

Linguistic Fieldwork: A Student Guide

DRAFT: 03-April-2006

Daniel L. Everett
Illinois State University
(formerly of
University of Manchester)

Preface

I have spent a large portion of my adult life in the Amazon, visiting over two dozen American Indian communities along the way. There have been times when I was so depressed, or so tired, or just so ill, that I wished I had chosen another area of linguistics, or even another profession (pizza delivery comes to mind). But only a few weeks after I return to my campus office, I am planning for the next field trip. Why? I don't like bugs, heat, humidity, cold, hunger, loneliness, or ridicule any better than *you* do. So my motivation is not masochism. I have two motives for doing fieldwork, people and science.

The greatest privileges of my life have been to get to know people like the Pirahã, the Kinsedji, the Banawá, the Wari, the Satere-Mawe, the Tzeltal, the Yarawara, and others I have visited in the course of my field research. These people's character, wisdom, humour and lack of pretension have challenged and enriched me deeply (and their demands and occasional impatience with me have helped me to 'grow' as a person). The many times I have had malaria, amoebic dysentery, infections, wounds, bruises, headaches, typhoid fever, even the half-dozen times my life has been threatened, have all been more than compensated for by the experience of getting to know these people. And from what I hear talking to field researchers around the world, this is a common conclusion.

The major reason I love fieldwork, though, is science. The excitement of discovery and analysis of facts that no other linguist has ever worked on before me is nearly addictive. As I review my curriculum vitae built up over these past couple of decades, each publication, each funded research project, each invited course and lecture on my fieldwork, recalls a fork in the road of my life, a place where I learned something new which altered my view of language in one way or another.

In this book, I hope to communicate some of this awe that fieldwork inspires. But I also plan to help the reader detour around the pits I have fallen into. My goal is that those who read this book will be better equipped for research than they would have been without it. Certainly, they will be better prepared by this book than I was when I first went to the field in 1977.

Suggestions on how to use this book are found in the introduction.

Acknowledgements

Many people have read portions of this manuscript and provided me with excellent comments, not all of which I used, but all of which I learned from. These include Peter Ladefoged, Bernard Comrie, Nigel Vincent, Jeanette Sakel, Martina Faller, Steve Sheldon, Alan Vogel, Brent Berlin, Johanna Nichols, Nikolaus Himmelmann, Bob Ladd, Keren Rice, Paul Postal, Geoffrey Pullum, Ljuba Veselinova, Cilene Camptela, Miguel Oliveira, Joan Baart, Carlota Rosa, Stuart McGill, Monica Macaulay, Sarah Grey Thomason, and Terrence Kaufman.

Two people in particular deserve special mention for without their support, this book would never have come to be. They are Prof. Steve Parker, Head of the School of Languages, Linguistics, and Culture at the University of Manchester, and Prof. Bernard Comrie, Director of the Department of Linguistics at the Max Planck Institute. Steve generously allowed me to take an early sabbatical leave, and Bernard generously offered me space and financial support to spend the bulk of the 2005-2006 academic year as a Visiting Scientist at the Max Planck Institute for Evolutionary Anthropology in Leipzig, Germany. The personnel at the Max Planck are incredibly helpful. In particular, Claudia Büchel and Julia Cissewski helped me make the transition to life in Leipzig and work at the MPI. I don't know what I would have done without their help. And Julia allowed me to join the MPI Choir, which gave me much-needed breaks from the strain of writing. My project secretary, Anabella Niculescu has improved the quality of my life by her initiative and competence in handling most administrative responsibilities of my research projects, allowing me more time and energy to write.

DEDICATION

This book is dedicated to the greatest linguist I have ever known, **Peter Ladefoged**. Perhaps no single linguist has ever done more for the documentation of the sounds of endangered languages than Peter. And no linguist was ever more caring, considerate, and respectful of their colleagues than Peter. He made many comments on this book as it was taking shape and I regret deeply that he could not see the final version.

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 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.

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 Maranhão. 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

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 Guaraní people, speakers of a Tupi-Guaraní 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 Guaraní 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

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 administrative 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 Guaraní 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 relinquishing 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 linguistics 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 dramatically? 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

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.'

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

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.

CHAPTER 2. PREFIELD PREPARATION AND NON-LINGUISTIC CONSIDERATIONS FOR FIELDWORK

2.1. Prefield preparation

You are going to the 'field' to study aspects of some language. You probably have a general idea of what you plan to study. But it is always useful to bring your research questions into ever sharper focus as your journey into the unknown takes shape. So, before you apply for a research grant, before you buy your ticket, ask yourself this: Am I clear on what I want to study? Can I explain it to others without being too wordy or abstruse? Can I explain it convincingly to the mirror? 'What is the exact object of my investigation?'

Part of getting an answer to these questions is to consider whether the object is to study something directly observable or something only inferable. An example of the former would be the measurements of formant frequencies of consonants and vowels across all speakers in a single village. An example of the latter would be, e.g. constraint rankings proposed to account for the morphological structure of the verb of language 'x'. This type of object clarification will affect your preparation, including your budget, your need for skills (e.g. in sound analysis software or constraint ranking evaluation, etc.), etc.

Another issue to consider in this regard is whether you are ultimately more interested in the explanation of similarities between languages or in the documentation of their differences. (Contrary to some opinions, the former is not necessarily a better goal than the latter!) Are you interested in corpus-based studies or speaker intuitions? Are you interested more in qualitative studies or quantitative studies, or a combination thereof? Is your principal objective connected in any way to ethnography of communication? And so forth.

A less obvious, but perhaps equally important, way to clarify your research objectives, once you have decided on your main question, is to carefully consider the ancillary questions implied by your 'big' research question(s). For example, if you read Cowart (), you may decide that your big theoretical research objective could benefit from some statistical analysis. How much time should you therefore give to the study of statistics? Or to learning about questionnaire preparation?

It is common for PhD students (in particular) to specialize, to deliberately focus on a narrower range of questions, to the exclusion of many other interesting, but not directly relevant issues. This is quite reasonable in most contexts. But it can be unreasonable and counter-productive in fieldwork, at least in an extreme form. A fieldworker not only needs to know more, because they will be faced with more information that requires knowledge to sort through, but they need more reflection because they cannot leave the field to get additional training if ancillary issues require it. This limitation has the corollary that very careful and detailed thought needs to go into the formulation of research questions to be asked in the field and that these questions and related issues should influence prefield training.

I suggest the following as a potential method of prefield research preparation. First, develop a list of the research questions you want to ask. The first versions of this list should be done hastily, just jotting down questions as they occur to you, things you *might* be interested in researching in the field. These questions should be formulated before, during, and after reading all you can about the field language and theoretical issues you expect to research. Second, narrow this list down to those questions that are most vital to your research and career objectives. Third, organize the questions (e.g. what are the main vs. ancillary questions? Which ancillary questions accompany which

main questions? And so forth). Fourth, *operationalize* each question – how can it be made 'behavioural', i.e. into something you need to *do* in order to investigate it? Never stray from the empirical core of your research at any stage of planning or execution. Refine and add to this list as you feel necessary. Next, build an initial plan. How might you ask and answer these questions in the field? (By the end of this book, I hope that operationalizing your questions will be easier.)

Let's move now to another prefield question of importance, namely, selecting a location for your field research. Here is a partial list of selection criteria you might find useful:

- (2.1) a. Language endangerment
b. Family history
c. Typological interest
d. Good things to eat
e. Geography
f. Funding agency priorities
g. Suggestion of advisor

For what follows, I suggest that each heading be thought of as a type of constraint. The individual field worker can then use them to guide her decision as to where to work by ranking them and trying to satisfy as many of them as possible, from most highly ranked down.

Language endangerment

If you are interested in the documentation and description of endangered languages, this will somewhat narrow down the range of language communities you have to choose from. You won't, for example, work in monolingual communities of tens of thousands of speakers undergoing no external pressure to switch languages or any obvious health or other external threat. Your concern will lead you most naturally to communities where there is a threat either to the physical survival of the people or economic or social pressure on them to switch to another language. To determine whether a given language is endangered, you will have to read on the socioeconomic conditions of the region, sociolinguistic relationships between languages in the area, speakers' attitudes towards themselves and their language, government policies on minority languages, likelihood of other linguistic studies of the language in question, and so on.

This is quite a worthy criterion. After all, if you study an endangered language, you could be contributing not 'merely' to linguistics but to history and the naturalistic record of *Homo sapiens*. Still, it is likely that the fieldworker will need more than these altruistic intellectual goals to see him or her through the long, lonely spells of frustration and ignorance that mark all initial periods of field research. So let's consider some other possible motivations.

Family history

Alex Haley's () *Roots* alerted and excited many people about the possibility of knowing about their family history. Haley's attention was initially caught by *linguistic* evidence of the similarity between words he heard from his grandmother and those of contemporary African languages overheard by him from fellow university students. If

he had been a linguist, he might have very legitimately chosen to conduct research on one of his identifiable ancestral languages from present-day Gambia.

The desire to research your family's linguistic history is quite a legitimate motivation for selecting a field language and location. This can further have the side benefit of creating family interest and support for your research and career choice, something lacking in most linguistic research projects, where family members, like the general public, often fail to appreciate linguistics.

Typological interest

If your goal is to advance our understanding of the linguistic possibilities of *Homo sapiens*, then a powerful motivation for field research could be the desire to advance typological knowledge, i.e. statistically valid clusterings of linguistic properties and their explanation. So, for example, you might go to Northern Brazil to study object-initial languages, which apparently only exist there. Or you might choose to work in Africa if you are interested in properties clustering in the phonologies of so-called 'click languages'. This selection criterion has the benefit of inserting your project from the outset into the current concerns of linguistic theory. And this is a very important advantage for the field researcher. As has been stated earlier, field research is a risky enterprise professionally. Your career options will be maximally enhanced if you can use your research results to challenge, refine, or advance current theorizing about grammar or methodology.

How then might one use typological considerations to select a field location? First, you should read widely in the typological literature. What are the main issues that stand out to you? Which ones are you most interested in? Discuss these with a typologist on your faculty or in another institution to make sure that you understand them and that they are indeed issues of current debate. Next, select an area of typology for concentrated learning. You need to master this area, reading, ideally, everything on the subject. Next, investigate regionally-focused journals and surveys (e.g. *International Journal of American Linguistics*, *Oceanic Linguistics*, *Journal of African Languages*, etc.). In this part of your preparation, you are looking for information discussed directly or simply mentioned and illustrated in some other context, that might bear on your research interests. What you would like to find, again ideally, is a region that you are attracted to and that seems to have languages in which the typological issue you wish to investigate is potentially quite relevant and widespread. Finally, and this advice goes for ALL field workers, *read grammars* from the linguistic/geographical area where you hope to work. Reading grammars is a vital component of the field researcher's healthy diet. Without knowledge of the intricacies of languages in the area, you enter that area under-prepared. This doesn't mean, of course, that you must agree with those grammars. But you must know them well. Form opinions, even become opinionated, but know the material and the analyses proposed by your predecessors in the area well.

Good things to eat

Some time ago, I attended a lecture by Professor Steven Anderson of Yale University where he suggested that an important criterion in selecting a field location would be 'where there are good things to eat'. I had never thought of that before. But it strikes me now as great advice. And it extends to more than gustatory attraction. Insofar as is possible, choose a place where there are things you enjoying seeing, eating, and doing in general. Feel free to choose a climate you enjoy. There is just no reason for

anyone to avoid their own comfort and pleasure in selecting a field location. It isn't always possible, of course, to satisfy these additional constraints, but it often may be and yet I suspect that many field linguists, if I am any measure, do not think about their own comfort, as though this were somehow 'wimping out' or anti-scientific.

In my own case, although I didn't use the 'pleasure criteria' in selecting my field location, I was fortunate nonetheless. Although the Pirahãs do not eat much I consider tasty, the surrounding Brazilian culture certainly does. The weather, scenery, rivers, beaches, music, people, and culture of Brazil have often inspired me to continue on with my work on Amazonian languages, even when I was linguistically discouraged, undergoing malaria treatment, or otherwise feeling tired of Amazonian field research. We all need such inspiration. This can come if we choose a place to work where, even if the research hits snags, we will still be able to enjoy where we are.

Funding agency priorities

Another important selection criterion for deciding where to work is the set of research priorities of the funding agencies most likely to support your research. Chapin (2004) is a useful and nearly comprehensive guide to research funding, based on Chapin's many years as Director of the Linguistics Program of the National Science Foundation of the USA. His book should be read by all younger scholars looking for funds to support their research. A vital part of field research is money. Know where to get it – and in large enough quantities to do your research well. Find out which funding agencies or programs within larger agencies fund research for the area of the world you propose to work in. Your university may have 'seed funds' to help newer scholars or students conduct pilot studies. Agencies like the NSF often have subprograms that set aside special funds for types of research, e.g. social sciences in specific regions of the world. In the United Kingdom funding agencies such as the Economics and Social Research Council and the Arts and Humanities Research Council have 'ring-fenced awards' that give special priority to specific types of research (e.g. studies in Modern European languages – great if you want to do field research on Basque or Northern Italian languages, for example). The success rate for most grant proposals hovers around 20-30%, depending on the size of the award, the research area, the funding agency, etc. A younger scholar will usually have to learn the hard way by competing for funds directly against senior scholars with plenty of fund-getting experience. So the newer scholar must invest time researching funding possibilities and agencies. Contact the relevant administrative staff at the agencies you are interested in. Most of them prefer to discuss potential proposals in advance, rather than merely getting them without prior discussion. Discussing the proposal in advance will usually make it more competitive (or help you discover that this agency is inappropriate for the project you have in mind).

A good programme officer at a major funding agency can be of inestimable benefit in helping the researcher prepare a successful research proposal. One of my earlier proposals to the National Science Foundation was rejected, though the comments from the reviewers were not so negative, mainly saying that my project was too ambitious. The Director of the Linguistics Program at the NSF in those days was Paul Chapin. He recommended that I bring the referee comments and my proposal to his office hours at the next Linguistic Society of America meeting. I did. He suggested that I keep the research objectives but triple the time and the budget (well, he did not suggest the latter, but that was entailed in the time suggestion). I did. I resubmitted. I got the

money. If it hadn't been for Chapin, I might have given up. He encouraged and helped me and thus is responsible for much of the research I have gone on to do over the past 25 years, almost all of it funded by the NSF, as I learned to become a more successful grant-writer.

Suggestion of advisor

For many linguistics graduate students, the choice of a language for fieldwork will be largely made for them by their advisor. However, even if the advisor is the driving force behind the selection of the language for study, the student should give some consideration to the factors mentioned here to better evaluate their chances of success before entering the project.

Self-Evaluation

In the movie, *Unforgiven*, Clint Eastwood reminds us that 'A man's got to know his limitations'. Fieldwork may sound like a great idea in the air-conditioned, bug-free library at Hometown University, but it may produce a different reaction when little children are pressing their hands against private body parts to determine your gender (or, as I sometimes suspect, your species), even as you are exhausted from a long trip and surprised by the smells and sounds of your new work environment. So be hard on yourself before you travel. A very important part of prefield preparation is self-evaluation: Do you have what it takes? Some of the components of the successful fieldworker include the following question, arguably the most important of all: do you have the talent and training for the job? And I suggest that the former outweighs the latter (as Boas himself emphasized to his students (Darnell ())). This talent and training will most clearly manifest themselves in the linguist's five senses (mental/physical data input devices) and her ability to interpret the results she gathers (mental data-processing). Do they think and read regularly about other languages? Do they have a well-developed ability to distinguish segments and prosodies? A talent for language-learning? An enjoyment of the exotic? A strong constitution? Ability to learn and teach with patience and clarity? Can they make friends easily and defuse tense situations? Can they tolerate lots of noise, successfully concentrate in a 'busy' environment; accept criticism for his or her government's politics, his or her skin colour (and many other things they can't change); tolerate lack of privacy and being laughed at every day? These are by far the most important toolkit the fieldlinguist will possess.

With regard to training, did the potential fieldworker's training include work with native speakers (ideally in their own environment)? Did they have teachers with field experience? Do they have cross-cultural experience of any kind?

There is a picture of me somewhere, where my back is covered with wasps and I am holding a microphone in one hand and a metal plate in the other, the latter in the futile attempt to ward the wasps away. I was stung about four times a day that summer. My right elbow was swollen to at least double size. My problem is that I sweat a lot in the tropics and the wasps apparently love the salt in my sweat (my theory). Anyway, I uttered many a foul word that summer. My sweat would trickle down my arm to my elbow, attracting wasps. Then I would rest my elbow on the table, without thinking, and that would anger the wasps and they would sting me.

Another time I was working just under a low thatched roof near the river's edge. River frogs would get into the thatch and croak. That is bad enough, because it affects the quality of your data recording. But what was worse was that snakes, some of them poisonous, would then come up into the thatch to eat the yummy frogs. This became so

common that I kept a foot-long, hardwood club by my feet. I would hear a rustling in the thatch. Then a frog would jump out. Then a snake would come slithering out behind it, often right above my desk. WHOMP! I killed many a reptile that field session. I even enjoyed it. In fact, if I could not figure out a particular construction, morpheme, etc. I looked forward to killing the next snake. WHOMP, indeed.

Now, if someone had told me these two stories before I went to the field I would probably have had two reactions. First, this is unlikely to happen to me. Second, I would freak out if it did. I was wrong on both accounts. But as I think about my background, growing up in a rural area of Southern California, spending a lot of time on my grandparents' farm, with chickens, cattle, pigs, and all the smells, sights, and accompanying creatures and bugs that go along with cow dung, chicken entrails, and pig blood (we ate these creatures, you see, and even while alive, they were unsanitary), I can see that this was all good preparation for field linguistics.

If you come from such a background, some aspects of field research will be less difficult for you. However, most academic researchers probably are not raised on farms. Most come from cities I would think. If you are from a city and intending to spend a serious period of time doing field research away from a major city, then you will face similar things. What you face in this regard will, of course, vary by climate.

Exercise is important, though not always because fieldwork is so physically demanding. There is no question, of course, that if a linguist moves from an urban university environment to a rural field environment, that daily routine will differ and will almost certainly require more lifting, walking, climbing, and general labor, as well as more direct exposure to the elements. Exercise will help prepare anyone for such a change. But exercise is valuable too because it can give the fieldworker a break from the mental strain of both prefield preparation and fieldwork itself. Doing linguistics in the field involves all of the work and pressure of doing linguistics in the city but adds to this the strain of responsibility, novelty, culture shock, and change in diet, comfort, and physical labor, etc. unique to the field. Trying to exercise or work in novel, less comfortable surroundings, altering one's diet for a while, etc. are useful training for the field, for relatively little personal cost.

2.2. Specific knowledge and skills preparation

In this section, I want to discuss the various general ways in which you can prepare yourself for a more successful field experience, where the latter includes scientific objectives as well as personal well-being, and contributions to the local community.

Preparation of research questions

Consider again the list of research questions suggested in the previous section. You need to learn as much as you can about each of those areas before you leave for the field. Some additional suggestions to prepare for linguistic research in the field are the following:

(2.2) Identify an advisor or mentor

You need an advisor or a mentor – a professor or a colleague whom you trust to ask you useful questions and offer useful suggestions. The exact role of this person will depend on your career stage. For graduate and undergraduate students, of course, an advisor is vital. You need someone who is willing to answer your formal, informal, and personal questions, including what we in the United Kingdom know as 'pastoral care' –

personal counseling along the lines clearly expected from Boas by Mead in the quote from her letter above. There may not be such a person at your home institution. It could well be the case that your mentor is across the hall. But it is equally possible that he or she is in another country, especially if your advisor and your mentor are different people (the former playing a formal, institutional role, the latter a more personal, intellectual role). With the internet and ever cheaper rates for international phone calls (especially with voice-over IP technology), there is no overwhelming reason why the latter couldn't be the case.

Ideally, you want someone with field experience, preferably in the geographical and linguistic area where you will be working. You want someone successful in getting data and publishing. You want someone who can put you in contact with local community of scholars where you'll be doing research and give you the perspective of the wider community of scholars who will be 'consumers' of your research. You should have someone who can advise you on the bureaucratic, mundane aspects of fieldwork (e.g. how to fill out forms in another language and culture, whether to buy a pressure cooker, how many kilos of books are worth taking, when the best time to be in a particular field location is, what the best local transport is, how to stay in touch with the 'outside world', etc. You also want someone who can read your work in progress and tell you how to get data, ask questions, and read more to make that work better. In fact, it is not inconceivable that you could benefit from multiple mentors. And these need not, again, correspond to an actual academic advisor if your research is for a thesis. But keep the number to less than three. Even with two, you will find opinions and advice will grow exponentially.

Prefield Literature Review

Read on the country and world region you'll be going to. Find out about its politics (especially its relations with and its populace's attitude towards both your home country and the minority group whose language you will be studying), history, geography, foods, language, culture, and laws, especially those governing foreign researchers (e.g. the constraints on research authorizations and visas).

Next, read all the linguistics you can find on the region you plan to visit. Even if the specific language you hope to work on is an isolate or part of an unstudied family, read all you can on languages of that region. Areal characteristics of languages are likely to be relevant and useful for you. As you read – THINK. How could this study have been improved? What kind of data is necessary to write a paper like this? How much time did the author spend with the people to collect the data upon which his or her studies are based (unfortunately, grammars and papers often omit this information or are imprecise in their description of it. For example, if an author says merely that 'I have worked on this language for twenty years, what does that mean? Is the twenty years in question eight hours a day in the field collecting data or three months in the field every two years, followed by writing and reflection)? What has been written already about the structure of the language you plan to study? How much has been written about the ways in which that structure is put to use or acquired? How much on the history of the language?

Take time to compile an exhaustive bibliography on the language, the family, and the area. Then do your best to read it all (this is relative – for some languages there will be very little, hence this assignment is easy, for others you won't be able to read it all or even list it all, but will need to be selective, exhaustive reading and compiling for some categories, very little for others). Your own research questions will have to guide

you in your efforts. Classify the works you have read. Know which are most useful. Often, you will want to take these with you to the field.

Three websites I recommend that supply tools for field research are the sites of the Max Planck Institute for Evolutionary Anthropology in Leipzig (<http://lingweb.eva.mpg.de/fieldtools/tools.htm>), The Electronic Metadata for Endangered Languages Data (EMELD, <http://emeld.org>), and that of the Max Planck Institute for Psycholinguistics in Nijmegen (<http://www.mpi.nl/tools/>).

Anthropology

Early in the history of North American linguistics, linguistic studies were seen as a branch of anthropology. Today, however, most linguists would likely not think of themselves as anthropologists, nor would most anthropologists identify even descriptive linguists as a subfield of anthropology. Nevertheless, because doing field linguistics is doing linguistics in a natural cultural setting, the field linguist cannot avoid culture. They can approach the cross-cultural linguistic experience ignorantly or informed – that is the only choice. Read a general text, e.g. Foley () or Duranti (), and then do follow-up reading on topics of personal interest via the references to these texts. Or, if you already have a background in anthropological linguistics, you can read in the major journals, eg. the **Journal of Linguistic Anthropology**, **Journal of Anthropological Linguistics**, and 'four-discipline' anthropology journals, e.g. **Current Anthropology**. I also recommend that all field linguists read Sapir (1921) and work by Lucy () and others on the neo-Whorfian approach to the language-culture interface.

I also recommend corresponding with anthropologists who have studied the people whose language you plan to study or with anthropologists who have worked nearby.

Other books looking at the connection between culture and grammar (language structure) should also be read and carefully considered (e.g. Enfield's 2003, *Ethnosyntax*). In chapter ____ below I give a series of examples of the interaction between language and culture. That chapter reinforces the importance of anthropological knowledge for the average field linguist.

Computers

Every researcher must use information technology in their research. It simply is no longer acceptable to go to the field without good technological support for collecting, recording, and analysing data. Residual Luddites that do attempt to do so, however, should at least recognize that they cannot document a language nearly as well without modern technological aids as they can with this technology.

Documenting a language involves creation of a multimedia record of the language in use by native speakers. Describing/analysing a language also benefits tremendously from and often requires technological support. Software and hardware for sound analysis, video-editing, transcription, and preparation of data for long-term storage is essential to field research.¹¹

¹¹ Several researchers have suggested that it is important to keep old versions of software for processing field data because there can be incompatibilities with newer versions and the old versions may be necessary, so long as one still has hardware that will run them, to access the data properly. But care should be given to data-storage and the software used for this purpose. Proprietary software such as Microsoft Word should be avoided and, instead, use of XML software should be the standard.

Time must therefore be invested to acquire at least an intermediate level of technological skills prior to departure for the field. You should familiarize yourself with your computer and its operating system and all relevant programs. Understand how and where application files and documents are stored. Design an effective (for you) filing system on your computer for your work. Treat it as a portable office. Make *sure* you have hardware to back up all your files and programs (fast transfer portable hard disks are very convenient for this).

Have calendars with alarms to keep you to your time schedule and goals.

Documentation

In the development of linguistic fieldwork, notions of *documentation* and *description* have perhaps not been as carefully distinguished as they ought to be. In earlier years of my career, they seemed to be used nearly interchangeably. To write a grammar of a language, for example, was to document a part of the language. Likewise a dictionary was a form of documentation. More recently, technological advances allow us to create interactive databases for long-term storage and usage of *primary* data on languages, i.e. audio and video files. Such data bases refine our concept of documentation (though of course in the selection of data for such data bases the researcher intrudes and obscures). In my opinion, 'primary documentation' is the recording of audio and visual data. Secondary, and perhaps tertiary, documentation may be thought of as data in increasingly interpretative matrices (e.g. grammars, theoretical articles, and so on). The more interpretative the documents produced, the farther removed from primary documentation in the view advanced here. Again, in times past, description doubled for documentation as primary sources were not made available to general linguists and all that we have/had on many languages were data as selected and interpreted by linguists, explorers, anthropologists, missionaries, and others.

So now let us consider documentation a bit more (though audio documentation is discussed in chapter __, section __ on phonetics).

If you want to share the pleasure of your birthday party with your friends, you could simply tell them about the party. Or you could show them photos and videos of the party. Your friends may not want to see all your photos, but at least they can judge for themselves whether Sally's dress was divine or the cake was lovely, etc. Still photos can isolate moments. It is very important, therefore, that a fieldworker be familiar with photography or at least that someone on the team have such knowledge. The quality of your equipment (see __ below as well for general considerations on equipment) will depend on the knowledge available to you to use it, your budget, your goals, etc. But at least a five pixel digital camera with an array of focus options should be part of your toolkit. Read an introductory text to Visual Anthropology, e.g. those listed at <http://www.visualanthropology.net/>. This is not absolutely crucial for field research in linguistics but it can be very important for documenting certain kinds of claims on meaning where facial expressions, gestures, and other visual cues can be crucial to understanding the pragmatics of the utterances in question.

Medical/first-aid training

Fieldworkers should have basic training in first-aid, treatment of diseases common to their chosen area of fieldwork. Ideally, if they are going to be working in extremely isolated situations, they consider some training in suturing and bone-setting.

They should have access to some basic bibliographic sources (e.g. **Where there is no doctor** and **Where there is no dentist**) and have emergency numbers to call locally and internationally (by satellite) for consultant help if necessary.

One afternoon among the Pirahãs, I was pursuing my never-ending quest to understand the structure of the Pirahã verb. Suddenly, I heard yelling at the river. When I looked, the Pirahãs were running towards the river, talking loudly. Someone came to tell me that a man from the village, /**abagi** 'Toucan', was hurt. Sure enough. As they brought him up into our house, his left arm was beet red, amazingly swollen, and oozing pus. He had had an accident in his canoe and an arrow had entered his forearm just above the wrist and emerged on the opposite side, below the elbow. He had a fever and was in obvious pain, something the Pirahãs only admit to in extreme circumstances. He freely admitted that the pain was nearly unbearable.

What was I supposed to do about this? It was clear that every Pirahã there expected me, the outsider, to have some western medicine and to know what to do. So I did what any courageous, knowledgeable, and resourceful field researcher might do – I called my wife, Keren.

Keren was able to treat Toucan and he fully recovered. How? Well, before we ever set foot in the Pirahã village we both took courses in first-aid. We also asked various people – doctors, nurses, missionaries, and others – what kinds of health problems we were most likely to encounter. We then purchased medicines accordingly. In Brazil, as in many countries, a much wider and more potent range of medicines can be purchased over the counter than in the USA. So we purchased several hundreds of dollars worth (this in 1978) malarial medicines, analgesics, snake anti-venom (antiophidic serum), local anaesthetics, syringes, sutures, and so forth. During our first couple of days among the Pirahãs, we organized our medical equipment and supplies on shelves, with our most useful medical manual, **Where there is no doctor**, by David Werner, in the front.¹²

There are many field locations where the fieldworkers would not be expected to provide health care. There are many places where unlicensed people dispensing medicines would be in violation of local laws. But in many isolated communities, a linguist or anthropologist may be the only hope for health care. Certainly the linguist may need to care for his or her own health, or their partner's or children's, depending on where they are. Therefore, training, reading, equipment, and medicines are all crucial components of any fieldworker's kit.

Survey

Finally, if there is no extant sociolinguistic survey of dialects, language attitudes, demographics, geographical distribution of the language, etc., this should be undertaken at some point during the first field research trip. There are books that offer crash courses in this kind of survey work. One such is Blair (1991).

2.3. NON-LINGUISTIC CONSIDERATIONS FOR TIME IN THE FIELD

2.3.1. *Paperwork and bureaucracy*

¹² All fieldworkers should have a copy of this book. It can be ordered via the internet: <http://www.hesperian.org> This website also contains a wealth of information and material on health care in rural environments, etc.

Going to 'the field' means crossing political boundaries. And this almost always entails getting two broad types of authorization: authorization to enter the country and authorization to do research. These permissions usually require the fieldworker to apply for special visas, to get medical exams, criminal checks, official translations of diplomas, etc. It will take time. And *nobody* enjoys the process. This book would be less honest or helpful than it should be if it ignored this unpleasant aspect of fieldwork, so I will try to highlight some of the bureaucratic processes involved and suggest ways to help the process run more smoothly.

Most countries will not allow fieldwork on a tourist visa. And in some countries tourist visas restrict the tourist so that they cannot visit minority communities outside of major cities. I know some linguists who have done fieldwork on a three-six month tourist visa, some even multiple times. Many an important linguistic study has been done without proper authorization. But not only is this failure to secure the proper visa unethical and illegal; if discovered it could bar the linguist from current or future funding for the research (most funding agencies require evidence, in advance, that the researcher has secured or will secure the proper legal documents for his or her research from the local government).

So you will need to get a visa. But you are likely also to need permission from a government department, ministry, etc. responsible for minority affairs. And your scientific project is likely to need authorization from the national research or science foundation. The latter may require that you have a national partner, i.e. a linguist or other appropriate specialist who is personally supportive of your research and is willing to be your academic sponsor. Often these various permissions seem to produce 'ordering paradoxes', 'infinite regresses', or 'Catch 22' situations, e.g. one source tells you that need government authorization before you can get scientific authorization, while another source tells you that you need scientific authorization before government authorization. I have seen foreign researchers spend a year or more getting authorizations in some countries.

In most countries these processes are all made easier and faster if you know the people responsible for the authorizations. It is almost never possible for you to simply handle this all with a phone call or over the internet. So how do you get to know the right people? You will need money, time, and a willingness to bite your tongue and keep silent at times.

First, where bureaucracy is especially labyrinthine, the prospective fieldworker should attempt to find 'seed money' for a trip, on a tourist visa, to the country where they would like to work. Before leaving, they should find out who the local linguists doing field research are. They are quite likely to be the people who will be asked, eventually, to evaluate the research proposal proper. If the linguist is lucky, they might be able to establish initial contact with someone who could help, a graduate student or a professor from the target country would could tell him/her who they need to see, where to go, etc. This person might even be able to make the initial contacts for the fieldworker, provide him/her with a letter of introduction, etc.

It would be useful if the fieldworker could speak the national language before venturing to the target country, but this is not necessary. Academic contacts will speak English and if all goes well they may be willing to help, advise, and even speak to the authorities on behalf of the fieldworker. There is, to be sure, occasionally some distrust of Western Europeans or North Americans, but by and large the international scientific community is interested in promoting high-quality research and the fieldworker is likely to be received warmly and make life-long good friends. So, although these initial

contacts can be tense and humbling, they often end up pleasant and rewarding, both professionally and personally. One should look at these initial steps as part of the fieldwork. They are.

On this first visit, enjoy the food, the nightlife, and the people of the country. Make the most of the experience. Life is short and you only go around once (so far as any one can prove). So make the most of it and have a good time. That would be neither unscientific nor unprofessional.

Expect the entire process of authorizations, once again, to take from 3-18 months and expect to travel at least once at the beginning of the process (and perhaps again after the process is well underway) to the target country to get help in speeding the process along. Plan ahead! Also, keep careful written records of your contacts, their expressions of support, and their willingness to help, etc. Get letters from them whenever possible. Funding agencies will want to know that you are aware of the need for authorization, etc. and that you have it well in hand.

Now I want to consider some additional nonlinguistic factors for time in the field.

2.3.2. *Entertainment*

When I took my first anthropology class, at Grossmont Junior College in El Cajon, California, in 1970, the teacher (who influenced me tremendously, but whose name I cannot remember), mentioned that what you read in the isolation of the field will affect you more than it would in your home community. That is, it will reach your emotions and mind more deeply. I have found that to be correct in my experience. General suggestions I have found useful in my own field experience include the following:

(2.3) Have a time for yourself: you need a time away from the language teachers, away from as much of the hustle and bustle of the language community as possible (if it is appropriate – it will not always be). Some time where you can simply reflect, relax, eat one of the chocolate bars you brought from the city, write letters to friends, etc. Time to just 'chill'.

(2.4) Read: take books to read in the evening or whenever the best time turns out to be in your field situation. I take a mix of novels, history, philosophy, and biographies. I find it necessary for myself to mix light and heavy reading. I can't take too much of either without a break.

(2.5) Movies: Taking movies to the field is easy these days. Take DVDs that you are willing to watch with members of the community. It will be possible at times to watch DVDs alone, with no one around, but assume that someone will want to watch DVDs with you. Choose programming that will entertain and relax you, but not offend the community's sensitivities. Also, it is important to remember that movies and other programs can be great educational tools for the community. So take a selection of DVDs just for showing the community. The Pirahãs, for example, tremendously enjoy material about other indigenous communities and their daily lives, as well as about animals of all kinds. National Geographic movies are very useful. On the other hand, the most popular movie among the Pirahãs is the old John Wayne movie, *Hatari*, about capturing wild animals in Africa.

(2.6) If your community is accessible to mobile phones, I am sure that you will take one. If the community is not accessible to mobile phones, then I suggest that you save up to purchase a satellite phone. There are several options on the market and most of them, though quite expensive with high per minute charges, can allow you to maintain

contact with friends, colleagues, and others in the most remote locations. Some people find this very useful. For example, with a satellite phone you can call for linguistic help, phoning your home institution, etc. to get expert advice on how to analyse or collect data on a particular subject (not to mention emergency help). Some satellite phones (e.g. the Nera World Phone that connects to Inmarsat) also can enable you to send and receive email, which can also be useful for getting advice or sending examples of constructions you have collected, along with your ideas on them, to another member of the research team, or a colleague or mentor, to ask for help, advice, etc. On the other hand, satellite phone communication is expensive so will not be a way to chat freely with your friends on the weekends.

2.3.3. *Journaling*

For the past three decades, I have, off and on at least, kept diaries, some of which have evolved into journals. I have to admit that my journals have by and large had no 'theory of journaling' behind them. They are unsystematic records of my emotions and activities for the most part. But they could be more useful to me in my research in my ability to perform well in fieldwork. I want to offer some advice that I wish someone had given me on how to keep a journal.

Journals

The first thing to get clear on is that a journal is not a diary. A journal is not simply a chronological record of what you have done during the day, though it might include that. So what is a journal for? What are the advantages of keeping one? Here is a partial list of ideas:

(2.7) Reasons to keep a journal

a. To maintain a personal record of your time in the community, e.g. what has been hard for you, what has provided new insights on language, life, culture, etc. what you are thinking about on a given day, thoughts about family and friends from your field site vantage point, etc.

b. To develop a record of your scientific findings, your incipient ideas about analysis, suggestions to yourself about what to collect the next day or week, ideas that did not work and ways that they might be retested, etc.

c. To have someone to talk to. The field gets very lonely. There are times when no one around speaks your native language. You need to talk to someone in your native language. Talk to your journal. I find that this is very useful. I can rant to my heart's content about something and get it out of my system. I do not write these parts of my journal for any present or future audience except myself, and would never want anyone else to see some of my 'rants'. But it is good therapy for me nonetheless and I highly recommend it. In fact, days or weeks later you may return to read your 'rant' and learn how wrong you were about the matter that so exercised you at the time, building up a valuable store of lessons on your growing understanding of the culture and yourself. Also record what pleases you about the community, the location, the research. Record positive observations too!

d. To have a place to focus and organize. Journals can provide you a space for focusing your thoughts and organizing your thoughts and future course of action in research, relationships, etc. A journal can be where you set and revise your goals, redefine success (as the saying goes 'If at first you don't succeed, redefine success'), and make notes of new things to read, new methods to try, etc.

e. To track your intellectual and linguistic development. Writing down what you think you know and what your attitudes are towards the work, the community, the general aspects of field research can, as you read your own entries over the months of fieldwork, provide an excellent source of data for you to study yourself and your own development as a fieldworker. This can be useful for setting further goals, but it can also be useful as an encouragement that you are not as bad as you thought you were at this job (or alternatively, this can be a source of humility if you had too high an opinion of yourself).

f. To record memories for post-field write ups of your experiences, your developing analyses, etc. Journals also and naturally provide a historical record of the linguist's time, but also of the community. If you are careful to record observations daily about the people (see below) then your journal, as a document that scientists (including you) might look at years hence can provide a record of cultural and historical change of the community under study and of the role and relationship of the fieldworker in these changes.

g. To develop your writing style and ability. Journals provide an excellent opportunity to become a better writer. Let yourself go. Be flowery. Try new ways of writing. Develop your writing style so that you can express your observations in such a way that your own personality comes through. This makes for better field writing.

Approaches to journal keeping

One approach, which I learned from Peter Ladefoged, is where the journal takes the form of a scrapbook, with photos, small bits of memorabilia (such as a feather, a flower, a fish hook, etc. – small things that are evidence of important or memorable events in the experience), as well as annotations. Keeping pictures of your language teachers can be very useful and provide a personal link with your field research for years to come.

Another approach is less interesting, perhaps, but it is my own, perhaps lazy, approach. I simply write in my journal, nothing else. My journals are simply written records that include the information suggested above and below.

Things to include in a journal

Here are some suggestions for what to include in a journal that I believe are useful for a scientific journal. (i) the date and time of the entry; (ii) the location where the entry is being recorded; (iii) the weather at the time the entry is recorded; (iv) your mental and physical health (i.e. how are you feeling?); (v) the village activities going on at the time of the journal entry and others planned for that day, i.e. before the next journal entry. An analysis of observed activities should also be included; (vi) track an individual, a family, or other cohesive group during the day and record their activities (gists of conversations, who did what during the day, where they went, how far it was from the community center, etc.); (vii) village health – is anyone ill as you record your entry? What is the illness? Is it contagious? etc; (viii) your ideas on the language, culture, nature, etc. (Theoretical, descriptive, musings, etc.)

Additional suggestions journal writing

Write freely. Allow yourself opportunities to release a 'stream of consciousness', without worrying about form or even much about content.

Describe scenes and events that catch your attention, rich in details. These details can help you write up your field experience later. Clifford Geertz () talks about 'thick' versus 'thin' descriptions. By this he means that:

“Cultural analysis is intrinsically incomplete. And, worse than that, the more deeply it goes the less complete it is... There are a number of ways of escaping this—turning culture into folklore and collecting it, turning it into traits and counting it, turning it into institutions and classifying it, turning it into structures and toying with it. But they are escapes. The fact is that to commit oneself to a semiotic concept of culture and an interpretive approach to the study of it is to commit oneself to a view of ethnographic assertion as... ‘essentially contestable.’ Anthropology, or at least interpretive anthropology, is a science whose progress is marked less by a perfection of the consensus than by a refinement of debate. What gets better is the precision with which we vex each other.” (29)¹³

In Geertz’s understanding, ethnography is by definition “thick description”—“an elaborate venture in.” Using the action of “winking,” Geertz examines how—in order to distinguish the winking from a social gesture, a twitch, etc.)—we must move beyond the action to both the particular social understanding of the “winking” as a gesture, the mens reaction, the state of mind of the winkers, their audience, and how they construe the meaning of the winking action itself. “Thin description” is the winking. “Thick” is the meaning behind it and its symbolic import in society or between communicators. The fieldworker should attempt to develop this kind of descriptive style in their journal. Among the cultural values that should be included are: community recipes, aphorisms, folk wisdom, myths, etc. It is also useful to provide pictures and samples of plants, insects, etc. whether you are interested in studying ethnobiology, or simply generally interested in these topics.

The form of the journal

First, you will need material to write with. You will need to decide whether you want to keep your journal on your computer or in hard copy. If the latter, are you going to keep your journal in a notebook or a hardbound book? I suggest that you use indelible, water-proof and acid-free ink and paper for your journal, and that the pen-paper combination you use doesn't result in the ink being absorbed in the paper over the years in such a way as to obscure what you have written. Let me close this section by a consideration of an alternative type of journal, the blog.

Blogs (Weblog)

Blogs are different from journals primarily in that they are written for an potentially large audience and located on a public or semi-public space on the internet. They can be useful for that very reason, by letting others see, in real time (as you post to the internet, perhaps from a satellite phone), what you are going through. They can enlist help and advice, they can later serve as journal records, and are otherwise every bit as useful as a journal. Therefore, what I have to say about journals also applies to blogs. In fact, there is no reason why a journal could not also serve as a source of blogs, leaving the private kinds of observations out.

¹³ This comes from: <http://academic.csuohio.edu/as227/spring2003/geertz.htm>

2.3.4. EQUIPMENT AND SUPPLIES

2.3.4.1. General issues

Any discussion of equipment or technology will date a study quickly. So I do not intend to spend much time on this. However, there are a couple of things to say in this regard that will be somewhat impervious to time.

First, technology is vital in field research. Even though I believe that I have very good 'ears', in my experience machines have been invaluable in helping me to notice sounds and patterns which my unaided ears had missed. And technology provides a record for the future, however outdated it eventually becomes. Consider, for example, the significance of the portable cassette tape-recorder for the history of field research. It is true, trivially, that early fieldworkers got by without this, now outdated, device, just as everyone gets by without inventions yet to come. But wouldn't it now be priceless to listen to audio tapes or watch video tapes made by Sapir, Boas, Newman, and others, checking their facts and interpretations more carefully, or possessing a more complete record of the languages they studied? As we recognize the need to study, for example, endangered languages, technology capable of accurately preserving and measuring the sights and sounds of these languages becomes ever more important.

Some questions to ask with regard to field-equipment are:

- (i) Who will be able to use the output of your equipment now and in the future?
- (ii) Is the equipment portable?
- (iii) Does the equipment provide state-of-the-art accuracy, or as close to it as the fieldworker can afford?
- (iv) Will the equipment help record both the grammar and its cultural matrix?
- (v) Does the equipment use a practical power source for the location in which it will be used (such as solar power)?
- (vi) Does the fieldworker's equipment include satellite capability, for email and phone contact from the field site to any part of the world?
- (vii) Do you have backup equipment for crucial items, e.g. extra microphones, computer(s), recorders, etc.?

Point (iv) may seem strange, but it can be taken as a reason for using, in today's terms, high-quality camcorders in the field, rather than relying exclusively on audio recordings. It is also a reason to use portable computers in the field which have state-of-the-art video and audio editing capabilities (e.g. the Mac G4 laptop in 2003). In purchasing and planning, remember that quality is not something to be overly economical with – pay top prices if necessary to get top equipment. There are other areas to be frugal in, if that is necessary (and of course it always is).

2.3.4.2. Portable power systems

There are a number of small, relatively inexpensive (but fossil-fuel consuming) generators available in most countries. These are extremely useful and convenient tools. In fact, I have a lightweight generator (approximately 20 pounds) that will supply all my energy needs in the field (lights, battery charging, etc. – it will even power a refrigerator). But generators have severe disadvantages, e.g. (i) they are environmentally harsh (use of fossil fuels); (ii) they require significant weight for fuel; (iii) they require maintenance and careful storage and they break down. Therefore, I suggest that the field linguist use a small solar-powered system when practical.

A solar power system would not have to be large if used only for charging a camcorder, computer, and audio recorder, and for small light bulbs. I recommend two 32-watt solar panels (roll-up rather than rigid, for easier transport). These are then

connected by cables to a 12 volt, deep-cycle battery (e.g. the kind used on many small boats in the US. Deep-cycle batteries charge better and have a wider voltage tolerance). This type of battery can be quite heavy, however, and needs to be replaced every one-two years. Smaller batteries (e.g. a motorcycle battery) can be used, but these are unable to provide as much power. You will then need a voltage converter to change the power coming out of the battery to 110 (or 220) from 12 volt. A voltage regulator would be useful because it can keep your batteries from overcharging or running while undercharged. It will shut off the relevant input or output to protect the battery. If you choose not to purchase and connect a voltage regulator, you should get a voltage meter to check the power in the battery. It should never go about 13-14 volts and never below 11 or you will be without it soon.

Finally, wherever possible, native speakers should be trained in the use of the equipment. This is important training for them and can be very helpful for the linguist, even avoiding the need for the linguist to return to the community for small samples of data that can now be collected by the native speakers.

2.3.4.4. Consumable study supplies

You will also need to take pens, papers, and other office supplies, even though you should rely more on the computer than on paper. Consumable study supplies I take to the field include:

(2.8) Consumables that might be useful:

- a. Flashlight batteries and AA-size batteries. I only take alkaline batteries. You can buy fewer of these than would be necessary for non-alkaline batteries since they last so much longer. Therefore, they require less space.
- b. Blank DVDs are important for backing up data or making copies of lab sessions, songs, etc.
- c. Indelible, acid-free, waterproof ink pens. Buy enough for one a week, minimum.
- d. Other pens: take various colors of pens for color-coding data and a range of other uses.
- e. At least 500 sheets of paper for a six-week stay, with side holes for including in notebooks (or take a paper-punch).
- f. Separate notebooks (appropriate for the paper just mentioned). You will need one notebook for every major division you want to make (e.g. 'Verbs', 'Nouns', 'Transitive clauses', 'Subordinate clauses', and so on).
- g. Paper clips, staples, stapler, clear adhesive tape, and plastic bags, e.g. rubbish bags of different sizes, sandwich bags, etc. The plastic bags will be vital, especially in rainy and humid areas, in preserving your notes and many other supplies (e.g. matches) from the effects of humidity and leaky roofs.
- h. Rice for dessicant and tupperware.
- i. Back-up computer battery: computer batteries do fail. I always take an extra computer battery to the field.
- j. Cottonettes and rubbing alcohol – these are useful for cleaning playing heads (on older model cassette recorders. But if possible avoid these and purchase a solid-state recorder). These are useful for personal hygiene as well, to help cut down on orifice fungus. (I recommend that you put alcohol mixed with vinegar (1:3 ratio) in your ears after every swim in the river, or hydrogen peroxide) .
- k. Recording tapes or memory cards (for solid state recorders).

1. Plastic binders/covers for loose papers. Papers which are not yet in particular notebooks should also be carefully stored and plastic binders or some other way to render your notes water-resistant are extremely useful.

2.3.5. Tools

Take a multiuse knife or two. I use both a Leatherman and a Swiss Army knife (since the two types have different tools). A hammer is also useful. The tools you choose will depend, obviously, on your particular field situation. If you use a boat motor, you will need to know how to repair it, at least basic maintenance and simple repairs, and you will need tools for that. I also recommend that if you use a boat, that you have replacement parts, especially ignition modules, waterpump, fuelpump, and spark plugs.

No fieldworker should travel without two essential aids: duct tape and durepox. The former can fix most broken things. Durepox (which comes in two clay-like sections, to be mixed together) forms a chemical bond which is strong enough to repair holes in boats and cars. It is very valuable in the field. A flashlight, e.g. one that can be attached to your head for hands-free lighting when trying to read or repair something is also useful. The list can always be added to, of course, but each item is more to carry, more to lose, more to potentially come between you and the community.

2.3.6. Care of equipment

The field is usually rough on equipment. Whatever you take with you is likely to be exposed to some combination of the following: high humidity, sand and dust, bugs, rain, and temperature extremes. As you travel and pack and repack, heavy bags and other objects are likely to be put on top of delicate equipment, your equipment will be dropped, perhaps even in a river, and will otherwise be treated very differently than it might, say, at your home institution.

There are special carrying cases with adjustable foam linings and quality rubber seals for laptops and other sensitive electronic equipment. These cases, when maximally useful, are of metal or hardened plastic (yet relatively lightweight), waterproof, and able to hold equipment tightly in place and secure, without rattling around inside. There must be room in the equipment case for a dessicant of some sort if you will be in a wet or humid environment. One popular dessicant is silicon gel. Cheaper alternatives are available, as discussed in ____.

We now turn to consider the last, but certainly not least, issue in non-linguistic fieldwork issues, having your partner and/or family accompany you to the field.

2.4. FAMILY IN THE FIELD¹⁴

2.4.1. Your partner

If you have a partner who enjoys the field experience, you are most fortunate. My former partner was raised in the Amazon by missionary parents. Living among Amazonian peoples turned out to be the most enjoyable activity in the world for her. She can tolerate the bugs, heat, and other hardships much better than most people can. And she is extremely organized, (which I am not), which helped tremendously in many

¹⁴ In 2005 I was interviewed on the BBC 4's Excess Baggage programme about raising children in the Amazon. This is available at:

http://www.bbc.co.uk/radio4/excessbaggage/index_20050917.shtml

ways, from shopping and packing, to organizing each day in the village. Her presence always enhanced my own ability to work and thrive in the field. When both you and your partner enjoy the field, this is a tremendous personal and professional help.¹⁵

I only went to tribal areas a couple of times without my family. In each case my productivity fell by more than half and I swore I would never do it again. People are very different. Whereas some linguists might relish the solitude of individual fieldwork, I have never found it easy to be alone in the field, though when my family has gone with me, i.e. most of the time, it was usually an enjoyable experience.

Even with the family it can be hard. My first visit to the Pirahãs, my oldest daughter and my wife got very severely infected with falciparum malaria. There were rarely any field visits where one of the family didn't get very ill.¹⁶

The first time that my family went with me to visit the Pirahã, 1979, was only the second time I had ever gone to the Pirahã area. Things were apparently going very well. One day I was lying in my hammock, swaying in the breeze off the river and memorizing Pirahã words. I remarked to my wife that 'this life is really rough', with lots of sarcasm. But I spoke too soon. It *can* be rough. Within 24 hours she and my oldest daughter, Shannon (8 years old at the time) had fevers of 104 degrees and severe headaches and backaches. I had taken introductory health classes during my SIL training, and I had several medical resource books. Since neither Keren nor Shannon were suffering from chills, I assumed that they could not be suffering from malaria. Also, I didn't think (why I have no idea) that malaria would begin in two people simultaneously, like any other infectious disease. (They both had falciparum malaria, the most virulent South American form.) So since their symptoms matched something I had suffered from on fieldwork in Mexico, typhoid fever, I began antibiotic treatments. They got worse by the day. I had no radio to call for help or advice because these were then prohibited for foreigners in tribal areas, and this was before the invention of satellite phones. A plane was due to arrive in a week, but Keren had now drifted into a coma, after being delirious for about 36 hours. Shannon was drifting in and out of consciousness. Neither one had eaten or had more than a liter to drink in four days. I had to do something or they might both die. And I was exhausted, taking care of them and watching my 5 year old daughter and 2 year old son. The Pirahãs seemed to me uncaring and cold during this time, though later I came to realize that this was part of their stoic philosophy of life, not a reflection of how much they cared for someone, nor a lack of concern. But I only had an old 9 horsepower motor, with very little gasoline, and no knowledge of the way out to the nearest doctor. I had done no geographical research, just assuming I would fly in and out of the village, as though I were going to a US regional airport. I am not sure what I would have done, but a Catholic lay missionary, an Italian man named Vincenzo, now deceased, came up to visit the Pirahãs

¹⁵ However, it is important, especially if you are both linguists, that you are not overly competitive with one another and that you are not defensive about your language-learning, linguistics, ability to cope with the environment, etc.

¹⁶ However, this is not standard for all fieldworkers! Desmond Derbyshire, a pioneering Amazonianist, told me that he had an excellent language teacher within his first thirty minutes among the Hixkaryana, who worked with him regularly for the next couple of decades, and that he could never recall having been sick in the village.

on behalf of a priest he knew. Seeing our plight he told me to take his boat, a small aluminum canoe, barely big enough for my family of five, with a 6.5 HP outboard motor. He told me how to get out. However, because he gave me all the place names in Italian and because I didn't yet speak Portuguese well, no one I encountered farther down the river knew what I was asking about. We left the village with Keren and Shannon lying down in the middle of the canoe and Kris and Caleb sitting near the back with me. The water was nearly up to the edge of the canoe. We were moving about 8 miles an hour and I had only 20 litres of gasoline, with no idea, in spite of Vincenzo's help, of how long it would take us to get to the next river, the huge Madeira, where boats could be hailed to take us to the nearest town. After several false stops and turns, almost out of gasoline, we arrived at a small settlement and several men hung hammocks on poles and carried Shannon and Keren through a jungle path to the Madeira river, about a one-hour hike. I followed along with supplies, carrying Caleb and Kris. A local family, Godofredo and Cesaria Monteiro, poor as all the others, took us in and fed us (I had taken no money – we were a complete liability for that family). After waiting for three days, with Keren ever nearer death, a boat came. Men from the community carried Keren and Shannon down the steep banks of the Madeira, at 3AM, to catch the big boat to the nearest town. Loaded on the boat, I was impatient to get to the hospital. The next day, I offered to pay the boat owner any price he wanted to take us in the motorboat he was towing to the nearest town, which would have saved us some 12 hours. He said 'Comrade, if it is your wife's time to die, she will die. Rushing will not save her.' Then he stopped the boat and his crew played the men of a local settlement a two-hour game of soccer, with me beside myself with rage and fear. Keren and Shannon were both losing weight and had terrible cases of diarrhea. My two year old and five year old were running about the boat and I kept trying to keep them still. Finally we reached the town of Porto Velho. The doctor examined the two and said that Shannon might live but that Keren would die that night and that I should call her family, which I did. She weighed 76 pounds and had lost a lot of blood (malaria causes you to lose blood through urine). But she lived. And we have been back many times. That was more excitement than I had bargained for. But it is, for better or worse, part of what fieldwork can be like if you are unprepared. Don't be. I have had malaria many times since then, as all of the family, except our son, Caleb, have. But being prepared has meant that none of the subsequent cases were all that serious (except for the time that I came down with malaria after just arriving from Brazil, while on a trip to Disneyland with my family. I came close to dying. But that is another story).

2.4.2. Children and fieldwork

Your children's mental, physical, and social health will be of utmost concern to you in the field. There are some dangers and things to worry about, but overall, the field experience can contribute to raising aware, resourceful, and independent children, respectful of other cultures and races. I highly recommend taking your family to the field, in spite of occasional danger and adversity.

In 1977 my family went to Chiapas, Mexico for 'jungle training' with the then Summer Institute of Linguistics. During our first few weeks in Chiapas, my six-week old son, Caleb, came down with meningitis. We gave him six adult doses of antibiotics per day for days (I forget the exact treatment). He was very, very ill. And his first set of teeth were all weakened and bad because of the antibiotics. And, as related above, in Brazil, my daughter Shannon nearly died with malaria. Another daughter, Kristene, came down with hepatitis. Health concerns should not be taken lightly. On the other hand,

they should not be overly dramatized. Kids are tough. And cuts, scrapes, sickness, and environments demanding adaptation are good for them. But it is important that their parents react well to and enjoy the same environments. If your children sense fear or guilt on your part for taking them, they will respond with fear and a sense that your guilt is well-deserved. I noticed early on that whenever something unusual or potentially unpleasant occurred, my children would immediately look at my wife or me to see our facial expression. If we showed fear, then they did too. If we seemed unperturbed, then so did they. You must be well-adjusted to the environment, secure, and confident in your ability to cope with most things that come your way before you take your children to the field. But if those conditions are met, they should thrive.¹⁷

Act like they are fortunate to have these experiences and you all will have a great time too! That is the secret.

Part of well-being for your children is to ensure that they have an adequate social life. This cannot be based around you and/or your partner. They need people their age. Therefore it is essential that they make friends in the community under study.

My daughters, Shannon and Kristene, went with me to live among the Tzeltal people when they were 6 and 3, respectively. They went with me to the Pirahãs when they were just a year older. I remember among the Tzeltals, waking up about 630AM on most days. That was late for the Tzeltals. Our 'door' was a set of poles placed upright in the doorway. Most mornings, a few poles were moved out of the way because, Kris, three years old, had left the house. I would find her in front of a Tzeltal's home, a huge cup of sweet coffee in one hand and a fistful of corn tortillas in the other hand. Shannon, then 6, would often spend the day visiting different Tzeltal villages with Tzeltal girls. Sometimes the girls would dress Caleb up in Tzeltal baby clothes (he was about 2 months old) and take him around with them to other villages (though not far because he was still nursing). Among the Pirahãs, Shannon and Kris would often disappear in a canoe with Pirahã girls in the morning and return in the afternoon, after a day of fishing, picking berries, and giggling. They seemed to enjoy themselves and tell me that they have good memories (they are now both in their 30s). On the other hand, the Pirahãs (like many peoples in the world) are sexually active very early and the entire culture sees sex at most ages as a natural and innocuous pastime. So my daughters were receiving sexual advances early on. But this need not be any big deal so long as all are prepared and they know that they can say no. This could be a problem in some societies, but the Pirahãs never expected us to go along with everything they did, because we were obviously different from them.

Children also need physical exercise and conditioning. In the normal course of events, fieldwork toughens them up physically, emotionally, and mentally. Let them do the normal village chores (gathering firewood, hauling water, collecting fruits, etc.) and they will almost certainly grow up fit. And they will be far from fastfoods and other 'junk' food. They will often mainly eat fish, wild fruits, and drink water. Not a bad diet for them, as a matter of fact.

¹⁷ I remember my first LaMaze (childbirth preparation) class, for my second child, Kristene, when I was 21. The instructor said, "Folks, there is no secret to raising children. If you show them love and respect, they will grow up healthy even if you raise them in the dark eating raw meat." Pretty close.

Of course, children's education in the field must be good enough to ensure that when they return to their home country they will be able to fit in (academically at least) with no serious problems. Both formal and informal education of children in the field will take fieldworker's time. Nevertheless, it is such a rich experience for all involved that it is, in my opinion, easily worth the time involved. Formal education of children in the field can be carried out as part of an accredited correspondence school, e.g. the University of Nebraska's course (<http://eeohawk.unl.edu/ishs/>) or the Calvert School (<http://www.calvertschool.org/engine/content.do>). These were great experiences for my children and they generally found normal US schools easy, and always were placed in the most advanced sections. Of course, children in the field can also learn about culture, linguistics, environment, nature, and a host of other things that are very hard for other children to learn as well by first-hand experience. Discussing cultural values of the Pirahãs, linguistic issues of the Tzeltal language, Banawa puberty rites, etc. with my children was enriching for all of us. And my children often asked better questions about culture than I did, helping me refine and improve my research in various ways. Buying a celestial map and showing your children constellations, stars, and other celestial objects is very enjoyable. (In the Pirahã village, we have even seen man-made satellites.) Your children can learn about local flora and fauna, about nature preservation and so on. There are limitless educational opportunities for children in fieldwork.

Children can also contribute to the work by noticing things that the linguist fails to notice, by learning the language quickly and better in many cases than the linguist, and by learning cultural values more directly than the linguist. They really can be an integral part of the research team. And I have found that having my children with me helps me fit into the community better and that the community trusts me much more if it sees me as a 'family man' than as a lone researcher.

When we first went to the Pirahãs' village, my children were upset. They thought that the Pirahãs were ugly, dirty, and weird. I tried to teach them about valuing differences, but my words were ineffectual. After nearly 8 months in the village at one stay, however, my children seemed to be making good friends. We were then visited by a Brazilian army major who had come to check out rumors of a gringo in the jungle exploiting Indians. The major said that the Pirahãs were the ugliest people he had ever seen. My children were angered immediately and opened up on the major, telling him that he didn't know what he was talking about, that the Pirahãs were beautiful, kind, and fun and that anyone who thought they were ugly needed to have their head examined. Not only did my children react in fluent Portuguese (because of their time on the field), but they showed that they had learned a beautiful cultural lesson, something no other situation could have taught them so well.

But there is no question that taking one's children to the field will have a huge logistic impact on your fieldwork, quadrupling perhaps the amount of equipment, books, and supplies you might otherwise take. You will need school books, candy, toys, presents, and so on. I have carried sets of encyclopedia through the jungle on my pack, for miles, so that my children could have reference works for their correspondence courses.

CHAPTER 3. GETTING STARTED

3.1. The first field session

Somewhere, sometime, somehow you bit the bullet and decided that you wanted to be a fieldworker. And now you are planning your first session in the field, wherever that happens to be. From the moment you first enter the field, you will never be the same. You will experience a professional and personal rite of passage. How do you plan for such an experience? Well, it involves both the mundane and the conceptually involved. Let's begin with the mundane by considering first the kinds of things the fieldworker might take to the field and then move on to more abstract aspects of planning for that first experience.

3.1.1. What do you take?

The things a fieldworker takes to the field, especially on the first field trip, can play a significant role in the success of the project and the well-being of the field linguist. Things to be taken fall into two broad categories: things for the work and things for the soul. The greater the cultural gap between the linguist's home community and the language community, the more the linguist will need to take things that enable him or her to feel a sense of 'continuous identity'. As Wengle (1988, 20ff) makes clear, 'identity maintenance' in the field is crucial for successful fieldwork (or, at least, it is crucial for a healthy fieldworker). The isolation of the field can be extremely disruptive psychologically. Wengle (1988, 7ff) advocates the idea that '... the stability of an individual's sense of identity depends directly on the "innumerable identifications" he has established with the familiar, personal and impersonal, concrete and abstract, animate and inanimate objects of his past and present existence.' Judicious selection of mementos, photos, books, and music, for example, can help the linguist to maintain a connection with her or his life outside of fieldwork.

Linguistic equipment is discussed in chapter _____. A list of the other kinds of things I usually take with me to the field includes the items in ():

- (3.1) List of personal items commonly taken by D.L. Everett to field sites in Amazonia:
- a. Three changes of clothes (one to wear, one to wash, and one drying), hat, flip-flops, closed shoes, and gym shorts for bathing. This would obviously not be an adequate wardrobe for fieldwork among the Inuit. What you take varies on where you are going. But the principle is to travel lightly.
 - b. Medicines (for first-aid and treatment of serious village health problems).
 - c. Multiple vitamins (my diet in some villages is largely starch and meat, with few if any fresh vegetables or fruit).
 - d. Food (the amount and kind depends on whether the group I will be working with has surplus food to sell or trade to me – not all do), including popcorn (people have different 'treats'. For me, the day ends better if I have popcorn).
 - e. Hammock and mosquito net.
 - f. DVDs of movies and concerts.
 - g. Pictures and letters from people I care for.
 - h. Satellite telephone.
 - i. Merchandise for paying language teachers (when money is not wanted. Cloth, machetes, files, hoes, axes, munition, fish hooks, sewing needles, flashlights, batteries, etc.).

- j. Toiletries and bathroom supplies (I recommend, strongly, deodorant – wherever you work, people are likely to think you smell bad, because you smell different).
- k. My entire music library (I have about all my music on iTUNES and nothing cheers me up more during a dull point in the day than ZZ Top or Lynyrd Skynyrd, etc. For you it could be Mozart).
- l. Books (linguistics, philosophy, nonfiction, and fiction – especially Henry James and PD James).
- m. Folding table and chairs.
- n. Backpacking stove.
- o. Kerosene.
- p. Batteries (12 volt and flashlight size).
- q. Solar panels and connections to charge 12 volt battery. (see ____)

If there are special treats (like popcorn for me), that you like to eat to cheer yourself up, be sure to take an adequate supply. However, there is an important caveat to consider before you decide what to take: things you take, no matter how innocent-looking to you, could and probably will, be desired by people in the language community. This can lead to theft, hard-feelings, jealousy, demand for equal treatment. Plan to take things to give as presents, to share your coffee and sugar (for instance), to give away most of what you have taken before leaving the village, and to make sure that nothing you take is too important to lose, be destroyed, or be taken. Never let 'things' come between you and the community. If things appear to create interpersonal problems, go without them. Never argue about them. And that entails avoiding taking anything you would be prepared to argue about.

3.1.2. How long to stay

The length of the first stay in the community should be based not only on the fieldworker's available time, but also on a realistic assessment of his or her ability to withstand the separation, newness, anxiety, and work responsibilities of the initial visit. The general rule is to underestimate your staying power, rather than to overestimate it.

I suggest less than one month for the length of the initial visit. There is a psychological/cultural issue involved in this recommendation. In general, when the fieldworker gives a date that they plan to leave the community, whether only to himself/herself or to others, this date is taken as a sort of promise, a kind of moral commitment.¹⁸ So if a fieldworker overestimates his or her staying ability for the initial visit, then they can feel that they are breaking a promise or 'wimping out', that is, that they have failed in some way. In fact, anyone who goes to the field and stays for a reasonable period of time should feel pride in their accomplishment, not guilt. If the fieldworker leaves after the first visit feeling guilty, this would be an unnecessarily inauspicious beginning to her or his field program. Therefore, I recommend that the first session be planned for three or four weeks. The fieldworker can stay longer if things work out well or return quickly if conditions permit.

3.1.3. Estimating the length of the entire project

¹⁸ This no doubt says more about Western Culture's view of language than about fieldwork.

Samarin (1967, p71):

"Criticizing linguists because they have not had enough time with informants is like condemning shipwrecked sailors for not having provided themselves with food and water. What we can more wisely do is evaluate how much was accomplished in the time a linguist had at his disposal."

The estimate of the length of the entire project clearly depends on several factors, including the nature of the project (e.g. to collect data for a grammar, dictionary, text collection, partial study of phonetics, phonology, constituent order typology, etc.), the degree of physical isolation of the community (from the fieldworker's perspective), the degree of bilingualism in the speech community, the state of documentation and description of the language of study, the state of documentation and description of related languages or languages of the area, the goodwill and cooperation of the community, and so forth. And, as the quote from Samarin says, all linguists would like more field time than they have had before writing up their results. Therefore, a *rough* time estimate for a comprehensive grammar (no dictionary, no carefully compiled text collection) of an Amazonian language lacking previous research on it or related languages would be, after all permissions are received (see ___ below), in the neighborhood of two full years of near-daily work and contact with the community. The writing up of the grammar would take at least an additional 6-12 months, including follow-up visits to gather, verify, or clarify data (see ___). For a language of North American, where at least some in the community speak English and related languages are well-described, the time for data-gathering might be somewhere between 6-10 months, again of regular, daily work with language teachers. I base these estimates on an assumption of three hours per day of *good* lab sessions, excellent language teacher help for data-processing in the field, good health, and no undue interruptions in the work routine. By this reckoning, a PhD program with the projected output of a grammar as a dissertation (in my opinion the most challenging, intellectually demanding, and personally rewarding type of dissertation), with courses, fieldwork, and writing up, should be estimated to take six years (in the British system, since no courses are required, four years are the maximum allowed by national regulations).

3.2. The first day

In early 1977, I began a four-month 'Field training course' offered by the Summer Institute of Linguistics (for more on SIL, see ___ below) in Chiapas, Mexico. To all participants, however, it was known informally as 'Jungle Camp'. Jungle Camp had five phases. First, there was an orientation to Mexico, cross-cultural relations, and equipment purchasing in Dallas, Texas. Next, there were several days of adaptation to Mexico, including visits to the Ballet Folklórico, Tenocitlan, Basílica de San Guadalupe, etc. After this general orientation to the country, we proceeded south to Chiapas, to SIL's initial training base, Yaxmiquilpan, where we learned to butcher beef, preserve meat, give injections, provide first aid care, travel in dugout canoes over rapids, repair kerosene lamps, live under primitive conditions, learn how not to get lost in the jungle, which jungle plants can be eaten, how to hang a hammock with mosquito net, etc. and we began physical conditioning, involving multiple day-long hikes of up to thirty miles through the jungle, in very hard conditions (90%+ humidity, knee-deep

mud, mosquitoes, etc.), including one fifty-mile hike to the village of the Lacandon people. The training's principal highlight was 'survival hike', when people were pulled out of the group at random and taken into the jungle to spend 3-6 days alone, with no food, no water, and only what they were wearing, no change of clothes. This meant that everyone walked around with rope, matches, a machete, and a few other tools that could be carried, in anticipation of the survival hike. The lesson learned was very simple indeed – be prepared (and all that preparation did help when I was called out and spent 5 days alone in the jungle, foraging and fishing and other things that linguists don't normally expect to do).

The Lacandon village was the first completely new cultural experience for me. All other trips I had made, I could at least identify a few points of similarity with my previous experiences. Not with the Lacandones, however. Everything seemed so different than anything I had ever experienced before, that I felt somewhat overwhelmed. Unfortunately, I had not received any suggestions on what to observe or how to observe in such circumstances, no background on the Lacandones, no linguistic orientation to them, etc. I arrived in utter ignorance and left no less ignorant and shocked at how different things were than I had expected to find. It is never excusable for a student of culture or language to enter another group without preparing themselves carefully first with good questions to answer.

3.2.1. The significance of the first day

No serious fieldworker should ever go as ignorantly to a location as I did to the Lacandon village (see box). So many things could have commanded my attention. For example, how was the village located in relation to the jungle, bodies of water, other villages or settlements? Who lived in each house and what were their kinship or other relations to people in other houses? What were the kinship constraints on spatial distance or proximity of dwellings? What greetings were used? (By the people for foreigners? Between men and women?, etc.) How were people dressed? (e.g. differences by age? By gender? By village prestige?) What were the dimensions of the houses? Of the open spaces between them? What were the terms for basic vocabulary (e.g. the Swadesh word list, see ___ below)? And on and on. I measured nothing. My photography had no real scientific objectives. My first day among the Lacandons was largely a day lost, a precious opportunity squandered. The first day in a new environment is the only time you will ever see the culture without previous experience of it. At the same time, from that first day, the sensory meter starts running. And this flow of sensory experience biases the mind, by accustoming it, by making the new old, by dulling curiosity. This in turn means that it is unlikely that you will ever be as acutely aware to the newness and strangeness of this culture and language as on the first day. It is your responsibility, therefore, to arm yourself with questions and to begin asking and trying to answer them within your first hour in the community.¹⁹

This first day is also the day when the community will form its opinions about you. Are you a trustworthy person? A nice person? What are your purposes here? How

¹⁹ In my favorite movie of all time, *Unforgiven*, Clint Eastwood shoots the unarmed owner of a brothel, in front of the local law officer, Gene Hackman. Hackman remarks that it is very cowardly to shoot an unarmed man. Eastwood says 'Well, he shoulda armed himself.' Opportunity does not always wait for one to arm oneself intellectually. A linguist needs to be prepared.

approachable are you? Will you give them things 'for free'? Are you someone people want to know better? Linguistically, you are more likely to impress positively by accurate mimicry and quick learning and use of phrases. As you unpack your bags, you should listen for question-like intonation and try to mimic it. Don't worry about mimicking things you do not understand. Of course you will embarrass yourself. Just do not take yourself too seriously and you will be fine. Have a good time.

This is also when you begin to form your working and personal relationships within the community. First impressions are very important. The consequences of a bad first impression are difficult to gauge. So it is better to give a good first impression and to make an effort to receive one. Yet, that can be tricky. If someone asks you for something, will giving it to them make you seem like a pushover, forever dooming you to nagging about giving away your possessions, or will it be seen as a sign of generosity, not necessarily inviting more requests? You must watch others, ask others, read, learn from others' experiences, etc.

On this initial day of '*National Geographic*' newness, you should be learning more than linguistics. For example, this is the time to draw maps of the community, learn who the community leadership is, where they are located, etc. Talk to the community leadership on this first day (and before ever entering the community if this is possible, by radio, one-day visit, in a nearby city, etc.). Explain your objectives, your aspirations, etc. Find out about theirs. Begin negotiating the understandings necessary for all to benefit from the research (see ____ below). Learn where and how to get water, take care of your rubbish and other waste. Find out about proper relations between foreigners (you!) and the community. Photographs are best taken at this stage, before jadedness sets in. This is also a vital time for gathering metalinguistic phrases, e.g. 'What is this?' 'What is she doing?' 'When are you going?', etc. Use this day to the fullest. It will never come again. And no other days will be remotely similar. Another important, crucial task of the first day or so in the community is the selection of language teacher(s). We turn then to consider that vital part of the first visit.

3.3. Language teachers

3.3.1. Selecting the teacher(s)

Before discussing the selection of a language teacher(s), we should consider what to call the person or people who work for the linguist to teach him or her the language. The traditional term for these members of the language community is 'informant'. But in today's world this sounds rather sinister. Even worse than 'sinister', however, the term 'informant' is bad because it implies that the language community participants in the linguistic research are passive, with no goals of their own, little more than inert sources of information for the fieldworker. The crucial desideratum for any term chosen in this regard is that the term reflect the active and pro-active role of the language community in shaping the research. I use the term 'teacher' here. There may be better terms, but 'teacher' seems to be simple and accurate. Other terms people might find useful are 'consultant', 'co-linguist', 'John' (i.e. simply refer to the teachers by their first names/preferred titles in the first instance), etc.

There may be a range of people with different backgrounds, linguistically, socially, and economically, to choose from. Ideally, one should choose to work with as representative a sample as possible, never being locked into just one language teacher. There are various reasons for this. Linguistically, one wants diversity and statistical significance (sometimes these conflict) in one's sample. But another reason for not working with a single teacher is that the linguist should avoid becoming a *patron* (in

the Latin sense), i.e. an employer responsible for the employee's overall well-being. I recommend working with at least 6 women and 6 men over time, from a variety of ages. (But at the same time the fieldworker must guard against confusing data from different dialects and offer suggestions on how to keep dialects separate.)

There are of course situations in which, initially at least, you may have little or no choice as to who your teacher is going to be. When I first began research on the Kisedje (Suyá) language, I saw several potentially good teachers. The best it seemed to me was a man who was the community's official translator. He was bilingual in Portuguese and Kisedje. But when I approached him, he said that he had no time for such work. Moreover, he said, it had already been decided by the chief that my teacher was to be his daughter. Faced with a community-based decision of this type, it may be acceptable to explain why you'd prefer someone else. But that can be risky at the outset of field research, when you do not know the people or the culture well. By and large the safest course of action is to make the best of the community's selected teacher(s) and later, when you are better attuned to values, decision-making, persons, and consequences in the cultural context, you can ask for a change in or additional teachers if you think it would work better. Once again, however, linguistic research always requires multiple teachers. This is because all conclusions are based on the assumption that they have inter-speaker validity. You cannot make this assumption if you work with only a single language teacher. (So don't worry too much about who you get as your first teacher if this person is selected for you.)

Let's assume that the linguist can work with anyone they please. How do they go about selecting a language teacher? Some qualities to look for in a language teacher are given in (3.2), although none of these are ultimately necessary or sufficient to guarantee a good teacher. But they are all helpful, I believe.

(3.2) Selecting a language teacher:

a. Good story teller

This is important because it will affect the quality of the texts you collect. Especially when collecting oral literature, you should attempt to work with someone respected in the community for telling stories of this sort well and appropriately.

b. Friendly

I have worked with good teachers who are rude or otherwise less than friendly. Other things being equal, it would be nice to work with someone that you have a good chemistry with, someone you consider friendly, for example.

c. Speaker of the correct dialect

It would be a disaster if you concluded a significant portion of your field time only to discover that one of the speakers you had worked with in fact spoke another dialect or had given you a very different register than what you thought you were getting. So you must check out from the outset whether your teachers are representative of the dialect you are studying.

d. Mother-tongue speaker

This sounds obvious, but if you have not worked on a language previously, you might mistake someone for a native speaker of the language when in fact it is their second or third language. You must find out what language their mother spoke to them and what language they now speak in their homes.

e. Patient

The fieldworker is continually asking questions that a child should now, from the perspective of the native speakers. And they ask questions about details that

are extremely uninteresting, asking one to repeat the same word over and over. Thus, working as a teacher for a linguist can be extremely tiring. A good teacher should be patient.

f. Reliable

The linguist's time in the community and the success of the entire field research program depends on the ability to plan. If a language teacher cannot be relied on to show up sometime near the pre-arranged time and if this goes on for too long (the 'too' to be determined by the linguist), then the teacher is not reliable and should therefore be switched, however delicately, to avoid offense, etc.

g. Unintimidated by linguists

Some language teachers are intimidated by the linguist's education, nationality, money, big words, cushy job, and so forth. Some of these teachers therefore do not like to correct the linguist's pronunciation, or his or her conclusions, data, etc.

h. No speech impediment

You need to be sure that the pronunciation you are hearing is a standard one in the dialect/language under study. If someone has an obvious speech impediment, you cannot be sure you are getting standard pronunciations. This can lead to faulty generalizations about the language's phonetics or phonology. Hence, at least for sound-system analysis, language teachers should be free from any speech impediment.

i. Shows ability to reflect on language as a formal system

For some language teachers, the linguist's structural investigations will have little interest or meaning (moving stress around in the word, building and testing sentence paradigms, i.e. avoiding questions on meaning for questions on structure. But there are rare teachers who (like rare university students) turn out to be talented, natural linguists and show an ability to reflect metalinguistically on their own grammar. Such speakers can suggest paradigms, find regularities before the fieldworker sees them, provide transcribed, translated data, etc. They are marvelous linguistic resources and should be looked for, cultivated, and trained. Where appropriate, the fieldworker should attempt to get linguistically talented teachers more formal training if they desire it.

Alan Vogel offers the following suggestions on training native speakers, based again on his Jarawara experiences:

"I think two kinds of training may be relevant here. First, I trained Jarawaras to transcribe tapes of recorded stories. These transcriptions are not perfect by any means, but they are a huge help to me. For one thing, they hear a lot of things that I would not. They gladly spend 8 hours a day transcribing tapes. Secondly, I made up a questionnaire on plants with about ten questions. I gave the younger Jarawaras, who write well, the questions, along with sheets of paper, each with a name of a plant that I had gotten, and asked them to get the answers to the questions from the old men who know all about the plants. It produced volumes of information on hundreds of plants. It's important to realize, of course, that when we started with the Jarawaras, no one knew how to read or write in Portuguese or their own language. We had to devise the orthography (adapting the Jamamadi orthography), and after members of another missionary organization made up a primer, my wife taught several young men how to read and write Jarawara. She taught them how to teach others, and they did. The other missionaries among the Jarawara did the same thing in Agua Branca, and today the Jarawaras are basically literate in their own language."

j. Well-respected in the community (at least not marginalized)

Occasionally someone available to work as a language teacher is ostracized from the community or seen as marginal (in fact they may be trying to get work as a teacher because they have no other employment). This can be detrimental to the research if when you proceed to check examples with other language teachers they reject all examples given to you by your 'marginal' teacher, simply because of that person's social status rather than because of their linguistic ability. But you will not be able to tell the difference easily, affecting the research in ways that might not be easy to predict.

k. Expects to teach language, not translate the Bible, etc.

In some field situations, communities with a strong or new church, with a history of close ties or cooperation with foreign missionaries, the community or individual language teachers might confuse the fieldworker with a missionary and expect that the linguistic work on the language is ultimately directed towards a Bible translation. The linguist must make it clear at the outset that this is not what they are there to do. Avoid creating false impressions. (See ___ below on ethics for further discussion.)

Regardless of how many or how few teachers you actually work with, it is important to know how to select them. As you select your teachers, you should also give thought to how you present your linguistic objectives, the nature of your job and the nature of your teacher's job to the community. Perhaps the community will have had previous experience with anthropologists (as with the Kisedje). In this case, it is important that you distinguish your objectives, because the linguist will work quite differently from the anthropologist, with objectives that are perhaps harder at times for the people to grasp or sympathize with. Avoid claiming that your objective is to learn their language unless you in fact intend to learn it. It can cause misunderstandings if you are cheerfully working away but with no marked progress in your ability to speak the language. If the teachers saw that as their primary goal yet you have not progressed much, this could lead them to conclude that they are bad teachers, you are a bad student, or both. This can lead in turn to a lack of interest in helping you.²⁰

3.4. The laboratory session

3.4.1. Linguistic planning of the session

The fieldworker must develop and maintain a high degree of professionalism in preparing the space, time, language teacher, and their own mind for the lab session. This section provides some suggestions for this preparation.

Have a point! Each lab session should begin with a well thought out, measurable objective. For example, 'Today I want to record and translate a narrative of 15-20 minutes in length.' Do not waste your time or your language teacher's by scheduling sessions for improvisation. As Peter Ladefoged told me, *"...when in the field spend more time thinking what you want to record than making recordings. I so often see students and others wasting time recording masses of useless stuff, hoping they can use it later when in fact it will simply delay their later work by their having to go through*

²⁰ My opinion is that you should do your utmost to learn to speak the language and to demonstrate regular and obvious progress at each meeting with the language teacher. It would be nice if others in the community complimented you and your teacher(s) for your progress (if complimenting is something they do).

*it all. **Think hard first, record later.**" [emphasis mine, DLE] Peter Ladefoged, email to Dan Everett November 26, 2003*

Additionally, you should have back-up plans. As Mick Jagger reminds us, 'You can't always get what you want.' For example, if the speaker(s) do not seem to want to give you a narrative text, you could have planned a series of questions, e.g. 'Tell me about your hunting trip', 'Can you tell me how to weave a basket', etc. That is, open-ended questions that likely will involve multi-sentence answers and can give you at least something text-like. The secondary goals need not be related to the primary goal. But time with the language teachers is precious, literally, and it should always be fully and productively utilized. On the other hand, do not keep the teacher with you if you have run out of planned material. There is no sense in paying someone to watch you sputter and spin your wheels. In fact, you could give them evidence that you have no idea what you are doing, which is not a good impression to make.

There are various types of elicitation that can be undertaken in the lab session (and in this section I am heavily indebted to Samarin (1967, 112ff), where some of these types are first spelled out).

(3.3) Types of elicitation

- a. Questionnaire-based: I often find it useful to follow a questionnaire, modifying it for intelligibility and local relevance, e.g. the Lingua Descriptive Questionnaire. (I have included an appendix to this book with a phonology questionnaire I developed, for the fieldworker thinking of doing more serious phonological studies, though it is not necessarily a tool appropriate for elicitation.)
- b. Complementary: looking for further examples of material already collected.
- c. Probing: asking open-ended questions, looking for explanations and paradigms provided by the language teacher. (Answers in such elicitation are simply one form of input and should never be taken as settling any issue.)
- d. Hypotheticals: testing lists of hypothetical words generated by computer (see William Poser's webpage (<http://www.ling.upenn.edu/~wjposer/>) for one such program).
- e. Translation: getting translations for texts and other material. Matthewson (2004) discusses semantic elicitation (see also __) and some of the pitfalls of eliciting translations. In spite of her well-taken warnings, though, getting translations of the data collected is vital and lab sessions need to be dedicated to this task. The fieldworker, however, must be careful to treat native speaker translations as only one source of data about the meanings of texts, sentences, etc.
- f. Reverse translation: another way of getting at the meanings of texts, etc., is to have one set of speakers translate a vernacular text (a text in the language under study) into the national or local trade language and then have a different set of speakers translate the translation back into the vernacular. The fieldworker can then compare and contrast the different forms given and design further lab sessions to probe and test the resultant hypotheses.
- g. Corrective: this type of elicitation tests the linguist's understanding of a certain set of constructions, expressions, texts, constraints, rules, etc. by discussing examples that have been devised based on the linguist's current understanding of the grammar. This is likely to have a low and unreliable rate of

return but, so long as the fieldworker bears this in mind, it can play a useful role in analyzing the relevant structures.

h. Filling in gaps: looking for missing paradigm forms, gaps in phonetic charts, sentence types, different participant lists, etc.

i. Experiments: It is important, where expertise is available, that some questions of structure, meaning, and understanding be tested by standard psycholinguistic experimentation. Although I have nothing to say about experimental design here, fieldworkers should either acquire the skills necessary to design, carry out, and interpret psycholinguistic experiments or they should recruit expert help, to either help them design experiments before leaving for the field or to accompany them to the field to carry out the experiments *en loco*.

3.4.2. Non-linguistic planning for the lab session

An early decision is where the lab session will be held. It would be nice of course to have a sound-protected location, comfortable, bug-free, dry, and free from distractions. However, this is not usually possible. But it is vital that the site chosen be as close to ideal as the fieldworker is able to get in the community. This could mean little more than taking a couple of folding chairs and table out of the village, busy neighborhood, etc. to a relatively calm, isolated area. Peter Ladefoged and I once commandeered a hut in a Pirahã village to carry out phonetic investigations and I simply asked people to keep the children and dogs away and to keep the general noise level as low as possible. They kindly accommodated us. Care in the choice of location will directly improve the research. And it can indirectly improve the quality of research by showing that this is serious business.

Of course, like any field activity, selecting and developing a location for lab sessions can have unintended consequences. For example, among the Pirahãs, I built a small wooden structure about 100 meters outside the village, raised above the ground, and screened in, with a lockable door. I discouraged people from looking in during sessions and tried to allow in only the teachers working in a given session, rather than them and all their friends. The Pirahãs do not mind this. But a Brazilian government agency investigating the activities of a 'gringo' in the area asked the Pirahãs about this small structure when I was absent from the village. The Pirahãs, in their nearly non-existent Portuguese, were able to communicate that I spent a lot of time in there and that few people were allowed in. The agency representatives wondered what sort of fiendish experiments I might be running out there. Finding out about this, I quickly visited the agency headquarters and gave them a full explanation.

I cannot imagine a successful lab session without the following equipment: headset microphone, digital and analog recorders, computer, pen and paper, chairs, table, and protection from the most intense features of the elements. In ____ below I discuss the linguistic equipment and its use in more detail.

The time of the lab session must take into account primarily the constraints of the community and the language teacher. When is it best for the community and the teacher for the lab session to take place? If the linguist can choose the time to work freely, then I suggest that the session take place while the community is quietest and calm. The linguist must keep lab sessions appointments and show that they take these times seriously. This could be in some cases part of the training of the language teachers as well – all participants must respect the research time. If language teachers show

themselves unable (for whatever reason) or unwilling to show up at agreed times, then they and the linguist should discuss possible solutions. If such discussions still fail to provide a satisfactory solution, the linguist may need to take the issue up with the community leadership, asking for advice.

3.4.3. During the session

Fieldwork is about getting high-quality data in the appropriate quantity (see ____). Nowhere is the data more likely to be gathered than the lab session. And the success of these sessions depends largely on the rapport, trust, and mutual understanding of the linguist and the language teacher. Therefore, the lab session must be a meeting of equals. The language teacher must be fully confident that they are respected by the linguist, and vice-versa. Care must be taken to ensure that the physical arrangement of the space (chair, table, etc.) reflects equality rather than hierarchy. The linguist's facial expressions, tone of voice, and body language must at once show professionalism, respect, and enjoyment of the task at hand.

Frequently in the sessions the linguist will find a particular response puzzling, apparently irrelevant, or otherwise unhelpful. Receive any 'unhelpful' response with the same gratitude that should mark the linguist's attitude towards every response and help from the language teacher. Respect means allowing the teacher to offer information that does not always coincide with the linguist's request.

As I have listened to some of my more than twenty-five year old tapes of the Pirahãs, I have, with my hard-earned ability to speak the language, realized that some of these 'unhelpful' responses were in fact attempts to correct some of my mistaken impressions, offering, instead of what I had ignorantly asked for, a response that was much more helpful, had I only recognized the Pirahãs' ability to teach me, rather than to simply answer my questions. That is, had I realized that they were teachers rather than mere passive 'informants'.

Further, the language teacher will look for evidence that the linguist has understood or at least heard correctly their response. The best way to show this (and that the linguist is paying attention) is to repeat every example back to the teacher. Make it clear that you want to be corrected.

One source of potential pressure on the language teacher that can lead to less useful responses is for the linguist to reveal too much about his or her predictions and analysis before it has been carefully verified. This could bias the results, leading the teacher to, in a friendly way, look for examples to confirm what you are saying or otherwise bias the nature of the results obtained. (This excellent bit of advice is taken from Boas (1911, 59)).

Finally, the linguist and teacher must know when the session should conclude. There are various criteria. First, conclude at the agreed time, unless there is mutual agreement that the session should continue for some reason. Second, conclude when the linguist runs out of material for the session. Third, the session should be concluded if this is a bad day for the teacher and they cannot get focussed. Fourth, conclude the session if there are too many distractions (e.g. a hunter has just brought back game and the language teacher is concerned about missing his or her share).

3.4.4. Third-party interpreters

In some field locations, it may not be possible to engage in either monolingual or bilingual work directly with the language teacher. In such cases, it may be necessary to employ a third-party interpreter (see also Samarin ()). Such a situation arises when the linguist and the language teacher fail to share a common language and the monolingual method is impractical for some reasons (e.g. when the time available to the linguist is too short, the teacher is one of a few survivors of a moribund language, etc.).

There are some special precautions to take in working with interpreters. First, the linguist must be clear in all instructions given. It can be safely assumed that all questions asked through an interpreter will be distorted to one degree or another. Ambiguity or vagueness in questions will create more severe problems when asked through a translator. Second, the linguist should be suspicious of the quality of the question, the translation, or both if short questions to the interpreter become very long questions or conversations between the interpreter and the language teacher. Answer in such circumstances should be viewed as highly suspect. Third, be equally suspicious of short answers by the teacher which require apparent circumlocutious translations to the linguist or whose translations are hesitant, unclear, etc. Finally, since the interpreter has a closer linguistic connection than the language teacher to the linguist and, perhaps, more formal schooling, there may be a tendency for the language teacher to perceive that they have lower status. Avoid this. Neither the linguist (!) nor the interpreter should communicate any attitude of superiority to the language teacher.

Lab sessions can involve culture shock and problems that the linguist did not foresee. For example, I was working once with a couple of Pirahã men who had contracted colds from visiting Brazilians on the TransAmazon Highway. As we were working, they would sneeze and then take the mucous and fling it away from them, often landing on my notebook paper or on me. Since the Pirahãs see absolutely nothing unhygienic about this, they could not understand my request that they not do this. 'We have colds', they remonstrated. 'This stuff comes out your nose when you have a cold. Don't you get colds, Dan?' Or another time I was working with a group that had apparently eaten something hard to digest. There were sounds and smells coming out of their nether regions that are hard to describe. As I told a friend later, they passed more gas than the Alaska pipeline. One time a female language teacher decided to flirt with me. She wasn't very subtle. She took her dress off and asked if I had ever had sex with women other than my wife. This was apparently no big deal to her, but it was, um, distracting in the midst of transcription to think of a way to say no politely. And then, of course, I offend others. I am hairy to Amazonian Indians and smell quite bad (Westerners perspire much more than Native Americans and thus do smell much worse in hot, humid climates). Be prepared to accept things with a smile that you might not have anticipated.

Monica Macaulay has written one of the best short pieces on culture shock (or culture strain) in the field that I have read. Here are some of her experiences:

"Let me describe some of the things I was dealing with:

First, I had trouble finding food. Eventually, I was able to establish a routine where I had a noon meal at a restaurant, and otherwise ate bananas and tomato sandwiches. But bananas, tomatoes and bread were all things I could only buy twice a week, on the two market days. If I didn't buy enough, and ran out of food, sometimes I could buy little packaged pastries at one of the stores, but that was about it. I lost a lot of weight.

Second, it was the rainy season, and I was at an altitude of about 9,000 feet. It was extremely cold, and I was not prepared for it. I wore layers and layers of clothes, and froze when I had to wash something. We did have electricity in the house I lived in—most houses in the village had it. But of course there was no heat, and often when it rained the lights would go out.

I almost immediately became covered in little red bites from invisible insects. No bug spray or lotion helped, and this continued for the entire time I was there...

I did manage to find consultants fairly easily, but they stood me up all the time. Of course the notion of scheduling appointments was not quite the same to the people I was dealing with as it was to me, with my Midwestern expectations of promptness and politeness. If the consultant wasn't available, the day was shot for me—it was very hard to get people to agree to work without at least a day's notice.

Then there was my work itself: the more I worked on the language, the more incompetent I felt. I had terrible fears that I was putting myself through this torture for nothing—that there would be no results to show for it when I returned home.

One thing that I expected that I actually did not find was unwanted attention from men. I had been in enough big cities in Mexico to know what to expect along those lines, but it did not happen in Chalcatongo, at least not from the locals. They watched me, certainly, but it was more like being an animal in a zoo than a woman being ogled by men.

Unfortunately, there were a few men there from bigger cities, and they did give me some trouble. One in particular was a real problem. He would get drunk and pound on my door, and say strange things to me. At times I felt completely confined to my room—that it simply was not safe to go out. This was especially problematic since of course the bathroom was across the courtyard.

Most of the time, though, I was simply a curiosity. Occasionally there was ridicule that I was aware of, but usually they just stared at me...
... how alien I felt, and how alien they regarded me as being."

3.4.6. Serial teaching, group teaching, and individual teachers

3.4.6.1. Group sessions

In any single laboratory session, the linguist will work with at least one language teacher. But they could also work with multiple teachers simultaneously. It is often useful for the linguist to work with small groups as well as with individuals. Group sessions (3-4 language teachers) have various advantages: (i) they can allow the linguist to put the language to use right away, by checking immediately what one speaker gives with another speaker in the same session; (ii) they immediately provide the linguist with alternative phrasings and pronunciations of the data and alternative ways of telling and/or interpreting a short text; (iii) they provide speakers a chance to discuss their language (by discussing their answers and the linguist's questions), developing linguistic sophistication and awareness of the linguist's objectives, at the same time that it also helps the linguist to learn more of the language and meta-language by listening attentively to the native speakers converse.

3.4.6.2. Serial teaching in lab sessions

The Pirahãs have never developed a great deal of patience for sitting and teaching me their language. So rather than have one person sit for long sessions, I schedule different language teachers for every 20-30 minutes for the entire morning, giving myself a coffee break or two in between sessions. This breaks up my day, keeps

language teachers from getting bored, and provides many opportunities for clarifying and checking other teachers' data. Serial sessions also have another significant advantage over working long periods with a single teacher. Speakers who only work 15-20 minutes per day will not usually see the linguist as substantially in debt to them for their time and services. They can see their participation as a break, as 'fun', as a change, etc. rather than serious work. And this lightened attitude and environment can translate into better exchanges and more openness between the linguist and the teacher.

The upshot of this is that the fieldworker should remember that there are various options that should be explored for working with language teachers beyond working with a single person for long periods of time.

It is not so much what you do, but what you do *next*

This may all make the lab session look like a traumatic experience. But in fact these sessions will almost certainly be rewarding and exciting experiences. A general rule of thumb is the old advice 'It's not what you *do* that matters, it is what you do *next*.' You *will* make lots of embarrassing, side-splittingly stupid and funny mistakes. You will murder the language, utter profanities unintentionally, and do any number of other things that will embarrass you and confuse the language teacher terribly. This is all unavoidable. But what the linguist does next, what *you* do next, that is, after the embarrassing mistake, is what really matters. Laugh it off. Go back to the drawing board and start again. Do that and you will succeed. Don't do it and you will fail. Period.

3.5. Training language teachers

3.5.1. Why train teachers

Why would the linguist, whose time is already far too limited, want to take any extra time away from his or her linguistic investigations to train language teachers in any skill not directly related to the linguist's own goals? The answer is primarily that this is an ethical responsibility. Just as the linguistic research is ultimately intended to enhance the linguist's career and improve the linguist's quality of life, so the research should enhance the teacher's quality of life. This is *empowering* fieldwork and it is not an option. Linguists and anthropologists, like missionaries, have the skeleton in the early histories of their disciplines of following in the wake of and contributing to attitudes of colonialism (every fieldworker should read Said's () *Orientalism*). Passing on skills and knowledge in mutual exchange and respect should be the hallmark of modern fieldwork. The linguist takes and must give back. And money is not enough.

When teachers are trained and interested in the research on their language, they can become pro-active co-researchers. As they come to understand and identify with the research on their language, they can gather a good deal of the data, record it, and transcribe it. They can also translate it themselves. The linguist can then review all the data they have collected and verify it against the developing analysis and verify it in light of the developing analysis and check it with other speakers for relevance, naturalness, accuracy, etc. I found that this method worked reasonably well with the Banawá. Alan Vogel (p.c.) tells me that this works well with the Jarawara people of Brazil.

3.5.2. What kind of training?

The linguist has an obligation to at least train the teachers and others in the community in technology, mechanics, medicine, linguistics, general world knowledge,

and any other kind of knowledge that the community sees as important that the linguist is able to help them with.

I have trained Pirahãs in the operation of my motor boat. We have had lots of fun driving this National Science Foundation-purchased boat and 40 horsepower engine around the Maici river. Pirahã men have piloted us right into jungle growth on the river bank and onto beaches. But we had a blast. And now when I am in the village, I know that if I am injured or ill, the Pirahãs themselves can pilot me out. So I get a direct return on this investment of time. And the Pirahãs now have a skill. They do not have motorboats, but they enjoy showing outsiders how they can pilot mine. The Kisedje people of the Xingu regions of Brazil already know how to drive cars, pilot motor boats, and handle other types of Western equipment. However, they need computers for some of the activities of their tribal association and want their people trained to use these computers. So my project provided them with a laptop computer and training in its use. We are on regular email contact now, no matter where I am in the world. I have also trained people in giving injections of anti-venom and in the use of anti-malarial pills. I regularly take in National Geographic and other movies about other regions of the world so that they can learn about other peoples, regions, animals, etc. These discussions are enjoyable activities for all of us.

How does one go about training language teachers? This will depend tremendously on the level of familiarity of the people with the things that the linguist brings to the field, e.g. education, equipment, medicine, etc. The more familiar they are, the more likely they are to have specific objectives in training in one or more of these areas. For linguistics, the linguist should discuss his or her objectives and the methodology of linguistics. And they must always be careful to explain global and local goals. Why did they come to the community? What are they trying to learn about in today's session. Why is the linguist asking this or that question? What are they trying to learn today? Training can also be facilitated by asking the teacher for his or her advice and insights. This type of reflection helps the teacher to think like a linguist about the language. Different approaches are required by different cultural contexts.

3.6. Styles of work

Some field linguists work with language teachers eight hours a day, seven days a week, the entire time that they are in the community. Others, myself included, find that a less intensive lab schedule works better. I recommend two-four hours per day of lab sessions, including verification of data collected with multiple teachers. The remainder of the day should be used for data-processings, analysis, hypothesis formation, language practice, and planning the next session. (See also section ___ on how I spend a typical day in the village.)

3.7. More on data-collection

3.7.1. Processing the data

Data that are not processed in the field, with the intensive participation of native speakers, are relatively useless. The fieldworker will waste much effort if texts and other data are collected to be processed when they return to her home institution. Lab sessions must be dedicated to transcribing, translating, and testing all material.

Part of the data processing, the easy part, is careful annotation of the data for storage (this is a form of 'metadata', though I avoid this term here because of its largely

computational connotations, taken up briefly in 5.8.). Suggestions for annotation are given in (3.4) below (which borrows from Samarin ((1967); see also the EMELD website, <http://emeld.org>).

(3.4) Data annotation

- a. Code: This gives the genre of the text (e.g. N(arrative), E(xpository), H(ortatory), P(rocedural).
- b. Sound file number, i.e. where the text is found in the audio database.
- c. Topic: What is the text about ('fishing story', 'story about jungle spirits', etc.)
- d. Date: When the text was collected and when the tape was transcribed (thus there will be two dates).
- e. Language teacher information: Who produced the text; who helped transcribe and translate the text; sociocultural information on language teachers (e.g. age, gender, marital status, occupation, birthplace, knowledge of other languages, religion, language of parents, societal status, etc.)
- f. Team member information: Which linguists worked on the text and what was the contribution of each?
- g. Place: where was the text recorded (village, city, university, etc.).
- h. Any notable characteristics of the text (e.g. this ritual text includes prosodic phrasing that appears quite unusual; the language teacher uses expression *x* a great deal, etc.).
- i. Other comments the linguist believes are relevant.
- j. Each line of the text should be numbered and cross-referenced to its location in the audio database (how can a reader find it to listen to it, quickly?).

3.7.2. Transcription

As a rule of thumb, never estimate less than a 4:1 ratio between transcription and recordings. That is, for each hour of recording, it will take the linguist at least three hours to transcribe the data, if they are already very familiar with the language and its sounds. If you are just beginning in a new language with unfamiliar sounds, the ratio is more likely to be 5:1 or 6:1, or even more, until you get used to transcribing this particular language.

I usually proceed as follows. First, I transcribe the text (or other data) on my own. Then I read my transcription to a native speaker. With a different color of ink, I write in that speaker's correction of my transcription, as I pronounced it. Where doubts and confusion arise, I play the original recording of the text and ask the teacher to tell me what was said and what it means. I use my own pronunciation initially because it is the best and most immediate check of my ability to pronounce and transcribe the language. The native speaker will almost always easily understand the original text, since it is by another native speaker. But I want to see if *I* have it right. I indicate whether I think any correction is of the actual phonetics or whether it is a 'prescriptive' correction, i.e. that it reflects what the second speaker believes that the first speaker *should* have said if speaking correctly (it is very common for speakers to make fun of one another and to think that the other's use or knowledge of the language is inferior to their own – common to anyone who has much experience around universities).

I then continue with this same language teacher to get a translation of the text as a whole, the individual lines or sentences of the text, and, to the degree possible, individual words and morphemes. Following this section, I study the text and translation, making notes of doubts I still have and structures that are unusual in

content, form, linguistic complexity, or cultural information. I often repeat this entire process with one or two other language teachers. Finally, I often go through the entire text, when it is particularly complex, with the original giver of the text.

In cases of unresolved doubt on the sounds (segments or prosodies) of the text, I look at wave forms and spectrograms. In cases of continuing doubts on meaning, I may continue to investigate with other language teachers or I may simply set the text aside for a while (perhaps even years).

I recommend too that indelible ink (waterproof at a minimum) be used for all transcriptions and that corrections be done only by strikethrough, never completely obscuring the corrected data, because that could, on subsequent reflection or investigation turn out to be the more accurate transcription after all.

3.7.3. Fieldwork without language teachers

David Gil (email, 14-October-2005) gave me an interesting answer to a question I put to him on working with language teachers:

'To be honest, for the last decade or so I've actually done very little informant work myself. Reasons are, I find other sources of evidence generally more reliable, and using other such sources more fun and more rewarding. In fact, I would go so far as to say that I haven't done any informant work at all in many years on any of the issues that REALLY interest me; the little that I have done sporadically is mostly to check things for other people, or for very mechanical things like collecting word lists... As a result, my main source of evidence is naturalistic speech (via either eavesdropping or recording and then transcribing longer stretches), with various types of experiments as an alternative.'

Eavesdropping can indeed often be a useful resource, even when the linguist is not in a situation similar to Gil's. One can often find what they need by spending unstructured time with native speakers and engaging in directed, selective eavesdropping. That is, the linguist can have a specific question in mind and listen for it to come up (obvious examples would be greetings and leavetakings, verb forms, pragmatic conventions, phonological features of natural speech as opposed to elicited speech, etc.). The linguist can also use eavesdropping for probing, i.e. looking for new structures that they have not encountered before (or doesn't remember encountering). But it is important then that the linguist always have pen and paper or, where practical, a small taperecorder, to hand at all times to jot things down. It is of no use to try to remember at the end of the day useful sentences overhead earlier.

3.7.4. Coaxing texts out of speakers

Here are some of my thoughts on ideas on collecting texts. It is not always easy to get a speaker to give you a natural text. I recommend Longacre () as a general overview of discourse analysis. The reader is also referred to ____ for more on discourse analysis.

Alan Vogel, who has worked for many years on the Jarawara language, recommends the following:

"One thing I do is always carry a list in my pocket of stories I want to record. When I am talking to people, and I hear a reference to some experience or story that sounds worth recording, I make a note to myself to record the story later, jotting down the

person's name and something about the subject matter. And I ask the person if he would be willing to record the story another day. Then later, when we both have time, I record it. This is a good way of avoiding the problem of sitting down with someone and asking them to tell you a story, and they don't tell a good story, because you don't know what stories they know (or maybe they are not a good story teller)."

NARRATIVES: Some speakers will not want to give informal narratives. They will want to provide the linguist with formalized/stylized texts of cultural significance, i.e. recognized oral literature. Such material is of course wonderful and the linguist certainly must collect a good deal of it. However, the linguist also needs informal narratives of the type 'What I did for my summer vacation', 'How my fishing trip went today', 'What I told my son to do to avoid jaguars', 'How my little girl made her dress', etc. Different speakers will have different abilities and preferences for different kinds of narrative texts. There is no magic answer. One simply has to experiment with different methods and speakers. But one should begin with the simple stories and work up to traditional stories, which are longer, often use archaic language, and are much more difficult to analyze. Moreover, in many communities only certain elders are allowed to provide, translate, or otherwise work on traditional stories with the linguist.

PROCEDURAL: This kind of discourse is easier to collect, because it involves a speaker telling the linguist how to make something, following a specific order of activities, e.g. a recipe, how to make a bow and arrow, etc. These were the first texts I ever collected among the Pirahãs. And they are among the easiest to understand, since you can pick out individual parts of the process and figure out fairly easily how the overall structure of the text works. It is an excellent genre for collecting imperatives, temporal connectives, and other natural features of recipes, etc.

EXPOSITORY: These are explanations, ideally of culturally important information. So a language teacher might explain how a man becomes a shaman, what the village headman was talking about last night in the text you could not understand, etc. Again, these are usually not too difficult to collect. They have characteristic aspects, tenses, and other features which set them off.

HORTATORY: These texts are often quite difficult to get in elicitation, though they are usually much easier in eavesdropping (and for this reason the effective eavesdropper should always be armed with a recorder and good microphone, ready to be activated at a second's notice).²¹

CONVERSATIONS: Conversations are at once the most important genre of text to collect (because they contain natural, everyday use of the grammar) and the most difficult. To set two or three speakers down together and then say to them 'Converse', is not conducive to natural discourse. But conversations can be recorded. In July 2004, for example, I was able to record a natural, long (30 minutes), and linguistically rich conversation from two Banawá men, Sabatao and Bido. They each wore headsets with high-quality unidirectional microphones and were recorded onto separate channels on a digital taperecorder. The conversation was then transcribed, glossed, and filed. It is the best data I have ever collected from the Banawá.

²¹ Of course, one must never record anyone without their permission. So the community should be asked if it is OK for the linguist to record things spontaneously. Assuming it grants permission, then for any recording made in eavesdropping conditions, the linguist must then get the specific and explicit permission of all those recorded for that recording to be kept, processed, and used in linguistic analysis.

At the same time, it proved much harder to collect a similarly natural conversation from the Pirahãs, whom I know much better. I tried various strategies after it became clear that just asking two people to converse was not going to give useful results. I have just set with people in their huts, with the recorder running, often getting natural conversation. However, since they are obviously not wearing headsets in such circumstances, the sound quality of such recordings is far inferior. Nevertheless, it is good enough for prosodic and morphological analyses in most cases.

3.8. Digitization of data and the internet

Before travelling to the field, the field researcher and all members of her team (which may include no one else but the lone fieldworker) should be trained in the field-collection of audio, video and pictorial primary data and determine the form of the notes and metadata to be associated with the collected data in the data base, e.g. Name, Tribe, Dialect; Gender; Age; etc. Any less-experienced members of the field team should also practice data-collection and field-analysis prior to the fieldwork seasons. Once in the field, members of the team should follow their pre-agreed upon plan to collect, transcribe, and conduct preliminary analysis of captured data. Again, unprocessed data is almost always useless away from the field situation. It is also vital that the team ensure that all collected data are secured and backed up to alternate media, e.g. DVDs and copied onto each team member's computer in the field (i.e. each team member's laptop should contain a full copy of the entire team's data). Returning to the home institution the data should be further backed-up, preferably using institution-wide resources that enjoy a long-term commitment of the institution's resources and administration.

In today's world, it is important that the fieldresearcher attempt to make her research results (at some stage of development, not necessarily including raw data) web-accessible, assuming that the speech community agrees. The planning process for this should include at least the following: (i) a list of desiderata for a preliminary website (e.g. data-retrieval, video-audio coordination on the website); (ii) a plan for the participation of a web-programmer in the site's design; (iii) a discussion for answers to questions like the following: (a) how can data be optimally accessible to linguists and other users via the Internet; (b) how the underlying data-base is best structured and constructed; (c) how will web-based users interact with the site; (d) what is the efficacy of different kinds of material to be made available for retrieval from the web data base; (e) how best to link video, audio, and labelled files to the orthographic and phonetic transcriptions of the texts collected. etc.

Finally, in designing the final site, the fieldresearcher/team should take a 'story-board' approach to experiment with design ideas, also inviting other university staff, colleagues, and students to participate. Presentations and discussions should be used to discover usability errors or conceptual design flaws before the latter implementation stages. At each stage of development of ideas on web-preservation expected outcomes should be clear to the field team.

These planning stages will then be followed by actual site construction. During this phase the research team, in conjunction with the web-programmer, will: (a) construct the underlying database; (b) populate the database from analysed field data; (c) construct the website front-end working from the agreed story-board; (d) program the agreed methodologies for the extraction and viewing of the underlying data; (e) conduct usability trials on the prototype system with interested parties (e.g. linguists experienced in web design and/or field linguistics); (f) consult with a wide-range of

experienced experts to ensure best-practice and comply with emerging international standards in web-archiving of field data, e.g. the Open Languages Archive Community (OLAC); Electronic Metastructure for Electronic Languages Data (EMELD); Oxford Text Archive (OTA) and others.

It is always useful for field researchers to handle and process the data multiple times to better control it. But fieldnotes in longhand also provide for creating symbols not available on computers initially, and can be done when not near one's computer, as often arises in field situations). All data should be copied to a separate server, e.g. the server of the linguist's home department. DVD copies are also made and then ideally would be made available to the appropriate institutions of the country of research, e.g. for Brazil, the Museu Nacional in Rio de Janeiro. At the home institution, initial field transcriptions are expanded to include as much of the phonetic nuances as possible, up to the point that members of the team agree that they have reached the 'point of diminishing returns' relevant to the project. Sound files are also analyzed in more detail, again using an advanced acoustic measurement program. Video files should be studied and discussed, e.g. as to how hand gestures, facial expressions, and other features (might) correlate with grammatical and cultural features of the texts being told. Tagging of texts, audio files, and video files will include any special/additional information agreed upon by the team.

Attention must also be given to then assessing the final product and ensuring that it continues to serve the agreed-upon purposes of the particular language project, e.g. (i) transcriptions of written texts downloadable for each text, both separately and along with their supporting audio (and/or) files. Readers might also have the options of downloading with the transcriptions other information, e.g. ToBI labellings (i.e. any combination of files, including downloading only audio or video files); (ii) each individual sentence of a transcribed text should ideally be downloadable individually, along with its supporting sound file and various annotations/labellings; (iii) some thought should be given to a general constraint to serve all interested parties with Internet access, including many in the third world with slow, dial-up connections. Therefore, during the pilot-webpage development phase, the fieldresearcher should be seeking feedback from all users to design a future site that can facilitate use by both high-end and low-end technology users. These are of course just a few ideas and their implementation and nature will vary dramatically from project to project.

Webpages should use platforms, software, and equipment backed by long-term institutional commitments.

CHAPTER 4. SEGMENTAL PHONOLOGY AND PHONETICS

4.1. Introduction

What are you trying to figure out when you begin to analyse the segments of a language's sound structure? Not everyone who sets out to study segmental phonology in a field situation has the same objectives. One person might study the segmental phonology so that they can have a consistent system for representing language data for research on other aspects of the grammar. Another might study the segmental phonology in order to help develop a literacy program with a particular community. Or one might study the segmental phonology because they are a phonologist or otherwise interested in theoretical issues of sound structure. No matter what one's overall objectives are, it is vital that they do a good job and that results be backed-up by high-quality digital sound files so that the analysis can in principle be replicated, without the need, insofar as possible, for additional field trips. The analysis of the segmental phonology of the fieldwork language should accomplish the following:

- I. Provide an explicit, clear account of the phonetics (contrast, variation, and make-up) and distribution of all sounds of the language.
- II. Offer a clear statement of the *phonemes* (or *distinctive segments*) of the language, with supporting analysis and data.
- III. Discuss or point out any typologically unusual or theoretically noteworthy characteristics

In my experience, phonology is usually the least developed portion of descriptive grammars. This is due to several reasons. First and most significant, perhaps, is that most linguists are interested in morphosyntax and semantics and many (wrongly) view phonology as unconnected to those goals or as inherently less interesting (see the next chapter on prosody for an effective refutation of that idea). Another reason is that field phonology requires a well-developed skill, namely, phonetic ability, i.e. ability to both produce and recognize sounds of languages in field situations, in particular well-developed auditory discernment. Field phonologists must have good ears. Instruments, while necessary, are not sufficient. Ears are necessary and nearly sufficient (but not quite).

But all linguists can do high-quality field phonology. They can *develop* their ears and use acoustic analysis as an aid (and much more!). The purpose of this chapter is to help the field linguist provide his or her colleagues with a useful, convincing, and testable analysis of the segmental phonology of the language under investigation.

4.2. THE PHONEME

A phoneme is a set of sounds that speakers hear as the same sound for reasons of grammar. Because it is label for a set of sounds, a phoneme is, properly speaking, never heard.²²

²² In the late 1980s I was asked to visit a newly arrived linguist who was working on a language in the state of Amazonas, Brazil, to help him get started on analysis of the language he wanted to work on. So I flew to the nearest airstrip and then, with an Indian guide, hiked about 8 hours through swamps (walking through water up to my chest), over logs, up and down the banks of many streams, with 'cut grass', thorns, bugs,

At one time, many linguists likely thought that generative phonology (Chomsky and Halle (1968, ---)) had done away with phonemes. But as researchers in Lexical Phonology (see Mohanan ()) pointed out, the phoneme, or something like it, needs to be retained in phonological theories for at least two reasons. First, native speakers have intuitions of phonemes. Second, phonemes are theoretically significant (in some theories) and practically important in orthography design. In Lexical Phonology (Mohanen ()), for example, the output of the lexical rules is roughly equivalent to a pre-generative phonemic representation. Also, discussing the segmental phonology in terms of phonemic analyses is perhaps a more theory-neutral way of presenting research (though still problematic, see ____).

On the other hand, there is no standard terminology or theory that a field linguist should feel obliged to conform to except that which produces quality and clarity of analysis and presentation. Finding generalizations and understanding the system, whether as rules, constraints, principles, or all three, is what the field linguist is after. Phonemic analysis is just one way of getting at this goal.

The field linguist should, however, be aware that his or her phonemic analysis has ethical and political, as well as scientific implications, especially if it turns out to be the first graphic representation of the language. This is too often overlooked. Entering a community as a linguist will, in many communities, mean that some see you as an expert whose work should be taken seriously. So as you conduct your phonological analysis many are likely to take your proposals more seriously than you might have imagined. To give an example of a problem and how it might develop, let's say that in your early efforts you confuse a morphophonemic rule with a phonemic rule. This could have serious implications for the community. To see how, consider the hypothetical analysis of English nasals, beginning with the facts in (1) and (2):

- (4.1) a. It is pink. [p^hiNk]
 b. I want a pint. [p^haⁱnt]
 c. He is a pimple. [p^hImp;]

- (4.2) a. It is inconceivable. [...Nk...]
 b. It is intolerable. [...nt...]
 c. It is impossible. [...mp...]
 d. It is illegible. [...ll...]
 e. It is inordinate. [...n...]
 f. It is unrealis. [...rr...]

etc. plaguing me along the way (plus an impatient guide who could not believe how slow I was or how frequently I fell down). Arriving in the village where the linguist was beginning work, we began to discuss the language. 'How is the phonology coming?' I asked. 'Nothing to it', came the answer. 'Why is it so easy?', I asked skeptically. 'Oh, because you can *hear* the phonemes' came the answer. This person was a friend so I did not strangle him and toss him out in the swamp for the wildlife. I realized that this person who had made it into a good graduate program and had come this far, was in need of remedial phonology instruction. You cannot hear analyses; you cannot hear phonemes; you cannot even hear allophones until after you have done an analysis. I may strangle the next person, though, who tells me this. I am older now and less patient.

The linguist will notice from examples like () that nasals and following obstruents are homorganic, i.e. that they share the same place of articulation. And the same is apparently true of the examples in ().

Now let us further assume that the linguist has found, say by minimal pairs or near-minimal pairs (see ____) like those in (), that the nasals in question are separate phonemes:

- (4.3) a. sing [siN] (at least in my Southern California dialect)
 b. sim [sIm] (as in 'my phone needs a new SIM card')
 c. sin [sIn]

- (4.4) a. mother
 b. nother (as in, 'Nother?')
 c. --- (no word-initial /N/ in English)

Based on (4.3) and (4.4), the linguist is reasonably sure that the nasals are each separate phonemes. Then, based on these facts, they could, wrongly, conclude, wrongly, that when immediately preceding an obstruent, the place of articulation contrast between nasals is neutralized. One might propose a rule along the lines of ():

(4.5)

	+nasal	→	[βplace]/	-sonorant
	αplace			βplace

But this would be wrong, problematic, and misleading. It would predict that there is only one kind of underlying representation and only one phonological process involved. To see why this is mistaken, compare the examples in () and ():

- (4.6) a. unobtrusive
 b. unreal (cf. *urreal)
 c. unpopular (cf. ?/*umpopular)
 d. untouchable
 e. unkillable (cf. ?/*uNkillable)

If rule () is correct, why does it fail to apply to the examples in ()? Well, it doesn't apply to () because it is incomplete. There are two 'neutralization' processes for nasals in English. The first is a constraint on nasal + obstruent sequences within morphemes and the other is a constraint on nasal + obstruent sequences across only a certain set of morpheme boundaries (or alternatively, only affecting certain morphemes, e.g. {in-}, but not {un-}).

It is true that the linguist could fix the initial bad analysis on their next visit (see ____ on morphophonemic analysis). What is the big deal? Just how could this possible become an *ethical* problem?

The latter could arise if the linguist presented his or her analysis to the community too hastily and, if consequently, orthographic decisions were made on the basis of it (e.g. if some more progressive elements of the community began to write their language as the linguist represented it to them). If the linguist then says 'Whoops, I made a mistake. We need to redo the writing system, folks.' This would lead to

confusing representations or difficulty in teaching native speakers to read their own language. To attempt to undo this harm would require telling the community that the linguist was wrong and that they were going to need to retool and relearn how to write their own language. This would likely seriously undermine community confidence in the literacy process. It could even contribute to a feeling in the community that their language is inferior to the national (etc.) language, since, as far as the community knows, no such inconsistencies are ever found in teaching the larger or more prestigious language. These are ethical problems. The political issue is that it is the community, not the linguist, that must eventually agree on and adopt a particular orthography. The linguist must avoid inadvertently or purposely bypassing the community in introducing the orthography. In fact, they should have nothing to do with such issues unless invited to do so by the community. And they should participate only when phonological analysis has been carefully refereed by other linguists, whether or not it is published. We want to do it right, as right as we can, and avoid such problems. That is part of what this chapter is about.

The chapter is organized as follows. First, I present the standard methodology for 'phonemic analysis'. Then I consider potential problems and shortcomings of this procedure, supplementing it with suggestions based on modern phonological theory. This is followed by a section considering segmental phonology in the wider grammatical context. Some suggestions are then made for phonetic fieldwork, though this section is small and I refer the reader to the best source available anywhere on phonetic fieldwork, Ladefoged ().

4.3. METHODOLOGY

4.3.1. Caveat

There is no guaranteed way of analysing anything in any language. You could in principle come up with an excellent analysis without following anyone's suggestions, due to luck or genius. Or you could fail to come up with a satisfying analysis after trying all the methodological suggestion of this and other books on field methods. All things being equal, however, it is best to plan to use a tested methodology rather than to rely exclusively on being a genius.

4.3.2. Train the ears

I would recommend that the first step the linguist take before trying phonological analysis in the field is to train their ears for transcribing sounds and prosodies. One form of training is to listen to tapes of other languages and practice imitating them and transcribing them, preferably having someone who knows the language make the corrections. A related form of training is to find speakers of other languages in one's hometown, university, etc. and practice fieldwork with them. You can also train your ears if you play a musical instrument by learning new styles of music 'by ear'.

Another training method I have found very useful is to memorize texts of the language under study before learning what they mean. Then repeat these texts to native speakers and see if or how well they understand you.

It is also useful to eavesdrop and then imitate what you hear (assuming that no one minds and that you never record anyone without permission).

An additional means of hearing and pronunciation training is to compare spectrograms of your pronunciations of words of the language under study with spectrograms from native speakers' pronunciation of the same strings of words or

sounds. The linguist then practices and works to make his or her spectrogram, wave form, etc. look more and more like the native speaker's.

Last but not least, perhaps the best way to train your ears for phonological analysis is to learn to speak the language (see chapter ____). This is not only useful ear-training, it is of inestimable value for all your linguistic objectives and is important socially and culturally.

However, one thing that is less than useful, except for some aspects of prosody, is to rely for your practice of the language on analog recordings in the absence of native speakers. The reason for this is that analog recordings simply are not good enough for distinguishing many sounds, especially voiceless consonants.

4.3.3. Chart all sounds

It is essential that all sounds transcribed be charted in a standard phonetic chart. The sounds of the phonetic chart can be referred to as 'surface segments' or, to use a more traditional term, 'phones'. (Following analysis, the linguist produces another chart. This one is of 'distinctive segments' (or 'underlying segments', or, depending on your analysis, as 'phonemes'), which are those from which the linguist believes all related allophones are derived or which they believe is a useful label for the particular set of sounds.)

Once the linguist has charted the surface segments they have encountered, analysis should begin. Do not wait. Analysis of the segmental phonology should begin on the first day of linguistic research, though it might not be concluded for several weeks or much longer. As one proposes analyses to oneself, one better equips oneself to look for more and better data on the phonology. Your analysis feeds and enhances your fieldwork. So it should begin immediately (and this is true for all areas of analysis). The first thing I do in studying the phonology of a 'new' language is to indicate on my chart of surface segments what I consider to be suspicious pairs and suspicious segments. A 'suspicious pair' of sounds is any two sounds that differ in no more than one manner or one place of articulation. A 'suspicious sound' is one that can serve both as a syllable nucleus or a syllable margin (i.e. onset or coda). Once you have these indicated to yourself you are ready for the next stage of analysis. Before going into that, however, let's 'walk through' an example of what is suggested in this paragraph.

So let us say that you are working on Pirahã. You will notice immediately that there are very few surface segments in the language. They are shown in the charts below (one for consonants, one for vowels).

Table One
Pirahã Surface Segments

Consonants

p	t	k	ʔ
		kʷ	
p̥	t̥		
p̥			
b		g	
b)	l&7		
m	n		

s	S	h
		h ₂ (with movement of tongue towards s position)
w	y	

Vowels

i	
I	
E	o
	a

So what is suspicious in the charts above? First, all the voiceless bilabials are suspect because they differ in no more than one manner of articulation. The voiced bilabials likewise. The weird lateral, [l&7] (see Everett (1982)), and [n], the two [h]s, the two sibilants, [s] and [S], and the front vowels should all be tested as groups to see if they are separate distinctive segments or members of the same phoneme (allophones). Second, the segments [w] and [y] are both suspect because they could simply be the phonetic realization of nonlow vowels ([i] and [o]) in onset position (Pirahã has no codas). How do we check these? Well, according to standard structuralist methodology, we first look for 'minimal pairs' or *Contrast in Identical Environments* (CIE). Herbert Landar (1980 IJAL 46:228) summarizes the structuralist viewpoint well when he says, "as with vowels and consonants, so with stress, pitch, and juncture: one minimal pair certifies phonemic integrity." CIE is defined in (4.7):

(4.7) CONTRAST IN IDENTICAL ENVIRONMENTS: Two sounds contrast in identical environments if and only if they are found in at least one set of two words that differ in meaning, where the only sound difference (segments and prosodies) distinguishing the words of the set are the sounds in question, and where the latter appear in exactly corresponding positions in each of the words in the set. The set is referred to as a *minimal pair*.

An example from English:

- (4.8) a. bit [bIt]
b. beet [bit]

We will return to problems with this notion directly. But what if the linguist cannot find a minimal pair or CIE for a pair of sounds? Then they should look for *Contrast in Analogous Environments* (CAE) which can be defined as in (4.9):

(4.9) CONTRAST IN ANALOGOUS ENVIRONMENTS: Two sounds contrast in analogous environments if and only if they are found in at least one set of two words that differ in meaning, where the sound differences (segments and prosodies) distinguishing the words of the set are the sounds in question plus some other sound(s), where the former appear in nearly the same corresponding positions in each of the words in the set and any other different sound(s) is(are) unlikely to be responsible for the differences in the manner or place of articulation of the sounds said to contrast.

- (4.10) a. spit [spIt]

b. Pete [p^hit]

Example (4.10) is CAE because the words compared differ in meaning and for the sounds in question (the vowels [i] and [I]), as well as the additional [s] at the beginning of (a). This [s], however, is unlikely to be responsible for the fact that the vowels in question differ as they do, i.e. by the feature, [Advanced Tongue Root] ([ATR]).

If the linguist finds neither CIE nor CAE, then they should turn to consider that the two sounds in question are in *Complementary Distribution* or, in other words, in a relationship of allophony. Complementary distribution is the Superman vs. Clark Kent relationship (The reason Lois Lane suspected Clark Kent of being Superman was that he was similar to Superman and he was never in the same place at the same time as Superman):

(4.11) COMPLEMENTARY DISTRIBUTION: Two sounds are in complementary distribution if one is found in a specific phonological environment and the other one is never found there.

Let's examine this notion in more detail. Recall from introductory phonology courses that there are two forms of complementary distribution, namely, *ambient* and *positional*.

Ambient conditioning refers to changes in the sound under study (or, alternatively, selection of an allophone of the phoneme under study) effected by the 'contamination' from surrounding sounds. Consider the English example in (4.12):

(4.12) can't [k^hQ)nt]

In this case, the oral vowel, /Q/, has been contaminated by the nasalization of the following nasal consonant. This is known as regressive assimilation (i.e. it spreads 'regressively', from right to left).

A further, well-known type of ambient conditioning is illustrated in Turkish, i.e. Vowel Harmony. Cases of Vowel Harmony can be very interesting because they show that conditioning can take place from non-adjacent items. Consider the following examples, which are standardly understood to illustrate the features of vocalic backness and lip rounding:

(4.13) Turkish Vowel Harmony: ip—in 'rope'; kız—in 'girl'; yüz—ün 'face'; pul—un 'stamp', etc.

Epenthetic vowels (vowels inserted to satisfy constraints on syllable structure usually) also undergo Vowel Harmony, receiving rounding and backness specification from neighboring vowels according to the same rules, as shown in (3).

(4.14) a. /koyn/ → koyun 'bosom'
b. /metn/ → metin 'text'
c. /sabr/ → sabır 'patience'

In this case, assimilation is progressive, the vowel to the left triggering harmony on the vowel to its right (though not necessarily its immediate right, just the first vowel to the right).

Assimilation may in some cases produce a slightly different kind of result. An alternative to simple harmonic assimilation, a more complete version, if you will, is Vowel Coalescence, i.e. where two vowels merge to form a single vowel. Consider the following examples from Wari' Vowel Coalescence (Everett and Kern (1997, 331)):

- (4.15) a. $i + i = i$
 b. $xi' + \text{-in} = xin$ [tSin]
 1pincl:rf -3sn
- (4.16) a. $i + e = i$
 b. $xi' + \text{-em} = xim$ [tSim]
- (4.17) a. $i + o = u$
 b. $'iri' + \text{-on} = \text{urun}$ [Y'RYn]
 1pincl:rp/p-3sm clitic cluster
- (4.18) a. $i + u = u$
 b. $xiri- + \text{-u} = \text{xuru}$ [tSY'RY]
 house- 1s 'my house'
- (4.19) a. $e + i = ei$
 b. $je + \text{-in} = \text{jein}$ [/y)eⁱ]
 2p:rf -3n clitic cluster
- (4.20) a. $e + o = u$
 b. $\text{hwe} + \text{-ocon} = \text{huhun}$ [hY'hYn]
 2p:rp/p -3pm clitic cluster
- (4.21) a. $e + a = e$
 b. $\text{hwe} + \text{-am} = \text{hwem}$ [h^wIm]²³
 2p:rp/p -3sf clitic cluster
- (4.22) a. $a + i = ai$
 b. $\text{wita-} + \text{-in} = \text{witain}$ [wi'tāⁱ]
 mat -3n 'its mat'
- (4.23) a. $a + e = e$
 b. $'ina + \text{-em} = \text{'inem}$ [/i'nIm]
 1s:rp/p -2s

²³ The phonetic form differs from the phonological form, /e/, here, but is a natural realization of /e/ before /m/.

- (4.24) a. a + o = o
b. ta + -on = ton [ton]
1s:rf -3sm
- (4.25) a. o + u = u
b. toco- -um tucum [tY'kYm]
eye- -2s 'your eye'

In general, what is happening is that the lower vowel is deleted and the higher vowel remains, except with [ei] and [ai]. This example is like vowel harmony except that that 'harmony' results in a blending of the vowels into one.

Positional conditioning is change produced by the location of the sound in question in the phonological or morphosyntactic constituent structure of the utterance, rather than by other sounds per se. Consider aspiration in English:

- (4.26) a. paper [p^heɪp̚]
b. tart [t^hart]
c. call [k^haL]
d. cheap [tʃ^hip]

Or consider the change in vowel quality effected by the vowel's location relative to stress placement in the word:

- (4.27) a. photo ['foto]
b. photography [fo'tɔgrə'fi]
c. photograph [fotə'grɒf]

These are the standard methodological teachings that linguists have received over the years. And this methodology is extremely useful, which is why I have given space to it here. All field linguists should familiarize themselves with it. In the next section, however, I consider some problems with this methodology and offer additional suggestions for phonological analysis.

4.4. The Structuralist Methodology Reconsidered

4.4.1. Reconsidering minimal pairs

In his important monograph of 1968, Postal points out that a major weakness of 'autonomous phonemics' (the source of the methodology just introduced) is its inability to allow exceptions. This observation has several applications, but the situation I am referring to here is where linguistic generalizations are either not observed or not described because the linguist has found apparent minimal pair contrast. Specifically referring to minimal pairs, Chomsky puts his finger on a crucial problem:

'In general it should be observed that 'minimal pair' is not an elementary notion. It cannot be defined in phonetic terms but only in terms of a completed phonemic analysis ...' (Chomsky 1964:97).

And Postal (p28) paraphrases this to say, '... contrary to almost every introductory exposition of autonomous phonemic theory or practice, the discovery of

phonetically minimal pairs does not *necessarily* [emphasis Postal; D.L.E.] permit an immediate conclusion about underlying phonological contrast'.

Postal goes on to argue, to me successfully, that analyses should be based on systematic regularities rather than 'static' exceptions. We do not thereby eliminate minimal pairs from analyses, but rather we bring the principle of their application into proper perspective.

Chomsky and Halle develop this notion further, as seen in statements such as: "Clearly, we must design our linguistic theory in such a way that the existence of exceptions does not prevent the systematic formulation of those regularities that remain" (1968:172).

The structuralist methodology from which CIE, for example, emerges has no elegant way of recognizing exceptions. What is clear is that regularities are the basis for linguistic analysis. Minimal pairs might, in fact, illustrate such regularities, in which case by all means use them. Or they might be useful predicting subregularities or creating doubt. Doubts refine hypotheses. But doubts caused by a single minimal pair, for example do not require, say, complete redesign of an orthography on the basis of two words.

Contrary to the Landar quote above, we do not 'certify' results by a methodological 'proof'. We do not do this in phonology, fieldwork, or science generally. Let's consider a couple of minimal pair problems to drive this point home.

Empirical and epistemological extensions

The misuse of minimal pairs has various empirical consequences. Consider the following examples.

Pirahã

Previous analyses of Pirahã tone (see Heinrichs () and Sheldon ()) based themselves primarily on series of minimal pairs. The following are some examples used to support the previous analysis proposing three phonemic tone levels.

- | | | |
|-------------------|------------------------|------------------|
| (4.28) [/a#òi] | MLH 'hand' | H, ' = high tone |
| (4.29) [/a#o#i##] | MMM 'ear' | M, - = mid tone |
| (4.30) [/àòi#] | LLM 'foreigner' | L, ` = low tone |
| (4.31) [/àòi] | LLH 'skin' | |
| (4.32) [/a#òi] | MLL 'Brazil-nut shell' | |
| (4.33) [/a#ói#] | MHM 'basket' | |

In a theoretical framework which allows "minimal pair shortcuts," it is clear that (4.28)–(4.29) argue strongly in favor of a three-tone analysis. But this is why minimal pairs can be dangerous.

Closer examination of this series, however, reveals that between the [o] and [i], in each of these examples a semivocalic glide [w] occurs. This is rather uninteresting phonetically since such a glide is pretty much expected in this position for obvious reasons. However, as any first-year linguistics student knows, such a glide has (at least) three possible interpretations: (i) this could be perceived as simply a trivial transition effect, needing nothing more than a footnote; (ii) the glide could be functioning as a consonant /w/ in the language; (iii) the glide could in fact be a vowel in underlying representation.

The determination of which option a–c is correct will have serious implications for the analysis of Pirahã. In my analysis, option c was selected. This not only allows for prediction of stress placement (Everett 1988), but notice the change in the tonal patterns of (4.34)–(4.39) (where [w] carries tone because it is an underlying vowel. Compare Everett, Forthcoming, for details.)

- (4.34) [/a#o\$w@í] MLHH ‘hand’
 (4.35) [/a#o#w#i #] MMMM ‘ear’
 (4.36) [/àòw#i#] LLMM ‘foreigner’
 (4.37) [/àòw@í] LLHH ‘skin’
 (4.38) [/a#òw\$ì] MLLL ‘Brazil-nut shell’
 (4.39) [/a#ów@ì] MHHL ‘basket’

These representations then allow us to write rules to derive the mid-tone from perturbations affecting both high and low tones. These rules are given below. This phonological decision completely alters our initial perspective of the minimal pairs in ()-(), showing that the concept of what is 'minimal' depends on our analysis.

Pirahã Tone Rules

TONE LOWERING:

$$(4.40) \left[\begin{array}{c} +\text{high} \\ -\text{low} \end{array} \right] \rightarrow [-\text{high}] / \left[\begin{array}{c} [+ \text{low}] \\ \# \end{array} \right] \text{ — } [+ \text{low}]$$

TONE RAISING:

$$(4.41) [+ \text{low}] \rightarrow [-\text{low}] / \left[\begin{array}{c} [-\text{low}] \\ \# \end{array} \right] \text{ — } \left[\begin{array}{c} [-\text{low}] \\ \# \end{array} \right]$$

Asyllabification:

$$(4.41) \left[\begin{array}{c} +\text{syl} \\ +\text{rd} \end{array} \right] [-\text{syl}] / \left[\begin{array}{c} +\text{syl} \\ +\text{rd} \end{array} \right] \text{ — } [+ \text{syl}]$$

And these rules are important for Pirahã since they also explain the rarity of tone 2, a mere 'allotone' or surface variant, according to this analysis.

Tonal displacement

A further example of empirical problems for a minimal pair analysis may be seen in languages which manifest what has been termed “tonal displacement”:

Richardson (1971) discusses a phenomenon which he calls displacement, whereby tonal contrasts are realized several syllables to the right of their original position. The words [ný-kòlò] 'sheep' and [ný-kòlò] 'heart' in Sukama differ, in that 'heart' etymologically carried a high tone on the last syllable. Both are pronounced identically in isolation. However, consider the following forms:

- (4.42) a. ný-kòlò ný-tàalè ‘big sheep’
 b. ný-kòlò ný-tàalé ‘big heart’

The original tonal contrast is realized on the adjective big (Hyman and Schuh 1974:103).

Hyman and Schuh proceed to give a feasible account of this phenomenon, with 'sheep' and 'heart' contrasting in underlying form. Note that such an analysis would have been 'messy' or very difficult to state in a structural framework wherein preanalytical minimal pairs are phonetic units 'certifying phonemic integrity'.

Portmanteau

A final example in support of the thesis expressed in this paper is the phenomenon known as Portmanteau (or Coalescence, as seen in the Wari' examples in ()-()). A discussion of this is found in E. V. Pike (1974a:24):

A portmanteau phone is one surface sound which is produced by the 'fusion' of two underlying or distinctive segments. A unique phone, the one not part of a symmetrical pattern may turn out to be a portmanteau phone. When the units which make up the portmanteau phone are recognized, their occurrence should help to make symmetrical one of the nonsymmetrical patterns.

Pike gives the following examples (among others) of Portmanteau:

'Harris (1951:92) describes a flapped nasal which occurs in some environments in some dialects of American English (as in, for example, painting) as actualizing the sequence /nt/.

In Quiotepec Chinantec, the sequence /mĩ/ is actualized as a syllabic bilabial nasal (Robbins 1961:245).

In Ayutla Mixtec, the sequence /ae/ is actualized as [æ] when following an alveopalatal consonant (Pankratz and Pike, E. V. 1967:289)....'

Discussion

One might possibly respond to this data by saying, "Well, the initial minimal pairs were incorrect. As a matter of fact, once the proper forms were defined the contrast became obvious." But this reasoning is fallacious. The 'correct form' of these minimal pairs was determined *phonologically*, not phonetically. That is, no phonetician could have told us that the semivocalic glide from [o] to [i] in the Pirahã data was an underlying /o/ (which carries tone). Nor is the phonetic data sufficient to determine the presence of a displaced tone in the Sukama examples. Portmanteau is even more effective in revealing the shortcomings of CIE/minimal pairs because sounds are 'lost' on the surface.

Another way of thinking about this is in terms of the more general notions of *dependent* vs. *independent* variable. These are defined in () and (), respectively:

(4.43) DEPENDENT VARIABLE: The item in an experiment or study whose changes are effected by one or more independent variables.

(4.44) INDEPENDENT VARIABLE: An item manipulated in an experiment or study that effects change in the dependent variable.

To give an example, let us assume that we are studying the nasalization of vowels. We might ask whether the nasalization on the vowel, the dependent variable, is caused by the nasalization of the consonant, the independent variable according to this hypothesis.

- (4,45) a. can't [k^hQ)nt]
 b. cat [k^hQt]

In (), the [n] is the independent variable because we are manipulating it to see if it causes the nasalation of the vowel [Q], which is our dependent variable. From this we can draw the modest conclusion that it is ambitious, to put it mildly, for the fieldworker to believe they have controlled and distinguished are variables in any pair of words at the beginning of field research. Rather, the understanding and recognition of such variables comes, if ever, only after a significant amount of rigorous analysis. Therefore, CIE, CAE, and CD, like other methodological notions, do not provide us with analytical algorithms but with heuristic procedures, rules of thumb.

To summarize what we learn from the cases above, the lesson is simply that if the fieldworker had stopped with the superficial phonetic form he could, according to structuralist methodology, have considered that:

- a. Pirahã has three tones and (by further data) unpredictable stress placement.
- b. Languages like Sukama have arbitrary allomorphs in grammatical sequences.
- c. Languages with portmanteau phenomena have arbitrary patterning in their phonemic inventories.

This type of data further illustrates the difficulties and dangers of minimal pair analysis for field linguists. Carrying the conclusion a step further, we might even say that a good analysis should determine minimal pairs rather than vice-versa. We need to remind ourselves that methodological suggestions are just that, suggestions. Ultimately, they are the 'icing' on a 'cake' of theoretically and typologically-informed analysis.²⁴

²⁴ As an epistemological aside, it might be noted that minimal pairs represent the effects of empiricist philosophy in linguistics. That is, they are the vestiges of the naive notion that “proofs” exist in science, in general. As Chomsky has frequently observed, data by itself is not sufficient for criticism of a given theory. Rather, one must say something about the data, which, by its very nature, is a theoretical activity. To criticize analysis a, for example, it is not enough to merely present contrary data. It is also necessary to (1) show how an analysis b would treat the data more effectively, and (2) how analysis a cannot be extended to handle this “extra information.” Pure inductivism is a dead-end road.

In astronomy, a researcher might criticize a colleague's theory by noting that light rays and planetary motion in a particular section of the galaxy do not conform to this colleague's theory. Then the colleague may simply respond by saying, “Well, there's this thing called a ‘black hole’ up there which, although invisible, exercises an effect.”

So, let's get some money from NASA and send up a rocket to check out the story. No black hole! Now we've got him! But, when presented with this new evidence, the shameless fellow replies, “You didn't find evidence of a black hole because your instruments were fouled up by magnetic clouds in the area (this example is largely from Marcelo Dascal, verbal communication).”

This type of thing can go on and on unless colleague b gets fed up and says, “Listen—I have had it with your old fairytales. I have developed a theory which

4.4.2. Effect of change

Fieldworkers can also be confused by historical change that has left synchronically strange effects in the grammar. As an example, consider once again the chart in () of Pirahã surface segments. Because the pairs [g] and [l&7] and [g] and [n] differ by multiple points and modes of articulation, they are not suspicious pairs so the fieldworker is unlikely to suspect them of being potential allophones/surface variants. And yet, they are surface variants of a single distinctive sound, as per the rules in ():

- (4.46)
- a. $\left[\begin{array}{l} -\text{continuant} \\ +\text{velar} \\ +\text{voiced} \end{array} \right] \rightarrow \left[\begin{array}{l} \mp\text{nasal} \\ +\text{coronal} \end{array} \right] / \# ___$
- b. $\left[\begin{array}{l} -\text{continuant} \\ -\text{syll} \\ +\text{velar} \end{array} \right] \rightarrow \left[\begin{array}{l} +\text{continuant} \\ +\text{lateral} \end{array} \right] \left[\begin{array}{l} /+\text{syll} \\ +\text{high} \\ +\text{round} \\ +\text{voiced} \end{array} \right] ___$
- c. $\left[\begin{array}{l} +\text{consonantal} \\ -\text{continuant} \\ +\text{voiced} \\ -\text{labial} \end{array} \right] \rightarrow [+ \text{posterior}] / ___$

This strange rule is, according to Everett (1979), the result of a historical process in which a diachronically prior sound [d] shifted to [g], while the remaining two sounds of the original phoneme remained as [+coronal]. This may be due to pressure to 'disperse' sounds more effectively to aid speaker perception (see X (), Y () for further development of the concept of 'dispersion'). But it is not expected from a purely synchronic view and, therefore, would pass beneath the radar of just about any methodology.

Another interesting case comes from the Ge language, Suyá (Kisedje). Consider the process of lenition in this language (Everett () and Foresti ()):

- 1) /t/ → [R] / _____ phrase boundary
- 2) /p/ → [w] / _____ phrase boundary
- 3) /k/ → [g] / _____ phrase boundary
-
- [tEp] → ['tEw^E] 'fish'
- [k^het] → [k^heRe] 'no' (negation)
- [s√k] → [s√g] ~ [s√k] ~ [s√k^h] ~ [s√g[√]] 'bird'

explains all of these phenomena, simply and satisfactorily without black holes, magnetic clouds, and so forth.”

So it is with minimal pairs. They are only acceptable as evidence within a theory. The lack of a theory rules all of the data in the world irrelevant.

Notice that although the coronal, /t/, and bilabial, /p/, stops lenite to continuants, the velar stop, /k/, is alternatively realized as [g], [k], [k^h], and [g^v]. As Everett () points out, there is no continuant realization of /k/ in the phrase-final position, contrary to realizations for /t/ and /p/ in this position, because there is no distinctive segment that is velar and continuant. According to Everett (), this process is structure-preserving, that is, it can only substitute one distinctive segment or phoneme for another. Since /R/ and /w/ are both distinctive segments in the language, they may be substituted for /t/ and /p/, respectively, in phrase-final position. But since there is no continuant velar distinctive segment, /k/ behaves differently, as already seen. This is interesting because it shows that some constraints (Structure Preservation in this case, see Kiparsky ()) may prevent rules from being maximally general, contrary to the standard fieldwork methodology, and it also shows a possible case of a change in progress. In other words, the behavior of /k/ could be seen as asystematic by new generations of Kisedje/Suyá and this could lead to a new phoneme (e.g. [F], which is continuant and velar) or to a removal of the Structure Preservation constraint). In any case, it is a case that is not completely compatible with the simple methodology in ---. So, once again, although that methodology is very helpful, one must be very careful to analyze and argue carefully for conclusions. And careful argumentation and analysis require knowledge of the literature, typological, theoretical, and descriptive.

For another caveat on the use of the standard methodology, see section ____ of chapter ____ on phonology and culture.

4.5. Orthography design

Phonological analysis is a necessary condition for orthography design, but it is not a sufficient condition. As is stated in ____ the choices involved in designing the visual representation of a language involve community values and discussion. And various other issues, such as non-trivial, but conflicting cultural values.

For example, do the people want to read in the first place? Do they want to read their language or the national language or both (each of these questions has implications for the literacy programme). Do they want their language to look like the national language or would they prefer that it look different, unique to them? Do they want the linguist's advice or not? Should the linguist offer advice even if it is not asked for (the linguist will have to answer this based on their understanding of the culture in question).

In 1980, Keren and I dedicated every night, six nights a week, to literacy and math classes among the Pirahãs, at the Pirahãs' request. Near the end of this time, we finally succeeded in getting the Pirahãs to read a word. We wrote the word **bigí** on the black board we were using. Everyone read the word out loud. Then they all started to laugh (about 30 people were present). I asked 'What's so funny?' They answered 'That sounds like our word for 'ground'. 'It IS your word for 'ground'', I replied. 'Oh, no' they said, 'We don't write our language. Is that what you are trying to teach us? Oh, we don't want that.' And they never returned to literacy classes.

Many agraphic societies will recognize the value of reading and writing their language, though certainly not all will. If you have an opportunity to contribute to literacy or other educational goals of the community, especially as these goals implicate your linguistic skills, then you, qua linguist, are making a valued contribution to community life. This is both personally rewarding and community-empowering.

Therefore, the fieldworker should feel privileged to contribute in this way. But at the same time, the fieldworker must take this privilege very seriously, recognizing the responsibility that it entails.

4.6. Back up your analysis with sound files

Until very recently it was impractical for the average reader of a grammar or phonological analysis to have the option of listening to the sounds upon which an analysis was based. Audio recordings would have been made on analog tapes stored exclusively at the linguist's home institution (by and large). However, digital files, servers, and the internet make it both possible and desirable for all phonological analysis to be backed up empirically such that any reader can replicate the measurements and analysis. This is a significant improvement over 'impressionistic', take-the-linguist's-word-for-it analysis. Sound files should be stored in .wav format (see the E-MELD 'best practice' page at: <http://emeld.org/school/>). For any phonological data archive or published study, each datum or analytical point should be accompanied by an address where the sound file which supports it can be downloaded.

4.7. Phonology in the wider context of the grammar

The procedures for segmental phonological analysis sketched above are incomplete because they treat phonology as an autonomous module unconnected with the rest of the grammar. However, as the examples from English in ()-() illustrate, morphosyntax can be causally implicated in phonological analysis. Although culture is also occasionally implicated in phonological analysis, this is treated in chapter _____. Here I want to consider ways in which morphology is crucial to understanding phonology. We begin by considering what Everett () refers to as encliticization, also discussed in Sheldon ().

Pirahã Encliticization

(4.47) tîi /ísitói /òògàbàgàí → tîi /ísito&ògàbàgàí
 lpsg egg want
 'I want an egg.'

(4.48) kàhàí/áàgàhá → kàhàa&àgàhá
 arrow is
 'It is an arrow.'

In these examples, we see that the final vowel of the first word, the verbal object, deletes, as does the initial glottal stop of the following verb. However, the tone does not delete, as shown by the rising tone on the vowel to the right of the deletion site in both examples above. This means that the only way to get at the correct analysis of Pirahã tones, i.e. that they are underlyingly level and not contour, is by taking into consideration the effect of 'cliticization' on the surface forms of the tones. If the forms to the right of the arrows were analyzed without regard to the forms to the left of the arrows, the tonal analysis would have severe problems.

Moreover, as Pike and Fries () observe in their pioneering study, loan words can affect the phonological system of a language. So, for example, consider the phoneme /Z/ in English. This sound only occurs in loan words, e.g. *azure*, *garage* (some dialects), *fromage*, etc.

4.8. PHONETICS FIELDWORK

This section is a brief and humble introduction to how to begin to answer questions of phonetic theory. Maddieson () and Ladefoged () provide excellent introductions to phonetic field research, the latter a book-length survey and this section is meant as little more than an annotated reference to those works.

There are two broad domains of phonetic research: the nature of contrast, i.e. how the sounds of a language are perceived as different by native speakers, and variation, i.e. how sounds change diachronically and synchronically. Phonetics fieldwork in some senses underwrites or warrants all other fieldwork. If the phonetics (articulatory or acoustic) is bad, the entire documentary-descriptive effort is bad. But phonetics is not, as some people might think, the first type of fieldwork undertaken by the fieldworker. Before you begin serious phonetic analysis, you should have other analyses well under way.

Moreover, with phonetic fieldwork, as with all types of research, you must first be clear on your methodology, purpose, and technology. As an example, let's say that you want to study the contrast between two closely related sounds. You will have already established that these sounds contrast phonologically. You should next look to your dictionary or word list in order to find systematic sets of examples to test. If you do not have a sufficiently complete dictionary to arrange the necessary examples, William Poser has written a programme that will generate lists of hypothetical words, based on an input of the language's syllable structure and segmental phonology.

What kind of word list do you need? What are the guiding principles in devising a word list for phonetics field work? The basic idea is quite simple: the linguist wants to construct examples for testing in which the dependent and independent variables are properly controlled. That is, you the linguist want to be sure that you are studying what you think you are studying. Consider how you might go about studying the difference between a voiced stop and a voiceless stop. First, assemble words with the sounds in minimal or near-minimal pairs (see ____ above). For example, suppose that you want to study the contrast between /g/ and /k/. Word lists like those in () and () would give you a reasonable start (where I assume that all words differ in meaning).

(4.49) gabi, bagi, gut, tug, grat

(4.50) karg, garg, gark, kark, kãg, gãk, etc.

(4.51) kabi, baki, kut, tuk, etc.

That is, each segment to be tested should be recorded preceding and following all vowels and all consonants and in word-medial, initial, and final positions. Once this is done, if the recordings are of adequate quality (see ____ below) and quantity, then you have the basis for comparing spectrograms of the two segments. Likely, the most interesting distinctive phonetic process you will find in this case is Voice Onset Timing. However, perhaps you have reason to believe that the articulation of the sounds is also different in some way. You may want to make palatograms of each of them. (How to do this is described in ____ below.) You may want to film speakers or use ultrasound, etc., depending on where you are at and what kind of research budget you have. I turn now to consider technical aspects of phonetic and phonological studies.

Recording

First, as mentioned in ____ above, the fewer the moving parts in your recording equipment, especially your recorder, the less chance there is for motor hum and other intrusive sounds to contaminate your recordings. Recording on solid-state digital recorders or directly onto your computer is therefore recommended over even quality digital recorders that use tapes, compact disks (CDs), or digital video disks (DVDs). Second, your microphone should be a dynamic, uni-directional mic, attached to a headset. (A capacitor microphone would be better if you were particularly interested in studying low-frequency sounds.) The purpose of the headset is to ensure a constant distance and angle between the microphone and the native speaker's mouth. Hand-held or table-top microphones are notorious for registering false differences in loudness, for example, as the speaker turns their head while recording or they hold the microphone at different distances from their mouth while speaking.

Care must be taken too to ensure that the recording volume on your recording device is set to maintain a clear signal to noise ratio (see ____ for a discussion of this ratio). That is, you do not want the recording volume so high that you get distortion at the upper ranges of speaker volume. But you also want the volume high enough so that the speaker's voice easily drowns out all ambient noise. The goal, visually, is to produce high-quality spectrograms, such that formants, vocal chord vibrations, nasalization traces, etc. are clearly visible. If the recording is well-controlled and in a silent environment, words will be separated on the spectrogram by white, clear space. This is almost never possible to achieve in the field environments, though approximating this ideal should be the goal.

When recording among the Banawa in 2004, I was delighted to collect the best natural conversation between two speakers that I had ever collected in all of my field research in the Amazon. Two language teachers, Sabatao Banawa and Bido Banawa, sat in front of me in a small study just out of the Banawa village (kindly provided for our use by SIL member Ernest Buller). Each wore a headset with a high-quality microphone. I recorded them onto a professional digital recorder (). I even recorded the entire session on digital video, which provided additional high-quality audio back-up as well, since most camcorders have superb microphones. Two things happened when we returned from the field, however. First, someone broke into our project office in Manchester and stole a valuable computer, and all of our video recordings from the entire first year. So I lost the video record. Next, as I looked at the audio recording of the conversation, I noticed a high-frequency band of noise going through the entire conversation. It turned out to be a cicada that I had not heard while recording, because I was so focussed on collecting the actual conversation. A bit disappointing. Nevertheless, the cicada and other ambient noise were relatively easy to work around because the signal-noise ratio was good.

An ideal addendum to your laboratory sessions would be to video-record all of them so as to capture at least all the words and expressions (facial, body posture, hand gestures) of the teacher. It would also be useful to record yourself at the same time. Your own actions, interactions, and reactions can be useful in determining the kind of feedback you might inadvertently be giving the language teacher (e.g. frowns, smiles, furrowed eyebrows, crossed arms, face in hands, etc.). All recordings should include a preface of the linguist speaking, giving at least the following information: date and time of recording, speaker name, location, objective of this recording, speaker gender and

age, speaker dialect, and speaker's level of bilingualism. Ideally, this tape should be a stand-alone document that any other linguist could use, e.g. students looking for thesis projects. Remember: backup all data files immediately and keep copies in a different building from the originals.

Palatography and linguography

Ladefoged () gives an excellent set of suggestions for field palatography. He suggests painting tongues or palates with either purified charcoal or the scrapings from burnt toast mixed with olive oil. These can be painted on the subject's palate (for linguography 'writing on the tongue') or tongue (for palatography 'writing on the palate') with a never before used or sterilized paintbrush, approximately 0.5 – 1.0 inches in width.

In my fieldmethods class at the University of Manchester, a couple of undergraduate students became enthusiastic about palatography and their fieldwork papers showed excellent palatograms. One of the students decided, however, that olive oil + burnt toast scrapings tasted 'yucky' so she used softened chocolate and butter. She got reasonable palatograms doing this. However, the disadvantage to tasty 'paint' is that it produces more salivation, smearing the palatogram and thus generally producing less useful results. So bad-tasting palatographic 'paint' may be better for science.

Linguography marks the palate and then photographs the subsequent markings on the tongue after the word or syllable has been pronounced. Palatography marks the tongue and then photographs the palate.

To photograph subjects' tongues and palates, use a high-resolution digital camera. Have the subject stick their painted tongue out and use a mirror to photograph the palate.

Airflow and air pressure (these are not the same thing) studies can also be useful in the field for more carefully examining supraglottal pressure and nasal airflow. The data collected from such studies can be essential in distinguishing stops from continuants, nasal sounds from oral sounds, and any other contrast involving the source and direction of airflow (e.g. pharyngeal vs. pulmonary air, etc.) or air pressure. For example, variations in air pressure can indicate increased or decreased energy in production of sounds and can be used, for example, to distinguish stressed vs. unstressed syllables. Greater airflow with lesser air pressure can, to give another example, indicate difference in phonation types (e.g. normal vs. 'breathy' voice). Ladefoged (2004, 55ff) has an excellent discussion of methodology in recording and measuring airflow and air pressure. There is a cost in undertaking such studies and that is the purchase and transport to the field of the equipment necessary to do the recordings. Such equipment these days is greatly reduced in bulk from what earlier field phoneticians had to contend with, but it still entails greater expense and inconvenience. Nevertheless, the more urgent the documentation of a particular language, the greater the argument for taking a range of phonetic equipment from the outset of field research on that language. Otherwise, my suggestion is that high-quality digital recordings and phonological analysis should take place for the first couple of sessions, to be followed later by phonetic field analysis. For most of what the average field linguist wants to do they can learn to do it themselves, and they should. On the other hand, if there is money in the budget and you can find a willing phonetician, it

could be useful to have an expert along to help you with your recordings and measurements for phonetic analysis.

In my own case, as I have mentioned before, I had the extreme privilege of having perhaps the world's greatest phonetician, Peter Ladefoged, accompany me to the Pirahãs, the Banawas, the Wari's, and the Oro Wins. But Peter wanted to come for two reasons. The first was to document the phonetics of endangered Amazonian languages for which there were at least initial phonological analyses. But the second was to check out some of the things I had been saying in the literature about Pirahã and Banawa stress. As I picked Peter up at the Porto Velho airport in Rondonia, Brazil, I felt like I was about to be audited by the US's Internal Revenue Service. I felt sure that my analyses were correct, yet at the same time, I knew that Peter Ladefoged would be concerned about the facts and not about what disproving everything I had said would do to my reputation! On the one hand this was a good feeling, as a scientist. But as a person I was tense. When Peter got off the plane and I was helping him to the car with his bags (and portable phonetics lab), he said that two of his colleagues had '... asked me to check out very carefully what you say about Pirahã stress because they are skeptical of your analysis.' And yet at the same time that I was concerned about whether my earlier statements would really stand up (paranoia), I was pleased that someone was taking this research seriously enough to check it out. Ultimately, the stress and tone that Peter identified in all the relevant words was what Everett & Everett (1984) had predicted. Thus Pirahã stress became a more widely accepted and important part of crosslinguistic stress studies. But the point is that replicability is a crucial part of science and having your results checked independently is a service to your own research and to the scientific community as a whole, even if your 'auditor' is perhaps not quite as distinguished as Peter Ladefoged.

The analysis of the entire grammar rests on the foundation of phonetics. If the phonetic quality of your data is poor, then everything from the morphology to the semantics is suspect.

Chapter 5. Prosodic fieldwork

by Nikolaus P. Himmelmann and D. Robert Ladd

This chapter is a little different from the preceding and following chapters, because it devotes a lot of space to basic issues that have nothing directly to do with fieldwork. The reason for this is simple. There's a lot of basic agreement about how segmental phonology and morphosyntax work, and most linguists know what kinds of things they will be looking for when they begin work on a new language. So in chapter 5 and 7 the focus is not on what to look for, but how to look. But with prosodic features – the kinds of things that often don't show up in a segmental transcription – many linguists feel that they are on shaky ground. They are insecure about hearing prosodic distinctions and unclear about the way these distinctions might be used in different languages. This means that an important goal of this chapter is to make you aware of what you might be looking for – not just how to look.

There are two fundamental ways that prosodic features differ from more familiar segmental features. One is that they are relevant at different levels of structure: there are both word-level or lexical prosodic features and sentence-level or “post-lexical” ones. Probably the best known typological difference based on this distinction is the one between “tone languages” like Chinese, where pitch serves to distinguish otherwise identical lexical items, and non-tonal languages like English, where pitch only serves to signal sentence-level differences of “intonation”. However, the use of prosodic features at different levels applies more widely as well: in English we can use stress at the lexical level to distinguish one word from another (e.g. *PERmit* [noun] and *perMIT* [verb]), but also at the post-lexical level to distinguish one sentence meaning from another (e.g. *I only put salt in the STEW* and *I only put SALT in the stew.*)

The other important property that sets prosodic features apart from familiar segmental features is that their sentence-level functions – like intonation and sentence-stress – are often broadly similar even in completely unrelated languages. For example, it is very common cross-linguistically to signal questions by the use of sustained high or rising pitch at the end of an utterance, even in languages that also have lexical tone. Because it works at different levels and because it has both universal and language-specific aspects, prosody is likely to seem mysterious and difficult. Speakers of a language that uses a given feature in one way are likely to find using it in a different way strange and exotic and (more practically) hard to hear: this is a common reaction of speakers of non-tonal languages when they encounter a tone language. Furthermore, sentence-level distinctions are probably inherently more difficult to think about than lexical distinctions: the difference between a pin and a bin is instantly obvious and easy to demonstrate, whereas the difference between the two versions of the sentence about the salt and the stew in the preceding paragraph takes careful explaining.

Nevertheless, prosody is an essential ingredient of every spoken language, and a description of prosody is an essential ingredient of every complete language description. In the following sections we will sketch some of the key phonetic, functional, and typological aspects of prosodic features, then go on to outline various techniques for achieving a satisfactory analysis of prosodic features in the field.

5.1 The phonetic fundamentals

We begin by briefly introducing four phonetic parameters which are relevant to prosody: pitch, duration, voice quality, and stress.

5.1.1 Pitch

Pitch is the property that distinguishes one musical note from another. In speech, pitch corresponds roughly to the fundamental frequency (F0) of the acoustic signal, which in turn corresponds roughly to the rate of vibration of the vocal cords. It is physically impossible to have voice without pitch – if the vocal cords are vibrating, they are necessarily vibrating at some frequency. In English and many other European languages we talk about pitch being “higher” or “lower” as the frequency of vibration gets faster or slower, but other sensory metaphors are used in other languages and cultures (“brighter/darker”, “sharper/duller”, etc.). Perhaps because pitch is a necessary property of voice, all languages – so far as we know – exploit pitch for communicative purposes.

The most striking thing about pitch is that it varies conspicuously from one speaker to another – men generally have lower voices than women. This means that the phonetic definition of pitch for linguistic purposes cannot be based on any absolute level of fundamental frequency but must be considered relative to the speaker’s voice range. Normalization for speaker differences must also deal with the fact that speakers can “raise their voice” without affecting the linguistic identity of pitch features. The details of how this normalization should be done are not fully clear but the basic principle is not in doubt. Moreover, this seldom causes serious practical difficulties in fieldwork, because we can usually hear whether a given pitch is relatively high or low in the speaker’s voice.

However, even if we find it relatively easy to abstract away from differences of overall pitch level, there are still major difficulties in the phonetic description of pitch. This is reflected in the lack of any agreement on an IPA system for transcription of pitch distinctions. One of the key issues for transcription is the relevance – or lack of relevance – of the syllable. In many tone languages, terms like “rise” and “fall” must be defined *relative to the syllable*: a sequence of a high-tone syllable and a low-tone syllable can be lexically completely different from a sequence of a falling-tone syllable and a low-tone syllable, even though both sequences involve an overall “fall” in pitch over the two syllables. In such a tone language, the overall “fall” is not relevant for phonetic description. In a language like English, on the other hand, a phonetic fall on a monosyllabic utterance (e.g. *John*) and a phonetic high-to-low sequence on a disyllabic one (e.g. *Johnny*) may be completely equivalent in the intonational system, which suggests that the “fall” must be regarded as a phonetic event regardless of the number of syllables it spans. This idea is strengthened by recent work showing that in languages like English and German functionally equivalent pitch movements can be “aligned” in different ways relative to syllables in different languages and language varieties.

In studying an unfamiliar language, in short, the fieldworker needs to be alert to the fact that descriptive assumptions can be hidden even in an apparently neutral label like “pitch fall”. For fieldwork, the most important thing to know about pitch is that a useful phonetic description of pitch depends on the way pitch is used in the language. More practically, the fieldworker whose native language works like English must be prepared to detect the syllable-by-syllable phonetic chunking of the pitch contour that is likely to be found in a language with lexical tone.

5.1.2. Segment duration

To the extent that we can divide an utterance into phonetic segments with clearly defined boundaries, we can measure the duration of the segments. In many languages duration is systematically manipulated for prosodic effect (e.g. distinctions between long and short vowels), but in all languages segment duration is affected by a whole range of other factors as well. These include some nearly universal allophonic effects (e.g. vowels tend to be longer before voiced consonants than before voiceless consonants; low vowels tend to be longer than high vowels; fricatives tend to be longer than stops) and effects of speaking rate (faster rate means shorter segments, but vowels are generally more compressible or expandable than consonants). Segment duration is also affected by other prosodic factors: specifically, stressed vowels tend to be longer than unstressed vowels; segments in phrase-final syllables tend to be longer than in other syllables; and word-initial and phrase-initial consonants tend to be longer than consonants in other positions.

For fieldwork, these differences mean that any suspected duration distinctions must always be checked in similar sentence contexts. In particular, if you ask someone to repeat two items that appear to be a duration-based minimal pair (like *Stadt* ‘city’ and *Staat* ‘state’ in German), it is important to hear the two members of the pair in both orders. That way you will not be misled by any lengthening (or occasionally, shortening) of whichever item is pronounced second.

5.1.3. Voice quality

The phonetic description of voice quality is less well advanced than that of other prosodic features. Many differences of voice quality – described by such impressionistic terms as “harsh”, “breathy”, “creaky” and so on – are based on different configurations of the glottis. As such they are difficult to observe directly, either in ourselves or in others, except by the use of special equipment. The standard work on the impressionistic description (and transcription) of voice quality is Laver 1980, which remains a useful reference for fieldwork. Much recent research has focused on understanding the acoustic correlates of voice quality differences and/or the glottal configurations that give rise to them. This work is not likely to be of much direct relevance to descriptive fieldwork, but good fieldwork can provide the basis for directing instrumental phonetic studies into fruitful areas of research.

5.1.4. Stress

Roughly speaking, stress is the property that makes one syllable in a word more prominent than its neighbours – for example, signaling the difference between the noun *PERmit* and the verb *perMIT*. Perhaps surprisingly, it is extremely difficult to provide a phonetic definition for this “greater prominence”. Impressionistically (for native speakers of most European languages), the phonetic basis of stress is “loudness” – the stressed syllable seems louder than neighbouring unstressed syllables – but perceived loudness is psychophysically very complicated, not just in speech but in all auditory stimuli. The most important correlate of loudness is intensity (sound energy), but duration and fundamental frequency have also been shown to play a role – for the same peak intensity, a longer or higher-pitched sound will sound louder than a shorter or lower-pitched one.

A possibly more useful phonetic definition of stress is “force of articulation”, which shows up less in effects on the overall energy in a segment or syllable and more in the distribution of energy in the spectrum of the sound. Specifically, it has recently been suggested that stressed vowels in Dutch have more energy at higher frequencies than unstressed vowels (they have “shallower spectral tilt” [Sluijter and van Heuven 1996]). There may also be effects of “force of articulation” on the relative duration of consonant and vowel portions of a syllable, although the details are not at all clear. Additionally, stressed syllables usually contain full (peripheral) vowels while unaccented syllables may contain reduced (centralized) vowels such as schwa; alternatively, a language may have only or mainly peripheral vowels, but stressed syllables may allow for larger vowel inventories than unstressed syllables. For example, Catalan distinguishes seven vowels /i e □ a □ o u/ in stressed syllables but only three /i □ u/ in unstressed ones.

Whatever the phonetic basis of stress turns out to be, and however much it turns out to differ from one language to another, it is clear that part of the problem defining the phonetic basis of “stress” is the existence of conceptual and theoretical problems with the classification and description of accentual systems generally. We return to this issue in the next section, and in section 6.3.3.

5.2. Typical functions of prosodic features

5.2.1. Lexical and morphological functions

The lexical functions of prosody are, on the whole, like the function of most segmental phonological distinctions: to distinguish between one lexical item and another. Just as English *pin* and *bin* differ minimally phonologically but are two unrelated lexical items, so pairs like Chinese *niàn* ‘study’ and *nián* ‘year’ or Dutch *man* ‘man’ and *maan* ‘moon’ or Greek [□□□□□] ‘space’ and [□□□□□] ‘dance’ involve unrelated lexical items that are minimally different phonologically. Similarly, just as segmental distinctions can be used to signal different morphological categories (for example, English *foot/feet* for singular/plural or *drink/drank* for present/past), so prosodic features can be used in the same way, as in the difference between Efik [□□□□□] ‘I buy’ and [□□□□□] ‘I would buy’ or Dinka [dèk] ‘drink (infinitive)’ and [dè:k] ‘s/he drinks’ or Italian [□□□□□] ‘I speak’ and [□□□□□] ‘s/he spoke’.

The examples just given illustrate the three most commonly encountered types of lexical prosodic distinctions: **tone** (as in the Chinese and Efik examples), **quantity** (as in the Dutch and Dinka examples) and **accent** (as in the Greek and Italian examples). It is common to treat the three of these together as “suprasegmental” features, and to identify them with the phonetic parameters of **pitch**, **duration** and **stress**. A classic statement of this view, still useful for the data it contains, is Lehiste’s book *Suprasegmentals* (1970). However, this view is misleading in two distinct ways. First, the linguistic categories of tone, quantity and accent are often cued in multiple phonetic ways. Tone is primarily a matter of pitch, but may also involve accompanying differences of segment duration and voice quality: for example, in Standard (Mandarin) Chinese syllables with “tone 3” are not only low in pitch but tend to be longer in duration and to have creaky or glottalised voice as well. Quantity distinctions are based on segment duration, but often involve differences of vowel quality or (in the case of consonants) manner of articulation as well: for example, German long vowels tend to be higher and less central than their short counterparts, in addition to being longer in duration. As for accent, there are so many different phonetic manifestations of things that have been called “stress” or “accent” that there is very little agreement on what these terms refer to. In short, it is at best a gross oversimplification to think of tone, quantity and accent as the linguistic functions of the phonetic features pitch, duration and stress.

The second reason for not treating tone, quantity and accent together is that they are functionally quite different. Where they exist, distinctions of tone and quantity are often functionally similar to segmental distinctions. Tone – especially in East Asia, West Africa, and parts of the Americas – generally has a high functional load, and it is not at all uncommon to find extensive minimal sets distinguished only by tone, for example Yoruba *igba* ‘two hundred’, *ìgbà* ‘calabash’, *ìgbà* ‘[type of tree]’, *ìgbà* ‘time’. Quantity systems are similar: in many languages with distinctive vowel or consonant quantity, all or almost all the vowels or consonants can appear both long and short in pairs of unrelated words, for example German *bitten* /bitn/ ‘request’ vs. *bieten* /bi:tn/ ‘offer’; *beten* /be:tn/ ‘pray’ vs. *Betten* /betn/ ‘beds’, etc.

By contrast, accentual differences are often rather marginal in the lexicon of a language as a whole, yielding few minimal pairs and/or involving some sort of morphological relatedness. For example, in English the lexical accent in a word is certainly a distinctive part of its phonological make-up, and a misplaced stress (e.g. in foreign pronunciation) can make word identification very difficult. Yet there are very few minimal pairs in English based on lexical accent, except for derivationally related noun-verb pairs like *OBject-obJECT* and *PERmit-perMIT*. This difference is due to the fact that accent involves a syntagmatic relation (the relative prominence of two syllables) whereas tone and quantity, like most segmental features, are a matter of paradigmatic contrasts between members of a set of possible phonological choices. It is clearly meaningful to say of a monosyllabic utterance that it has a long vowel or a high tone, because these terms can be defined without reference to other syllables. It is often less clear what it means to say that a monosyllabic utterance is “stressed” or “accented”.

Finally, we should mention lexical distinctions of voice quality, which are often not considered under the heading of “prosody” at all. In some languages there are

phonemic distinctions of voice quality which are associated with specific consonantal contrasts: for example, in Gujarati voice quality distinctions are historically related to the distinction between “voiced” and “voiced aspirated” stops in other languages of Northern India, and are found only in the presence of certain specific stops. Similarly, in many East Asian tone languages there are characteristic differences of voice quality that accompany pitch differences in distinguishing between one tone phoneme and another, and which are therefore generally described as part of the tonal system. (This is the case with the glottalisation that often accompanies Mandarin “Tone 3”, as we just saw above.) However, voice quality distinctions can be independent of both segmental and tonal distinctions: for example, in Dinka, the two distinctive voice qualities (often described as “creaky” and “breathy”) can cooccur with any of the tone phonemes, any of the distinctive quantity categories, and most of the vowel and consonant phonemes.

5.2.2 Sentence-level functions

At sentence level, prosodic features typically play a role in marking three general functions: sentence modality and speaker attitude, phrasing and discourse segmentation, and information structure and focus. However, there is nothing intrinsically “prosodic” about any of these functions: all of them may also be marked in a non-prosodic way in addition to, or instead of, a prosodic marking. Thus, for example, while sentence modality and focus are often marked by intonational means in many European languages, many other languages employ particles or affixes in the same functions (e.g. focus particles in Cushitic languages, question marking clitics in western Austronesian languages).

An important problem in studying the prosodic signaling of these functions is that many pitch-related phenomena are quasi-universal, which reflects their link to prelinguistic ways of communicating that we share with other species. As noted in section 6.1.1, women have higher-pitched voices than men, and individuals can “raise” and “lower” their voices for various expressive purposes. These “paralinguistic” functions of pitch and voice quality are broadly similar the world around, though there are big differences between cultures in the way the paralinguistic functions are evaluated. For example, a voice raised in anger sounds much the same in any language, but raising the voice in that way may be dramatically less acceptable in one culture than in another. Similarly, in some cultures it is highly valued for males to have very low voices and/or for females to have very high voices, and speakers tend to exaggerate the biologically based differences, whereas in other cultures little importance is attached to such differences.

5.2.2.1 Sentence modality and speaker attitude

The prosodic expression of modality and attitude is most closely identified with speech melody and voice quality. Together, these are the characteristics we are most likely to think of as the “intonation” of an utterance. Typical examples include the use of overall falling pitch in statements, overall rising pitch in yes-no questions, or the use of overall high pitch in polite utterances.

These examples are also typical examples of the difficulty of distinguishing linguistic and paralinguistic functions of pitch. For example, there have been disagreements about

whether overall rising pitch in “question intonation” is part of a language-specific intonational phonology or merely based on the universal use of high pitch to signal tentativeness or incompleteness. Our view is that it is necessary and appropriate to talk of “intonational phonology” for at least some sentence-level uses of pitch (see further section 6.3.1 below). It is important to remember that languages may diverge considerably from the quasi-universal tendencies mentioned above: there are languages such as Hungarian or some dialects of Italian, where question intonation includes the kind of final fall which is typical of statements in other western European languages. Nevertheless, we acknowledge that there is genuine empirical uncertainty about how to distinguish phonologized uses of pitch from universal patterns of human paralinguistic communication.

5.2.2.2 Phrasing and discourse segmentation

In all languages, so far as we know, longer stretches of speech are divided up into prosodically defined chunks often called intonation units (IUs) or intonation(al) phrases (IPs). To some extent this division is determined by the need for speakers to breathe in order to continue speaking, and in the literature the term breath group may also be found for what we are here calling IU. However, it is important not to think of IUs purely as units of speech production, because they almost certainly have a role in higher-level linguistic processing as well, both for the speaker and the hearer. That is, intonation units are also basic units of information (e.g. Halliday 1967, Chafe 1994, Croft 1995) or of syntax (e.g. Selkirk 1984, Steedman 2000). Closely related to the issue of segmentation into IUs are the prosodic cues that help control the smooth flow of conversation (e.g. signals of the end of one speaker’s turn) and the cues that signal hierarchical topic structure in longer monologues such as narratives (e.g. “paragraph” cues). An eventual theory of prosodic phrasing will cover all these phenomena.

The phonetic manifestations of phrasing and discourse chunking are extremely varied. The clearest phonetic marker of a boundary between two prosodic chunks is a silent pause, but boundaries can be unambiguously signaled without any silent pauses, and not all silent pauses occur at a boundary. Other cues to the presence of a boundary include various changes in voice quality and/or intensity (for example, change to creaky voice at the end of a unit), substantial pitch change over the last few syllables preceding the boundary (such as an utterance-final fall), pitch discontinuities across a boundary (in particular, “resetting” the overall pitch to a higher level at the beginning of a new unit), and marked changes in segment duration (especially longer segments just preceding a major boundary). However, it is also important to note that there are extensive segmental cues to phrasing as well, especially different applications of segmental sandhi rules. For example, in French, “liaison” – the pronunciation of word-final consonants before a following vowel – is largely restricted to small phrases and does not occur across phrase boundaries: *allons-y* ‘let’s go’ (lit. ‘let’s go there’) is pronounced [alɔ̃zi] but *allons à la plage* ‘let’s go to the beach’ is normally pronounced [alɔ̃alaplɑ̃], signalling the presence of a boundary between *allons* and *à la plage*.

An important conceptual problem in discussing phrasing and discourse segmentation is that we need to recognize different levels of prosodic structure, and there is no agreement on how to do this. In corpora of ordinary spontaneous speech it will often be easy enough to distinguish a basic level of IU, perhaps 6-10 syllables long, set off

by relatively clear boundaries signaled by silent pauses and other cues. However, merely dividing texts into a single level of IUs tells us nothing either about the smaller units that distinguish one syntactic structure from another, nor about the larger units (often called “episodes” or “paragraphs”) that signal higher-level textual organization in monologues. This important topic is unfortunately beyond the scope of this chapter.

5.2.2.3 Information structure and focus

Related to the marking of boundaries and cohesion is the use of prosody to signal semantic and pragmatic features often collectively known as “information structure”. This includes notions like “contrast”, “focus” and “topic”, and refers to the way new entities and new information are introduced into a discourse and to the way in which entities and information already present in a discourse are signaled as such. One important means of conveying this kind of information is to put specific words or phrases in prosodically prominent or non-prominent positions. In some languages word order can be extensively manipulated in order to achieve this, whereas in other languages the same string of words can have different prosodic structures. Both strategies are exemplified in English constructions involving direct and indirect objects: we can say either *I gave the driver a dollar* or *I gave a dollar to the driver*, putting either the amount of money or the recipient in the prosodically prominent final position. Other things being equal, the first construction is used when the amount of money is more informative in the discourse context and the second when the point of the sentence is to convey something about the recipient. However, we can achieve similar effects by restructuring the prosody so that the major sentence-level prosodic prominence occurs on a non-final word: *I gave the DRIVER a dollar* (... not the waiter) or, somewhat less naturally, *I gave a DOLLAR to the driver* (...not a euro).

There is an extensive literature on these matters, especially in the European languages; the reader is referred to Lambrecht (1994) and Ladd (1996) for useful summaries. Fieldworkers should probably be wary of expecting to find close analogues of European phenomena in languages in other parts of the world.

5.3. Phonology of tone, intonation and accent

From the foregoing sections it will be clear that “prosodic” features – defined on the basis of phonetic properties that are not normally indicated in a segmental transcription – do not form a linguistically coherent set. Among other things, this means that there is no way of knowing ahead of time how the phonetic features loosely referred to as “prosodic” – pitch, duration, and so on – are going to be put to phonological use in any given language. Speakers of all languages produce and perceive differences in pitch, duration, voice quality, and probably relative prominence, but they may interpret these differences in radically different ways. There is no unique relation between a given phonetic feature and its phonological function.

As we suggested earlier, some “prosodic” distinctions turn out to work in ways that are no surprise to any linguist, while others – sometimes involving the same phonetic raw material – are still in need of extensive new theoretical understanding before we can be sure that our descriptions make sense. What seems fairly clear is that the “unsurprising” prosodic features (like lexical tone and quantity) involve linguistic

elements that are grouped into strings and contrast paradigmatically with other elements, like most segmental phonemes. The “problematical” prosodic features (like accent and phrasing) are somehow involved in signaling phonological structure, the grouping of linguistic elements into larger chunks. In this section of the chapter we provide a little more detail on two problematical topics: the tonal structure of intonation, and the nature of “accent”.

5.3.1 Tone and intonation

As we’ve already seen, pitch provides the main phonetic basis for prosodic distinctions both at the word level (“tone”) and at the sentence level (“intonation”). Tone languages are extremely varied, and it would be possible to devote this entire chapter just to describing the many varied phenomena of lexical and grammatical tone. However, since there are good descriptions of numerous prototypical tone languages from around the world and a substantial body of literature discussing various aspects of their analysis, it would be pointless to attempt a mere summary here. The textbook by Yip (2002) provides a comprehensive survey, and is a useful guide to various descriptive and theoretical problems. Anyone embarking on the study of a language known or suspected to have lexical and/or grammatical tone should be well acquainted with this literature before leaving for the field.

We focus here instead on intonation. We use the term here in a strict sense, to refer to sentence-level uses of pitch that convey distinctions related to sentence modality and speaker attitude, phrasing and discourse grouping, and information structure. The phonological structure of intonation is better understood now than it was a few decades ago, but there are undoubtedly plenty of intonational phenomena waiting to be discovered in undocumented languages, and plenty of things that we will understand better once we have a fuller idea of the range of possibilities. What we present here is a minimal framework for investigating intonation in a new language. Our discussion is based on the now widely accepted “autosegmental-metrical” theory of intonation (for a review see Ladd 1996).

The most important phonological distinction to be drawn is the one between intonational features at major prominent syllables and intonational features at boundaries: in current terminology, the distinction is between “pitch accents” and “boundary tones”. The existence of such a distinction has been recognized by some investigators since the 1940s, and is made explicit in current autosegmental-metrical transcription systems for numerous (mostly European) languages. The difference between the two can be readily appreciated in English when we apply the same intonational tune to sentences with markedly different numbers of syllables and/or markedly different accent patterns. For example, imagine two different possible astonished questions in response to the sentence *I hear Sue’s taking a course to become a driving instructor*. One might respond *Sue?! or one might respond A driving instructor?! In the first case, the pitch of the astonished question rises and then falls and then rises again, all on the vowel of the single syllable Sue. In the second case, the pitch is briefly fairly level at the beginning, then there is a steep rise in pitch on the lexically stressed syllable dri-, immediately followed by a fall, then a level low-pitched stretch until the very end of the utterance, at which point there is an abrupt rise. [Really need a sound file and a picture here!///] At a minimum, therefore, the contour consists*

of two separable parts: a rising-falling movement at the main stressed syllable and a rise at the very end. On the monosyllabic utterance *Sue* these two parts are compressed onto the single available syllable, which is both the main stressed syllable and the end of the utterance. But with a somewhat longer phrase the separateness of the two prosodic events becomes clear.

One important clue to the correctness of the distinction between pitch accents and boundary tones is the fact that in some lexical tone languages, where pitch primarily conveys lexical information, there are nevertheless intonational pitch effects at the ends of phrases or sentences. These effects typically involve modifications of the lexically-specified pitch contour on the pre-boundary syllable. Early descriptions of this effect were given by Chuang 1958 for Szechuan Mandarin and by Abramson 1962 for Thai. This coexistence of lexical and intonational pitch can be described easily if we recognize boundary tones: in these languages the pitch contour of an utterance is principally determined by the lexical tones of the words that happen to make it up, but at the edges of phrases it is possible to add an additional tonal specification - a boundary tone.

However, it should be emphasized that not all lexical tone languages use intonational boundary tones; for example, some West African tone languages appear not to have them, so that in these languages the pitch contour of an utterance is almost completely determined by the string of lexical tones. Conversely, there appear to be languages with intonational boundary tones that have neither pitch accents nor lexical tonal specifications. In these languages, all intonational effects are conveyed by pitch movements at the edges of phrases, and “nothing happens” phonologically in between. Obviously, there is phonetic pitch wherever there is voicing, but the linguistically significant pitch effects are restricted to phrase edges, and the pitch in between is determined by simple interpolation. Clear descriptions of such systems are given by Robert and Rialland (2001) for Wolof and Jun (1998) for Korean.

Current transcription systems for pitch accents and boundary tones, based largely on the ToBI system first designed for English in the early 1990s, analyze these pitch movements further: the astonished question contour just discussed would probably be transcribed as a L+H* pitch accent, an immediately following L- “phrase accent”, and a H% or L+H% boundary tone. The details are well beyond the scope of this chapter, but the reader who expects to deal with an unfamiliar intonation system in a language without lexical tone should consult the ToBI web site (URL <http://www.ling.ohio-state.edu/~tobi/>) and its extensive series of links to ToBI systems that have been designed for a number of other languages.

Before we leave the subject of intonation, we must note that in addition to pitch accents and boundary tones, intonation can make crucial use of what we might call “register effects”. Recall that the phonetic realization of pitch distinctions is somehow relative to the speaker’s pitch range: “high” does not refer to some absolute fundamental frequency level, but a level that is high for a given speaker in a given context. This even applies within a single utterance: as a result of the widespread phenomenon of “declination” – a gradual lowering of pitch across a phrase or utterance – the pitch of a “high” tone at the end of an utterance may be lower than that of a “low” tone at the beginning. That is, the phonological interpretation of pitch level is somehow relative to

a frame of reference that varies not only from speaker to speaker and from context to context but also from one part of an utterance to another. Such changes of the frame of reference during the course of an utterance can be exploited for communicative purposes in various ways, and these are what we are calling “register effects”. The clearest examples of such effects involve the interaction of lexical tone and overall pitch level to signal questions. In Chinese, for example, it is possible (though not very usual) to distinguish yes-no questions from statements in this way.

5.3.2 Lexical accent systems

The existence of tone languages is such a remarkable fact from the point of view of speakers of non-tonal languages that there are at least two typological schemes – devised by speakers of non-tonal languages – that attempt to accommodate lexical/grammatical tone in a larger theoretical understanding. One of these is based on the “domain” of pitch distinctions, while the other is based on a typology of “word prosody”. Looking at the domain of pitch, languages have been divided into “tone languages” (where the domain of pitch distinctions is the syllable), “melodic accent languages” (where the domain of pitch distinctions is the word), and “intonation languages” (where the domain of pitch distinctions is the phrase or utterance). This typology goes back at least to Pike (1945) and is found in work as recent as Cruttenden (1997). Looking instead at the lexical uses to which “prosodic” features are put, we can divide languages into “tone languages” (in which each syllable has different tonal possibilities), “melodic accent languages” (in which one syllable in a word or similar domain is marked by pitch in some way), and “dynamic accent languages” (in which one syllable in a word or similar domain is marked by stress in some way). This typology is suggested by Jun 2005. Both typologies have obvious problems (e.g. the existence of intonational distinctions in tone languages, the existence of languages like Swedish with both dynamic accent and lexically specified melodic accent), and neither commands wide acceptance.

In our view, the problems with these typologies result from trying to incorporate tone and accent in the same scheme. As we pointed out earlier, tone often functions like segmental distinctions: it involves a choice of categories from a paradigmatic set, and it is meaningful to talk about e.g. a high tone on one syllable without reference to the tone on any other syllable. Accentual distinctions, on the other hand, are syntagmatic distinctions: they involve contrast with immediately adjacent syllables in a string. Consequently, we believe that it is quite misleading to see, as in Pike’s typology, a continuum from tone to melodic accent to intonation, and equally misleading, but in a different way, to take “tone” and “stress” as different kinds of “word prosody” that a language may have. Rather, we think it will be useful to discuss the ways in which accentual systems can differ without necessarily trying to place them into a typological scheme that places them in the same dimension as intonation and tone (i.e. the typology of prosodic systems should involve three, at least partially independent dimensions: tone, accent, and intonation).

A general and possibly universally valid definition of lexical accent is the singling out of a specific syllable in a word or similar domain (such as the “foot”) for some sort of prominence or other special prosodic treatment. Lexical accent, as conceived of this way, is an abstract structural notion, and says nothing about how exactly the “special

prosodic treatment” is manifested in the acoustic signal. In some languages, the special status of the accented syllable is based entirely on association with a specific pitch feature; in other languages, the accented syllable is distinguished from other syllables by phonetic “stress” – greater force of articulation leading to some combination of longer duration, greater intensity, more peripheral vowel quality, shallower spectral tilt, etc. (cf. section 6.1.4). This suggests a distinction between “melodic” and “dynamic” accent, a traditional distinction recently reestablished by Beckman (1986).

The distinction between melodic and dynamic accent is a phonetic one. Other typological dimensions on which accentual systems appear to differ involve structural properties. These include obligatoriness, culminativity, recursivity, transitivity, intonational anchoring, and lexical distinctiveness. We briefly outline these six properties here:

Obligatoriness: In some accentual systems, an accent must occur within each domain of the specified size: if the “prosodic word” is the domain of accent, then each prosodic word must have an accent. In other systems, the accent may or may not occur in a given domain. For example, in Japanese, words can be accented or unaccented, whereas in English any word of more than one syllable must have at least one syllable that stands out as more prominent when the word is pronounced in isolation.

Culminativity: In some systems, for every accent domain there is a single major prominence peak. This does not preclude the possibility that other syllables in the same domain may also be prominent relative to surrounding syllables (see further below under RECURSIVITY), but there is only one which is the most prominent one of them all. In a non-culminative system, there may be two prominences within the same domain without either of them being more prominent than the other one (in some languages, e.g. Chinese, accentuation in compounds appears to be non-culminative).

It is a matter of debate whether it is useful to distinguish obligatoriness and culminativity. The alternative is to operate with a single parameter, usually also called simply culminativity, defined as the property where every lexical accent domain has a single major accentuation. If one separates culminativity (in a narrow sense) and obligatoriness, languages such as Japanese have a non-obligatory, but culminative accent-system (i.e. not every word has to have an accent, but those that have an accent have only one). If one operates with a single parameter culminative (in a broad sense), then Japanese is non-culminative since not every word has an accent.

Recursivity: In some languages, it is possible and useful to distinguish different levels of lexical accentuation. Thus for English, for example, one commonly distinguishes at least three different levels of syllable prominence: primary accent, secondary accent, and unaccented. Primary accent is assigned to the most prominent syllable in a word (as the English accent system is culminative, there can be only one such syllable). Secondary accents are assigned to syllables which are also somewhat prominent and in certain contexts can actually become carriers for the primary accent. There can be several of these in an English word, as in *èxtramètricálicity* (using grave accents to mark secondary accents). However, in some languages there is no evidence – or at best very weak evidence – for anything resembling secondary accent: a single accent is assigned to a word domain, and all the other syllables are simply “unaccented”.

One widely-adopted analysis of such secondary accents in languages that have them is in terms of sub-word domains called (*metrical*) *feet*. In a word with secondary accent, the word domain consists of two or more feet, each with its own most prominent syllable, and one foot is singled out as the most prominent foot of the word. The prominent syllable of the prominent foot is the primary accent; the prominent syllables of the other feet are secondary accents. In languages without secondary accent, we may say either that there is no level of structure corresponding to the foot, or that the feet are “unbounded”, i.e. that they are coextensive with the word. See Ewen & van der Hulst (2001) for a comprehensive introduction to metrical structure.

Transitivity: Just as accentual prominence may apply within domains smaller than the word, so we may also find accentual prominence relations at the phrasal level when words are joined together to form phrases. Within a phrase such as *yellow paper* one word (normally *paper*) is more prominent than the other word, which entails that its most prominent syllable is more prominent than the most prominent syllable of the other word. That is, the most prominent syllable of the most prominent word becomes the most prominent syllable of the phrase, often called phrasal prominence or *sentence stress*. However, not all accent systems have this feature of transitivity, and then it is not possible to single out one accented word as the most prominent in its phrase.

Phrasal prominence can be analysed in the same way as lexical secondary accent, in terms of nested domains each with its own most prominent constituent. However, not everyone accepts this point of view. In some analyses, phrasal prominence is treated as being qualitatively different from lexical prominence: on this view, lexical prominence is usually described as “stress”, and phrasal prominence is described in terms of intonational “pitch accent” (see e.g. Selkirk 1984 or Shattuck-Hufnagel and Turk 1996). For this reason it is extremely difficult to make reliable and generally acceptable typological statements about these matters.

Intonational anchoring: In many languages, as we saw in sec. 6.3.1., a lexically accented syllable serves as the ‘anchor’ for the pitch accents that make up the intonational tune. This means that in e.g. English and German the lexically most prominent syllable of the most prominent word in an utterance also carries an intonational pitch accent. This is the basis for the view of transitivity sketched in the preceding paragraph: according to this view, lexical accent is phonetically “stress”, while phrasal prominence is “pitch accent”. We prefer to see this as a fact about the relation between the accentual system and the intonational system of a given language; lexical accents may or may not serve the role of intonational anchors. In Japanese and many other languages with melodic accent, for example, there is no additional intonational feature that targets accented syllables. But this is not a function of having a melodic rather than a dynamic lexical accent: in Swedish and Basque, syllables marked with a melodic lexical accent may additionally also serve as anchors for an intonational pitch accent. Conversely, recent work on the Papuan language Kuot (Lindström & Remijsen 2005) suggests that it has dynamic lexical accent (phonetic stress) but that the intonational pitch accents do not have to occur on a stressed syllable.

Lexical distinctiveness: Finally, another commonly drawn typological distinction among accentual systems is that between fixed or predictable accent and lexically distinctive accent. In both Greek and Japanese, despite the fact that the former uses

dynamic accent and the latter melodic accent, the location of accent can be used to signal differences between one lexical item or another. In other languages, the position of stress is either completely fixed (as on the initial syllable in Hungarian or Czech) or entirely predictable (e.g. Latin, where the penultimate syllable is accented if heavy, otherwise the antepenultimate).

The dimensions of accentual typology just discussed are probably not completely independent. Accentual systems with dynamic accent (or phonetic stress) typically have obligatory and culminative lexical accent, exhibit recursivity and transitivity, and involve intonational anchoring, and in fact it is widely assumed that all dynamic accent systems exhibit these properties more or less by default. Although there is no doubt that the dynamic accent systems of Europe typically show this cluster of features, for fieldworking purposes we strongly advise you not to take this as given. Kuot and Wolof are examples of languages with phonetic stress which show that one should be prepared to encounter unusual combinations and to try to provide substantial evidence for each of the parameters.

Finally, since melodic accents are realized primarily by pitch changes, they are sometimes difficult to distinguish from tonal distinctions, and in a number of cases there is an ongoing discussion whether a given language is better analysed as a tone language or a melodic accent language. This problem typically arises when there are only two distinct pitch patterns (high/low or marked/unmarked) and when the pitch pattern changes only once per lexical item. This type of accent system is widely attested African and Papuan languages and often discussed under the heading of ‘word melody’ (see Donohue 1997, Hyman 2001, Gussenhoven 2004, for examples and discussion). The core issue in analyzing these languages is whether tonal marking has essentially a paradigmatic function, distinguishing one lexical item from the other, or rather a syntagmatic (or organizational) function, rendering the marked syllable(s) prominent in comparison to the neighbouring syllables. While this distinction is reasonably clear on the conceptual level, there are many borderline cases in actually attested systems which may be quite difficult to assign to either category (Hyman (2001) uses a set of parameters similar to the ones above for distinguishing typical tone and accentual systems). The existence of such borderline cases is not surprising given the fact that prototypical lexical tone systems may change into melodic accent systems and vice versa.

In concluding this section, a note on the ambiguity of the term “pitch accent” as used in much of the literature is in order. This term is now regularly used in two distinct ways: on the one hand, it refers to the sentence-level (intonational) pitch features that may accompany prominent syllables in an utterance in a language like English; on the other hand it refers to the word-level – lexically specified – pitch features that accompany accented syllables in a language like Japanese. In this chapter, we have opted to use the term *pitch accent* only for intonational pitch features and use *melodic accent* for lexically specified accentual pitch features.

5.4 Working on prosody in the field

In approaching the analysis of segmental phonology or morphosyntax in an unfamiliar language, there are various well-tested techniques for determining the elements and structures one is dealing with. These include things like minimal pair tests and permutation tests, which are described in more detail in Chapters 5 and 7. For certain purposes, these are also relevant for prosody – for example, we have already described the existence of lexical minimal pairs that differ only in tone, and once you have determined that you are dealing with a lexical tone language it may be both possible and appropriate to elicit minimal pairs for tone in exactly the same way that you would for segmental differences. However, to the extent that prosodic features are not organised like ordinary segmental phonological and morphosyntactic features, different techniques are required.

The most important problems in studying prosody in the field are the fact that prosody is pervasive – you can't have an utterance (even a single elicited word) without prosody – and the fact that it is influenced by both lexical and sentence-level factors and may thus be contextually variable in ways that are difficult to anticipate, or to notice. For example, if you were asked out of context to give the name of the famous park in the middle of London where people come to make speeches to anyone who happens to want to listen, you would say *Hyde Park*, with the two words about equally prominent. However, if you were in a conversation about great urban parks – like Grant Park in Chicago or Central Park in New York or Stanley Park in Vancouver – you would probably say *HYDE Park*, with the main prominence on *Hyde*. (In fact, if you read the previous sentence aloud you will find it is very difficult to say the list of park names without putting the main prominence in each on the proper name and de-emphasising *Park* in each case.) If you were doing fieldwork on English and knew nothing about the language, you would have to become aware of this contextual effect before you could accurately describe the prosody of expressions like *Eiffel Tower* or *Princes Street* or *Van Diemen's Land* that consist of a proper noun and a common noun.

In this section, therefore, we will discuss research procedures which are particularly useful in prosodic research but rarely used in working on other aspects of the grammar of a given language. We begin by describing some useful “first steps” to take in the prosodic analysis of a previously undescribed language.

5.4.1 First steps

It is important to establish early what sort of lexical prosodic features are found in the language you are working on. The literature on neighbouring and related languages may provide important pointers in this regard, but it is obviously necessary to remain open to all possibilities until clear language-internal evidence points in one direction or the other. If you are working on a language with distinctions of quantity or of lexical accent (whether dynamic accent or melodic accent), it may take some time to become aware of the distinctions, because as we noted earlier the functional load of such distinctions may be relatively low. If you are working on a prototypical lexical tone language, it is likely to become evident quite quickly, because native speakers will usually point out to you that items that you appear to consider homophonous are not homophonous but clearly distinct for them. However, unless you are working with speakers who are also familiar with a well-described tone language, they will not

necessarily make reference to tone (or pitch) in pointing out these differences. They may simply assert that the items in question sound very different, sometimes perhaps even claiming that the vowels are different.

Although there may be some languages with no lexical prosodic features whatever, in general it will be a useful starting hypothesis that in any given utterance some prosodic features will be lexically determined and some determined at the phrase or sentence level. Both levels are inextricably intertwined; there is nothing in the signal to tell you whether a given pitch movement is lexically motivated (e.g. lexical tone), intonationally motivated (e.g. sentence accent), or even both (e.g. the combinations of lexical and intonation tone commonly found on sentence-final syllables in Chinese or Thai). This problem is of central importance when analyzing pitch, but sometimes affects the analysis of quantity and accent as well. Perhaps the most important lesson to begin with is that recording and analyzing words in isolation does not in any way provide direct, untarnished access to lexical features. This is a classic mistake, unfortunately widely attested in the literature. A single word elicited in isolation is an utterance, and consequently cannot be produced without utterance-level prosodic features. For example, if you compare ordinary citation forms of the English words *PERmit* (noun) and *perMIT* (verb) [link to sound files], you might conclude that high pitch, followed by a fall, is a feature of lexical stress in English. However, high pitch associated with the stressed syllable is actually a feature of declarative statement intonation in short utterances. If you utter the same words as surprised questions, the stressed syllables will be low, followed by a rise in pitch to the end. If you utter the same words as part of a long and boring list, the stressed syllables may be high, but followed by a rise in pitch (rather than the fall seen in the isolated citation form). In short, even for single word utterances it is not a straightforward matter to distinguish between lexical and intonational prosodic features. There is no intonationally unmarked ‘citation form’; every utterance has intonation.

In order to separate the two levels, lexical items have to be observed in a number of different syntactic and semantic-pragmatic contexts. Whatever prosodic features remain constant across these contexts most likely pertain to the lexical level; features which change may relate to the sentence level. But especially in dealing with lexical tone languages, even this statement needs qualifying, because in many such languages there are complex locally-conditioned variations in tonal pattern, sometimes called *tone-sandhi* (see Yip 2002 for examples and discussion).

In order to elicit target words in different contexts, one can construct short clauses or phrases where the target words may occur in different positions (i.e. initial, medial, final). A particularly useful variant of this technique is to record short (3-5 word) lists of target words with the words in different positions in the list. If speakers produce a coherent list rather than a sequence of minimal utterances, the result is likely to be a contrast between list intonation and minimal declarative utterance intonation. This may allow you to distinguish word-level prosodic effects. More generally, list intonation may be particularly useful in the initial stages of such an analysis for three reasons. First, it is relatively easy to elicit naturally: the act of listing elicited items does not differ in principle from listing items as part of a procedural description, whereas enacting a question is quite different from actually asking a question. Second, list intonation tends to be fairly simple in the sense that there is usually only an opposition

between non-final and final members, or sometimes a three-way distinction between non-final, penultimate and final. In particular, there are no differences of information structure (focus, topic) in lists, which often complicate the interpretation of prosodic features in other types of examples (see also section 6.2.2.3 above). Third, list intonation may be more consistent across speakers, which makes it easier to recognize the same intonational targets across speakers and at the same time provide an indication of inter-speaker variability.

5.4.2 Elicitation

All modern descriptive and documentary fieldwork includes the recording of a substantial corpus of (more or less) spontaneous ‘texts’ (where ‘text’ subsumes all kinds of communicative events including conversations, narratives, oratories, etc.). If these recordings are done with reasonable quality, they can form the basis for subsequent auditory and instrumental analysis of many prosodic features of connected speech, features that may be difficult to observe in structured interview sessions and difficult for most native speakers to be aware of. However, just as you would not expect to study phonology or syntax solely on the basis of a recorded corpus, so in the case of prosody it is important to complement recorded texts with elicited data.

In eliciting data for prosodic analysis it is important to keep various factors in mind that are of only secondary importance for eliciting many other kinds of data. First and most important, it is essential to keep in mind the kind of effects that context may have, and to adjust elicitation procedures accordingly. For example, in English it is common for WH-questions to be pronounced with an overall falling contour in neutral contexts (*Where is he going?*), a relatively high level followed by a low rise at the end in polite contexts (*Where would I find Dr. Anderson?*), and an overall rising contour in repetition or reminder contexts (*Where did you say you were from?*). Eliciting such distinctions may require you to get native speakers to put themselves mentally in different contexts, which is not necessarily easy to do. We return to this topic at some length in the next section.

Second, it is important to record several speakers rather than relying on one or two primary consultants. One reason for this is the conspicuous difference of voice pitch between males and females; another is that many prosodic features vary more between individuals and between socially defined groups than do centrally “linguistic” features. Fieldwork situations will usually put severe limits on how many speakers you can work with, but if at all possible it will be valuable to record elicited material from at least 4 and as many as 8 or 10 speakers. Gender balance is an important concern in putting together a set of speakers. In situations where it is impossible to find several speakers for the same task, it may be useful to record the same material with the same speaker a few days or weeks apart. There is little use in recording the same example set twice as part of the same session because this will almost certainly produce repetition effects.

Third and finally, it is important to keep in mind that instrumental acoustic analysis is increasingly regarded as an essential part of reliable descriptions of prosody, and that preliminary instrumental work in the field may be invaluable for guiding your work. This means that elicitation must be done in such a way that the resulting recordings are usable for instrumental analysis. In devising test examples for prosodic features, it is

important to pay attention to the segmental make-up of the example in order to minimize microprosodic effects (see sec. 6.4.5). However, it is often not possible to come up with materials that perfectly control for microprosody; either the phonotactics of the language may prohibit certain sequences that would be useful to include in your materials, or the only lexical exemplars of a particular sequence may create meaningless, obscene or ridiculous sentences that native speakers may refuse to say or will be unable to say naturally. As usual in experimental work, there is a trade-off between naturalness and the control of interfering variables.

5.4.3 Problems in prompting speakers

As the example of English WH-question intonation makes clear, eliciting example sentences for prosodic research requires attention to various factors that are not usually of concern to fieldworkers, and makes demands on speakers that ordinary phonological and syntactic fieldwork may not. Suppose you carefully construct a question-answer pair, paying attention to both pragmatic plausibility and segmental make-up. It is not enough to get native speakers to produce the segments of which the example sequence consists; they have to produce the first part as a question, the second as an answer. Do not underestimate the problems involved in explaining the idea of pretending to pose a question or give an answer. Moreover, be aware that some speakers may be unable to do things like this naturally, even if they understand the idea. This is one of the reasons why it is important to record multiple speakers wherever possible: without being able to compare across a sample there is no way of knowing who is acting reasonably well and who is doing something else.

We just spoke of carefully constructing question-answer pairs for native speakers to produce, but there is a significant problem of how to present tokens for prosodic research without unduly influencing the speakers. It is of little use to have a speaker repeat what the fieldworker is saying, since there may be direct effects of repetition on the speaker's production, or the speaker may in some way imitate the researcher's model. If you are working in a literate community, reading can be a good method for eliciting intonational data, provided that the speakers understand the need to vocally enact the illocutionary force of the example sentences. Unfortunately, it often happens that even literate speakers are unable to read fluently in their native language; it is common to find speakers who are literate in a majority or national language but have little practice or experience reading their native language. One technique that has been successfully used with such speakers is to present them with material written in the language they are comfortable reading, and ask them to give equivalents in their own language. But only some speakers will produce natural-sounding utterances under such conditions. It is also known from work on major European languages that the intonation patterns found in reading do not perfectly match those found in spontaneous conversation. Here the influence of the standard norm may be a major issue.

If reading is not feasible, various role-playing and experimental tasks may be useful. For example, rather than constructing question-answer sequences in advance and asking speakers to 'enact' them as naturally as possible, one may try to involve speakers in some kind of game or role play which requires them to ask questions. A technique widely used for this purpose involves matching tasks where one speaker instructs another speaker in reconstructing an arrangement of figures, pictures or points

on a map which is only visible to the instructing speaker (see map task, space games, etc. /REF/ and chapter ??[needs input by Dan]). Another technique is to have speakers look at a picture sequence or watch video clips and then to describe these or comment on them (pear film, frog story, etc. /REF/ ?? [needs input by Dan]). The big advantage of these techniques is that speakers are prompted with non-linguistic materials, and relatively spontaneously produce naturalistic speech. Moreover, unlike completely open-ended tasks such as recounting narratives or engaging in free conversation, these tasks permit a certain degree of control over what speakers will do, which makes it possible to collect comparable data from several different speakers. While it is rare that speakers produce completely identical utterances in these circumstances, a well-devised task usually requires them to use particular words, phrases or constructions and to engage in specific linguistic routines such as asking questions or giving directions.

Such tasks are not without their problems, however. The major problem is that speakers in small and remote communities are generally not familiar with the idea of role-playing or experiment and may be unable or unwilling to participate. It is not unknown, for example, that speakers who are asked to retell a video clip they just watched comment on the colors of the main participant's clothes or the nature of the setting rather than the action depicted in the clip. Considerable time and ingenuity may thus be required in adapting the experimental set-up to the specific circumstances found in a given speech community and in explaining the task.

5.4.4 Perception experiments

For prosodic analyses it may also be desirable to obtain some perceptual data in addition to the production data generated with experimental tasks or documented in narratives and conversations. Perceptual data are needed to answer questions such as: Do native speakers actually perceive prominences at those locations where they appear in the acoustic data (or where they are perceived by the fieldworker)? Which of the various factors contributing to a given prominence (intensity, duration, vowel quality, change and height of pitch) is the one actually of major importance in the language at hand? Which parts of a pitch contour are actually perceived as major cues for question intonation? Such questions can generally only be answered with some degree of certainty by devising perceptual tests, i.e. manipulating the prosodies of example clauses or phrases and testing speakers' reactions to them. For example, one may reduce the duration of putatively stressed syllables and ask speakers to identify stressed syllables in tokens computationally modified in this way, comparing the results with results obtained when identifying stressed syllables in naturalistic (unmodified) tokens. See van Zanten et al. (2003) and Connell 2004 for detailed descriptions of such experiments.

Once again, however, it has to be pointed out that administering such experiments is not a straightforward matter and will not necessarily produce satisfactory results. Apart from problems involved in getting speakers to participate at all in a listening experiment (in some instances, putting on a headset may already be a problem), the main problem pertains to defining a task which speakers are able to perform and which also generates relevant data. In most non-literate societies, it will be impossible to use concepts such as syllable or prominence in explaining a task. Task types that may work

- to a certain degree at least - are: a) asking speakers to comment in a general way on prosodically modified examples (which produces very heterogeneous and non-specific results but may still be useful in providing pointers to relevant parameters); b) tasks which involve the comparison or ranking of similar tokens (Which of these two items sounds 'better'/'foreign'? Which token would you use when speaking to your mother? etc.).

5.4.5 Computer-aided acoustic analysis

Perception experiments of the kind just mentioned presuppose the use of programs for acoustic analysis such as Praat, Emu, Wave Surfer or Speech Analyzer (see chapter ??). Use of such programs is strongly recommended for all kinds of prosodic analyses. The main reason for using them is that they may be of help in overcoming biases in one's own perception of prosodic data and in detecting phenomena one has not been listening for. As further discussed shortly, acoustic data are always in need of interpretation and auditory crosschecking. Nevertheless, they provide the only objective source of prosodic data, and an analysis which goes against major acoustic evidence is almost certainly false.

The programs just mentioned provide fairly reliable acoustic analyses of duration, intensity and F0. These can be done on a laptop in relatively short time and hence are feasible also in field situations provided that laptops can be used at all. Handling the programs can be learned in a few hours (in particular in the case of Speech Analyzer or Wave Surfer). Hence, it would be most inefficient not to use these tools when tackling the prosodic analysis of a previously undescribed language.

The current section briefly reviews the most important parameters to keep in mind when interpreting F0 extraction. For effective fieldwork it is not necessary to understand the mathematical and engineering aspects of F0 extraction. However, it is necessary to know something about the factors that affect F0 in order to interpret pitch contour displays appropriately and to select speech materials for phonetic analysis. It is easy to be misled by what you see on the screen, and easy to make instrumental measurements that are nearly worthless.

The rate of vibration of the vocal cords can be briefly but substantially affected by supraglottal activity – that is, by the fact that specific vowels or consonants are being articulated at the same time as the vocal cords are vibrating. Such effects are often collectively referred to as microprosody. Fig. /// shows instrumental displays of two English utterances, pronounced with pitch patterns that are impressionistically the same (listen to sound files ///). However, it can be seen that the two pitch contours look very different.

a Is that Betty's kitten?

b Are you Molly Newman?

The most obvious difference is that in (b) the contour is continuous, whereas in (a) there are many interruptions. This makes sense if we recall that we must have voice to have pitch: voiceless sounds have no periodic vibration and therefore no F0. As listeners we are scarcely aware of these interruptions, but on the screen they are very

conspicuous. Even more conspicuous is the fact that the F0 in the immediate vicinity of the interruptions jumps around a lot. These so-called “obstruent perturbations” are caused in part by irregular phonation as the voicing is suspended for the duration of an obstruent, or (in the case of voiced obstruents) by changes in airflow and glottis position as the speaker maintains phonation during partial or complete supraglottal closure. Even an alveolar tap (as in *Betty*) often causes a brief local dip in F0; a glottal stop (as in *kitten*) often causes a much greater local dip. The consequence of such obstruent perturbations is often that the pitch contour on a vowel flanked by obstruents (like the first syllable of *kitten*) looks like an abrupt fall on the visual display, even though perceptually and linguistically there is no significant pitch change on the syllable at all.

Methodologically, the existence of obstruent perturbations means that great care must be taken in interpreting visual displays of F0. Beginners tend to overinterpret what they see on the screen. In case of a conflict between what you see on the screen and what you hear, trust your ears! Obstruent perturbations also mean that the best samples of speech for making instrumental measurements of pitch are stretches containing as few obstruents as possible.

The other type of microprosodic effect that it is important to be aware of is “intrinsic pitch” or “intrinsic F0” of vowels. The phenomenon here is very simply stated: vowel quality affects pitch. Other things being equal, a high vowel like [i] or [u] will have higher F0 than a low vowel like [a]. If you say *to Lima* and *a llama* using the same intonation pattern and being careful not to raise or lower your voice between the two, the F0 peak on *to Lima* will be higher than that on *a llama* even though they sound exactly the same. This effect appears to have some biomechanical basis, although it is not entirely clear what that basis is. No language has ever been discovered to be without intrinsic F0 effects, although in some languages with more than two lexically distinct level tones the effect may be smaller than in other languages.

The methodological significance of intrinsic F0 is that if you want to measure F0 level instrumentally, you need to control vowel quality. Don’t try to compare measurements of mid tones and high tones if all the mid tones occur on [i] and all the high tones occur on [o]. Be sure to compare like with like.

5.5. A final thought

In addition to being a central part of any language description, prosody is relevant to the fieldworker in a very different way, because it may affect communication with native speakers and local authorities. It has frequently been suggested that misunderstandings in cross-cultural communication can be caused by misinterpreting prosodic cues. Although there are certainly generalizations about the sentence-level uses of prosody that are valid in language after language, the details may differ in crucial ways. What sounds rude and aggressive to one party may just be the normal way of marking emphasis for the other. A noticeable fall in pitch at the end of a unit may signal a simple assertion to the non-native hearer, but the speaker actually intended to pose a polite question. And misunderstandings may occur even if the fieldworker and the community members use a contact language to communicate, because both

parties will tend to bring their native prosodic systems to the contact language. So an appreciation of the ways in which prosody can differ from language to language is in itself an essential tool for successful fieldwork.

References

Abramson 1962

Beckman, Mary E., 1986, *Stress and non-stress accent*, Dordrecht: Foris

Chafe, Wallace L., 1994, *Discourse, Consciousness, and Time*, Chicago: The University of Chicago Press

Chuang 1958

Croft, William, 1995, "Intonation units and grammatical structure", *Linguistics* 33:839-882

Cruttenden, Alan, 1997, *Intonation*, 2nd ed., Cambridge: Cambridge University Press

Donohue, Mark, 1997, "Tone system in New Guinea", *Linguistic Typology* 1:347-386

Ewen, Colin J. & Hulst, Harry van der, 2001, *The phonological structure of words*, Cambridge: Cambridge University Press

Gussenhoven, Carlos, 2004, *The phonology of tone*, Cambridge: Cambridge University Press

Hyman, Larry M., 2001, "Tone systems", in: Martin Haspelmath, Ekkehard König, Wulf Oesterreicher & Wolfgang Raible (eds), *Language Typology and Language Universals*, Berlin: de Gruyter, 1367-1380

Jun, Sun-Ah, 1998, "The Accentual Phrase in the Korean prosodic hierarchy", *Phonology* 15: 189-226

Ladd, D. Robert, 1996, *Intonational phonology*, Cambridge: Cambridge University Press

Lambrecht, Knud, 1994, *Information structure and sentence form : topic, focus, and the mental representations of discourse referents*, Cambridge: Cambridge Univ. Press

Laver, John. 1980. *The phonetic description of voice quality*. CUP

Lehiste, 1970

Lindström, Eva & Bert Remijsen, 2005, "Aspects of the prosody of Kuot, a language where intonation ignores stress", *Linguistics* 43(4), 839-870

Pike, Kenneth L., 19

Rialland, Annie & Stéphane Robert, 2001, "The intonational system of Wolof", *Linguistics* 39: 893-939

Selkirk, Elisabeth O., 1984, *Phonology and Syntax: The relation between sound and structure*. Cambridge, Mass.: The MIT Press

Sluijter and van Heuven 1996

Steedman, Mark, 2000, *The syntactic process*, Cambridge, MA: The MIT Press.

Van Zanten, Ellen, Robert W.N. Goedemans and J.J. Pacilly, 2003, "The status of word stress in Indonesian", in: Van de Weijer, Jeroen M., Vincent J.J.P. van Heuven and Harry G. van der Hulst (eds), 2003, *The Phonological Spectrum II: Suprasegmental Structure*, Amsterdam: Benjamins, 151-175.

Yip, Moira, 2002, *Tone*, Cambridge: Cambridge University Press

"One of the first tasks which confront the linguist in examining a new language with a view to discovering and describing its structure is the identification of the minimal meaningful units of which the language is composed." Nida (1974 [1946], p6)

CHAPTER 6. MORPHOLOGICAL FIELDWORK

6.1 INTRODUCTION

In my early days of research among the Pirahãs, every single utterance was nearly impossible to understand. Since the Pirahãs do not speak Portuguese, there was no way for me to get a translation of anything they said. And, for reasons I was not to understand for many years, they didn't seem to have simple verbs. Where I expected a simple verb for, say, smelling, I would get long multisyllabic utterances. Each word I was able to isolate and understand was a costly accomplishment. Learning what a particular morpheme meant was cause for celebration.

In polysynthetic languages, e.g. many American Indian languages, the hardest nut to crack is the morphology. And the difficulty of understanding the meaning of individual morphemes is not likely to be significantly easier in any language, even in languages that lack inflectional morphology (e.g. Wari', Everett (2005a, 2005b) or even have no morphology (if there are in fact such languages). If a language has little in the way of morphology proper, this will simply move the problem of understanding the meanings and functions normally conveyed by affixes one level higher, to the word or, more likely, to the phrasal level. In other words, no fieldworker can avoid analyses of words and how they fit into the grammar, a matter that always entails morphological principles.

Therefore, what I want to do in this chapter is to help equip the fieldworker for successful morphological field research. Of course, a single chapter, or two or three, is not really enough space to provide significant detail about all aspects of morphological analysis. There are, after all, entire books dedicated to these matters (e.g. Nida () and Pike (). Nevertheless, this chapter aims to provide at least a 'check list', with examples to remind the fieldworker of the minimum required for data-collection and morphological analysis in the field.

One might think we could begin by simply answering the question 'What is morphology?' and then get on with a methodology for it. Morphology can be usefully defined as the study of words and how they are formed (see Everett (in progress) for various approaches to the study of word formation).

And yet it is not always clear where the dividing line falls between morphology and syntax, a problem that can confound both the fieldworker and the theoretician. Syntax is often defined (at least informally) as the study of how words get put together to form sentences. But in practice, the fieldworker can often only understand word structures if they understand the sentential contexts (structural and semantic) in which the various word-forms may appear and vice-versa. So one needs to study syntax and morphology together, even though one might believe that morphology and syntax are ultimately distinct components of grammar (e.g. that they are organized in different ways and/or have different principles – see Ackema and Neeleman (2004) for useful discussion).

Although the morphology and syntax of a language need to be studied together, there are still principles and methodological procedures unique to word structure. So I focus on those here. But where necessary, syntax is brought into the discussion to

explicate morphological concepts and their implications.²⁵ I want to offer three caveats on morphological fieldwork before beginning with a consideration of morphological fieldwork proper.

6.2. CAVEATS

6.2.1. PHONOLOGY IS NOT MORPHOLOGY

You will hear a cacaphony of confusion when you first enter an area where you do not speak the language (e.g. as I do now as I write this in Leipzig and *ich spreche nur ein bisschen Deutsch*.) And it can depress you if you think too long about that fact that it is your responsibility to bring order to your confusion, to find euphony in the apparent cacaphony.

In my first linguistics course, Professor Kenneth L. Pike asked the class how we might analyze the form in (1), which he claimed to be common in English (class lectures, University of Oklahoma, 1976):

(6.1) **squeat**

I did not know what he was talking about. What does this mean? Pretending for the moment that we do not speak English, let's approach this problem like fieldworkers. We first need to consider the context in which () is uttered. It turns out that it is usually uttered by a member of a group preparing to dine together. The linguist must observe this context him/herself. They cannot ask speakers directly what an utterance means without supplying them with a context. In fact, native speakers should only be asked to opine about data – and then only when they fully understand the relevant context of the items under discussion in very carefully controlled circumstances. After providing a context, on the other hand, if we then asked speakers of this dialect what *squeat* means, some speakers might understand it and might be coaxed to repeat it more carefully. If they do, they are likely to say something like:

(6.2) Oh, you mean '*let's go eat*'.

With this more careful repetition, it becomes clearer that the phonology of () obscures its morphology. This is an important point. To see this perhaps more clearly, let us go deeper into the phrase. How might a field linguist analyze the **s** at the end of [lEt], for example? They might be tempted to conclude that this **s** is part of the word [lEt], basing this morphological decision on the phonetics of the word form.²⁶ But this

²⁵ Interestingly, in my experience when a language has a complex morphology, that morphology is more difficult to analyze than the syntax of the language, however complex the latter.

²⁶ This is not a textbook on morphology, but some terminology would be useful here nonetheless:

word form: a phonological 'word', without regard to its morphological composition, a constituent which fits the phonological definition of word in the language. So, for example, **let's** is a word form in English. As is **am**, **is**, **were**, **gone**, **going**, etc.

would be wrong, clearly. Although *s* is indeed part of the *phonological* unit that precedes it, it is best analyzed as an independent word syntactically, a shortened form of the pronoun, [Us]. This is what Zwicky (1977, page?) labels a 'simple clitic'. In other words, the phonological data in this (simple) case once again conflicts with the morphosyntactic data. This leads me to offer the following related warning:

(6.3) Be careful not to misanalyze clitics, affixes, and function words because of their phonology.

A subtype of (3) is (4):

(6.4) Cliticization is often confused with affixation.

Consider, for example, how a novice fieldworker might analyze the English examples in (5) and (6):

- (6.5) a. John's hat.
b. The man's hat.
c. The man who was running's hat.
d. The man you spoke to's hat.

(6.6) I'll come tomorrow.

In (5) there is an enclitic which attaches to the final word of the noun phrase. This enclitic, as it turns out, is indeed morphologically, syntactically, and phonologically part of the preceding phrase. By analogy, the fieldworker might analyze the *'ll* of (6) in the same way, assuming that it too 'goes with' the word on its left. But semantically and syntactically, *'ll* forms a unit with the verb that follows it, not with the word that precedes it. That is, its phonological host is unrelated to it syntactically. This phenomenon is often labelled 'wrong-way' cliticization in the literature, a case where the phonology and morphosyntax conflict. An additional example of wrong-way cliticization, further underscoring the importance of keeping phonology and morphology separate in fieldwork, comes from Yagua, a language spoken in Peru, from (Everett 1989, 343, who takes it from Payne & Payne ()):

- (6.7) Sa_i -pu7@u7@chi(*-ní) Pauro_i rooriy(*-ní) viimú -ní_j Anita_j.
3sg.cl -carry Paul house into 3sg.cl Anita
'Paul carries Anita into the house'

word: an abstract morphosyntactic unit that is not heard, but posited to account for morphosyntactically relevant word behaviors.

lexeme: the minimal unit of the lexicon.

Similar (though not identical) definitions for these are offered in Matthews (1991, 24ff).

Everett (1989) attempts to explain, among other things, the observations of Payne () and Payne and Payne () that Yagua object clitics, like English auxiliaries, undergo 'wrong-way' cliticization. The clitic –*níí* in () must attach to the immediate left of the object, in this case, **Anita**. It cannot attach directly to the object, but it must nevertheless be immediately to its left. So although the clitic and the object do not form a constituent phonologically, they do syntactically (at least according to Everett (1989)).

To sum up these points aphoristically: *What you hear is not what you get*. Or more scientifically, *articulation obscures morphosyntactic boundaries in normal speech*. You must think, analyze, and argue for all your conclusions. Nothing is obvious in linguistic analysis, especially not in morphological analysis. Do not be fooled by confusing phonology with morphology! Let us consider now another caveat, namely, that:

6.2.2. SEMANTICS IS NOT MORPHOLOGY

The fieldworker must also be careful not to assume a direct or transparent relationship between semantics and morphology. A meaning that is expressed as a single morpheme in one language may not be expressed at all in another language or it may be expressed by multiple words or morphemes. Although semantic arguments, like phonological and phonetic arguments, can be useful in identifying and classifying words, word classes, morphemes, etc. in a language, there is no algorithm for translating from one to the other or any necessarily direct relationship.

To understand this a bit better consider the category of tense. Consider the question, "Does English have a future tense?" in light of the examples in ():

- (6.8) a. I will go tomorrow.
b. I am going to go tomorrow.

Are either of these the future tense in English? Well, if you mean semantics, then the answer is 'yes', they both express (among other things) a future meaning. But if you mean morphology, then the answer is 'no', neither of these examples contains a morphological future tense. In fact, morphological and semantic categories are often confused in just this way in the professional literature. Everett (1993), for example, argues that Pirahã has no morphological tenses, though it does have 'absolute tenses' (Comrie (1985)) semantically. But I argue there as well that Pirahã has no relative tenses, either morphologically or semantically.

It is common as well for novice field linguists to find it difficult to keep semantics and syntax separated as they prepare a grammar of an unstudied language. For example, linguists I have worked with in following some sort of questionnaire e.g. the Lingua Descriptive Series Questionnaire (LDSQ), often initially answer affirmatively to some of the structural or constructional questions of the LDSQ (of the type 'Does the language have relative clauses?') even when the language in question lacks such structures, just because there is a way to express that meaning in the language under investigation.²⁷ So, for example, does English have 'evidentials' (see section ____ below) – morphological markers that indicate the source/type/quality of evidence for a given assertion? Morphologically, no, English does not have such markers. But of course an

²⁷ The LDSQ can be found at: <http://lingweb.eva.mpg.de/fieldtools/linguaQ.html>

English speaker can always add to a statement, 'I saw that with my own eyes' or 'So they say', using these expressions like evidentials semantically. So English can express evidentiality, even though it has no evidentials. This is a very important point that is not nearly as easy to recognize in fieldwork as it is in the linguistic classroom. In other words, when linguists investigate *structure*, they are not thereby investigating the circumlocutions that might be used to express the meaning or function of the structure. They are concerned with understanding both form and meaning, of course, recognizing however that they should be studied separately (initially at least) in order to better understand each. An excellent discussion of these issues is found in Comrie, et. al. (1993).

A final caveat before beginning our discussion of morphological fieldwork proper is that:

6.2.3. SYNTAX IS NOT MORPHOLOGY

Although, as I said, syntax and morphology need to be analyzed nearly simultaneously due to their mutual influence, it *is* possible to tease them apart. They are not the same thing, at least not operationally in the field (see Ackema and Neeleman (2004) for interesting discussion of how these might be distinguished in the grammar (or not) and the consequences of different decisions). They differ most strongly, in my opinion, in that word formation rules are *paradigmatically constrained* (see Everett (2005) and ____ below).

And yet, morphology and syntax often appear to overlap – which can and will confuse the fieldworker unprepared for this. As Ackema and Neeleman (2004, 48ff) show, morphology and syntax can often 'compete' with one another. So, for example, consider the contrast in (9) and (10) (again, see Everett (in progress) for a survey of approaches to morphological analysis):

- (6.9) a. *felicitouser
b. more felicitous

- (6.10) a. bigger
b. *more big/more bigger

If an adjective cannot take the comparative suffix, *-er*, then it is made comparative syntactically, or periphrastically. The suffix might be said to *block* the use of the syntactic form. That is, the syntactic or periphrastic form and the affixal form are not in free variation in most dialects (though this is an empirical question that has not been well-investigated to my knowledge, even though its consequences for morphosyntactic theory are potentially important). We will see more evidence for this in section ____ on periphrasis. Such interactions reinforce both suggestions made here, namely, that syntax and morphology must be analyzed together by the fieldworker, yet they are not the same thing. Understanding English comparatives, in other words, requires us to understand the different roles of syntax and morphology in constructing comparatives, as it also requires that we analyze both together to see the full set of patterns emerge from the data. These are the main caveats. Now we are ready to begin our discussion of morphology fieldwork proper. I prefer to discuss morphological field analysis in terms of semantic distinctions and how these are marked, since as a fieldworker, I found this to be the most natural way to think about the issue.

6.3 SEMANTIC AND GRAMMATICAL DISTINCTIONS

6.3.1. NOMINAL CATEGORIES

The two most important questions for morphosyntactic fieldwork are (i) What semantic and grammatical distinctions are marked on words? and (ii) How are these distinctions marked? That is really all there is to morphology. There is a large, but fortunately, not too large, number of answers to each of these questions and many combinations thereof. But we can in fact offer a couple of simplified lists that, I believe, will to better conceptualize the issues.

I am using 'semantic distinctions' to refer to properties of the lexicon (as I see it) that are relevant for the syntax, e.g. aspect, number, class, case, tense, etc. Another way of putting this is as in (11):

(6.11) A morphologically relevant semantic distinction is any meaning contrast of a language x that is reflected in the *words* of x.

The terms we use at this point are not crucial. What we are after here is a counterpart to the philosopher J.L. Austin's (1962) famous *How to do things with words*, namely 'how to do things *to* words'. The most important rule of thumb in morphosyntactic field research is to *be alert* and *be creative* in your thinking, following *Postal's Maxims* (see ____).

We begin with an overview of the major semantic distinctions encountered on nouns, grouped under my headings:

6.3.1.1. REFERENCE: specific vs. referential vs. definite

This is a fundamental distinction that is perhaps a semantic universal, though its morphological reflex (marking on word forms) is less so.

Consider the following sentences:

- (6.12) a. John bought a book about Vietnam.
 b. John bought the book about Vietnam.

These examples are, respectively, indefinite and definite. In general (but see Lyons (1999)) the definite article indicates shared knowledge between speaker and hearer, or old information from the discourse. The indefinite is often used to introduce new participants into a discourse, or to offer a different type of quantificational reading. But there is more to them than this. In both of these examples the speaker can either have a very specific book in mind or not. For example, both can be continued as in (13) and (14):

- (6.13) a. John bought a book about Vietnam, in fact he bought *Fire in the Lake*, by Frances FitzGerald.
 a. John bought a book about Vietnam, but I don't know which one.

- (6.14) a. John bought the book about Vietnam, or at least that is how he was describing it, I really don't know anything else about it.
 b. John bought the book about Vietnam, you know, the one in the window we saw yesterday?

What is going on in these examples? Well, they show that in addition to definiteness, examples can be crossclassified by the notion of *referentiality*, that is, whether they refer to a specific individual, i.e. one known to the speaker. In some languages, this can be marked morphologically or syntactically. So consider the following example from Persian (from the LDSQ):

- (6.15) a. *Hasan yek kita:b-ra: xarid*
 Hasan bought a specific book
 b. *Hasan yek kita:b xarid*
 Hasan bought some book or other

Both of these examples are indefinite. But the special marking, **-ra** on **kita:b** 'book' in (15a) is there to indicate that the example is *referential*, that is, that it refers to a specific individual. Such distinctions, though 'only' semantic in English, can be vital to understanding the morphology of some languages.

Another semantic distinction vital for morphosyntactic analysis is between major participants of the designated verbal event or state:

6.3.1.2. GRAMMATICAL ROLES GRAMMATICAL RELATIONS

Perlmutter & Postal () launched an extremely interesting and very influential research program on grammatical relations, Relational Grammar, by explicitly adopting the hypothesis that grammatical relations are syntactic primitives which are causally implicated in numerous syntactic operations, especially those based on VOICE (see ___) and INFORMATION STRUCTURE (see ___). One of the appeals of Relational Grammar was that its concept of grammatical relations and their relation to syntactic operations was intuitively very appealing. Initially, at least, the theory also seemed to enjoy a great deal of empirical success and conceptual simplification in handling numerous operations that were much more clumsily handled in standard Transformational Grammar, for example. Many linguists still find it convenient to talk about notions like subject, direct object, indirect object, and so on. To see how such notions can be useful, consider that the passive in English and other languages can be generalized to a rule like ():

(6.16) Passive: Promote the direct object to subject position and demote the subject to an oblique relation.

- (6.17) a. John hit Bill.
 b. Bill was hit *by John* (John has been 'demoted' to the oblique object of *by*).

Unlike transformations that operate on syntactic categories, the use of grammatical relations rather than categories such as NP, PP, etc. as the basis of grammatical rules offers clearer and more satisfying cross-linguistic accounts of voice alternations. The reader is urged to consult Perlmutter & Postal (), and the large literature on Relational Grammar for further discussion of the usefulness of grammatical relations in understanding syntactic processes.

MACRORoles

An alternative to grammatical relations is proposed in Role and Reference Grammar (Van Valin and La Polla ()), under the label of MACROROLES. Macroroles are different from Grammatical Relations in that, among other things, they are linked simultaneously to the semantic roles specified in a verb's lexical entry and the ways in which the specific nature of these lexical roles are 'neutralized' in the syntax. Macroroles overlap with Grammatical Relations in many simple cases, but they are by no means isomorphic to Grammatical Relations.

For example, ACTOR is a generic term for all semantic roles that are associated with causing or controlling, etc., an action in a specific clause. That is, it is the label for the set of roles whose specific semantics are neutralized in a particular construction. UNDERGOER is the label for the neutralization of roles associated with arguments that suffer the consequences of the action in some sense. Consider the following examples, from Van Valin (), with the actor vs. undergoer roles labeled:

- (6.18) a. The *teacher* is reading the words. Actor of transitive
V
b. The *teacher* is singing. Actor of intransitive
V
c. The *teacher* fainted. Undergoer of
intransitive V

Example (6.18c) is interesting because it shows that subjects of simple clauses in which there is no obvious dislocation can also be Undergoers, i.e. that subjects do not map directly to Actors.

- (6.19)
a. *The *teacher* are reading the words. *Undergoer of transitive V
b. The *words* are being read by the teacher. Undergoer of passive V

Example (6.19a) shows that Macroroles do not directly govern agreement in English. In Relational Grammar this is because subjects, not macroroles govern agreement in English. However, Van Valin and La Polla (1997) argue that Grammatical Relations are really not what is needed to account for agreement and other apparently Grammatical Relation-sensitive processes in syntax. Consider other ways in which Macroroles may be manifested in the syntax.

- (6.20) a. *Chris* wants to drink a beer. Actor of transitive V
b. *Chris* wants to sing in the park. Actor of intransitive V
c. *Chris* wants to be stronger. Undergoer of intransitive V
d. **Chris*_i doesn't want the journalist to interview _____j. *Undergoer of transitive V
e. *Chris* doesn't want to be interviewed by the journalist. Undergoer of passive V

This example is interesting because it shows that Macroroles can only govern verb agreement or appear in a dislocated position if certain conditions on Voice are met. The next examples show that English is quite liberal in the semantic roles that may

head relative clauses. In many languages only the Actor or the Actor plus the Undergoer roles can head relatives (all such cases are discussed in detail in Van Valin (2005)).

- (6.21) Mary talked to the man (a) who [A] bought the house down the street.
 (b) who [U] the dog bit.
 (c) to whom [recipient] Bill showed the house.
 Mary looked at the box (d) in which [locative] the jewelry was kept.
 (e) out of which [source] the jewelry had been taken.

Verbs have a semantic structure that, among other things, specifies for a given verb how many arguments it needs and what the semantic relationship of those arguments is to the verb.²⁸ Languages rarely, if ever, mark these various lexical relations directly in the morphology or the syntax (though cf. VVLP's treatment of Acehnese (pp255ff)). More commonly, languages choose to 'neutralize' (VVLP pp251ff) some number of these distinctions by grouping the more finely-grained lexical argument distinctions under larger categories. Tagmemics (Pike & Pike (1976)) and Role and Reference Grammar (VVLP), suggest the labels 'Actor' and 'Undergoer' as the two basic 'macroroles' (VVLP, 139ff), under which the more specific semantic roles are neutralized. These macroroles can have important consequences for a language's morphology, as seen in examples like (16)-(19), from Kulina, an Arawan language (Wright 1988):

- (6.22) **Kodzo** **ts^hitei** **-na** **-hari.**
 lizard.M shoot **3on3** -AUX-COMP.M
'S/he shot the lizard.'

- (6.23) **Aoi dzoho** **i** **-na** **-haro.**
 tapir.F carry **3on3** -AUX-COMP.F
'S/he carried the tapir.'

- (6.24) **Kodzo** **ts^hiteØ** **-na** **-hari.**
 lizard.M shoot **3** -AUX-COMP.M
*'He (*She) shot the lizard.'*

- (6.25) **Aoi dzoho** **Ø** **-na** **-hari.**
 tapir.F carry **3** -AUX-COMP.M
*'He (*She) carried the tapir.'*

Wright (1988) takes the examples with the **i-** '3on3' prefix (third person Actor and third person Undergoer) ((16) and (17)) to be the basic/underlying forms. If one makes this decision, then in the basic form there is agreement with the object. In the 'derived' form, the agreement is with the subject and the object no longer triggers agreement. From this perspective, the **i-/Ø** alternation's effect on gender agreement looks (somewhat) like an ergative/antipassive alternation (see ___ below on ergativity and antipassive). On the other hand, if one takes the **Ø**-forms to be basic, and the **i-**

²⁸ Arguments are expressions required to *saturate* a verb's valence (see ___ below).

forms to be derived, then the structure will look more like a passive (this is the analysis of Campbell and Campbell 1990 for Yamamadi (a language closely related to Kulina), where *hi-* is the cognate of *i-*. It should be noted that that the *hi-/i-* prefixes only appear when both subject and object are third person (any number). When either or both is nonthird person, there is no special agreement prefix and only the gender alternation is seen. Cf. (20) and (21):

(6.26) **Kodzo** **ts^hiteo** **-na** **-haro**.
 lizard.M shoot *I* -AUX-COMP.F
 'I shot the lizard.'²⁹

(6.27) **Kodzo** **ts^hiteo** **-na** **-hari**.
 lizard.M shoot *I*-AUX-COMP.M
 'I shot the lizard.'

In these examples the only visible difference is the gender of the verb. In (20) it is governed by the subject and in (21) by the object.

For our purposes, the crucial observation is that Arawan has morphological strategies for distinguishing different pragmatic functions of Actors vs. Undergoers. It is not necessary to label them as either 'passive' or 'antipassive'. One way to think of the alternations in Arawan, as well as related, but nonidentical, alternations in other languages, e.g. passive and antipassive, is that a language can select either the Undergoer or the Actor as the unmarked morphological (and pragmatic as well, in most cases) argument. When the topicality of that unmarked argument is altered (by becoming more or less topical), then the morphology will usually 'kick in' to signal this.³⁰

6.3.1.3. POSSESSION: alienable, inalienable, intermediate

Languages often distinguish between *alienable* vs. *inalienable* possession, i.e. nouns that need not be marked as possessed in the morphosyntax and nouns which *must* be so marked (or, alternatively, must have a special form to signal that they are violating the normal expectation that they be possessed). Below I illustrate all of these points from Banawá and Wari.

As in other Arawan languages, there are two main syntactic types of possession in Banawá, alienable and inalienable. The former is subdivided into kinship vs. nonkinship. All nonkinship alienably possessed NPs include the clitic **ka** 'possession' occurring between the possessor and possessum. Kinship terms do not always take the **ka** clitic and in such cases are similar to inalienably possessed forms. They are *unlike*

²⁹ First and second persons and all plural forms are feminine throughout the Arawan languages, regardless of the sex of the real-world referent.

³⁰ In addition, Actors, Undergoers, and other semantic relations, can be further neutralized by interposing another level of organization on the verb's arguments, namely, grammatical relations, e.g. Subject, Object, etc. But not all theories recognize these grammatical relations as having any usefulness to morphosyntactic analysis. Even theories as otherwise distinct as Role and Reference Grammar and Minimalism reject any role for grammatical relations in their view of grammar.

inalienably possessed forms, however, in that the gender of a possessed kinship term is determined by the kinship term rather than its possessor.

Alienable possession:

- (6.28) **Inaso ka yomai rabikei no, bede ka -wei.**³¹
 Ignacio ka dog.M sick.M past, run comit -cont.M
'Ignacio's dog got sick and ran away.'

Inalienable Possession

- (6.29) **kosiba efe wiri to -witi -ei**
 babassu:palm leaf strip away-end.point -M
The babassu palm leaf was stripped off to the end.'

In example (6.29), the gender agreement on the verb can be controlled by the possessor (**kosiba** 'babassu palm'), but not the possessed item (**efe** 'leaf') and there is no **ka** possession marker allowed. These are two of the criteria for recognizing inalienable possession in Banawá. Banawá also shows a somewhat intermediate category of possession for kinship:

Kinship possession:

- (6.30) **oda ka ami -rawa oda da -daba -bisa towa -maro**
 1P ka mother -pl.F 1P red -accomp -also be -past
'We stayed with our mothers also.'

- (6.31) **ami da -daba -bisa towa -maro**
 mother -pl.F red -accomp -also be -past
'You stayed with your mother also.'

In example (25), the possessor of **ami** 'mother' is omitted, but trivially recoverable because **ami** is the suppletive term for second person's mother. With third person kinship terms (also marked by suppletion in Arawa) following Campbell (), the

³¹ In personal communication, Alan Vogel suggests that the crucial portions of the examples in ()-() in the text should in fact be analysed as below:

- () *bede kawei*
bede ka- na -wei
run COMIT- AUX -now+M
- () *wiri towitiei*
wiri to- na -witiei
strip.off away- AUX -out+M
- () *towamaro*
to- ha -maro
INCH- AUX -past

Vogel may well be right. But this relatively abstract analysis has not yet been demonstrated to my satisfaction.

possessor of the kinship term can be omitted if it is recoverable from the discourse, unlike normal alienable possession, but like inalienable possession. Thus kinship can occasionally behave intermediate between the two types. Everett (in progress) accounts for this in terms of qualia (see ____ below).

Kinship terms and possession in Wari' are discussed in detail in Everett and Kern (1997:434ff.). There are two formation classes of kinship terms in Wari'. The first includes what Everett and Kern (1997) call *-xi'* nouns, where *-xi'* is the first-person plural inclusive possession suffix. Only inalienably possessed nouns take possession suffixes and these in turn can be inputs to other word-formation rules, as discussed in Everett and Kern (1997:235ff.). In these forms, the citation or base for the paradigm is first-person plural inclusive. The first-person singular inflection of six of the terms is suppletive; otherwise they all inflect for possession with the *-xi'* suffix. The complete list of *-xi'* nouns, all in their '1pl inclusive' forms, is given below (first-person singular alienably possessed forms are in parentheses).

Simplex:

- (6.32) a. *'aramanaxi'* (*'aramana*) 'sister' (lit., female of the species; male ego)
 b. *cainaxi'* (*caina*) 'daughter' (female ego)
 c. *cawinaxi'* (*cawiji*) 'son' (female ego)
 d. *humajixi'* (*humaju*) 'children' (female ego)
 e. *japinaxi'* (*wijapi*) 'wife's mother'
 f. *jinaxi'* (*jina*) 'granddaughter' (male ego or female ego's daughter's daughter)
 g. *manaxi'* (*mana*) 'wife' ('hole'; *mana* 'my wife/my hole')
 h. *namorinaxi'* (*namori*) 'wife's sibling'
 i. *tamanaxi'* (*tamana*) 'husband's mother'
 j. *taramajixi'* (*taramaju*) 'brother' (male of the species; female ego)
 k. *taxixi'* (*taxi*) 'husband'
 l. *wijinaxi'* (*wiji*) 'grandchild' (female ego's son's child)
 m. *winaxi'* (*wina*) 'grandson' (male ego or female ego's daughter's son)
 n. *xerexi'* (*xere*) 'sibling'
 o. *xinaxi'* (*wixi*) 'sister's son' (male ego)
 p. *xi'* (*na*) 'mother'

The only way to understand how '*-xi'* nouns' differ from other nouns semantically and morphologically in Wari' is to recognize the (non-universal) semantic distinction of inalienable possession (note that it is *not* necessary to understand the semantic basis per se as to why a given noun is an inalienably possessed noun. This can be helpful, but for diachronic reasons can be fairly asystematic).

Moreover, even languages that do not have inalienable possession morphologically can show traces of it in their syntax, as the following examples from Portuguese show:

- (6.33) Joao operou a perna.
 (i) John_i operated on the or his_{i/j} leg.
 (ii) John's leg was operated on.

In this example, quite common in Brazilian Portuguese, the 'passive' reading in (6.33ii) is available for part-whole relations, the kind that are often the basis for inalienable possession. Consider also:

- (6.34) O carro furou o pneu.
'The car's tire was punctured' (literally, 'The car punctured the tire.'

This example, also common in Brazilian Portuguese, is possible because *pneu* 'tire' is part of *carro* 'car'. Everett () explores these constructions in detail. The relevance of part-whole/inalienable possession relations to the syntax of such constructions is shown by ungrammatical examples like ():

- (6.35) *A casa roubou o carro.
'The house was robbed of the car.'

Although the syntax of this example is the same as (6.34) (perhaps coming from an underlying form e.g. ____ *roubou o carro da casa*, again, see Everett () for details), the example is ungrammatical because () does not involve a part-whole relation (cars are not parts of houses).

Notice too that whereas inalienable possession shows up (mainly) in the morphology of Wari' and Banawá, it appears in the syntax of Brazilian Portuguese. This again shows how semantic distinctions should not be confused with their modes of expression. Form is not function.

From the semantic category of possession, let's move to consider the category of number.

6.3.1.4. NUMBER: singular, plural, intermediate

Apparently all languages of the world, except Pirahã (Corbett (2001)), mark number morphologically. The most common distinction is singular (one entity) vs. plural (more than entity). But there are many languages that make intermediate number distinctions, e.g. 'dual' (two) and 'paucal' (3-5 or so), in addition to plural vs. singular. Therefore, when eliciting number, be sure to test for all these categories, especially if you already know that other languages of the area or the family make such distinctions. You can do this fairly simply by eliciting phrases, e.g. 'One man came'; 'Two men came'; 'Three men came'; etc. plus 'I saw one man'; 'I saw two men'; 'I saw three men'; up to five at least (be sure to collect numbers in at least the subject and object positions). This will cover most of the possibilities. However, once again, always 'keep your guard up' for distinctions that didn't show up in initial, simple tests, but which might appear in more subtle ways, i.e. elsewhere in the phrase, as you progress in your understanding of the language.

Another important, number-based semantic distinction made in most languages is the distinction between *count* vs. *mass* nouns, i.e. nouns that can be counted and nouns that cannot be, at least grammatically. So consider English examples like 'Some water' vs. *'Two waters' (where 'water' refers strictly to the substance H₂O, and not some standardized forms for containing or channelling it, e.g. cups or rivers) or 'Two arrows' vs. *'Some arrow'. Although linguists almost universally attribute these distinctions to countability, in my view, they may arise from something even more basic than counting, namely, visual perception. For example, the Pirahã language of Brazil (Everett 2005) lacks number, numerals, and counting, yet it apparently has a 'count' vs. 'mass' distinction. This could be evidence for the hypothesis that 'count' vs. 'mass' is a distinction of some 'universal grammar', so that it is found even if a language lacks means of expression for the related cognitive faculty of counting. But the simpler

hypothesis is that this derives from the fact that some entities are easier to perceive as made up of smaller units (i.e. they can be 'individuated'), whereas others are not so obviously decomposable visually. Thus it is possible that the distinction here has nothing to do with universal properties of grammar but, rather, with visual perception. The point is that although the fieldworker should be aware of distinctions drawn in a huge percentage of the world's languages, they must always exercise creativity, thinking 'laterally' to use a term from de Bono (1973).

The next important semantic distinction languages often make in one way or another on nouns (or their containing phrases; see section ____ below on ways of marking semantic distinctions) is class membership.

6.3.1.5. CLASS MEMBERSHIP: gender and more

The classes into which languages may divide their nouns are likely to have some sort of semantic motivation, at least diachronically. But in general, this motivation is obscured, perhaps completely, by normal historical change of the language.

The simplest class distinction is masculine vs. feminine. Languages that use this simple class system, as well as those that include a third class of 'neuter', are said to have gender systems. This is fine as a label, but the linguist should not forget that this is nothing more than a simple class system and that it is absurd to think that speakers of a language with 'gender' actually see every object in their universe as male or female. A number of objects in each linguistic gender class will indeed have a link to male or female in the real world (e.g. 'man', 'woman', 'girl', etc.) but certainly not all. For example, in Brazilian Portuguese, one says *A cola* 'The (feminine) cola', but *O guarana* 'The (masculine) guarana', where in this case guarana and cola are just two forms of softdrink. No Brazilian I have ever talked to thinks that *cola* is female while *guarana* is male in the real world. More complex classification systems have less obvious semantic unity in each class, by and large, and it is almost always a waste of time (i.e. a violation of the 'law of diminishing returns') to look for semantic motivation for each member of each class. What the field linguist really needs to understand is how these classes, such as they are, are causally implicated in the morphosyntax of their containing clauses and discourses. This is not to say that it will always be pointless to look for the semantic motivation behind class membership, not at all. But it is the case that looking for such motivation is less likely to be useful to initial fieldwork than carefully describing how the classes work structurally.

Class is often marked as a separate morphological category. For example, consider how gender can be marked in the Arawan language, Banawá. Banawá is interesting in that person and number agreement are marked on the rightmost boundary of the verb, i.e. as prefixes or proclitics, whereas gender is marked at the end of the verb, as a suffix. Moreover, gender agreement is not directly governed by either the subject or the object, but by the topic of the sentence which can be subject, object, or the inalienable possessor of the subject or object (see Everett (in progress). Dixon (2004) discusses similar facts in the related language, Jarawara).

We saw some examples of Arawan gender marking earlier from Kulina ((-)). When the Actor is the topic (what Dixon () refers to as the A-construction), the Actor governs gender agreement on the verb:

- (6.36) a. kobaya mere waka-nei
 pig wild:M 3P.OBJ break-AUX:M
 'He killed pigs.'

- b. **o_i** **-man_k** **wete** **Ø_j** **-ka** **-sei_j**
 1S.F -arm.M tie.up 3 -comitative -aspect.M
'He tied up my arms.'

When the Undergoer is the topic (Dixon's () O-construction), it controls gender agreement:

- (6.37) **dee** **ka** **-maki** **yomai** **owa** **nabowa bone**³²
 2P MOT -follow:F jaguar:M 1S.OBJ kill INTENT:F

awa **-tee** **o** **-ka** **abi** **ati**
 maybe -RECENT.PST.QUOT+F 1S -POSS father say

-nei
-AUX:M

'You all come here. The jaguar will kill me, my father said.'

But, just as with Kulina, when the Actor or the Undergoer is inalienably possessed, its possessor controls gender agreement:

- (6.38) **Mate,** **mate** **ime** **yama** **hi-** **kaba** **metemone**
 buttock:F buttock:F flesh:F thing:F 3- eat long.ago:F
mate
 buttock:F

hodi **wa** **-metemone,** **mate** **tone** **wa**
 hole:F stand-long.ago:F buttock:F bone:F stand -

-fara **-metemone.**
 in.open.area -long.ago:F

'The flesh of her upper thigh was being consumed by something, it was open, and her thigh bone was exposed.'

When the Undergoer is treated as topic and where the Undergoer is inalienably possessed, the possessor of the Undergoer, rather than the Undergoer itself, governs gender agreement:

Possessor (of Undergoer) controls gender agreement:

- (6.39) **o_i** **-mano_j** **wete** **hi** **-ka** **-sa_i.**
 1S.F -arm.M tie.up 3/3 -comitative -aspect.F
'My arms were tied up by him.'

³² Vogel (p.c.) also says that **bone** 'intent feminine' should in fact be **bona** 'intent masculine'. But my source is fairly clearly **bone**. I suspect that Vogel is right here, knowing far more than I about these languages, but I leave it this way just the same.

These examples are very useful because they show the interaction of possession types, voice alternations, and gender. Each of these semantic distinctions and the way that each is marked must be carefully analyzed or their interactions will be mysterious.

Everett () argues that the simplest analysis of the facts is that gender agreement is pragmatically governed, whereas person and number agreement are syntactically governed. Pragmatic government of the process requires the whole rather than the part to be the topic. That is, to consider (), you cannot tie up my arms without affecting me. It is 'my' that is the topic, therefore, not 'arms'. The point relevant to this guide is that CLASS is subtle, has effects throughout the clause, and care must be taken in its analysis.

6.3.1.6. PRAGMATICS

Arguably, transitivity alternations in languages have the function, among others, of tracking the pragmatic functions of nominals.

It is useful for languages to mark such alternations. We have already seen examples of this from Salish () – () and Arawan () – (). The point of this section is simply to remind the fieldworker to look for a variety of possible correlations between pragmatics and morphology. And the fieldworker must be aware that there are also important morphology-pragmatics connections unrelated to information structure, e.g. politeness markers and honorifics. So consider the examples below from Brazilian Portuguese and Japanese:

- (6.40) Tu vai(s) para o cinema hoje, rapaz?
'Are you (most informal) going to the cinema today, fellow?
- (6.41) Voce vai para o cinema hoje, rapaz?
'Are you (informal) going to the cinema today, fellow?'
- (6.42) O senhor vai para o cinema (, % rapaz)?³³
'Are you (formal) going to the cinema?'

Generally dialects of Brazilian Portuguese differ as to whether the informal usage is expressed by *tu* or *voce*. However, in some dialects, there is a three-way distinction, with (34) least formal and (36) most formal. Although not all dialects of Brazilian Portuguese that use *tu* mark the verb differently (hence the parentheses on the *s* in ()), some do. These examples thus show that politeness forms can affect word-formation.

In Japanese, on the other hand, there is a much more elaborate pragmatic-based system, with the forms referred to as honorifics (Potts & Kawahara (2004, 1)):

Japanese Honorifics

- (6.43) a. **Sam -ga warat -ta.**
Sam -NOM laugh -PAST
'Sam laughed.'
- b. **Sam -ga o -warai -ninat -ta**
Sam -NOM subj.hon -laugh -subj.hon -PAST

³³ % indicates that the addition of this word would be pragmatically strange.

- i. 'Sam laughed.'
- ii. 'The speaker honors Sam.' [subject honorific]

- c. **Sam -ga warai -yagat -ta**
 Sam -NOM laugh -antihon -PAST
 i. 'Sam laughed.'
 ii. 'The speaker views Sam negatively.' [antihonorific]

The authors go on to illustrate how honorific marking can be used to mark speaker attitude not only to other individuals but to the content of propositions as well (p2ff):

- (6.44) **Kathryn -wa Sam -o hai -ken -shi -nakat -ta**
 Kathryn -TOP Sam -ACC obj.hon -see -do -not PAST
 i. 'Kathryn did not see Sam.'
 ii. 'The speaker respects Sam.' [object honorific]

"We divide our attention between these cases and the honorifics represented in (39) and (40)[my numbers, DLE], which associate semantically with some aspect of the propositional content rather than with the denotation of an argument nominal."

- (6.45) **Mary -ga ringo-o tabe -mashi -ta**
 Mary -NOM apple -ACC eat -perf.hon -PAST
 i. 'Mary ate the apple.'
 ii. 'I am speaking nicely to you.' [performative honorification]

- (6.46) **John -wa [Mary -ga nesugoshi -chimat -ta] -koto-o**
 John -TOP Mary -NOM oversleep -antihon -PAST -fact

shitteiru.

know

- i. 'John knows that Mary overslept.'
- ii. 'It sucks that Mary overslept.' [antihonorification]

As they continue:

*"The first is called performative honorification in Harada (1976) (certainly a suggestive name). It goes by the name 'polite speech' as well. We call the second kind antihonorification. Here, the morpheme **chimau**, which appears as **chimat**-, signals that the speaker has contempt for the proposition expressed by the clause in which it appears."*

The Japanese case thus illustrates that pragmatic markers like this can be 'co-opted' for other grammatical functions. Therefore, such categories are doubly important to recognize, with potential ramifications that go beyond their original functions.

6.3.1.7. CASE

Verbs are said to 'govern' cases, which means that in some languages verbs partially determine the shape of the nouns that are specified in their lexical representation. English has vestiges of this in its pronoun system:

- (6.47) a. I saw him.
 b. He saw me.
 c. *Me saw he.

Of course, anyone who has studied Latin, Greek, Finnish, or any one of a huge number of languages will have learned about case systems much more complex than English. Case is an interesting feature because it is in a sense 'displaced'. It is perhaps best understood as a verbal feature that is borne by nouns. That is, what the case will be on a noun depends on what the verb is. But the verb itself does not bear case. In Minimalism (Chomsky 1995), case is one of the 'uninterpretable' features. But these features in Minimalism seem to be exactly those features that are displaced in this specific sense. To understand case in a language requires a solid understanding of verbal lexical structure.

Case is functionally straightforward. It is used to help interlocutors track actions and their participants. Consider the template for a simple transitive clause (order irrelevant):

- (6.48) S V O

Now in a language like English, with rigid word order, the use of additional word markers (case affixes or prepositions) to indicate that one NP is Subject or Actor and the other is Object or Undergoer is unnecessary. A language could distinctively mark these participants but it does not need to. On the other hand, if a language allows its nominals to be ordered more freely than English, a morphological marker on one or both arguments of the verb becomes very useful. Marking just one of them is more efficient (a lesser effort to results ratio) than marking both, although all possible options are found in natural languages. The three possibilities are shown in (43):

- (6.49) a. S_{mark} V O
 b. S_{mark1} V O_{mark2}
 c. S V O_{mark}

The system in (6.49a) is called 'ergative' or 'ergative-absolutive'. The case that is marked on the subject is called the *ergative* case and the unmarked case on the object is called the *absolutive* case. The case marked on the object in (6.49c) is called the *accusative* case and the unmarked case on the subject is referred to as the *nominative* case. The (6.49b) example is rarer, because less efficient, but can fall into either ergative or accusative patterns or yet a separate kind of pattern. Part of knowing how to interpret (6.49b) is learning how the language marks the single argument of an intransitive clause:

- (6.50) a. S_{mark} V
 b. S V

Marking of the sole argument of an intransitive clause is unnecessary, so (b) is the most common case. When coupled with (c) the null case on the S is called nominative. When coupled with (a) it is called the absolutive. When coupled with (b), if the case of the intransitive S matches the case of the transitive S, we have a nominative-accusative system. If it matches the case of the O, then the system is ergative. If it matches neither, the system is neither. (But again, labels are not all that crucial. Clearly presented, accurate analyses and descriptions are.) Variations are possible, the two most common being split-ergativity and active-marking.³⁴

In a split-ergative system, the case marking is nominative-accusative for some purposes and ergative-absolutive for others. From our definition in () below of *prototypical transitivity*, it is not surprising to learn that the dividing line is often punctiliar past and/or definiteness of the arguments. In general, in split-systems, ergative marking is more common with the punctiliar past and nominative-accusative with deviations from prototypical transitivity. But there are other ways to divide the case-marking system. So *beware*. One final observation is that languages do not have to exploit case-marking to show at least traces of ergativity. In Wari' for example (Everett & Kern (1997, 331ff), many verbs take a suppletive form when the Object or the intransitive Subject is plural. Since those two are grouped together by this process, the process is ergative in a sense. Or consider (from Lyons ()) the vestiges of ergativity in English:

- (6.51) a. John ate breakfast.
 b. John ate.
 c. *Breakfast ate.
- (6.52) a. John broke the window.
 b. The window broke.
 c. *John broke.

Verbs like *break* and *eat* in English, as in many other languages appear in transitive and intransitive clauses. However, when they do, the *eat* verbs allow the Actor to appear as their sole argument, never the Undergoer (c), while the *break* verbs allow only the Undergoer as their sole object, never the Actor (c). Therefore, the *break* verbs group together the Subject of the intransitive form with the Object of the transitive form and, as Lyons () so rightly pointed out, they are ergative verbs in this sense.

Another kind of distinction, the last we consider for now, is what some call an *active-stative* case system. Consider the hypothetical examples in (47):

- (6.53) a. John_{mark} fell down (on purpose/*accidentally).
 b. John fell down (*on purpose/accidentally).

What is going on in such examples? In this type of system, volitional Actors (acting intentionally) are case-marked while non-volitional Actors (things happen to

³⁴ The best crosslinguistic typological comparison is WALS, Haspelmath, Martin & Dryer, Matthew & Gil, David & Comrie, Bernard (eds.) 2005. *The World Atlas of Language Structures*. (Book with interactive CD-ROM) Oxford: Oxford University Press. ISBN: 0-19-925591-1.

them without their intention), are not case-marked. Such systems of case-marking are called active-stative.

The key to case analysis is to be alert for ways that nominals are being grouped together. This is rarely arbitrary (modulo vestiges of some historical process now almost gone from the language). Labels are not that important initially. Recognizing patterns is.

Let's move now to another important semantic domain with morphosyntactic consequences for nominals, *qualia*.

6.3.1.8. QUALIA

Morphosyntactic research must also pay attention to a semantic set of categories used in classifying nouns in subtle ways. Pustejovsky (1996, 76ff and 85-105) discusses these in a detailed and helpful manner, labeling them *qualia*. As Pustejovsky (85) explains these, the qualia are the four factors which 'drive our basic understanding of an object or a relation in the world. They furthermore contribute to (or in fact determine) our ability to name an object with a certain predication'.

What this means for the fieldworker is that until they at least roughly understand the qualia of a particular lexical item, they cannot fully understand that item's appearance in sentence structure. This is relevant for morphosyntactic field research because subregularities in word behavior, both in verb and nominal or NP structures (e.g. selection of determiners, numerals, and, occasionally, affixation), can be determined by the qualia of a given item. There are four qualia, according to Pustejovsky (85-86).

(6.54) "1. CONSTITUTIVE: The relation between an object and its constituents, or proper parts.

- i. Material
- ii. Weight
- iii. Parts and component elements

2. FORMAL: That which distinguishes the object within a larger domain.

- i. Orientation
- ii. Magnitude
- iii. Shape
- iv. Dimensionality
- v. Color
- vi. Position

3. TELIC: Purpose and function of the object.

- i. Purpose that an agent has in performing an act.
- ii. Built-in function or aim which specifies certain activities

4. AGENTIVE: Factors involved in the origin or 'bringing about' of an object.

- i. Creator
- ii. Artifact
- iii. Natural Kind
- iv. Causal Chain"

For example, it is possible that qualia can be reflected in noun classification. It is likely that qualia can cause some nouns to behave in otherwise confusing ways, so that they would group with some nouns for some purposes according to qualia, but with others, according to, say, gender/class. Consider, for example, the following examples from Brazilian Portuguese:

(6.55) *João usou uma faca nova no frango.*
'John used a new knife on the (cooked) chicken.'

(6.56) *João usou um terçado novo no leitão.*
'John used a new machete on the (whole cooked) pig.'

Because of what *faca* 'knife' and *terçado* 'machete' mean at the level of qualia, we can use a generic verb like *usar* 'to use' and still know that cutting/chopping is being talked about. If a language morphosyntactically distinguishes, say, an aspectual difference for cutting vs. other actions, then () and () could function identically with regard to aspect, as a result of the qualia of the nouns involved, yet fall into separate classes with respect to gender (as in this case, feminine vs. masculine, respectively).³⁵

Or consider an analysis of the Banawá possession facts in terms of qualia. As Pustejovsky (1995, 79) observes, "The CONSTITUTIVE (henceforth CONST) quale refers not only to the parts or material of an object, but defines, for an object, what that object is logically part of, if such a relation exists." (p98)

To illustrate let us use Pustejovsky's (1995, 99) lexical structure for **hand**:

(6.57)
$$\left[\begin{array}{l} \mathbf{hand} \\ \text{ARGSTR} = \left[\begin{array}{l} \text{ARG1} = \mathbf{x:limb} \\ \text{QUALIA} = \left[\begin{array}{l} \text{FORMAL} = \mathbf{x} \\ \text{CONST} = \mathbf{part_of(x,y:body)} \end{array} \right] \end{array} \right] \end{array} \right]$$

"That is, the relation in the CONST allows for reference to what something is constituted of as well as what it constitutes, in part; i.e., a hand is part of a body, and a body has a hand..." (Pustejovsky (1995, 99))

My conclusion is that topic-agreement (gender) in Banawá picks out the index of the whole (*body* in (51)) in the qualia structure for its referential index. For inalienable possession this will be the possessor (that is the whole) but for alienable possession it

³⁵ This has been useful to me in my on-going analysis of Banawá possession. Topic-agreement (gender) picks out the index of the whole in the qualia structure for its referential index. For inalienable possession this will be the possessor (that is the whole) but for alienable possession it will be the possessed item (because that is the whole). Moreover, this allows us to say why kinship is intermediate. It is like inalienable morphologically (the cases without *ka(a)*) but like alienable syntactically (in terms of agreement). The latter fact is because the possessor of a kinship term is not the whole. The former fact is because it IS inalienable possession without *ka(a)*. In other words, there are two things happening: (i) inalienable vs. alienable possession and (ii) part-whole relations. Each is important in different ways.

will be the possessed item (because that is the whole). Moreover, this allows us to say why kinship is intermediate. It is like inalienable possession morphologically (the cases without **ka**) but like alienable syntactically (in terms of agreement). The latter fact is because the possessor of a kinship term is not the whole. The former fact is because it IS inalienable possession without **ka**. In other words, there are two things happening: (i) inalienable vs. alienable possession and (ii) part-whole relations. Each is important in different ways.

Other semantic distinctions may be less 'deep' or not inherent. One such property is comparison.

6.3.1.9. COMPARISON: more, less, better, best, etc.

Comparison is a ranking of objects relative to one another and is thus not inherent in a single object, by and large, as qualia are. Not all languages have comparison as a morphosyntactic category, but most do. So consider English:

- (6.58) a. A good man.
 b. A better man.
 c. The best man.

Not only is comparison reflected on the modifier of the English NP, choices of comparison (e.g. (c)) can also affect other characteristics of the NP, e.g. marking for definiteness. Comparison, like other semantic categories, may be marked on the N, NP, Verb, or Clause (section ____ below). Be aware that regularities on one word may be caused by or associated with another word. Therefore, it is vital once again that phonology not be confused with morphology (see ____ above). Let's move now to consider a different type of semantic categorization which can affect the morphology of a given language, namely, possible distinctions for different classes and subclasses of words, e.g. common nouns, proper nouns, pronouns, abstract nouns, concrete nouns, quantifiers, number words, etc. All or any of these might play a significant role in the morphosyntax of the language under study. We consider just a few possible subclasses of nouns here.

6.3.1.10. NOUN SUBCLASSES

PROPER NOUNS:

Proper nouns are those which name specific individuals, e.g. *John, Fido, Bank of America, Brazil*, and so on. Morphosyntactically they *usually* do not cooccur with definiteness marking (since they are inherently definite), plurals, or much other morphology usually associated with common nouns.³⁶ And they behave differently in discourse in some if not all languages. Recognizing them, however, is not always as easy as it sounds. In Pirahã, for example, all proper nouns are morphologically complex or derived from common nouns. Here are a few:

- (6.59) a. /ahoaógií 'big night'

³⁶ But consider Brazilian Portuguese examples like ():

- (i) Vamos na casa *do* João.
 'Let's go to *the* John's house.'

- b. Kohoibiíhai 'blood eater'
- c. Toisií 'eagle'
- d. Piihoábigí 'frog' (literally: 'water-side-dirt')
- e. Piiopí 'capyvara' (literally: 'enter water')

The first proper noun in (53) is a modified noun, the second a nominalized verb + object, the third comes from a common noun, the fourth from a phrase which is most commonly used as a common noun, and the last from a verb + goal, and is used commonly as the noun for capyvaras. In other words, there is little in Pirahã to distinguish proper nouns from other nouns morphosyntactically, at least in isolation. Only in context can proper nouns be distinguished (mainly by syntactic position and the type of clitic used preverbally), as in (54):

- (6.60) a. *Piiopí* *hi* */iaagá*.
 name masc. is hungry
 'Piiopí is hungry.'
- b. *Piiopí* */isi* */iaagá*.
 animal
 'The capyvara is hungry.'

The proclitic on the verb tells us whether the noun is a proper noun (referring to a person named capyvara), as in (54a) or a common noun (a capyvara) as in (54b).

PRONOUNS, CLITICS, AND AFFIXES

The first caveat offered at the beginning of this chapter is that the fieldworker must be on guard to avoid confusing phonological facts with morphological facts. This is perhaps nowhere truer than in the analysis of clitics, pronouns, and nominal affixes. To get an idea of why this is hard, consider the following examples from Wari' (Everett ()):

- (6.61) **Quep** ***na*** ***-in*** **xirim** **te** **pane** **ta**.
 do 3s:rp/p -3n house father:1s rem:past emph
 '*My father made a house long ago.*'
- (6.62) **Ten** ***ta*** **wao'**.
 weave pass:3s type of basket
 '*Baskets are woven.*'
- (6.63) **Mi'** ***non*** ***-on*** **con** **hwam** **hwijima'** **mon** **tarama'**.
 give 3p:rp/p -3pm prep:3sm fish children coll man
 '*The men gave the children fish.*'

The italicized material almost always is found to the immediate right of the verb, as in the examples shown. Should this material be analyzed as a sequence of pronouns, as clitics, or as suffixes on the verb? Such decisions are non-trivial and require consideration of syntactic, morphological, phonological, and, occasionally, semantic evidence. In the case of Wari', we conclude that these are clitics for three main reasons: (i) Vowel Harmony operates within words, but is blocked from spreading from a verb

to an agreement marker, i.e. across a clitic boundary; (ii) stress falls on the last syllable of each word, but clitics are never stressed as part of any adjacent word, receiving either no stress or an independent stress in clitic 'clusters' (as in ()-()); (iii) these constituents may follow embedded sentences, which are not words, and thus can appear without any potential word for them to affix themselves to (Everett (in progress)). Perhaps the best summary of the differences between clitics and affixes is given in a well-known article by Arnold Zwicky and Geoffrey Pullum (19--). Another useful discussion on distinguishing clitics, affixes, and pronouns (or words more generally) comes from the article by Anderson and Zwicky () in the second edition of the Oxford International Encyclopedia of Linguistics:

"Since the unusual properties of clitics ... are bound up with their ambiguous status between affixes and words, we may consider some criteria which distinguish affixes (determined, bound, reduced) from words (undetermined, free, full):

- (a) The typical word, but not the typical affix, has an independent accent.*
- (b) The phonological shape of a word must be listed in the lexicon, but the phonology of an affix is described in general by saying how the shape of some stem is altered (so that affixes can have 'process', as well as affix, realizations) [See Affixation].*
- (c) Separate, language-specific restrictions can govern the possible phonological shapes of words vs. affixes. In particular, affixes, but not words, are often non-syllabic.*
- (d) Syntactically, words belong to (lexical) categories, i.e., word classes; but the assignment of affixes to such categories is problematic...*
- (e) Syntactic rules introduce word classes as co-constituents with other syntactic categories; but an affix is syntactically dependent, described by rules which locate it by reference to syntactic elements (e.g., on Nouns, on the head of VP, in the first word of S, at the right edge of NP).*
- (f) For each affix, morphological rules specify the class of words with which it can occur, and the properties of the resultant combination; but the syntactic rules distributing words typically make reference to phrasal categories rather than to word classes. From this, it follows that affixes are typically very selective in the word classes with which they occur, but words are unconstrained with respect to the word classes that happen to occur adjacent to them.*
- (g) Syntactic rules cannot alter morphological structure. In particular, syntactic rules cannot allow a word to interrupt a stem + affix combination; a word attached to such a combination must have edge position.*
- (h) Syntactic rules which introduce a lexical category are blind to the morphology and phonology of its co-constituents, but rules which introduce an affix may be contingent on such properties of its stem. From this it follows that there can be arbitrary gaps and morphophonological idiosyncrasies, including suppletion [q.v.], in the set of stem + affix combinations, but not in the set of word + word combinations.*
- (i) Alternative orders of words within a constituent are common, but the ordering of an affix – with respect to its stem, and to other such affixes – is fixed (although the same affixes may combine in different orders to express different meanings: e.g., the passive of a causative is not the same as the causative of a passive, and the affixers involved may reflect this in their ordering)."*

The question here, of course, is how do we formalize such observations and turn them into rules of thumb for fieldworkers? First, consider the basic structures distinguishing clitics from affixes proposed in Everett (1997, PAGE) (in these examples ()s = phonological boundaries and []s = morphological boundaries):

- (6.64) a. Y is a free word ([x]) ([y])
 b. Y is a clitic ([x] [y])
 c. Y is an affix ([x [y]])

Affixes are morphologically parts of words. Clitics are phonologically dependent (though their placement may also be syntactically constrained, e.g. Everett (1989) argues for Yagua). Free words are independent in both senses. The problem is how to determine the type of dependency, morphological or phonological. Thus, by the view represented in (58), the terms 'clitic' and 'affix' are nothing more than labels for morphophonological relationships. It is those relationships which are crucial for the field researcher. Zwicky and Anderson's discussion offers very useful considerations for this.

The three questions the fieldworker must answer, put in a very general way, for affixes, clitics, and free words are:

- (6.65) a. Does the item behave like a free word?
 b. Or does it behave like an affix?
 c. Or does it have mixed qualities, e.g. behaving like a word syntactically but like an affix phonologically?

Put in this way, it is clear that correctly understanding a particular item as a clitic, affix, or free word requires knowledge of linguistics, the language, and, especially, how to define a word in the language under study. Let us turn now to consider another subset of nominals/nominal modifiers (depending on the language).

NUMERALS AND QUANTIFIERS

There are also special classes of nouns that can vary a good deal from language to language as to whether they have idiosyncratic or relatively normal noun behavior. These include, but are not limited to, the following: number words, quantifiers, abstract nouns, and color words.

To take number words as the first example, consider the following examples from Russian (Rappaport (2002, 329)):

"Numeral phrases in Russian have long presented a puzzle to those attempting a theory of the syntax of this language. The puzzle itself is not difficult to formulate. When the phrase as a whole stands in a position to which the syntax assigns a direct case (nominative or accusative), the numeral acts like a nominal head of the phrase, assigning the genitive case to the quantified noun (and its modifiers) as if it were a complement. When this same phrase stands in a position assigned an oblique case, the quantified noun acts like the head of the phrase, with the numeral falling into line with the other modifiers in agreeing with that quantified noun. For example, contrast (60a,b)[my numbers, DLE]:

- (6.66) a. **videt'pjat'** **krasivyx** **ptic&ek.**
 to see five_{ACC=NOM} beautiful_{GEN.PL} birds_{GEN.PL}
 'to see five beautiful birds'

b.voxis&c&at'sja pjat'ju krasivymi ptic&kami.
to-be-enthralled-by five_{INST} beautiful_{INST.PL} birds_{INST.PL}
'to be enthralled by five beautiful birds''

Or consider the English quantifier *all*:

- (6.67) a. All the children came today.
b. *The all children came today.
c. The children all came today.
d. *The children came all today.
e. *The children came today all.

The English quantifier, *all*, 'floats', that is, it can appear in different positions in the phrase, unlike other words. In this case, it shows no morphological irregularity, though its syntax is clearly linked to its morphological class. Therefore it is important in classifying words, part of morphological analysis, to determine what special features they have phonologically, morphologically, or syntactically.

6.3.1.11. NOMINAL TENSE

Fieldworkers will expect certain semantic categories to be marked on a closely corresponding set of syntactic categories for the most part. For example, it is natural to expect tense to be marked on verbs because a principal function of tense is to situate events, expressed almost always by verbs, in time. On the other hand, tense is not marked exclusively on verbs crosslinguistically. For example, a word like **ex-wife** in English arguably marks the tense of 'wife' as past by the prefix **ex-**. Nordlinger & Sadler () have studied the distribution and typology of nominal tense extensively. They say that:

"Although the categories of tense, aspect and (to a lesser extent) mood are traditionally considered to be properties of verbs, the morphological expression of tense, aspect and/or mood (henceforth TAM) on nominals is attested across a range of languages. We distinguish two major subcases of nominal tense, that is, of cases in which TAM marking occurs on a nominal or other constituent of NP/DP. In some cases, a dependent nominal or nominal phrase (of whatever grammatical function) bears some TAM marking which serves to temporally, aspectually or modally specify the clausal predicate which is itself distinct from the nominal argument. This is illustrated by examples (62)-(63)[my numbers, DLE] in which the case marking of the dependent NPs changes to reflect the tense (future vs. nonfuture) of the clausal predicate:

- (6.68) Ngamari -lu ngunytyi -ka
mother -ERG give -PAST
- ngali -nha mangarni -marru -nga -nha
we.DU -ACC bone -having -GEN -ACC
- kathi -nha.
meat -ACC.

'Mother gave us the doctor's meat.' (Pitta Pitta (Australia), Blake 1987:60, ex.

4.12)

- (6.69) Ngamari -ngu ngunytyi ngali -ku
mother -NOM.FUT give we.DU -ACC.FUT
- mangarni -marru -nga -ku
bone -having -GEN -ACC.FUT

kathi -ku.
meat -ACC.FUT.

'Mother will give us the doctor's meat.' (op. cit., ex. 4.14)"

Thus, initial expectations based on training notwithstanding, we must be careful in fieldwork to look for tense on categories other than the verb. In general we must look for the possibility that many of our semantic categories will not match up as expected with our expected morphosyntactic categories.

6.3.2. VERB CATEGORIES

We now turn to consider the hardest nut to crack in morphological fieldwork, the verb. As one example, verbs in Pirahã have sixteen suffix positions (giving 2^{16} possible verb forms, modulo semantic incompatibilities), none of which include tense or number or person. Pirahã affixes encode notions like evidentiality, aspect, directionality, negation, and mood, among others (see Everett (1986, ---)). So although Pirahã's verbal morphology is very complex, one of the most complex systems I have encountered, its morphological categories overlap surprisingly little with those of English.

Nevertheless, many categories are commonly marked in verbal morphology cross-linguistically and I want to survey those very briefly here.

6.3.2.1. TENSE

Tense is a deictic ('pointing') category that locates an action or event with respect to the moment of utterance (absolute tenses) or another tense referenced on the verb (relative tenses). (See Comrie (1985)).) Consider the examples from Portuguese:

- | | | | |
|--------|----|--------------------------|------------------------|
| (6.70) | a. | Ele fala. | 'He speaks.' |
| | b. | Ele falou. | 'He spoke.' |
| | c. | Ele falará. | 'He will speak.' |
| | d. | Ele falara/tinha falado. | 'He had spoken.' |
| | e. | Ele terá falado. | 'He will have spoken.' |

The first three tenses are absolute. This is because they are defined with respect to the moment of utterance, i.e. when they are spoken. So each means 'he speaks as I am speaking' or 'he spoke relative to my current speaking' or 'he will speak after I am done talking now', respectively. The last two examples are relative tenses. They refer to events situated in relation to other events. So you might use (64d), for example, in a sentence like (6.71):

- | | |
|--------|---------------------------------------------|
| (6.71) | Ele tinha falado quando eu cheguei na sala. |
| | 'He had spoken when I arrived in the room.' |

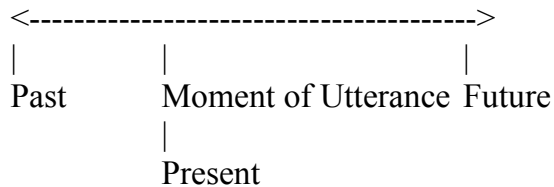
And you might use (64e) in a context like:

- | | |
|--------|------------------------------------------------|
| (6.72) | Ele terá falado quando voce terminar de comer. |
| | 'He will have spoken when you finish eating.' |

In (6.71) and (6.72) the events in the initial clauses are situated temporally in relation to the events of the second, embedded clauses, rather than directly to the moment of utterance.

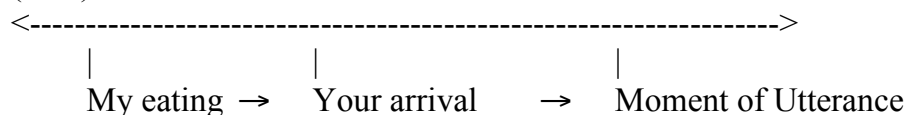
Not all languages necessarily encode tense as a linear category. So for European languages, which do seem to categorize temporality as principally linear, we may represent tense by a line extending metaphorically into the past and into the future, generally representing the future as leading off to the right and the past as to the left.

(6.73) Metaphorical representation of tense as temporal linearity:



By this line, Past, Present and Future are all absolute tenses because they are defined directly with respect to the moment of utterance (simultaneous = present; preceding = past; following = future). Relative tenses are defined on the line with respect to the temporal placement of other events. So, for an example like 'When you arrived I had eaten', our linear representation would look like:

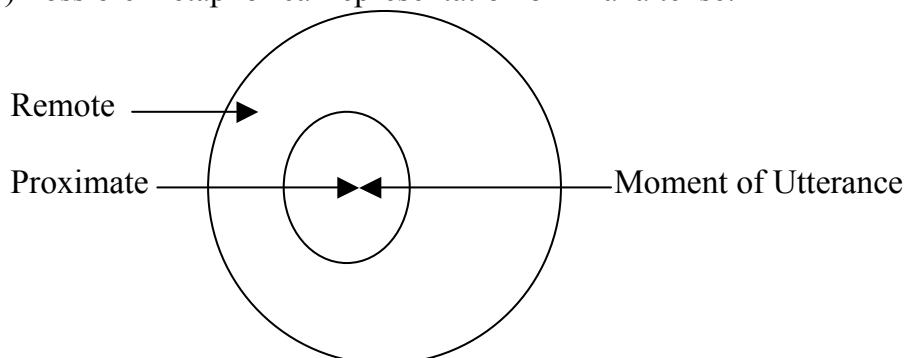
(6.74)



So here the 'eating' event of 'I had eaten' is located before the 'arrival' event of 'when you arrived'. Thus the former is not situated in relation to the Moment of Utterance but in relation to the arrival event.

On the other hand, the linear conception of time is not the only one imaginable nor even the only one implemented in natural languages. For example, in Piraha, according to Everett (1986 – an analysis I would not swear by anymore. I need to do some more thinking about this) there are two tense-like suffixes, **-a** and **-i**, marking 'remote' and 'proximate', respectively. Remote in this analysis refers to events either remote in time from the present (moment of utterance) whether future or past, or out of the speaker's control. Proximate on the other hand refers to the converse, something close to the moment of utterance temporally or within the speaker's control. If this analysis is correct, then rather than a linear conception of time, the best way to express the Piraha conception would be as 'concentric' layers:

(6.75) Possible metaphorical representation of Piraha tense:



6.3.2.2. ASPECT

Understanding a report about an event or state can be made much easier if the speaker gives his or her perspective on how to classify the temporal states being reported. Such temporal classification is known as 'aspect'. So consider the examples below from Brazilian Portuguese:

- (6.76) a. Eu estive com ele ontem.
 I was with him yesterday.
- b. Eu estava com ele ontem.
 I was with him yesterday.

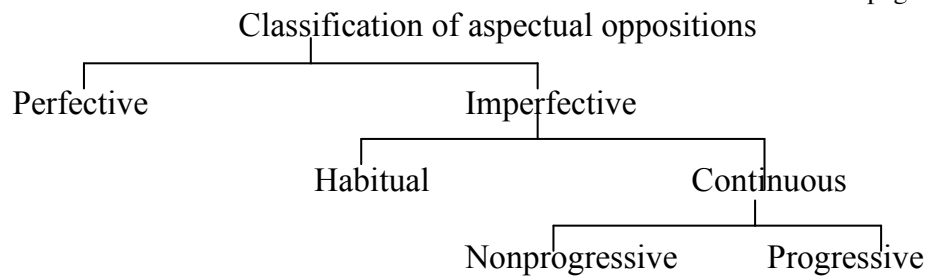
These examples are distinguished not by tense but by aspect, a speaker's perspective on the classification of the 'measuring out' (Tenny (1989)) of an event or the non-deictic temporal properties of an event or state. Both are past tense. The punctiliar aspect in (67a), i.e. a reference to a single being with him at a point in time, is contrasted with the non-punctiliar 'smearing' across time in (67b).

Languages can draw many distinctions based on aspect. A few of these are:

- (6.77) a. Perfective and Imperfective
 b. Habitual
 c. Telic and Atelic
 d. Progressive
 e. Punctual and Durative
 f. State and Dynamic
 g. Iterative
 h. Simultaneous
 i. Terminative
 j. Ingressive
 k. Semelfactive

Perfective aspect marks the verb to represent that the speaker is viewing the event as an indivisible whole (Comrie 1976, 16ff). Imperfectivity, on the other hand, is used to make "explicit reference to the internal temporal structure of a situation" (Comrie 1976, 24ff). Comrie's important introductory work goes on to break down a number of aspectual relationships into a single hierarchical relationship, as in ():

(6.78)



This chart represents the broader set of classifications likely to be found in field research. There are other possibilities, however, as languages introduce 'fine-tuning' or specific twists into this classification. Like other linguistic categories, the semantics and formal nature of aspect can only be gotten at more or less reliably by (i) studying the distributions of forms in the grammar and (ii) following a semantic methodology based on conditions of felicity and truth-conditions (see ___ below).

Some other aspectual distinctions the fieldworker may encounter include things like those in the remainder of the list in (). Aspectual distinctions are fairly standard across theories, though different theories may emphasize different features of the oppositions. Just to get a better feel for these oppositions, let us consider the one that I consider the most important distinction for the syntax, the telic vs. atelic opposition. This is relevant for the syntax because, as Tenny (1990) shows, it is crucial for understanding the relationship of the verb + undergoer to the actor.

Telic vs. Atelic

Telic refers to an end point, what Tenny () refers to in relation to verbs as the 'measuring out' of the event, in particular, marking when the event has reached its lexically specified ending point or goal. This need not be a morphological category, but is often reflected in the language independent of morphology, e.g. in the syntax of English:

- (6.79) a. John ate spaghetti. (Atelic)
 b. John ate the spaghetti. (Telic)

In (6.79a) 'spaghetti' is treated like an unspecified mass and so there is no claim that 'John' finished eating the spaghetti, merely that he engaged in the activity of eating it. In (6.79b), on the other hand, the definite article draws a boundary around the spaghetti and indicates that there is an identifiable whole of spaghetti, such that when 'John' eats that he can in fact be said to have completed the goal of his eating. Hence this is a telic clause. Telicity (as Van Valin and La Polla (1997, ---) show) has implications for the syntactic behavior of clauses. But in many languages, e.g. Piraha, telicity is also a morphological category:

- (6.80) **Tí so/óá/ísigíhíi kohoáip -á.**
 1sg. already meat eat -telic
 'I already ate (the) meat.'

- (6.81) **Tí so/óá/ísigíhíi kohoáip -i.**
 1sg. already meat eat -atelic
 'I already ate meat.'

Note that in the examples given, the presence of the telic suffix favors an interpretation of the direct object as definite, even though Piraha otherwise lacks a definite/nondefinite distinction morphosyntactically. This is a natural correlate of telicity, however, as we saw in the English examples in ().

There are various other possibilities that involve differential marking (syntactic vs. morphological) vs. semantics of aspect. Although the fieldworker should be familiar with the basic categories mentioned here, they must be careful to arrive at conclusions about 'their' specific language by distributional studies specific to the language in question.

6.4.3. VOICE, VALENCE, AND TRANSITIVITY

Voice is, roughly, is the relationship between the action or state expressed by a verb, and its arguments (nominals to which it assigns a semantic role, e.g. agent, patient, goal, etc.). It is a rather sloppy term that is often used to refer to either Valence (also referred to as Valency) or Transitivity. When applied to Transitivity, different voices increase or decrease the degree of transitivity of a clause, as in the Montana Salish **-(e)m** morpheme discussed below. When applied to Valence, these mark changes from avalent to trivalent:

There is a great deal of confusion in the literature, even in books which ought otherwise to clear up this confusion, between VALENCE and TRANSITIVITY. The former refers to the lexically specified number of arguments of a verb. The latter refers to the way that these arguments are 'mapped' to the syntax. Let's consider a simple example from English and then consider some more complex examples from Montana Salish. This issue is not all that complex, but it is very important and often overlooked (though Hopper & Thompson (1980) and VanValin and LaPolla (1997, 147ff) are both noteworthy for their care in distinguishing these concepts. Consider the following examples from English which illustrate the differences between the two:

- (6.82) a. The captain sank the ship in order to collect the insurance.
 b. The ship was sunk (by the captain) in order to collect the insurance.
 c. The ship sank.
 d. *The ship sank in order to collect the insurance.
- (6.83) a. The captain scuttled the ship in order to collect the insurance.
 b. The ship was scuttled (by the captain) in order to collect the insurance.
 c. *The ship scuttled.

The a. and b. examples in (6.82) and (6.83) are bivalent. That is there are two arguments lexically required by the verb 'to sink'. In the a. examples and in the parenthetical material in the b. examples, each of the two arguments is overt. But when the parenthetical information in the b. examples is missing, the two arguments are still both there, one explicit and one *implicit* (i.e. understood to be present in the meaning but not heard, like a well-behaved child). This can be seen by the fact that the purpose clause 'in order to collect the insurance' is still grammatical, whether or not the Actor is overt. But purpose clauses can only be used when an agent is either present or understood as part of the meaning of the clause. When the Actor is implicit, it is often nevertheless indicated by voice morphology, e.g. the English passive, which signals the presence of a less topical, potentially implicit Actor (if it appears in the 'by phrase' of English, e.g. 'It was seen *by John*' then it is overt). The c. examples are what some refer

to as the 'middle voice'. With middle voice, the agent is not even implicit. Therefore, with not even an implicit agent allowed with the middle, no purpose clause is permitted in (d).

It is interesting that there is no middle voice for the verb 'to scuttle', (c). The reason is that the middle voice eliminates the agent altogether, i.e. it can be thought of as altering the lexical representation of the verb/deriving from a separate lexical representation without an agent. But 'to scuttle' will lack a middle voice altogether because it has a very specific, non-alterable meaning of 'an agent sinking/destroying' a vessel. Therefore, (c) is ungrammatical. With regard to Valence vs. Transitivity, we can say that the a. examples are bivalent and transitive; the b. examples are bivalent and intransitive; the c. examples are monovalent and intransitive.

Another example might be helpful. In English, as for so many semantic categories, the distinction between causative vs. non-causative is periphrastic or syntactic:

- (6.84) a. John cried. (non-causative)
b. Mary made John cry. (periphrastic causative)

A mark on a word that increases the number of arguments required for a lexical item, as in () or periphrastic representation of an added argument, as in English, is said to increase the valency of the lexical item. The causative is one of the most common valency-increasing operations. (Others include the applicative, 'dative shift', or, arguably, simple object-addition, as in English 'Bill ate' vs. 'Bill ate eggs'.) In Piraha, causative is a morphological category:

- (6.85) a. Baóhoipaí /i koabáipí
cat it died
'The cat died.'
- b. Baóhoipaí /i koabá -a -ip
cat it die -causative -completive
-í
-proximate
'Someone killed the cat.'

There are several types of voice alternation in natural language, including (with my rough descriptions) Antipassive (the detopicalization of an Undergoer); Passive (detopicalization of an Actor), Applicative (increased topicalization of a benefactive or similar prepositional object), Dative Shift (increased topicalization of a GOAL argument. To illustrate some of these, let's consider the extremely interesting case of Montana Salish (from work by Thomason, Thomason, and Everett ()). In this language, as in most Salish languages, there is a morpheme, **-(e)m**, that marks a number of apparently unrelated types of meaning. However, when looked at more closely in relation to the Valence vs. Transitivity distinction, a simple analysis emerges. Here are some of the facts.

First, **-(e)m** can mark Antipassives, that is a voice alternation whereby the Undergoer of an action is marked (and is in many languages further indicated by

marking the Undergoer with an oblique case) as less topical in the current discourse context. In the (73a) and (74a) examples, we have the antipassive, compared to the normal transitive in the (73b) and (74b) examples. In these examples, the morpheme appears in the position normally reserved for agreement with the Undergoer.

- (6.86) a. **tSn** **tS'n'eyn** **-m** **t** **o'ol'ó**
 1sgSitr trap -ANTIPASSIVE OBLIQUE marten (small weasel)
 'I trapped some/a marten'

- b. **tS'n'ey** **-s** **-t** **-Ø** **-n** **lu** **o'ol'ó**
 trap -TR -TRANS -3OTr -1sgSitr 2ndary marten
 'I trapped the marten'

- (6.87) a. **Ø** **-n** **-'úlx^W** **-m** **t** **lúk'^W**
 3Sitr -LOC:in -bring -ANTIPASSIV OBLIQUE wood
 'He brought in some wood'

- b. **n** **-'úlx^W** **-st** **-Ø** **-s** **lu** **lúk'^W**
 LOC:in -bring -TRANS -3OTr -3Str2ndary wood
 'He brought in the wood'

This same morpheme can also be used to mark the passive, an unusual fact. To get a passive reading from **-(e)m**, simply put **-(e)m** in the position on the verb normally reserved for agreement with the Actor:

- (6.88) a. **'ín** **-Ø** **-Ø** **-m**
 eat -TRANS -3OTr -BCK.AG
 'Someone ate it'

- b. **'ín** **-Ø** **-Ø** **-m** **lu** **scnc'á** **t** **wílwl̥t**
 eat -TRANS -3OTr -BCK.AG 2ndary frybread OBL.
 scoundrel
 'The fry-bread got eaten by a scoundrel/scoundrels'

Compare:

- (6.89) **'íhi** **-Ø** **-Ø** **-s** **lu** **scnc'á** **t** **wílwl̥t**
 eat -TRANS -3OTr -3Str2ndary frybread OBL scoundrel
 'The scoundrel(s) ate the fry-bread'

- (6.90) a. **x^Wúy** **-st** **-Ø** **-m**
 go -TRANS -3OTr -BCK.AG
 'Someone made him go'

- b. **x^Wúy** **-st** **-Ø** **-m** **lu** **Coní** **t** **Malí**
 go-TRANS -3OTr -BCK.AG 2ndary Johnny OBL Mary
 'Johnny got made to go by Mary'

The Salish **-(e)m** can also be used to mark what Thomason, Thomason, and Everett () refer to as 'Derived Transitives'. The traditional Salishanist understanding is that in Derived Transitives a verb not normally transitive (in fact monovalent) becomes transitive (more accurately bivalent), i.e. the number of arguments increases. Or, as L. Thomason () puts it, more rigorously:

"Atelic stems can be turned into telic stems through the addition of -m and a transitive marker. The meaning of such derived stems is lexicalized to a greater or lesser degree; locative prefixes frequently help to direct the newly-telic action towards an object. Two examples of derived transitives are given in [...]:"

(6.91) a. **tSn 'áyχ^w-t**

1sg Sitr tire-STATIVE
'I am tired'

b. **'áyχ^w -t -m -n -Ø -Ø -Ø**
tire -STATIVE -DER.TR -TR -TRANS -3OTr -1sgStr
'I tire of him/it, I find him/it tiresome'

c. **'áyχ^w -t -m -n -Ø -Ø-Ø lu s-q^wllú-s**
tire -STATIVE -DER.TR -TR -TRANS -3 OTr-1sg Str NOM-
story-3 Poss
'I tire of his story/stories'

Another use of this morpheme is to mark continuative aspect on transitive verbs, as in (79) and (80), in which case it follows the object marker. This is referred to as the 'Transitive Continuative':

(6.92) **es -ʔ'e' -Ø- ém -s lu ululím**
stative -seek-3OTr-CONT -3Poss 2ndary money

t Coní

OBL Johnny
'Johnny is searching for the money'

(6.93) **k^wu es -'ác'χ -m -s t Coní**
1sgOTr stative -look.at -CONT -3Poss OBL Johnny
'Johnny is looking at me'

The reader may have also noticed a suffix **-n(t)** (e.g. examples (73), (74), and (78) above) which marks the verb as being greater than monovalent. Although it is referred to in the Salish literature (and it is roughly pan-Salishan) as a transitivizer or as 'the transitive apparatus', its real function is to mark valence (Thomason, Thomason, and Everett ()), whereas the **-(e)m** suffix marks deviations with regard to prototypical transitivity, (). There are two questions the Salish morphologist must answer with respect to these examples. First, why does the language have both the valency marker, **-n(t)**, and, second, why does **-(e)m** have so many functions? If we distinguish, as we

should, the lexical notion of Valence, a nongradient concept, from the syntactic notion of Transitivity, which is gradient, then we predict that, as in Salish, a language can mark variations in each with separate morphology (just as we predict that they can vary independently in the first place). And the different functions of the transitivity morpheme, **-(e)m**, can be understood as marking, in the first instance, deviations from the prototypical notion of transitivity in (81) (which is meant not as a theoretical statement or commitment to prototype theory at all, but merely as a very useful rule of thumb):

(6.94) Prototypical Transitivity: The prototypical transitive situation is one in which a definite Actor affects a definite Undergoer in punctiliar past.

In light of (81), consider the different uses of the transitive morpheme just looked at. Why does the same morpheme mark both antipassive and passive, for example? Because it indicates, by appearing in the relevant 'slot' on the verb, that the Actor or Undergoer is indefinite, hence that there is a deviation from Prototypical Transitivity. Why does it mark the continuative in transitive verbs? Same reason – a deviation from (). How can it mark a Derived Transitive? In this instance, it marks a valence change not a transitivity change. It is crucial to note that in the Derived Transitive, **-(e)m** appears to the left (i.e. closer to the root) of the **-n(t)** morpheme. This relative proximity to the root means that it (following Bybee (1985)) is likely to affect the meaning of the root more severely. In this case that is the right prediction – it does indeed affect the meaning more – it alters the valence (a lexical property of verb meaning), not merely the transitivity (a property of clauses). In these cases it moves the verb closer to Prototypical Transitivity, by signalling that the verb has a sufficient number of arguments to be transitive, thus licensing the occurrence of the bivalence marker/'transitive apparatus'. In other words, unless we carefully distinguish transitivity from valence, we have no account of Salish verbal morphology.

Now why have I given so much attention to this particular analysis of Salish here in a field methods guide? Because it is a very clear illustration of the vital distinction between valence and transitivity that, if it is not controlled for, can mislead or puzzle the field researcher for a long period of time. And because most linguists fail to recognize the distinction.

6.4.4. MOOD (or MODALITY):

Mood is used to indicate the relationship of the reported event or state to reality. There are various kinds. The simplest mood system with opposing moods is two: realis (action or state is perceived as real) vs. irrealis (action or state perceived as potential, hypothetical, future, etc). There are more complex mood systems, e.g. indicative, subjunctive, imperative, hortatory, optative, etc. in Romance languages. Mood is often marked on the verb directly, but not always. Consider English (82) vs. Portuguese (83):

- (6.95)
- a. If I were you.
 - b. If I was you.
 - c. If it had rained, I would not have come.
 - d. If it rains tomorrow, I will not come.
 - e. ?It rains tomorrow.

Although the English subjunctive is disappearing, a mood used to indicate hypothetical or conditional statements, a vestige of it is seen in (82a). For dialects that use (82a), this is a subjunctive form (it would be ungrammatical if used as an indicative, as in *I were too here yesterday'). And (82e) needs the help of *might*, as in 'It might rain tomorrow', with the indicative ending –s missing from the verb, to indicate the subjunctive mood.

- (6.96)
- a. Se eu fosse voce.
 - b. *Se eu sou voce.
 - c. Se tivesse chovido, nao teria vindo.
 - d. Se chouver amanha, nao virei.

In Portuguese, unlike English, as (83) shows, mood is an obligatory morphological category.

Mood is also important (as are numerous other verbal categories) for syntactic reasons as well, because it can indicate the degree of formal relatedness between clauses, as the Portuguese examples below show:

- (6.97)
- a. Se fosse você, não viria/vinha/*vim/*verei amanhã.
if went:pastsubj. you not would come/was coming/came/will
come tomorrow
'If I were you, I would not come/was not coming/*came/*will come
tomorrow.'
 - b. Se for amanhã, terá/tem/*tinha/*tive que levar um casaco.
if go:fut.subj. will have/have/*was having/had to take a coat.
'If you go tomorrow, you will need to take a coat.'

The correlation between the mood of the protasis (the embedded 'if clause') and the tense and mood of the apodosis (the matrix 'then clause') indicates embedding in Portuguese and can indicate different degrees of clausal interconnection in other languages, so can serve as a useful diagnostic for embedding and other aspects of analyses of interclausal relationships.

6.4.5. DIRECTIONALS

Verbs are further restricted in some languages by affixation of directionals. These affixes may be used *in addition to* a similar range of prepositions or they may obviate the need for most prepositions (so I advise the fieldworker to look with particular care for verbal directionals if there are only one or two prepositions in the language!). Directionals primarily function to fix the event in space. But they are often extended in meaning. Intransitive prepositions in English often function as directionals, as seen in (6.98):

- (6.98)
- a. The plane takes off
 - b. Look up the tree; look down the street.
 - c. He threw up; he upchucked (from the stomach to the mouth is seen as an upward motion, but the extension of the directional meaning is also present).

But also (see Lakoff and Johnson for further extensions of verb + particle constructions in English):

- (6.99) a. Look him up vs. ?/*look up him.
 b. Look Bill up vs. look up Bill.
 c. He is moving up in the world.

To my mind the contrast in (a) shows an interaction of constraints on English cliticization (where the prepositions are simple clitics, merely phonologically reduced forms needing a 'host' in Zwicky's (1977) terminology). A pronominal clitic in English, e.g. [m] 'him', attaches to what it is an argument of. If it is an argument of the verb, it attaches to the verb, as in (). If it is an argument of a preposition, as in (), then it attaches to the preposition, producing a very different reading. As with other morphological categories, directionals have a fairly straightforward and literal basic function with subtle semantic extensions that can take considerable research time to understand.

So compare () above with (): (see Lakoff and Johnson () for a large number of such extensions) consider:

- (6.100) a. Look him up; */?Look up him.
 b. Look Bill up; Look up Bill.
 c. Take on city hall.
 d. He is moving up in the world.

The contrast in (a) shows an interaction between the constraints on English cliticization (simple pronoun clitics, i.e. those which are merely phonologically reduced forms of full pronouns, but otherwise unexceptional syntactically, see Zwicky ()), such that a pronominal clitic must attach to the verb before a prepositional clitic. A pronominal clitic in English, e.g. *him* [Im] in (), attaches to what it is an argument of. If it is an argument of the preposition it attaches to the preposition, as in (), but if it is an argument of the verb it must attach to the verb. Thus () is bad but () is good. The reading where it is an argument of the preposition *up*, (), is quite different, of course, than where the pronoun is an argument of the verb.

Examples like (), and many more in Lakoff & Johnson, illustrate how the uses of directionals can be extended to be interpreted in ways other than their etymological source might indicate. As with other morphological categories, directionals have a fairly straightforward and literal basic function with subtle semantic extensions that can take considerable semantic research to understand.

6.4.6. AKTIONSART

Aktionsart, 'kind of action', is a semantic classification of verbs, based on the inherent temporal characterization of a verb's specified action or event.³⁷ In VanValin and LaPolla (1997, __), as in many other works, the aktionsart of verbs is an important component in the analysis of their morphological (e.g. the range of affixes they can

³⁷ This section borrows heavily from the excellent introduction to verbal semantics in Van Valin ().

take) and syntactic (e.g. the range of constructions in which they may appear) behavior. This notion is introduced clearly by Van Valin (---):

"Verbs can be subclassified in various ways. Some researchers have found some ways of subclassifying them more useful than others. One system that has been widely adopted is to subclassify verbs based on Aktionsart ('kind of action', from Vendler ()), the inherent temporal characterization of a verb's specified action or event. There are four subtypes of verbs in the traditional Aktionsart classification proposed by Vendler:

- (6.101) *a. States: be sick, be tall, be dead, love, know, believe*
 b. Achievements: pop, explode, shatter (the intransitive versions)
 c. Accomplishments: melt, freeze, dry (the intransitive versions); learn
 d. Activities: march, walk, roll (the intransitive versions); swim, think, snow, write, drink

States depict static situations which are inherently temporally unbounded (atelic), and both achievements and accomplishments express changes of state, which are inherently temporally bounded (telic): achievements are instantaneous, while accomplishments are not. Activities are dynamic, inherently temporally unbounded (atelic), states of affairs. Vendler proposed this taxonomy based solely on the analysis of English verbs, and yet it has proved to be of great cross-linguistic validity.

It would be reasonable to hypothesize that these distinctions are the universal basis of the organization of verbal systems in human language. There is one important non-Vendlerian Aktionsart class, namely semelfactives (Smith 1997). Semelfactives are punctual events which have no result state. Examples are given in

- (6.102) *a. The light flashed.*
 b. Chris coughed.
 c. The tree branch tapped on the window.
 d. Dana glimpsed Kim.

There is a derivational relation between two classes which is very important crosslinguistically, namely that between activities and what are called ACTIVE ACCOMPLISHMENTS, the telic use of activity verbs. This general pattern relates activity verbs of motion (e.g. run), consumption (e.g. eat), and creation (e.g. paint) to the corresponding active accomplishment verbs. This is illustrated in (87) for English.[my numbers, DLE]

- (6.103) *a. The soldiers marched in the park. Activity*
 a'. The soldiers marched to the park. Active Accomplishment
 b. Dana ate fish. Activity
 b'. Dana ate the fish. Active Accomplishment2
 c. Leslie painted (for several hours). Activity
 c'. Leslie painted Mary's portrait. Active Accomplishment

There are a few verbs in English that are lexical active accomplishments, e.g. devour and go; that is, they do not alternate with an activity counterpart like the verbs in (6.104).

Most fundamental is the distinction between static and non-static verbs, which distinguishes verbs which code a 'happening' from those which code a 'non-happening'. In other words, with reference to some state of affairs, one could ask,

'what happened?' or 'what is happening?'. If, for example, a sentence like *Bob just ran out the door* could be the answer to this question, then the verb *run* is [– static]. On the other hand, a sentence like *John knows Bill well* could not be the answer to this question, because nothing is taking place. Hence *know* is a [+ static] verb. By this criterion activities, achievements, semelfactives, accomplishments and active accomplishments are [– static]. States, however, are [+ static]."

However, it would be a mistake, a serious one, for a fieldworker to attempt to simply translate the diagnostics for Aktionsart from English. They will likely not be useful in many other languages, at least without considerable adaptation. Finding useful diagnostics for linguistic categories, especially diagnostics that give such (apparently at least) clear-cut distinctions is not easy and in a particular field situation it is possible that nothing corresponding to these will be found. Aktionsart is an important concept to the fieldworker not because it will always be useful in every field situation, but because it *may* be very useful in figuring out how the morphology works in some languages.

But beware, again, of simple solutions. It may be very difficult to come up with satisfying, watertight classifications. Nevertheless, your morphological research will benefit tremendously by investigating at least the possibility of verb or verb combinations classified on the basis of Aktionsart.

6.4.7. FINITENESS

Verbs can be more or less *finite*. Finiteness is the degree to which the verb is referentially anchored. The greater the amount of inflection on the verb, the more precisely is it anchored referentially. Consider the following examples from English:

(6.104) He owns the place.

- (6.105) a. He wants to own this place.
b. He thinks he can own this place.

(6.106) Owning is not always a good idea.

In (88) the verb is marked for present tense, indicative mood, third person, singular number, and has an aspectual reading that indicates on-going ownership. This verb thus provides the hearer with a good deal of information to help situate the 'owning' in time, reality, and relationship to identity of owner, among other things. But in (89) the same verb is not marked for these semantic categories. This less finite form can be overtly marked by an item such as syncategorematic *to* (Pullum ()), (89a) or by no marking at all, (89b). But in these examples it is temporally-aspectually unanchored, i.e. it refers to no specific 'owning' of 'this place'. Its gerundive form in (90) likewise marks it as lacking anchored temporal-aspectual, etc. reference. And the gerundive marking further indicates that it is 'less verby' (see __), becoming more like a thing (an abstract thing, i.e. the act of ownership).

Finiteness can manifest itself in different ways in different languages. So consider the examples below from Karitiana (Arikem family) and Pirahã:

- (6.107) **ŷn** **nāka** **paka** **-j** **ŷn** **pykyp**
 I affirmative clean -tense I clothes
 'I will clean the clothes.'

- (6.108) **ÿn i paka ÿn pykyp**
 I it clean I clothes
 'I will not clean the clothes.'
- (6.109) **ti gáí -sai kó'ói hi kaháp -íí**
 1 say -nom. name he leave -intention
 'I said that kó'ói intends to leave.' (literally 'My saying kó'ói intend-
 leaves')
- (6.110) a. **hi ob -áa'ái kahaí kai -sai**
 he see -attractive arrow make -nom.
 'He knows how to make arrows well.' (lit: 'He sees attractively arrow-
 making')
- b. **kahaí kai -sai hi ob -áa'ái**
 arrow make -nom. he see -attractive
- c. ***hi kahaí kai -sai ob -áa'ái**
 he arrow make -nom. see -attractive

Karitiana is interesting here because a negated verb takes a non-finite verb form. Pirahã is interesting for other reasons. Let us consider first the Karitiana example. Apparently in Karitiana, the negation of a verb removes, in standard cases, its verbal referentiality. It no longer takes finite verbal morphology because a negated action or state simply never occurred and thus can be left unanchored.

The Pirahã case is perhaps even more interesting. Everett (2005) argues that Pirahã lacks recursion, at least in the syntax. Yet, at the same time, some functions of clauses are less finite, corresponding partially, but by no means completely, to what in other languages would be embedded clauses. On the other hand, the reduction of finiteness in this language does not mark embedding. For example, in (), it is (what most linguists would take to be) the *matrix* verb that receives the **-sai** suffix, where as in () it is what might otherwise be taken to be the embedded clause which is so marked. This difference in marking violates normal expectations of finiteness as an embedding marker (see Koptjevskaja-Tamm (1993a and 1993b for detailed discussion of nominalizations and finiteness more generally). Everett and VanValin (in progress) analyse the **-sai** suffix as an information marker, indicating topical or old information.

Nonfiniteness of the verb can mark embedding. One reason that this is common is because, as Cristofaro (2003) argues, subordinate clauses are not assertions. Therefore, it is common for their verbs to be less referential in the sense of this section. However, that is not the only function of nonfiniteness. It can also indicate that the verb has 'moved' in the sense of Ross's (1973) scale towards 'nouniness'. An analysis associating nonfiniteness exclusively with embedding could not capture the generalization that Pirahã otherwise lacks any evidence for embedding.

6.4.8. EVIDENTIALS

Many languages employ morphological marking to indicate the speaker's epistemological stance with regard to their assertions, as an indication for the hearer(s) of the reliability of or the warrant for the content of the speaker's assertion.

For example, La Polla (2003) discusses the interesting case of the Tibeto-Burman language, Qiang. This language has a suffix, **-k**, which is labelled as 'inferential/mirative', depending partially on the Aktionsart of its verbal host. That is it is 'inferential' with activities, but 'mirative' (i.e. marking unexpected or new information) with resultatives and states. For example:

- (6.111) a. the: ,d,yta: 'a q' -k
 3sg Chengdu:loc or go inferential
 'He went to Chengdu (I infer based on the evidence, e.g. bags are missing and was told earlier about the trip, etc.)

And from Shipibo (Camacho, Elias-Ulloa ()), '...an inferential can be combined with a reportative, yielding a potentially ambiguous sequence: either the speaker reports someone else's inference, or s/he expresses an inference from reported evidence."

- (6.112) Ani -ronki i -bira [a] -i jawen jema.
 large rep be -inferential-incl poss3 village:absolutive
 'It appears the door is open/apparently the door is open.'

As Camacho and Elias-Ulloa go on to observe:

"Several studies have tried to explore whether propositional source-marking obeys some kind of cross-linguistic tendency. Willett (1988, p. 57), for example, finds in a survey of 38 languages, that evidentiality falls within three categories: attested (which can include subdivisions that include visual, auditory or other sensory information), reported, and inferred. Speas (2004, p. 4), on the other hand, proposes four hierarchical categories: personal experience, direct (sensory) evidence, indirect evidence and reported evidence (hearsay).

Aikhenvald & Dixon (2003, p. 3), suggest two broad types of evidential systems: those that state the existence of a source of evidence without specifying it, and those that specify the source. Within the second type, they discuss several subtypes:

(6.113) *Two-distinction systems:*

1. *Eyewitness and noneyewitness.*
2. *Nonfirsthand and everything else.*
3. *Reported and everything else.*

(6.114) *Three-distinction systems:*

1. *Visual, inferred, reported.*
2. *Visual, nonvisual sensory, inferred.*

(6.115) *Four-distinction systems:*

1. *Visual, nonvisual sensory, inferred, reported.*
2. *Visual, inferred (2), reported.*
3. *Nonvisual sensory, inferred (2), reported.*

4. *Visual, inferred, reported (2).*

It seems clear, particularly for three and four-distinction systems, that they are built on Willett's three basic distinctions, with some possible subdivisions within each of those categories. Speas, following Oswalt (1986), suggests an additional category different from visual-sensory: personal experience. Willett, Oswalt and Speas suggest that there is an underlying hierarchy corresponding to the degree in which the source directly involves the speaker's evidence. This scale goes from more direct experience to no experience at all (Speas 2004, pg.258):

(6.116) personal experience >> direct (e.g. sensory) evidence >> indirect evidence >> hearsay."

Evidentials are interesting for what they have to teach us about the interplay of semantic scope between different types of affixes, about what they reveal of different epistemologies, about the nature of morphosyntactic structure, and about the cultures that produce them.

6.4.9. DISLOCATION³⁸

Modern syntactic theories focus (to an inordinate degree in my opinion) on just one of the many items that we must 'memorize' about a language, namely, whether or not it allows for constituents to be displaced or dislocated from their purportedly 'underlying' or (perhaps more accurately) their unmarked positions. The functions of dislocation in a language (aside from the extremely theory-internal arguments of some syntactic theories) include marking the *information structure* of the sentence (see Lambrecht () and section ____ of chapter ____ below) and scope alternations. But of course dislocation is simply one among many different strategies for marking different information structures and scope, inter alia. Otherways of marking these include prosody, morphological marking, separate construction types, or a combination of some subset of these options, among other strategies. Still, it is true that many languages choose dislocation as perhaps the principal means of pragmatic highlighting, scope alternations, or information structure changing. Occasionally, dislocated elements may have consequences for the morphology of a language, which is why dislocation is important to us in this section. So consider question-related dislocation and morphological marking in Wari:

(6.117) **Ma'** **co** **tomi'na?**³⁹
 that:prox:hearer m/f:rp/p speak 3s:rp/p
 'Who is speaking?'

(6.118) a. **Ma'** **co** **tomi'ca?**

³⁸ See also the section on dislocation in chapter 8 below.

³⁹ Example (4) is interesting because it illustrates that questioning the subject of the sentence requires tense in second position, to the immediate right of the question word, and also immediately to the right of the verb. WH-questions of subjects require that tense be expressed twice in the sentence. This, as (5) – (7) show, is not true of any other questioned constituent.

that:prox:hearer m/f:rp/p speak 3sm
'Of whom is he speaking?'

- b. **Ma' carawa ca pa' caca mon**
that:prox:hearer animal nrp/p kill 3pm coll

tarama'?
man
'What thing/animal did the men kill?'

- (6.119) **Ma' ca para 'aca ca piye ma'?**
that:prox:hearer nrp/p why cry 3sm child that:prox:hearer
'Why is that child crying?'

- (6.120) **'om ca mao ca.**
not:exist nrp/p go(sg) 3sm
'He did not go.'

Dislocation is morphologically relevant in Wari' because the nature of the grammatical dislocation (e.g. subject vs. direct object vs. adjunct) results in different kinds of agreement patterns. First, any dislocation causes tense to appear before the verb. This is due to a simple (and crosslinguistically very common) constraint that requires tense to appear following the first constituent of the clause. But notice that in (), when the subject appears in sentence-initial position, tense appears *both* in second position and immediately following the verb (it's 'normal' position). Only one occurrence of tense, however – in second position of the clause, is found when objects and adjuncts are 'dislocated' to the beginning of the clause (()) and (()). Whatever the reason for this asymmetry, it is evidence that dislocation can have morphological and not merely syntactic effects.

Another morphological configuration that some languages allow is Incorporation.

6.4.10. INCORPORATION

The first clear reference to Incorporation (though not by that name) I am aware of is Montoya (1585-1652), Jesuit missionary and linguist who worked among the Guaraní of Paraguay. Like Dislocation, Incorporation is a common device for marking changes in information structure. It can also be used for other functions, e.g. additional specification of the verb. As Mithun (1986, --) says:

"Noun incorporation is perhaps the most nearly syntactic of all morphological processes. Examination of the phenomenon across a large number of geographically and genetically diverse languages indicates that, where syntax and morphology diverge, incorporation is a solidly morphological device that derives lexical items, not sentences. It is used for four different but related purposes; these fall into an implicational hierarchy which in turn suggests a path along which incorporation develops historically. Differences in its productivity from language to language show that this development may be arrested at any point – resulting either in the eventual disappearance of the process, or in its resurgence as a productive system of affixation."

An example from Yucatec Mayan

- (6.121) a. **tinč'akah če'**
t- in- č'ak- Ø- ah če'
COMP- I- chop- it ASP
- b. **č'akče'nahen**
č'ak- če'- n- ah- en
tree chop- tree- APASS- ASP- I.ABS
"I chopped a tree."

Noun-incorporation has various manifestations and a range of functions. In the examples of Piraha encliticization repeated below, there is no real incorporation, but the phonological divisions between object and verb have been reduced in a way that doesn't happen with subjects:

- (6.122) tî /ísitòí /òògàbàgàí → tî /ísitò&ògàbàgàí
lpsg egg want
'I want an egg.'

- (6.123) kàhàí /ààgàhá → kàhàa&àgàhá
arrow is
'It is an arrow.'

There is something 'iconic' in this in that some structures show less phonology to represent a tighter semantic relationship. What that semantic 'tightness' actually means in any given language has to be determined by the linguist by careful analysis and argumentation.

6.4.11. AGREEMENT

Agreement is a way of keeping track of verb participants by marking them morphologically, either on the verb (as in English, *John runs*) or elsewhere in the clause, with Wari's second-position verb agreement markers:

Most languages will have agreement in one form or another. As Everett (1996) observes, it is imperative to analyze carefully the relationship between pronouns, pronominal clitics, and agreement affixes. Everett (1996) considers a variety of languages in which these categories interact to produce a type of 'agreement mosaic'. This is also discussed in the section on clitics above and illustrated in many examples throughout this book.

Let us turn now to strategies commonly employed to mark the distinctions discussed above.

6.5. MORPHOLOGICAL REFLEXES OF GRAMMATICAL AND SEMANTIC DISTINCTIONS

6.5.1. CONCATENATION

Easily the most common morphological marking strategy is the addition of extra material to the root (the base of a word from which all morphology has been stripped), what morphologists know as *concatenation*. Material may be added to the root at the right or left margins (prefixation and suffixation, respectively), at both simultaneously (circumfixation), or within the root (infixation). Of these various strategies, suffixation

is the most common, followed by prefixation, then perhaps infixation, then circumfixation. These are illustrated in (6.124) – (6.126):

Suffixation: adding to the end of a particular morphological constituent (word, root, or stem)

(6.124) John runs.

Prefixation: adding to the beginning of a particular morphological constituent (word, root, or stem)

(6.125) John *en*livened the party.

Infixation

(6.126) Tagalog Infixation (McCarthy & Prince 1993)

- a. /um + /alis/ → //um-alis/ 'leave'
- b. /um + tawag/ → /t-um-awag/ 'call' pf., actor trigger
- c. /um + gradwet/ → /gr-um-adwet/ 'graduate'

Circumfixation is the simultaneous addition of material preceding and following the root, stem or word to mark a single distinction. Circumfixation is perhaps best known from the German past participle (*ge-* *-t* for regular verbs). The verb *spielen*, for example, has the participle *gespielt*.

6.5.2. SUPPLETION

Another common strategy for marking distinctions like those discussed above morphologically is suppletion. Suppletion is the label given to alternations where there is no rule-based phonological relationship between the various allomorphs. It is quite common in the world's languages. So consider the examples below from English and Portuguese:

English *to be*

(6.127) I go, you go, he goes, they go, we go, I went, you went, etc.

Portuguese *ir*

(6.128) Eu fui; voce foi; eles foram; Eu vou; voce vai; nos vamos, eles vao.

It is common to find suppletion in more frequent verbs, e.g. 'to be' or 'to go'. This is likely because it is easier to remember the forms when they are heard more often.

Another not uncommon type of suppletion is found in Wari', where the suppletion follows an ergative pattern, the form of the verb governed by the object of the transitive or the subject of the intransitive. Not all verbs participate in this process (and it would be an interesting study to see if these verbs follow any particular pattern). But those that do behave as in (), from Everett & Kern (1997, 331):

- (6.129)
- | | | | |
|----|----------|---------|-------|
| a. | Toc | nana | com. |
| | drink:sg | 3p:rp/p | water |

'They drank water.'

- b. Tototoc nana com.
 drink:pl 3p:rp/p water
 'They drank and drank water.'

This example is interesting, because although 'water' has no plural in Wari', the suppletive plural of the verb can also indicate a plurality of events. This scope variation of suppletion, i.e. marking either object plurality or event plurality, is common. Abdias, Walker, and Everett (1984) discuss a similar case for the Mayan language, Huastec. But there are many other examples where the object itself governs plurality:

- c. Cao' 'ac cacama na.
 eat:sg travel 3pf 3s:rp/p
 'Then they ate.'
- d. Mon cacacao' 'ac cacama -in capam' con
 slowly eat:pl travel 3pf -3n cornbread prep:3sm

 jowin ma' na.
 monkey:species that:prox:hearer 3s:rp/p
 'Then they ate cornbread and *jowin* monkey for a long time.'

The Wari' case is very interesting because the suppletion shows an ergative pattern (see __ above). For a very useful and detailed study of morphological suppletion, the reader should consult Veselinova (2004).

Let's consider one more, often overlooked strategy for morphological marking, *periphrasis*.

6.5.3. PERIPHRAISIS⁴⁰

Consider the contrast between aspectual marking in English vs. Portuguese:

- (6.130) a. Eu estava aqui ontem.
 b. Eu estive aqui ontem.
- (6.131) Eu tomava café, sim.
- (6.132) a. I was here yesterday for a while.
 b. I was here yesterday.
 c. I used to eat breakfast.
 d. I was eating breakfast.

Portuguese marks punctiliar, continuative, and habitual aspects morphologically. English marks the latter two periphrastically. The fieldworker can easily mistake periphrasis, a morphological strategy, for syntax. But periphrasis, as Börjars, Vincent,

⁴⁰ This section assumes the paradigm to be a theoretically primitive of linguistic theory. For eliciting lists of related lexical items commonly known as paradigms in a non-theoretical sense, the reader is referred to __ of chapter __ on monolingual fieldwork.

and Chapman (1997) show, is an important component of natural language morphology. To see this more clearly, consider the case of periphrastic pronouns in Wari', discussed in Everett ():

TABLE ONE
Paradigm of spatial demonstrative pronouns

	Proximate to Speaker	Proximate to Hearer	Distal
Masculine singular	<i>co cwa'</i>	<i>co 'ma'</i>	<i>co cwain</i>
Feminine singular	<i>cam cwa'</i>	<i>cam 'ma'</i>	<i>cam cwain</i>
Neuter	<i>'i ca'</i>	<i>'i 'ma'</i>	<i>'i cain</i>
Plural	<i>caram cwa'</i>	<i>caram 'ma'</i>	<i>caram cwain</i>

TABLE TWO
Paradigm of temporal demonstrative pronouns

	Heard/not seen	Recently absent	Long absent
Masculine singular	<i>co paca'</i>	<i>co pacara ne</i>	<i>co pacara pane</i>
Feminine singular	<i>cam paca'</i>	<i>cam pacara ne</i>	<i>cam pacara pane</i>
Neuter	<i>'i cara</i>	<i>'i cara ne</i>	<i>'i cara pane</i>
Plural	<i>caram paca'</i>	<i>caram pacara ne</i>	<i>caram pacara pane</i>

The constructions in these tables are not simply words. Phonologically, words in Wari' manifest two important characteristics that these periphrastic forms fail to show. First, Wari' words disallow internal consonant clusters where one of the consonants is a glottal stop. Thus, the initial glottal stop on *'ma'* [/ma/] 'that proximate to hearer' in Table 1, as in *co 'ma'* 'that masculine one proximate to hearer' and *'i 'ma'* 'that neuter one proximate to hearer', should not occur, since word-medial glottal-consonant clusters do not otherwise occur. By this otherwise inviolable criterion, in the cases of *co 'ma'* and *'i 'ma'*, *co* and *'i* must be interpreted as morphologically independent and not, say, as prefixes. We interpret them, as well as *cam* and *caram*, as clitics (as discussed in Everett and Kern (1997, 413ff)). Second, in Wari' words prefixes always undergo Vowel Harmony with their hosts. The kinship terms discussed in the penultimate section of this paper, which are analyzed as words, rather than periphrastic forms, do indeed show Vowel Harmony between the *co* and the root. The absence of Vowel Harmony in the forms in Tables 1 and 2 is therefore additional strong evidence that these are not words (or, at the very least, that they violate expectations on word phonology).

Among the many arguments Everett (2005) gives that these pronouns are periphrastic morphology, rather than syntax, is the following. Periphrastic pronouns in Wari' mark all three Wari' genders. But neuter gender marking is accomplished by

using the morpheme *cara*, just in case it is in a periphrastic construction. *Cara* 'heard' otherwise has no inherent gender association (Everett (2005))). This means that *cara*'s interpretation in the tables above is a function of the *paradigm* (an abstract constraint of morphology but not syntax), rather than the syntax and is thus non-compositional and morphological. Therefore, Everett (2005) concludes that periphrasis is an important morphological strategy in Wari', a way of co-opting phrases to function as 'hybrid' words.

Methodologically, therefore, it is always useful to try to discover unifying principles of morphological groupings, assuming the paradigm as at least a methodological convenience if not a theoretical primitive. This way it will be easier to spot the use of syntax for morphological means as well as 'mixed categories'.

6.5.6. INTERCALATION

Intercalation is most often referred to in the literature as 'non-concatenative morphology'. However, from what we have seen in this section, there are various kinds non-concatenative strategies. So I prefer to refer to this type of morphological marking as *Intercalation*. This is illustrated in (110) from Arabic:

(6.133) *TH: Gloss: Morphological analysis:*

- a. **qātal** 'he killed' 3 p. masc. sg. perfect
- b. **qittēl** 'he murdered' 3 p. masc. sg. perfect intensive
- c. **qotēl** 'killing' active participle (qal)
- d. **jiqtol** 'he will kill' 3 p. masc. sg. imperfect/future
- e. **niqtal** 'he killed himself' 3 p. masc. perfect reflexive
- f. **q_e tālām** 'he killed them' 3 p. masc. sg. perfect + pers. pron.

One way to conceive of this is that the morphology of Arabic accesses structures for vowels separately from structures for consonants and then, following precise constraints (e.g. those in McCarthy (1979)) intercalates them.

6.5.7. PROSODY

The marking of morphological distinctions can also be accomplished by prosodic features such as tone or intonation, length, and stress. For example, consider the way that Pirahã indicates an inherent vs. an accidental property of an individual via the tone on the verb:

(6.134) Hi /ii /aagá.
'He hungry is.'

(6.135) Hi hiaitíihí /áagá.
'He is a Pirahã.'

Or consider the distinction between some nouns and verbs in English:

(6.136) *permit* (verb) vs. *permit* (noun); *contract* (verb) vs. *contract* (noun), etc.

6.5.8. REDUPLICATION – KAMAIURA

Another common morphological marking device is reduplication, where some

part of the root or stem is duplicated in order to mark a semantic distinction. One well-known example comes from Kamaiurá (Everett & Seki 1986, McCarthy & Prince 1993):

(6.137)

- a. *ohuka* → *ohuka-huka* 'he laughed/he kept laughing'
- b. *apot* → *apo-apo-t* 'I jump/I jump repeatedly' (**apot-apot*)
- c. *o-mo-tumuõ* → *omotumu-tumu-õ* 'he shook it/repeatedly'
- d. *je-umirik* → *jeumiri-miri-k* 'I tie up/repeatedly'
- e. *o-etun* → *oetu-etu-n* 'he smells/keeps on smelling'
- f. *o-ekvj* → *oekv-ekv-j* 'he pulls/repeatedly'

According to Everett & Seki (1986), these are examples of suffixation. According to McCarthy & Prince (1993) they are best analyzed as infixation. Whatever the analysis, it is important for the fieldworker to be familiar with this type of marking. A more detailed study of reduplication (but to my mind one with the defect of predicting the absence of the Tupi pattern just investigated) is Inkelas and Zoll ().

6.6. WHERE TO MARK DISTINCTIONS

Now let us consider where the morphological strategies discussed above may appear. For many features they can differentially appear, crosslinguistically, on either the head of the phrase or a dependent (daughter) of the head. This distinction is very important, as Nichols (), the first one to make the distinction in these terms, points out:

"Morphological marking of grammatical relations may appear on either the head or the dependent member of the constituent (or on both, or on neither). Grammatical relations – and whole languages – may be classified according to their propensity for using one of these types of marking. Implicational relations among various marking patterns can be stated: languages display a tendency to use one type consistently throughout their grammar. The difference in patterns provides a typological metric and a functional explanation for certain word-order preferences. For historical linguistics, it provides a diagnostically conservative feature and a clue to genetic relatedness. Although the head-marked pattern is cross-linguistically favored, grammatical theory is strongly biased toward the dependent-marked patterns that happen to dominate in Indo-European."

Consider the two English examples in (115) – (117). The first one marks the relevant morphological distinction on the head of the phrase (the verb in this case), while the other marks it on a dependent of the head (the embedded NP in the possessor position in (116) and the case on the pronoun in (117)):

- (6.138) a. The man runs.
- b. The men run.

(6.139) John's book is wet.

- (6.140) a. I saw *him*.
- b. *He* saw *me*.

Likewise in Portuguese, the gender of the noun is marked on its dependents:

- (6.141) a. O homen bonito.
 b. A mulher bonita.

Or, as in Wari clausal morphology, the marking may appear on neither the head nor one of its dependents, but in the clausal 'second position', also known as Wackernagel's Position, from the work of the first person to draw attention to this position for clitic placement, Wackernagel (1892).

6.7. DERIVATION VS. INFLECTION

A venerable distinction in morphological theory is the subclassification of morphological processes which change the category of a root to form a stem as *derivational* and morphology added to the stem, and usually agreeing with a semantic distinction on another word in the phrase, as *inflectional*. Not all morphologists attribute primitive theoretical status to this distinction. Here I want to briefly consider some of the classical reasons for distinguishing these two subclasses of morphology – not because I necessarily endorse the labels, but because recognizing the concepts behind the labels is useful for any fieldworker. Consider the examples in (6.142)–(6.143) from English:

- (6.142) a. farm
 b. farms
 c. farmer
 d. farmers
- (6.143) a. live
 b. liven (as in *Liven up the party!*)
 c. enliven (as in *Enliven (*up) the party!*)

The suffix *-s* is an inflectional morpheme. It is used to produce 'fit' between a lexeme and its syntactic context (e.g. its phrase or clause), as well as to signal that a meaning option (plurality) has been selected from the relevant inventory of semantic distinctions (number in this case).

Morphology that changes the meaning of a *root* to form a *stem* (an internally-complex morphological input to inflection) often is less productive, more idiosyncratic in meaning and morphology, more constant in its phonological form, among other things, than morphology that does not affect core lexical meaning and that applies to either roots (from which all morphology is absent) or stems. Whatever you call it, this distinction crops up in language after language and it is to the fieldworker's advantage to be aware of it, look for it, and carefully subclassify this and any other distinct morphological processes that appear to be operative in a given language.

Up to this point, we have considered distinctions often made in natural languages and how and where these may be marked. I now want to offer practical suggestions for morphological fieldwork, i.e. how to collect, analyze, and process morphological data.

6.8. METHODOLOGY FOR MORPHOLOGICAL ANALYSIS

6.8.1. INTRODUCTION

Returning now to elicitation of morphosyntactic data, the researcher must first answer the obvious question of what it means to 'describe', 'document', or 'analyze' *the* morphology of a language. Simply put you have two goals in morphosyntactic description: to identify the primary semantic distinctions in word meanings and to determine how these distinctions are signalled. The task is therefore easy to conceptualize.

Of course, the actual doing of it is much harder. Let's say you hear the two forms in (6.144), for example:

- (6.144) a. pandapa 'I ate'
 b. pandapã 'You ate'

On the surface, it would look like 2nd person is marked by nasalization of the final vowel, /a/. But here are some obvious questions: (i) are you *sure* there is no nasalization on the final vowel of the first word? (ii) did you get this pair repeated from multiple speakers? If so, did they all use nasalization in (b) and none in (a)? (iii) can (b) also be uttered without nasalization on the final vowel without changing the meaning? These questions may seem obvious now, but they are not quite so obvious in a field situation. The field researcher will soon realize that there is an enormous difference between analyzing printed data and analyzing spoken data. Having 'good ears' is crucial to good morphosyntactic analysis. Acoustic analysis will help, of course, but without good ears, you will be reduced to hours and hours of unnecessary spectrographic labors.

Linguists disagree on how to collect data for morphosyntactic and semantic analysis in the field. Some advocate a text-only approach to data collection. Some claim that specific sentence elicitation can play a useful role in field data collection.

The problem to me is similar to the problem faced by any second language learner. Do you learn a language from books and classes, or from natural conversations in a community of native speakers? The choice is a false one, at least to me. One must converse and develop a *need* to use the second language. But all other sources are also of great potential usefulness and will almost always complement the information otherwise learned, helping in fact to learn constructions and expressions that might have otherwise gone unperceived, at least for a considerable period of time.

Likewise in morphosyntactic research, we need data from a variety of sources. Some linguists are concerned about a 'data problem', arguing that elicited data (one sentence at a time, for example) should be avoided in favor of conversational or text-based data in which sentences appear in natural contexts. Only in such 'natural contexts' we are told, are data reliable. However, I disagree. I believe that elicited data, natural texts, conversations, corpora, and any other source of data can and should be used in constructing a theory of a particular grammar. The 'data problem' is to me a judgment problem. Gathering data requires experience, intuition, a lot of linguistic knowledge, security in what you are trying to do, and the ability to distinguish contrived data from data that could actually emerge in a natural way from the mouths of speakers. (We don't know if they have it in their grammar until we have heard it from their mouth.)

6.8.2. Record, transcribe, annotate (the context and what you think it means)

As you perambulate about the speech community where you are working you will hear words, both words you ask for and words that are offered to you, or just words and

expressions in the environment. You probably will not know at the beginning of your work whether these are words or sentences or phrases (consider the *gavagai* problem discussed in section ____). And you will almost certainly not have a secure understanding of their meaning. Many things you encounter, e.g. rare animal or rituals, or descriptions of culturally sensitive activities you might not be welcome to see again (e.g. something you just innocently stumbled across). So always be prepared to transcribe and, ideally, record in audio and video what you hear. This is where a small, analog recorder, with easy rewind and record functions is very useful. However, there are often occasions that it would be useful to record an image along with the word. My personal digital camera (not a camcorder) allows me to take a picture and a brief recording associated with that picture. This is useful, for example, if I am collecting the names for flora and fauna. I am not a zoologist. Thus whenever I see a new tree or species of rodent, I usually do not know what it is called in English, much less in Latin (i.e. its scientific name). Some fieldworkers do know this. But in my case, when I get back to the village, I pull out one of the flora and fauna books I have taken (see ____ below) and look for what I have just photographed. Failing to find it, I can take the photo with me to a specialist upon returning to my home institution.

6.8.3. Test with native speakers:

You haven't learned a morpheme until you have it down articulatorily and acoustically. You need to test your pronunciation of it (or the words in which it occurs) by practicing with native speakers. As you practice, you need to also listen to make sure you understand how to use the morpheme appropriately in context and any extensions that it has. This is partially pragmatic knowledge of the morpheme and partially semantic. Semantic methodology is presented in ____ below.

6.8.4. Get translations and paraphrases from a variety of speakers.

Never rely on the pronunciation, usage, or judgements of a single speaker when analyzing a morpheme. You need to test your hypotheses and judgements with a variety of speakers, at least 3-6 men and women before feeling that you have reached a definitive conclusion.

6.8.6. Text tracking

One of the best examples I am aware of in the literature that traces a linguist's analytical history in relation to a set of morphemes is found in Lowe (1990). Lowe (1990, 544ff) states his problem as follows: "In Nambiquara, four morphemes, namely *-jau³*-, *jut³*-, *-kxai³*-, *-kxe³*- figure centrally in expressing the interclausal relationship of cause/reason. Each one of these morphemes occurs in two-clause constructions which can be faithfully translated as expressing a cause or reason relationship. There is the added complication, however that each one of these morphemes is used in other contexts where a causal relationship does NOT [emphasis Lowe's, DLE] seem to be present." Lowe proceeds to discuss the difficulties of analysing these morphemes, showing clearly that looking at their uses exclusively in isolated sentences will lead largely to confusion rather than conclusion. He then shows how an examination of their functions in a wider discursive context leads to a fairly straightforward analysis. Before reviewing his findings, very useful from a methodological standpoint, I quote his foundational assumption: "The analysis presented in this paper must start with the assumption that each of the morphemes above has a unitary meaning, even though the same morpheme may appear to have different meanings in different contexts. Such an

assumption is justified provided it leads to consistent and insightful descriptions of the function of each morpheme. The danger of assigning disparate glosses for the same surface morpheme in different contexts is that each one of these disparate glosses will be assigned from an English speaker's point of view; *until one struggles to find what, if anything, they have in common, the foreign viewpoint will be all that one has.* [emphasis mine, DLE].

Lowe goes on to demonstrate, convincingly, that one cannot understand either the notion of causality in Nambiquara, nor the ways that it is marked linguistically, without understanding both the cultural meaning of causality in Nambiquara and the various discursive uses of the different morphemes. In the course of his study, he also strongly supports his assumption of 'unitary meaning' as a vital methodological tool.

Another, similar, example comes from my own study of the intersentential connective, **hoagá** in Pirahã. Consider the following examples and the contexts in which I initially recorded them.

- (6.145) **Hi toio/aagá, hoagá hi opaohoaibaái.**
 he old is *hoagá* he works a lot.
 'He is old ?.. he works a lot.'

- (6.146) a. **Hi gogiísoi /igí -ai?**
 he what-big:thing with is
 b. **Ti hoagá pi-obaibaai.**

- (6.147) **Ti gi hoagá poogaihiaí bagaboi.**
 'I you *hoagá* give banana.'

6.8.7. Bi-directional translate texts with language teachers

Another potentially useful method of verifying morpheme meanings is to translate with native speakers, who are also fluent in the language of the translation, the words containing the morpheme into the trade, national, or other relevant surrounding language. Then with other speakers, you should translate back into the target language. If you and they have fully understand the morpheme, the translations should produce similar results. If you are wrong, however, you should get unexpected variations.

6.9. PERSPECTIVES ON MORPHOLOGICAL ALTERNATIONS

HOMONYMY, POLYSEMY, AND ALLMORPHY

The fieldworker will find it useful, but not always essential to distinguish polysemy (a single word with multiple meanings) from homonymy (multiple words with a single phonological form). For example, consider the different uses of the clitic *se* in Portuguese. Are these multiple morphemes with one form or multiple meanings of one morpheme? (Each 'se' is preceded by a label for that particular function.)

Reflexive

- (6.148) Sérgio se matou.
 'Sergio killed himself.'

Argumental Impersonal

- (6.149) Se recebe socos facilmente.

'One receives punches easily.'

Passive

- (6.150) Maças *se* vendem por aqui.
'Apples are sold around here.'

Ergative

- (6.151) A janela quebrou *se* (*a proposito).
'The window broke.' (*on purpose)

Inherent

- (6.152) Tres meninos *se* desmaiaram. (Brazilian Portuguese)

Each of these uses of *se* from Portuguese (and the phenomenon repeats itself throughout Romance) is different in function. So is there one *se* or five? Just about every hypothesis imaginable has been proposed in the literature at one time or another (see Everett (1996, ---) for a summary).

The answer to this particular problem is not relevant to our present concerns (though I believe that this is only a case of apparent polysemy and that all these functions are related in a simple way). However, the general problem is relevant. What evidence could a fieldworker bring to bear on such a question if encountered in the field (and the odds are high that something like this will confront most fieldworkers). Historical evidence might help, but it is problematic for several reasons. First, it is often unavailable to the fieldworker. Second, even if it could be proven that *se* entered Latin as a single morpheme, that would not entail that it has been preserved as such in any modern Romance language. Things change! We saw a similar problem with the Salish facts in () – () above.

The ultimate answer to this question of homonymy vs. polysemy is found in the linguist's analysis and argumentation. The most satisfying, clearest, and simplest analysis will likely be the one linguists adopt as 'the answer'.

6.10. CLEANING UP YOUR MESSSES –FOLLOW-UP VISITS

Field researchers must always allow time following the evaluation of data they have collected to find 'holes' in the patterns of their data, for follow-up visits. Follow-up field trips should be scheduled into a project in response to the possibility of discovery, incomplete corpora, change in focus, etc. This is a way to build in error-correction into your project, using these follow up visits to fill in missing bits of the data and to 'clean up your messes', that is, verify conflicting transcriptions, recordings, textual organizations or understandings, etc.

CHAPTER 7. SYNTAX AND SEMANTIC FIELD RESEARCH

7.1. Introduction

Syntactic fieldwork, like all the other structural domains we have considered until now is very complex and, again, exceeds the capacities of a single chapter. In this chapter I follow the approach adopted in the previous chapter on morphology and present *types of phenomena* that should be accounted for by any fieldworker. I present these mainly in terms of communicative functions and the formal realizations of these functions in the syntax.

7.1.1. What is syntax

Let's begin with a definition of syntax. I understand syntax roughly as the set of restrictions on how words are put together into larger units, the structure of those units, and the way that these units present and structure information.

What should the linguist give priority to among the potential areas of syntactic investigation that emerge from this definition? Whatever they discuss about the syntax and other aspects of the language under study, there will be large, disparate audiences ready to pore over what has been said, looking for data related to themes they are interested in, which may not overlap with the fieldworker's original interests in the description. Of course, no linguist can satisfy everyone. But there is no getting away from a certain responsibility that field workers incur to make their results useful and clear to as wide an audience as possible. Therefore, the fieldworker, to repeat a recurrent theme of this book, must endeavor to read widely and inform themselves with a systematic reading program.⁴¹

In the first part of this chapter, I focus primarily on questions of structure, though I attempt to give the reader a taste for all the other approaches just mentioned. This reflects my own training, personal history, and continuing interests, just as any scientist's decisions are affected by their background. In the next section of the chapter I discuss the syntactic hierarchy and various assumptions that are widespread among linguists, some of which need to be rethought, at least by the fieldworker.

The final section of this chapter addresses the role and method of argumentation in linguistic description. All too often in my experience field linguists fail to recognize that even descriptive accounts of linguistic items must argue for all their conclusions. Therefore, I consider the components of good descriptive argumentation.

We begin with a discussion of syntactic field methodology, move to semantic field methodology, and move, finally, to a discussion of argumentation. Before beginning syntax proper, let us consider some additional relevant considerations for syntactic field research.

7.1.2. The fieldworker is not a blank slate

Occasionally one hears fieldworkers advocate – or be advised to produce – so-called theory-neutral descriptions, in order to serve as wide an audience as possible (and so as not to be overly biased at the outset of the fieldwork). Insofar as this means avoiding theory-specific jargon when writing for a general audience, it is reasonable advice. But it is otherwise naive. Like newborns, fieldworkers are not blank slates. Moreover, they should not be. The purpose of training is not to make us 'open-minded'

⁴¹ I suggest to my graduate students the program I maintained for a good part of my career, namely, reading 50 pages per day, minimum, seven days a week, ranging across topics e.g. phonology, syntax, semantics, descriptive linguistics, etc.

but to make us keenly observant, able to recognize from among subtle alternatives the best-motivated analysis. To maintain the 'observer's edge', we need to be slightly biased.

The fieldworker cannot avoid learning and interacting with at least some linguistic theory. This is because all of us accumulate a perspective on how language works during our linguistic training and experience, either from our teachers, our reading, or our natural predispositions coupled with our experiences analysing linguistic data. And theories should not be avoided. They provide a framework of expectations (a source of the 'abductive surprise' discussed in ____ below) and a source of ideas (the 'abductive 'B's/hypotheses' also discussed in ____). And theories provide strategies and boundaries for arguing for conclusions. They are essential training for thinking more precisely. The choice of a specific theory is less important than the recognition that some theory must be selected. However, other things being equal, it is better to choose as your theoretical base a theory that has a track-record of crosslinguistic usefulness (which is why I suggest that fieldworkers familiarize themselves with *ROLE AND REFERENCE GRAMMAR* (VanValin and LaPolla (1997) in addition to any other theory they might choose to work with).

Theories can be misused of course. They are misused when a linguist fails to consider analyses or overlooks facts that have no obvious 'niche' in his or her own theory. They are also abused when the linguist simply *translates* their analysis into theoretical terminology, i.e. when the theory is only used 'for show' and is never causally implicated in the analysis proper, an all too common occurrence. Syntactic fieldwork always confronts the fieldworker with structures unlike any the have confronted previously. Often a structure that is 'weird' and apparently irrelevant for a particular theory's objectives will be ignored. But the same structure could turn out to be very important for linguistics more generally. This is why a fieldworker must not only familiarize him or herself with a particular theory, but also read widely, beyond the boundaries of that theory.

Ultimately, the fieldworker should strive to simply give an honest day's work for an honest day's pay. That means that they do their best to present the language (the aspect(s) of it they are concerned with at any rate) clearly and fully (never intentionally sweeping facts under the rug which appear to bear on analysis at the center of the fieldworker's description). In return they receive career-related emoluments. To do this work, they certain basic concepts about syntactic field research, to which we now turn.

7.1.3. Crucial syntactic concepts for fieldwork

Probably ever syntactic theory recognizes that the syntax of most, if not all, languages will be composed of constituents of different sizes (though see ____ below on recursion for a discussion of ways syntacticians differ). Almost all linguists agree on the following, in one way or another:

(7.1) Syntactic hierarchy

- a. words
- b. phrases
- (c. clauses)
- d. sentences
- (e. paragraphs)
- (f. discourses)
- (g. conversations)

Some theories, not all, recognize clauses, paragraphs, and conversations (as indicated by the parentheses). All linguists recognize that discourses and conversations exist, but not all theories have a place for them (most lack any well-grounded treatment of conversations). But the items in () not in parentheses are universally recognized. So at a minimum the syntactic description of any language should include a discussion of each of these constituent types, how they are put together, how they relate to one another, how they are like one another, and how they differ from one another. In addition, some linguists believe that there is a correct order of analysis, such that one should begin with the largest constituent, e.g. the conversation (but this is theory-dependent), and then work one's way down the hierarchy, one layer or level at a time.

Each constituent is built out of or 'manifested by' constituents at the next level down, though the precise way in which one constituent is built out of another varies. The standard article on the building or analysis of syntactic constituents is Wells (), still worth reading today. The methodological implications of this hierarchy are discussed in _____ below.

Note however, that there are huge differences in the cohesiveness of syntactic units as we proceed 'up' the hierarchy. This is important because it ultimately entails different methodologies and forms of analysis and argumentation for different levels. It is also important theoretically, because it could indicate that levels of this hierarchy have different theoretical status (consider, for example, Chomskyan theory's refusal to consider anything above the sentence as syntax proper. This is not an entirely unmotivated position to take, as Everett () argues in depth. But, methodologically at least, discourses and sentences must be studied in relation to one another.) A conversation can vary tremendously in the units that compose each of its exchanges and these could in principle be rearranged in a number of ways without loss of intelligibility. At the other extreme, the word level, constituents are much more rigidly ordered (but see Bickel, et. al. (2005) for a potential counterexample). Moreover, speakers' varying ability in constructing these units becomes at once more obvious and more crucial as the units get larger. For example, it makes little sense to ask 'Who forms words best around here?' But it does make sense, and the fieldworker should ask, 'Who tells stories best around here?' before deciding on a language teacher for (certain kinds of) discourse studies and text collection.

Therefore, each level of the syntactic hierarchy is associated with a specific methodology. I want to begin the discussion of syntax proper by a review of fundamental notions of syntax shared by most linguists.

7.2. Dominance and precedence (or Configurationality and linearity)

Two fundamental ways in which discourses and conversations differ from constituents lower on the hierarchy are the relevant restrictions of configurationality and linearity (or dominance and precedence) at each level. Consider the following:

(7.2) Words

- a. running
- b. *ingrun

Word-internal constituents, in general, perhaps universally, cannot be moved. So placing *-ing* at the beginning of the word, as in (b) produces severe ungrammaticality. Moreover words allow a range of configurational relationships, ranging from binary

branching (as in the English examples above) to the periphrastic constructions discussed in chapter _____. Phrases, on the other hand, have slightly less rigid linearity constraints:

(7.3) Phrases

- a. John's book.
- b. *book John's
- c. book of John
- d. *of John book
- e. book of John's

Phrase-internal order is also fairly rigid (and (e) shows that *of* and *-s* are not simply 'allomorphs'. Yet, as (a), (c), and (e) show, it does allow some flexibility. Clauses and sentences show even lesser constraints on precedence and, in some theories, greater freedom in configurational relationships. So, for example, in some languages prepositional phrases are only binary branching, with only one level of branching, though clauses in the same language may have various levels of branching, such as when prepositions allow no modifiers, only a head and a complement, whereas clauses are complex. (Yarawara, Dixon (2005) is one such example.)

(7.4) Clauses and sentences⁴²

- a. John ran to the store.
- b. ?Ran John to the store.
- c. To the store ran John.
- d. To the store John ran.
- e. *To the ran John store.
- f. *To the ran store John.
- g. Yesterday, John ran to the store.
- h. John ran to the store yesterday.
- i. John ran, yesterday, to the store. etc.

And discourse orderings are freer yet:

(7.5) Discourse

- a. John came in. Peter asked him if he wanted a drink. John sat down later.
- b. %Peter asked him if he wanted a drink. John sat down later. John came in.
- c. %John sat down later. John came in. Peter asked him if he wanted a drink.
- d. John was writing. Peter was writing. Susy was writing.
- e. Peter was writing. John was writing. Susy was writing.
- f. Susy was writing. Peter was writing. John was writing.

⁴² Morphological constraints can supplant syntactic restrictions. So in Latin (and other languages with rich morphology) word order is relatively free, because morphology is so consistent.

Discourse orderings are interestingly and significantly different from clause orderings because they are constrained by very different principles. For example, (b) and (c) are anomalous (%) because in the way we normally understand the discourse, Peter cannot ask John anything until he comes in. And John can't sit down until he comes in. But now imagine that John is just outside the window near a lawn chair. Peter asks him, through the window if he wants a drink. John sits down after that, thinking he is going to be served outside. He finds out he needs to come in for his drink, so he does. This little scenario renders (b) just fine. And a bit of rethinking makes (c) similarly fine. So the % only indicates that the order is strange in some contexts, but not in all. And yet the ungrammatical examples of words, phrases, and clauses seems unsalvagable by context. They are always bad. Conversational linearity is constrained by yet different principles:

(7.6) Conversation

- a. A Hi. Wanna beer?
B Yeah, sure.
- b. % A Yeah sure.
B Wanna beer.
- c. A So, what were you up to yesterday?
B Oh, about 5'8". Ha ha. Oh, not much really.
- d. % A Oh, about 5'8". Ha Ha. Oh, not much really.
B So, what were you up to yesterday?

Generally, conversations are structured in pairs of utterances, at a minimum, exchanges between different conversational participants. There are ways to think of the %'d examples such that they become more or less acceptable, however, as with the discourse examples. Although discourse does not share the 'interlocutor exchange' structure with conversation, it does share with conversation the fact that context and imagination can render otherwise (context-free) anomalous utterances or exchanges sensible and acceptable.

Therefore, at each level of the syntactic hierarchy, the linearity principles differ. Also, the overall structure, the configurationality of the constituents at different levels of the hierarchy is achieved by different principles (one structures a novel differently from a noun phrase, for example).

No one theory of syntax offers a comprehensive treatment of the syntactic hierarchy (indeed, as indicated by the parentheses in (), not all theories even recognize all the constituents posited). Tagmemics (Pike & Pike (1976)) attempted to do so, but the results were hard to follow at best and unconvincing and confusing to most linguists. Therefore, the fieldworker will have to supplement his or her chosen theory of expertise with additional, eclectic readings on the other constituents, rather than finding all the answers in any one study, book, or theory.

We now want to consider another notion that is basic for many linguists.

7.3. Basic word order

Linguists often talk about the 'basic word order' of a language. My guess is that most linguists that use this phrase know that it is fundamentally imprecise. Each word of the phrase is problematic. So, for example, 'basic' has no scientific significance, i.e. it is not a technical term of linguistics. The term 'word' is also imprecise because what most linguists mean here is 'immediate constituents of the sentence or clause', rather

than words, which are usually themselves not immediate constituents of the sentence or clause. And, finally, 'order' is not very useful either since to many (but by no means all) linguists what is crucial is the hierarchical structure of phrases, not their linearity.

Given this imprecision, it is not surprising that what linguists actually mean by 'basic word order' includes several concepts, not all clearly related. At least the following are included in one way or another:

- (7.7) a. Information-theoretic default constituent ordering.
 b. Discourse-functional constituent ordering.
 c. Initial/underlying constituent configuration – the hypothesized configuration that serves as the basis for non-default orderings.

Consider (a). In most or all languages there is a default information-theoretic ordering of constituents (see, for example, Van Valin (2005) and Lambrecht (1994)) and ___ below. English usually reserves the subject position for old information and the direct object position for new information. In fact, many languages (Van Valin and LaPolla (1997--)) place new information immediately to the left or right of the verb and the old or topical information farther out or on the other side of the verb. To get a feel for how information structure works, consider the familiar examples in (), from Van Valin (), in ___ below.

So for a given language, it is important that the fieldworker understand (by examining constituent orderings and intonation), among other things, how topic and focus are distinguished (see ___ below). To some people, this will be the core meaning of 'basic word order'. But there is complementary concept that any fieldworker could profitably consider, namely, discourse function of different constituent orders, not so directly tied to information structure.

Discourse functions of sentences involve things such as the use of passive clauses to mark backgrounded information or information with some other special purpose in structuring the discourse, or the use of one constituent ordering to open a discourse but a different ordering to mark denouement. Or subject-less or object-less sentences to mark the discourse body but use of full NP subjects or objects to indicate the beginning of end of a discourse. If such discourse notions are found to be useful in a given language, they may very well differ practically from the other concepts in (). If such different discourse functions exist, they may be discovered in one of several ways. First, there is no available excellent software for discourse filing (see the websites mentioned in ___). Second, there are some still useful manual methods. One method I used to employ was to color code sentence types throughout texts (e.g. red for active, blue for passive, etc., depending on the types of sentences you are interested in tracking) and then compare discourse genres and discourse functions or positions (e.g. beginning, climax, denouement, end) to see if there were patterns to the colors. This is a psychedelic experience for those of my generation while also being a useful field methodology.

Let's proceed in our discussion of major syntax notions to the concept of dislocation.

7.4. Dislocation

Most formal theories, to one degree or another, concern themselves with the phenomenon of 'dislocation', i.e. the situation where related meanings are expressed by

different word orders or where some constituents are not where they might be in a simple declarative. These theories often take one of the orders as underlying or default or basic and derive the other orders from this. So consider examples like those in (7.8):

- (7.8) a. John saw James.
 b. James was seen by John.
- (7.9) a. The mail is here.
 b. Is the mail here?
- (7.10) a. John likes Mary.
 b. Who does John like?
 c. John likes who(m)?
- (7.11) a. John thinks you like Mary.
 b. Who does John think you like?
 c. Who does John think likes Mary?
- (7.12) a. John thinks that you like Mary.
 b. Who does John think that you like?
 c. *Who does John think that likes Mary?

In most theories, the (7.8a)-(7.12a) examples represent the basic forms and the other forms the derived forms. The question words in (7.10b)-(7.12b) are said to be 'dislocated', i.e. not in their 'expected' positions (where they would be in a simple declarative). Harris (1947) proposed that such sentence alternations should be understood in terms of specific discourse functions for each alternate word order (which he analysed as 'transformations', but without a sense that one was derived from the other). Formal theories have more elaborate means of accounting for dislocation. But whether one works in a formal theory or not, the fieldworker must come to grips with Harris's observations, because they are vital to understanding the grammar of the language in question. It is perhaps always the case that different word orders have different discourse functions. And describing those functions is part of an adequate descriptive grammar or understanding of a language's syntax. Whatever theoretical account of such facts the fieldworker wishes to suggest is welcome, though many (myself included) would see this as secondary to finding out the communicative functions of the alternations or so-called dislocations.⁴³

Also, it is important to note that dislocation, while a prominent part of the syntax of many languages, is insignificant in many others (except in very abstract, theory-internal ways that are of little use to the average fieldworker, at least in my experience). Therefore, if the fieldworker goes to the field primarily trained in a theory with a strong emphasis on dislocation, to the exclusion of, say, information structure, then, however

⁴³ Urban (1998) contains a number of suggestive analyses illustrating potential cultural implications of discourse structures, e.g. the use of the passive vs. the active and the types of heroes that emerge, say, in cultures with high percentages of passive sentences for major characters, vs. those in which active sentences express the actions of main characters. .

Before proceeding to a consideration of discourse analysis and syntactic elicitation methodology, I want to consider another notion that is widely assumed by many syntacticians to be universal, but which also seems to have exceptions. Such exceptions, if valid, require that the concept not be used as an obligatory mold for structures in the grammar.

7.5.1. Endocentricity

(7.13) [S [Nuc(1eus) [S [Nuc **Ma'** **co** **mao**] **na** **-in**]
that:prox:hearer m/frp/p go(sg) 3s:rp/p -3n
Guajarái]
Guajarái (Brazilian city)

(7.14) [S [Nuc [S **Cao' xi'** **carawa**]] **nana** **hwijima'**].
 eat 1pincl:rf animal 3p:rp/p children
'The children will eat food.' (lit: *"We will eat food," the children (say).'*)

7.5.2. Simple syntax vs. complex syntax

"Simpler Syntax Hypothesis (SSH)

Culicover and Jackendoff go on to demonstrate throughout their book why the structure hypothesized for a given construction should be guided by the SSH rather than the standard 'theorems' of phrase structure in syntactocentric theories, e.g. Minimalism (Chomsky 1995). Most linguists share the view that figuring out the

hierarchical constituent structure of a given language is a very important and essential step to understanding the language. Naturally, for such an important area of linguistics, there are various theories on the constraints on possible tree structures, including the SSH.

Since Kayne ()'s work on restricting tree structures, many generative linguists have become persuaded that trees are binary branching, i.e. that (), but not (), is a possible structure of a human language. The SSH, on the other hand, would allow either, depending on the facts of the language under investigation.

(7.15) /\

 /\

 /\

 etc.

(7.16) /|\

 /\

 /\

 /\

 etc.

It may in fact be possible to fit any language into either of these structures, more or less convincingly. In fact, it might even be possible (see section ____) to avoid tree structures altogether and use only violable (e.g. Optimality Theoretic, Prince and Smolensky (1993)) constraints on linear precedence in conjunction with semantic-based adjacency to account for the syntax of some languages. We might call the binary branching hypothesis 'complex syntax', the non-binary branching hypothesis in () the 'simple syntax' hypothesis, and the absence of tree structure in a language (whether the entire language or only some structures) the 'no syntax' hypothesis. From my own experience, each one of these views has advantages and disadvantages for the fieldworker (though the inductive simple syntax and no syntax possibilities are spiritual kin, complex syntax has nothing in common with them in its strong deductive approach, forcing structures without considering alternatives).

Following the complex syntax hypothesis may be a requirement of the theory you have adopted. But it is no less a procrustean bed for that. If a field linguist refuses to consider non-binary structures, they may indeed produce a description fully compatible with their chosen theory, yet utterly lacking in insight into various aspects of the language and of little long-lasting value (remember the fate of the famous Hidatsa grammar, discussed in ____).

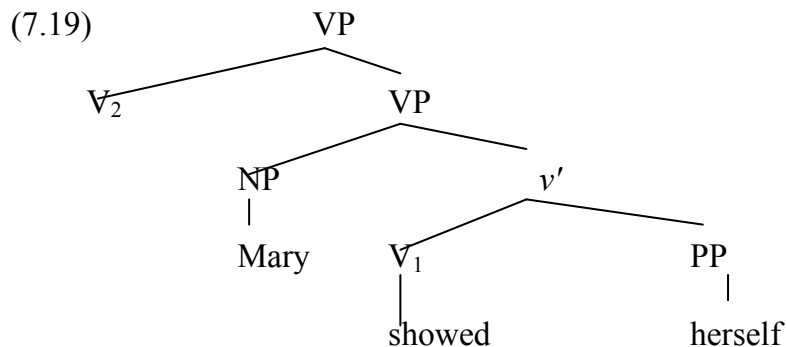
The simpler syntax model may or may not be restrictive enough. My general rule of thumb is to propose the most restrictive trees possible (binary) initially, but always look for (and *accept*) evidence that this analysis is wrong, either relaxing it to (b) or (c) (on the latter also see ____). For example, consider the following discussion of English syntax from Culicover and Jackendoff, based on English examples like those in (7.17) and (7.18):

(7.17) I showed Mary_i herself_i.

(7.18) *I showed herself_i Mary_i.

Following the standard assumptions of complex syntax, i.e. imposing a deductive structure on the data that requires *c-command* to capture binding facts, without concern

for the thesis of SSH, Larson (1988) argues that such data require binary trees plus a special 'verb shell', with subsequent movement from V_1 to V_2 :



However, Culicover and Jackendoff (2004, 52ff) argue that simple precedence could account for such examples by requiring antecedents in the VP to precede their anaphors (this grossly oversimplifies their account, so the reader is advised to consult Culicover and Jackendoff (2004) for full details of their model).

Let's move now to briefly consider what a language without hierarchical syntax might be like, drawing from some suggestions based on my own work on Pirahã.

7.5.3. No syntax

Almost every language manifests the property of recursion, what some linguists consider to be the foundation of syntax (see Hauser, Chomsky, and Fitch (2004)). Simplifying slightly, recursion has two forms:

Embedding:

(7.20) $A \rightarrow AB$

System recursion:

(7.21) a. $A \rightarrow BC$
 b. $B \rightarrow DE$
 c. $C \rightarrow AF$

Linguists vary in the importance they attach to recursion. So, for example, Hauser, Fitch, and Chomsky (200??) argue that recursion may be the only (or core, depending on interpretation) component of Universal Grammar. Others recognize it, but do not attribute such genetic or species-defining importance to it.

Everett (2005) argues that there is no evidence for recursion in Pirahã. And there is some reason to believe (Hale (), Dixon (), Austin (p.c.), Nordlinger (p.c.) that recursion may be largely lacking from some Australian languages as well. In any case, the fieldworker must exercise care and caution in attributing the property of recursion (or indeed any property) to syntactic structures. This is because recursion is not the only way of combining syntactic units of one level of the hierarchy into another. Another way to put units together is *parataxis* (Bloomfield ()), where two more units are set side by side as it were, with no further structure, in particular without one structure being contained inside another of the same level. So, for example, () is the result of recursion, () is parataxis, and () is arguably parataxis as well:

(7.22) $[_{NP} \text{John's } [_{NP} \text{father's } [_{N} \text{hat}]]]$ is nice.

(7.22) is recursion, because one NP is found inside of another.

(7.23) The lion, the witch, the wardrobe, what have you, are all fictional objects in this story.

The first part of (7.23) is potentially a case of parataxis. The phrases (the lion, the witch, the wardrobe) are linked paratactically, rather than recursively (at least by my analysis). As another example of parataxis, consider Tagalog.

Some linguists, e.g. Schachter and Otnes (1972 - **PAGE**) and others have pointed out that some Tagalog sentence structures are like equations, where the two sides are brought together in a larger unit (the sentence) via parataxis:

- | | | |
|--------|---------------------------------------------------------------------|--------------------------------|
| (7.24) | Predicate/Comment | Topic |
| (7.25) | <i>Titser</i>
teacher
'The woman (is a) teacher.' | <i>ang babae.</i>
nom woman |
| (7.26) | <i>Maganda</i>
stative-beauty
'The woman (is) beautiful.' | <i>ang babae.</i>
nom woman |
| (7.27) | <i>Umalis</i>
leave-actor focus, completive
'The woman left.' | <i>ang babae</i>
nom woman |

Therefore, we can conclude that, at least for some constructions in some languages, recursion is irrelevant. Further examples can be found in Pirahã, according to Everett (2005). Consider the following:

First Pirahã lacks coordination, disjunction, and embedding. It uses non-recursive 'circumlocutions' to express each one of these structural types.

COORDINATION:

- (7.28) *Ti ?isigihii kohoaipi. ?itii?isi pi-ai.*
I eat meat. Fish does also/now.
- (7.29) *Ko?oi (hi) kohoaipi. ?aibigai (hi) pi-ai.*
Ko?oi he eats. ?aibigai does also/now.

DISJUNCTION:

- (7.30) *Ko?oi (hi) kohoaipi. Hai, ?aibigai (hi) kohoaipi.*
Ko?oi eats. Hmm. ?aibigai eats.
- (7.31) *Hi ?isigihii kohoaipi. Hai hi ?itii?isi kohoaipi. Ti kos-aaga.*
He eats meat. Hmm. He eats fish. I am ignorant (do not know).

Notice that in the following quotative, it is the verb of saying that bears what Everett (1986) analyzes as the nominalizer, rather than the otherwise 'embedded verb'.

And yet in the subsequent apparently embedded clause 'arrow-making', the 'embedded' verb has the nominalizer. Everett and Van Valin (in progress) analyze this **–sai** as a marker of secondary or topical information, rather than as a nominalizer. The same analysis would hold true for (). The suffix **–sai** is in fact a verbal form and the language has a nominal corresponding to **–sai**, namely **–si** (found on the autodenomination of the language of the Pirahã, **?apaitĩsi**). The nominal suffix marks nominal secondary or topical information (see Everett (1986)) for details. Many other examples are discussed by Everett (in progress) on Pirahã and language evolution.

EMBEDDING:

- (7.32) **ti gái -sai kó'óihi kaháp -íí**
 1 say -nom. namehe leave -intention
 'I said that kó'ói intends to leave.' (literally 'My saying **kó'ói** intend-leaves')
- (7.33) **hi ob -áa'ái kahaí kai -sai**
 he see -attractive arrow make-nom.
 'He knows how to make arrows well.' (lit: 'He sees attractively arrow-making')

One way to account for the apparent lack of recursion or configurationality of Pirahã is to adopt a no syntax hypothesis and analyze the Pirahã facts in terms of lexical semantics and linear precedence, as in (7.44):

- (7.45) Linear Precedence + Lexical Semantics (> = 'immediately precedes'):
 Agreement > NUCLEUS
 NUCLEUS > Modifier
 Topic > Comment
 Argument > Agreement

These rules capture most of the facts of Pirahã 'syntax' without recourse to recursion. And I would be surprised if Pirahã were the only language for which the no syntax hypothesis is the simplest account of the data. This brings us to a more recent development of syntactic theory, another place where hierarchical, recursive structures have little role, namely, syntactic 'constructions'.⁴⁴

7.6. Constructions

Words (and for some linguists morphemes as well) are considered by most linguists to exemplify the Saussurian sign, an arbitrary association of sound and meaning. Syntax, as the general reasoning (used to at least) goes is not arbitrary but strictly the output of regular constraints or rules. Goldberg (2006, 3) summarizes an alternative position,

⁴⁴ We might well ask, of course, why such considerations are relevant for the fieldworker, as opposed to the syntactic theoretician. The reason it is crucial for fieldworkers to be knowledgeable and argue well is that if they simply apply the complex syntax model or some other theory blindly, however 'basic' or 'uncontroversial' they take it to be, their work can fail to teach all of us what it should have taught us about the language in question. The fieldworker should take no structures for granted.

"Many linguists with varying backgrounds have converged on several key insights that have given rise to a family of ... constructionist approaches. ... constructionist approaches emphasize the role of grammatical constructions: conventionalized pairings of form and function."

In other words, there are many linguists who are persuaded that some or most of syntax is convention and not merely the output of formal rules or constraints. (But cf. Borer () for an alternative view of constructions.)

Some of the evidence for constructions does indeed seem very strong. So, for example, consider the 'Xer the Yer' or co-variational construction of English:

- (7.46)
- a. The more you think about it, the less you understand.
 - b. The hurrieder I go, the behinder I get.
 - c. The bigger they are, the harder they fall.

This co-variational construction, according to Goldberg (2006, 6) is "... interpreted as involving an independent variable (identified by the first phrase) and a dependent variable (identified by the second phrase)." These properties of the construction do not follow merely by reading the semantics directly off of tree structures, as some theories might maintain. Rather, they are 'added' to the sentence by convention, the latter following from the matrix culture (see the papers in Enfield (2004)). In the view of many linguists, all languages have constructions for which part of the meaning is compositional and structure-based but where other parts of the meaning are added by convention. If these linguists are correct, then a fieldworker armed only with tree structures and form-based constraints will fail to account for vital, construction-based components of the grammar.

So how would one go about identifying and describing constructions? I suggest the conceptually simple, though methodologically rigorous, procedure in ():

- (7.47) Methodology for identification of constructions
- a. Understand the meanings of words and morphemes in the language (morphology fieldwork, see chapter ____).
 - b. Understand the constraints on syntactic object formation in the language (chapter ____).
 - c. Understand the syntax-semantics mapping in the language (section ____ below).
 - d. See if any meaning is leftover that is constant for each instance of the hypothesized construction.

If a particular phrase, clause, or sentence is a construction in the technical sense, its meaning will not be exhausted by the preliminary steps in (a-c) above, but it will instead be circumscribed by them (i.e. it will not be completely arbitrary, e.g. an idiom like 'kick the bucket', but neither will its meaning be a simple sum of the meanings of its constituents). In other words, the fieldworker is satisfied that they understand (a-c) in the language under study, the part of the word or phrase that is left over, and that this leftover portion follows regularly from the construction in its various manifestations.

7.7. Discourse-down analysis

In this section we consider why it is important to collect the vast majority of one's data in the form of natural texts. It is the best method for collecting *acceptable*, natural examples. We discuss the basic text genres that should be represented in any data sample – especially what I consider to be the four basic genres: *expository* (e.g. teaching someone why certain things in the culture work the way they do), *narrative* (e.g. a historical text), *procedural* (e.g. how to make a particular object or perform a particular ritual), and *hortatory* discourses (e.g. exhortations on how to solve a community problem, sermons, etc.).

Not all cultures have stories about topics we might expect them to have stories about. For example, when I first told people that the Pirahãs had no creation myths or discourses about their history, anthropologists were skeptical.⁴⁵ One anthropologist actually went to the Pirahã village with the express purpose of collecting creation myths. His method was to ask in Portuguese and hope for the best, since he immediately recognized the Pirahãs' lack of knowledge of more than very rudimentary Portuguese. "How was your world created?" he asked. He recorded his questions and the answers for me, in order for me to help him translate the material he was collecting. After asking the question in Portuguese, he waited for the Pirahãs to translate the question into Pirahã. "The world is created," replies one of the assembled men in his own language. "Tell me how your god made all this?" the anthropologist presses on. "All things are made," comes the answer. The interview lurches on for a few more minutes, until suddenly, the question and answer session is overtaken by a deluge of excited banter as the assembled Pirahã vie to be heard.

"I've cracked it," said the anthropologist to me as he handed me his tape recording a few weeks later. "Here is the Pirahã creation myth." I must admit to being a bit dubious. In the past three decades, I have spent a total of seven years living with the Pirahã in the Amazon rainforest and am one of just three outsiders, along with my ex-wife and a missionary who spent time with them in the 1960s and 1970s, who is fluent in their language. I have long maintained that they are among the few people on Earth who have not devised a story to explain their existence. Others, including this particular anthropologist, find the idea difficult to accept.

So I listened to the tape. After the short, stilted exchange, some bright spark points out that this guy asking them odd questions doesn't know their language, so he will need to get help from me to translate the tapes. "Hello, Dan!" comes a chorus of Pirahã voices. "How are you?" "When will we see you?" "When you come, bring us some matches." "And bananas." "And whisky." And so on. Nice try, but no creation myth here.

7.7.1. Discourse and its relation to syntax

Understanding what the principal formal genres or 'distinguishing parameters' of discourse are in a given language, how they are structured, and how sentences are distributed within them, what kinds of information are talked about, what kinds of information are avoided, and what kinds are implicit, are each fundamentally important

⁴⁵ This box on creation myths is take largely from the report on my work by Kate Douglass in *New Scientist*, March 18, 2006, a story I told her during her research.

to the fieldworker's quest to understand the language they are describing. As Chafe (1994), among many others, demonstrates, there is much to learn about culture, cognition, and grammar in different languages via the study of discourse. With regard to content and cognition, as James (1890, 1:243) [The principles of psychology, 2 volumes, New York: Henry Holt. Reprinted 1950 by Dover Publications, New York.]puts it (also cited by Chafe (1994, 3)):

"As we take, in fact a general view of the wonderful stream of our consciousness, what strikes us first is this different pace of its parts. Like a bird's life, it seems to be made of an alternation of flights and perchings. The rhythm of language expresses this, where every thought is expressed in a sentence, and every sentence closed by a period."

On the other hand, as the generative tradition has shown over the past several decades, a large portion of syntactic structure does not simply 'emerge' as a by-product of discourse study. Among other things, discourses do not provide systematic ungrammatical examples – and yet these are vital for adequate analyses. The fieldworker should therefore conduct careful investigations of discourse structures, realizing, however, that these investigations will not be sufficient, however necessary, for a full analysis of the syntax of the language.

However, there *are* significant methodological advantages in beginning one's syntactic investigations by working 'downward' from the discourse. We consider a few of these in the remainder of this section.

Before you have learned to speak the language or studied its structure extensively, there is a plethora of opportunities to be misled as to the appropriateness, grammaticality, or translation of different sentences. There is ultimately no way to avoid this (another reason to remember Postal's Maxims). On the other hand, beginning syntactic study with the discourse ensures that all data are embedded in an appropriate context, are (likely) felicitous, and are grammatical (or at least acceptable). Thus, in this sense, the data are more reliable at the initial stages of analysis than the isolated, de-contextualized test sentences that might otherwise be used in elicitation.

Also, by studying sentences in their discourse context, the way the language presents and structures information can be studied more easily, naturally, and effectively in the initial effort to understand how it works. Eventually, other methods will be necessary, but it is difficult to imagine a more effective way of beginning studies of information structure than via the discourse.

Further, intonation can be more studied with more confidence at the outset (see Chapter ____ below for many more detailed suggestions on the study of intonation) because it is naturally contextualized when looked at initially in discourse. Initial hypotheses on intonation can at least assume that the intonation recorded in discourse is appropriate and natural and that it fits the particular information structuring of that part of the discourse.

Another advantage of beginning syntactic analysis with the study of discourse is that the entire array of sentential features, e.g. grammatical voices, constituent orderings, moods, evidentials, pronoun omission, etc. are observed in natural distributions. This is invaluable to undertaking their study. Finding them in appropriate linguistic environments is almost inconceivable without initial study of natural discourses. Of course, the initial analysis will not exhaust their function and structures, but it will provide a better beginning point.

Functions of different sentence types will also be more readily visible when looked at in natural discourse initially. For example, are certain sentence structural types, certain phrase types, or words more common at the beginning, middle, or end of a particular discourse genre? Even such simple questions could provide important clues and insights into the final analysis of specific structures and constructions. But this simple source of insights depends crucially on studying discourse at the outset of syntactic research. And, as we see in ____, Lowe () argues that understanding discourse is vital to understanding morphology and lexical meanings in many languages.

7.7.2. Discourse genres and analytic methodology

This section makes no attempt to offer anything approaching a full introduction to discourse analysis. The main reason for this lack of ambition is that discourse analysis is an interdisciplinary set of research programs and not a simple step-by-step set of tasks. This interdisciplinarity is natural since discourse is the nexus of culture, philosophy, language, literature, cognition, social relationships and conventions, and grammar, among other things. Arguably, our discourses are essential to identify and distinguish languages, cultures, and psyches of individuals and groups. For that very reason, discourse analysis is a vital part of field research. That is, if I am right to argue that an understanding of a given grammar should go hand in hand with an understanding of the containing culture, then discourse study becomes more important yet, as the crucial link between the two.

7.7.2.1. Discourse genres

Discourse genres or types or varieties, etc. are hard to summarize. What might be to one fieldworker two tokens of the same genre could be to another fieldworker examples of very different types of discourse. Distinguishing discourse genres is largely a matter of what your background, expectations, and objectives are. Nevertheless, there are some distinctions that one should keep in mind, as a set of suggestions.

One type of genre that might be encountered in many societies is what we can label 'stylized or belletristic texts', that is texts that form part of the acknowledged cultural traditions and oral literature of a particular culture. These texts, regardless of their content, are very important because they represent careful reflection on both form and content and can reveal a tremendous amount about the culture and about the linguistics of discourse. However, at the outset of the field research they can be somewhat problematic. The reasons include the facts that they often include stylized or archaic language that is not idiomatic (which will be important to the eventual analysis but confusing at first) and they can be very long. They will be much more involved, harder to figure out in some ways. Moreover, since transcription time is roughly 5-6 times greater than actual recorded material and in fact much more at the beginning, this type of text can compromise your freedom to look at other things in the grammar. And if texts are not processed right away, at least not while in the field, they are simply worthless. Therefore, though no grammar would be complete without a careful discussion and analysis of such material, I recommend that one try to begin field analysis of discourse with simpler texts, e.g. simple reports.

The field linguist should also be sensitive to the possibility that speakers of the language may have their own recognized genres which may or may not correspond to the field linguist's own categories. Treat these differently if you do encounter such

cases. Almost certainly there is something in content or form that will distinguish them from one another and it will be worth your time to attempt to figure this out.

For a linguist, the most important classification of discourses will be in terms of their *structural distinctions*, things such as predominant tenses, aspects, construction types, overall organizational principles, etc. For example, let us consider four types I find useful to distinguish in my own fieldwork, namely, narrative, procedural, expository, and hortatory. To these we can also add religious, brief reports and informal discourse, and, last but by no means least, conversations.

Narratives are the easiest to study. They tend to use mainly indicative or a related mood, e.g. realis, and an array of past tenses. It is interesting to observe how the narratives are organized. For example, are events and participants introduced or presented in chronological order or in some other way, e.g. cultural relevance, logic, etc.? How are main participants distinguished from secondary participants? What kinds of sentences mark the main event line, as you understand the text? Do different kinds of sentences mark events that do not advance the main events of the discourse? How about the way that new participants are introduced? Are all characters of the discourse introduced in the same way? Or does it turn out that characters central to the telling are introduced differently (e.g. more sentences about them, different aspects or moods, more relative clauses, etc.) than peripheral characters? Are mythological characters treated formally different from 'real' characters? And so on. The possibilities are almost literally endless. Part of what distinguishes a mediocre field research from an insightful fieldworker is the effort invested to interrogate structures from different perspectives, formal, functional, cultural, societal, historical, etc.

Procedural discourses, things like how to make a bow and arrow, how to bake a cake, how to catch a fish, etc. involve step-by-step procedures that can illustrate moods and tenses (e.g. imperative and sequential, respectively) that are not found as readily in narratives. Since the subject of most procedurals is the addressee ('You do this, then you do that', etc.) then there is more likely to be subject omission in procedural discourses, due to the facility of subject identification.

Expository discourses explain things about the world. They may not turn out to be a lot different from simple narratives. But it is useful to begin by assuming that simple story-telling could be different from explanations and to examine these types of texts carefully for distinguishing formal and cultural characteristics.

And finally, hortatory texts are a superb source of cultural and linguistic insights (if the language in fact formally distinguishes them, though it likely will). These will be a rich source of deontic modals, different types and scopes of negation, cultural values, aspects, subject forms, etc.

With this barest list of suggestions on how to approach the general issue of discourse, let us consider in more detail an accompanying methodology.

7.7.2.2. Discourse analytical methodology

First, it is vital to have video or audio recording equipment present at different events associated with different discourses or the whenever there is a likelihood of recording specific texts, e.g. when someone returns from hunting, preparation for ritual, etc. Second, you must be creative in getting what you are after across clearly to your language teacher. Often in my experience, language teachers have felt that it is somehow wrong to give complex answers and prefer to give short, one sentence or one word answers to questions. Unless one can effectively communicate to the language teacher what one is after, the field session can be very harrowing and unrewarding. And

it is often just hard to get speakers to give a discourse in such cases. One suggestion is to build up texts line by line, by going back and forth between speakers to get more and more information on a particular topic and then ask someone to recap, rewarding the person who tells the longest story about it.

Taperecorders have a use beyond recording of sounds. They are essential in discourse analysis as well. For analyzing discourse, I recommend that you use at least two recorders, or a recorder and a computer, etc. That is, that you have one device dedicated to playback and another device dedicated to recording. Simple analog recorders with 'cue/review' buttons are the most convenient, but if you have a computer for playback this can be almost as easy to use. First, record a text with a native speaker. Second, transcribe the text and translate it with a native speaker. Third, go through the text with another native speaker (there are some ethical considerations here: make sure that the first speaker has given you permission to play the tape to others in the village, that the tape contains no sensitive or embarrassing material, and that the genre is one appropriate for a general audience, or carefully select language teachers that can and are willing to help you with sensitive material). This is done by playing back a portion of the tape and asking the new language teacher to repeat what was said, slowly. Record what the new language teacher says, including their comments about the text. This slower repetition will almost certainly introduce changes of pronunciation and lexical choice, but that is good. You want paraphrases and alternative pronunciations. The commentary provided will provide cultural and linguistic insights that are difficult to obtain in any other way. For a particularly interesting or challenging text, I would ask up to three additional speakers for comments, corrections, and translations.

After collecting texts, however obtained, they need to be processed. One set of suggestions that I have found useful is: (i) record the text digitally, with the speaker using a headset (this is discussed in the chapter on phonetics); (ii) transcribe it the first time alone, with no speaker present; (iii) check your transcription with the speaker who gave the text originally and write their corrections and comments in a different colour of ink. This will require also the use of two recorders. One to play back the original recording and one to record the speaker's comments. (Alternatively, one can record directly onto the computer. But I recommend separate digital or analog recorders and then transferring the data to the computer so as to have automatic backups of the data, among other reasons.); (iv) check the second transcription with another speaker and use two recorders, again, one to play the original text and one to record the second speaker's comments, corrections, etc.; (v) do a four-line transcription of the entire corrected text (all of this in the field!), with the following lines: Line 1 = morphophonemic transcription; Line 2 = phonemic/underlying segments transcription – these will differ occasionally; Line 3 = gloss; Line 4 = free translation. These are illustrated in the following example from Wari':

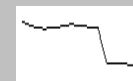
- (7.48) **Mon** **womu** **cara** **ne**
 ma'-on **womi-u**
 that:prox:hearer-3sm cotton-1s that:rec. rec:past
 'Where are my clothes that were just here a minute ago?' (lit: '... my recently
 absent clothes?')

Last but not least, all field linguists need to attempt to record conversation. The box here gives some initial bits of a conversation recorded in July 2004, among the Banawa people of Amazonas, Brazil.

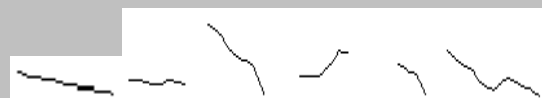
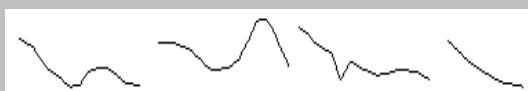
Speaker A: Batao
Speaker B: Bido



2. B: m ee



4. B: m



5. A: Danieo kamei hidabani fame / eremee e atini me no fa fa i e

‘Daniel and the others came all together. They like our language much’

This type of study is extremely important at various levels of study of the language and culture, revealing not only formal components of conversational organization in the language, but natural intonation, intersentential anaphora, and so on.

By carefully looking at syntax, intonation, and discourse from natural data, we have an extremely useful tool and data source for subsequent analysis.

Of course, getting two people to sit in front of a researcher and converse naturally is not easy. The Banawa were particularly good at this, however, and although the conversation began slowly, within a few seconds, both interlocutors were conversing naturally, looking at each other and not at the researchers, the recorders, or the video camera. Further, both speakers, Sabatao and Bidu, wore headset microphones and each recorded onto a separate channel of our stereo digital recorder. If you are unlucky and cannot get people to converse naturally in front of you (the Pirahã won't for me, even after all these years), then there are other ways to approach the problem. You can get the people's permission to leave a voice-activated recorder in their house. You can try to record phone conversations (our first Suyá text was one side of a phone conversation, recorded more or less by mistake as the language teacher's cellphone rang during a language lab session). These all have obvious disadvantages, but still they can produce useful data for discourse and conversational analysis. Other possibilities are to record radio broadcasts, video discussions of community beliefs, or rituals, etc. When asking questions you hope will produce discursive answers, be sure that the questions are framed broadly enough so that a free-ranging answer is more appropriate than a simple yes or no. Watch how they ask questions on your favorite talk show (modulo the rehearsal that goes into these overproduced spectacles). How does the talk show host get guests to open up and talk freely (at least between commercials)? How does a reporter interviewing a nervous subject? Take notes on technique and apply what you learn to your discourse-collection techniques. It is particularly important that researchers avoid framing questions so as to impose their own categories, values, orderings, abstractions, and length expectations, among others, on the language teacher (i.e. insofar as possible. It is never possible to completely eliminate our own culture, nor do we want to because our grammar-writing goals are a *cultural* value of our own. The very doing of field research and research more generally are themselves cultural goals, so we are obviously as bound by our culture during the data-collecting process as the language teacher is, if not more so.)

Finally, let me conclude with a brief mention of the importance of implicit information – what is *not* said (e.g. sample Pirahã text) in studying natural language discourse. This kind of information can be important in understanding how the culture works. The reason for that is that what is not said often reflects values and knowledge that all hearers and speakers of the culture hold in common. Consider for example, the following hypothetical exchange:

- (7.49) Speaker A: Do you want drinks at the reception?
 Speaker B: If you want to clean up and referee.

This is a simple example. Members of American (and other Western) cultures will know that this conversation is about alcoholic beverages and that people who drink such beverages can get messy and rowdy. That is what the references to 'clean up' and 'referee' refer to, this implicit cultural information which is simply not present in any theory of the literal semantics of this exchange. This seems obvious to members of the

culture, but it is not obvious to outsiders and this kind of thing will be puzzling for the field researcher until they have mastered the culture of the language under study (which they may never do of course).

7.7.3. Syntactic methods

When a discourse begins, we can be sure that its beginning marks one and probably several layers of morphosyntactic boundaries (e.g. morpheme, word, phrase, clause, sentence, discourse). Likewise, the end of a discourse will be the right boundary of at least one and likely several morphosyntactic constituents (and layers of constituency).

Also, we can identify morphosyntactic boundaries via discourses by identifying points at which speakers backtrack to reinterpret the sentence (e.g. for hard to follow or garden-path sentences, etc.), to correct errors, and so on, even without sophisticated experimental equipment).

Spontaneous discourses also are marked by hesitations of different kinds by the speaker. These hesitations will often come at morphosyntactic or prosodic boundaries and so may be used as a source of hypotheses for investigating these boundaries.

Each discourse genre can also be expected a priori to be a likely source of certain kinds of grammatical features. So, for example, in a narrative discourse, most sentences are likely to be in past tense and indicative mood in most languages. In hortatory discourses, a large number of sentences are likely to manifest imperative mood and conditional or other special tenses. In procedural discourses we will get sequential relationships between sentences, present tenses, indicative and imperative mood, and so on. In other words, regardless of the theoretical status or relationship between discourses and sentences (and see Everett () for a review of many ways in which they might be profoundly different), the study of discourses can provide extremely important methodological support to understanding sentences. Let's consider some methodological rules of thumb that I have found useful over the years.

As I have said, textual information alone is inadequate to underwrite a grammar of a given language. This is because individual sentences need to be studied and permuted for tests of grammaticality. Nevertheless, sentences are usefully extracted from discourses for further testing by the following procedures.

First, isolate clauses in the text, using phonology and phonetics to probe for relevant constituent boundaries, as well as any knowledge available of the morphosyntax. Once each sentence of a particular discourse has been identified, then these can be used to generate paradigms, where each position and hypothesized constituent of the sentence can be checked by substitution and distribution tests. So, for example, assume that you have extracted the sentence, *John will probably eat beans on Wednesday*. Then this sentence can be extracted and tested as per (), listing English grammaticality judgments (my own) by way of illustration:

- (7.50)
- a. John will probably eat beans on Wednesday.
 - b. ?John will on Wednesday eat beans probably.
 - c. *Beans will probably eat John on Wednesday.
 - d. The boy will probably eat beans on Wednesday.
 - e. The girl will probably eat beans on Wed.
 - f. Bob will probably eat beans tomorrow.
 - g. Bob will probably smoke beans tomorrow.
 - h. etc.

As you work through every sentence in every text in this way, adding to this process data observed in perambulatory elicitation, you will quickly come to have a very solid foundation for analysis and 'attacking' the entire grammar of the language.

Filing your data is vital for your own subsequent analysis as well as for linguists and others of the future for whom your data will be important.

Some linguists, usually those of the above 40 category, will prefer to file their data on paper. There are in fact advantages to hard-copy filing. For example the linguist works 'manually' with their data in this type of filing and for some people, this can be an aid to remembering the data. Certainly your field transcriptions, hand-written notes, etc. will be filed in hard copy.

By and large, however, I recommend the use of the very useful software programs available for language data storage. A number of such programs are available through SIL International, for example (<http://www.sil.org/computing/catalog/index.asp>). Other software producers include the Nijmegen Max Planck Institute for Psycholinguistics (<http://www.mpi.nl/DOBES/INFOpages/applicants/dobes-techframe-main.html>). The general rule of thumb for filing of any kind is to provide ready and clear access to all categories of grammar and semantics that the fieldworker has determined to be of relevance and significance for understanding the language in question. Thus, filing will not be the same from one language to another, but will require the judgment of the field linguist. Categories such as discourse type, clause valency, subordinate vs. matrix clauses, aspect, tense, mood, case, and so on are all obvious initial divisions in the filing system adopted for any language. The software collected will also provide numerous useful ideas for filing. On the other hand, since judgment is important in establishing, maintaining, subdividing, and applying categories appropriate for each language, the linguist will want the software to exhibit flexibility. Some programs can turn out to be fairly 'wooden' in practice. Therefore, before investing time and money in data filing software, you should inquire from the software producer and several users of that software something of its problems, advantages, and overall rating or utility. On the other hand, the more flexible the software, the less user-friendly and more demanding of the fieldworker's computer knowledge is likely to be.

7.8. Semantic fieldwork

7.8.1. Introduction

Semantics fieldwork is difficult under the best of circumstances. Part of the reason for this, in fact a large part of the reason for this, is that semantics, the study of meaning, must involve knowledge of culture, since in a sense, culture is the source of a great deal of meaning in natural languages. Another part of the reason is that for the large part of how meaning is related to syntax in the language in question, as well as various formal aspects of meaning (as discussed below) are extremely difficult to get at in the best of circumstances. Here methodology is crucial. In this chapter, I provide a brief overview of some of more salient issues in semantic fieldwork, drawing both from my own experience and from the very important article on semantic methodology by Matthewson (2005).

Semantic fieldwork can involve at least the following topics:

Ethnoclassification and Semantic fields – These related areas have to do with the way that cultures organize the world around them into meaningful categories. The world we live in can appear to be self-classifying to the naive observer. There are things like water, forest, fire, sky, ground, animals, people and minerals in the world.

Why would the linguist's task be any more complicated than working through 'Adam's task' in reverse? (Where by 'Adam's task' I mean just the biblical reference to Adam giving names to the things around him as though his pre-existing language matched one-to-one the world around him, without the necessity for an intervening culture to give meaning to that world.) That is, why doesn't the linguist simply collect the names for things in the a priori universe? Let's consider this problem in more detail, via the real world entities that we refer to in English as 'sky' and 'ground'. Isn't it perfectly obvious that these are distinct entities in the world?

In my first day of research among the Pirahã, I learned that they used a single word to refer to the entities labeled by 'sky' and 'ground' in English, their word **bigí**. But it took many more years of research to find out why this was the case. Initially I thought that perhaps I might have gotten the tones wrong or that there were actually two forms that shared a single truncated form. But it wasn't too difficult to determine that in fact it is the same word. So could the two words be homonyms, like 'too' and 'two' in English or 'janus' words (words that are their own antonyms) e.g. English *fast* 'to move quickly' and *fast* 'immobile' ('to stand fast'). These hypotheses seemed self-serving and unrevealing. There was clearly something going on here that I was missing. Ultimately, it emerged that the Pirahãs view the universe differently than we do. To the Pirahãs, the universe is structured in layers and the name for the boundary between any two such layers is **bigí**. So above the 'sky' is one layer of the universe. Below that is another (our biosphere). Below that is another, the boundary corresponding in a very broad way to what English calls 'ground'. Thus there is no simple mapping between English words for nature and the Pirahã **bigí**. Pirahãs and English speakers structure these parts of their universe differently.

The field linguist, if they are to give a comprehensive or accurate account of the semantics of the target language must provide at least an initial list of categories and subcategories of the natural and mythical worlds found in the language under study. But this will clearly involve a study of the culture of the native speakers – at least a synchronic study and often diachronic studies as well. Further, for detailed ethnosemantic studies it is crucial to enlist the support and advice of ethnobiologists, either in the field or in your prefield preparation. A great number of important pieces of cultural knowledge, linguistic structures, and meaning relationships depend on careful ethnosemantics.⁴⁶

Semantics proper

Semantics-syntax interface: How does the syntactic form of a sentence constrain its meaning? Is semantics simply a summation of the meaning of the words of a sentence plus the contribution of a specific word order or hierarchical structure? We have already seen in ____ that grammatical *constructions* are important because, among other reasons, important aspects of their meaning are not derived by strict composition in the sense just given, but also involve what we might refer to as 'annotations', specific bits of noncompositional meaning associated with a particular construction which must be memorized as part of one's knowledge of the language in question. This then raises the question of what other types of meanings and meaning-form relationships might be introduced in the syntax. To get at the meanings that are linked to different syntactic

⁴⁶ A very good study of this kind in Brazil is Jensen's () study of bird classification among the Wayampi of Northern Brazil. A huge portion of Brent Berlin's research program has been dedicated to such classification, with important insights for all of us interested in the language-culture-real world connection (see Berlin (), (), ()).

forms and to tease apart different kinds of semantic meaning requires, to put it mildly, careful and painstaking work. In this subsection, we look at some suggestions on how to proceed in this area, borrowing heavily from Matthewson (2004).

Word and sentence meaning: Our basic question in this aspect of meaning is this: what does one know when one knows the meaning of a word or sentence? There are two broad components of the answer to this question of importance to most linguists. First, to understand the meaning of a word or a sentence is to know when it is appropriate to utter it. Saying 'John is a bastard' when meaning to say that 'John's parents were not married at the time of his conception' might be literally true in some archaic form of bastard. But this would not be a felicitous use of the term in general since to most speakers 'bastard' is principally used to comment negatively on the character of the individual in question, rather than the marital status of his parents. The second basic component of our knowledge of the meanings of words and sentences are what the world would have to be like for the utterance of the word or sentence to be true. The first component of meaning mentioned here is referred to as FELICITY CONDITIONS. The second component is known as TRUTH CONDITIONS. On the other hand, as important as these two notions are in developing a theory of meaning or describing meanings in a particular language, there are other fundamental concepts that must be explored: ENTAILMENT; IMPLICATURE; AMBIGUITY; and VAGUENESS. For sentences there is an additional meaning component, discussed in ___, viz., information structure. Because we discuss information structure separately, I want to focus here exclusively on these other concepts just mentioned, all of which are causally implicated in an understanding of sentence or word meaning.

First let us consider the truth conditions of a sentence or word. Arguably the most important component of meaning is found via the truth conditions. To see this, consider the following examples:

(7.51) He ate *all* the fish.

(7.52) He ate *none* of the fish.

How must the world be like to native speakers of English to judge the truth value of each of these examples? Let's first consider how we might go about determining this in a monolingual setting. One idea would be to put some food in front of a helper (not the language teacher), having previously instructed them to eat it all as soon as you give the signal. Then you could repeat the experiment, having them eat *most* but not all of the fish. In the simplest case, the language teacher will accept your description of 'He ate *all* the fish' in the first instance and reject it in the second, where part of the fish was not eaten. A very similar set of scenarios could be created to get at the truth conditions of *none*. On the other hand, it is clearly vital that *much* thought needs to go into testing for truth conditions. The linguist tests for truth conditions using complete, grammatical sentences, to check meanings that are already largely understood. Checking for truth conditions is not a tool for initial field research.

Using a metalanguage, the procedure is easier in some respects. For example, consider how we might probe the truth conditions of 'all' using a metalanguage. We will assume that the same third party eats the fish again. You ask, 'Can I say 'he ate all the fish'? (in the metalanguage) The speaker will presumably then say 'Yes, you can say that he ate all of the fish'. (Again, make sure that all exchanges between linguist and native speaker are complete sentences. It is easy to misinterpret results if partial utterances are given. And partial utterances cannot be appropriately labeled as either

grammatical or ungrammatical.) Further probing will reveal that when the speaker eats only part of the fish, even the major part, that the native speaker will say that 'No, then you cannot say he ate all of the fish, because there is some left.'

To get at felicity conditions, consider the following example from Matthewson (2004, 401):

"(7.53) Situation: There are two cats in the room, and they are both asleep.
The cats are awake. FALSE
The cat is asleep. INFELICITOUS"

How does one distinguish reliably between the falsity of a sentence vs. the infelicity of a sentence? This involves discussion with native speakers, attempts to paraphrase, or, as in (-), changing grammatical marking of some sort or another.

7.8.2. Diagnostics

There are a number of useful diagnostics for the study of meaning in well-known languages. One such test is called the 'wait a minute' test, first introduced by Kai von Fintel.

This test (Kai von Fintel, cited in Matthewson (2004, 34ff)) is useful for distinguishing infelicity from falsity, a crucial distinction for the semantic fieldworker. But like so many other diagnostics, it often does not travel well from one language to another. Consider Matthewson's (2004, 34ff) discussion of this in the box.

"The issue of distinguishing falsity from infelicity is particularly tricky with respect to St'át'imcets and Straits at least, and possibly other Salish languages, because one easy language-internal clue to infelicity due to presupposition failure simply does not work in these languages. The test is the so-called wait-a-minute test, invented by Kai von Fintel (personal communication). Since presuppositions are propositions that discourse participants are assumed to know already at the time of utterance, a presupposition which is not known can be challenged with "wait a minute!" (or another similar expression). On the other hand, an assertion which is news to the hearer cannot be challenged by "wait a minute!" English examples are given in (52) and (53).

(52) Presupposition of stop:

A: Felicia has stopped smoking
B1: Wait a minute! I didn't even know she smoked!
(presupposition unknown)
B2: #Wait a minute! I didn't even know she stopped!
(assertion unknown)

(53) Existence presupposition of the:

A: Barnaby won the semantics prize.
B1: Wait a minute! What semantics prize?
(presupposition unknown)
B2: #Wait a minute! I didn't know he won it!
(assertion unknown)

Unfortunately for researchers working on Salish, for some reason this test does not work. Even items that are difficult to conceive of as lacking presuppositions (such as ‘stop’) do not give rise to a “surprise” response with any consultant tested so far. Even when consultants offer or accept overt denials or questionings of the failed presupposition, as in (54), this does not distinguish presuppositions from assertions (as the English “wait a minute” test crucially does). Exactly parallel denials and questions are judged appropriate for assertions which are not already known, as shown in (55).

- (54) A: plan cukw kw-a-s mán:x-’m s-Bob
 already finish det-impf-nom smoke-mid nom-Bob
 ‘Bob stopped smoking’.
 B1: ?az L:u? kw-’n-s-wá zwát-’n kw-s
 neg just det-1sg.poss-nom-impf know-dir det-nom
 tu? mán:x-’m s-Bob
 past smoke-mid nom-Bob
 ‘I didn’t know Bob smoked’. (presupposition unknown)
 B2: wa? ha tu? mán:x-’m kw-s Bob?
 impf ynq past smoke-mid det-nom Bob
 ‘Did Bob smoke?’ (presupposition unknown)
- (55) A: plan cukw kw-a-s mán:x-’m s-Bob
 already finish det-impf-nom smoke-mid nom-Bob
 ‘Bob stopped smoking’.
 B1: ?ay L:u? kw-’n-s-wá zwát-’n kw-s
 neg just det-1sg.poss-nom.impf know-dir det-nom
 cukw-s
 finish-3sg.poss
 ‘I didn’t know he stopped’. (assertion unknown)
 B2: wa? ha tu? cukw
 impf ynq past finish
 ‘Did he already stop?’ (assertion unknown)

Why the “wait a minute” test should be inapplicable in some languages is at present not known. For fieldworkers investigating a language that does allow “wait a minute” responses, the test is a very useful tool for establishing infelicity."

Again, there is no magic set of crosslinguistic tests that will enable you to make all the distinctions you want to make clearly. But there are numerous suggestions that, when coupled with your own creativity and thought can provide useful inways into the semantics of the language in question.

Let's move to a final area of study, one of the links between semantics and syntax in fact, *information structure*.

7.9. Information structure

Information structure is to me the heart of syntactic analysis. It is the way that language formally distinguishes between relationships among types of information. To use common, though not completely accurate terminology, it is where we distinguish new, contrastive, topical, and related types of propositional relations and content in the language.

In general, as students of language have recognized for centuries, the sentence contains a basic pragmatic division roughly understood as the contrast between old vs. new information. In general prosody (see ___) is used to provide clues as to the location of focus in a sentence. Thus, since the predicate generally carries new information it will likewise tend to bear the primary sentence stress. This can be seen in the examples below, from Lambrecht (1994, ___), in which predicate forms are illustrated from different languages:

(7.54) Predicate focus

Q: What happened to your car?

- A: a. My car/It broke DOWN. English
 b. (La mia macchina) si è ROTTA. Italian
 c. Auto se POKVARIO/POKVARIO se. Croatian
 d. (Ma voiture) elle est en PANNE. French
 e. (Kuruma wa) KOSYOO-si-ta. Japanese

- (7.55) Sentence: *My car broke DOWN.*
 Presupposition: 'Speaker's car is available as a topic for comment x'
 Assertion: 'x = broke down'
 Focus: 'broke down'
 Focus domain: verb plus remaining post-verbal core constituents

The ingredients of information in Lambrecht's theory are the *presupposition*, i.e. what the speaker assumes that the hearer knows; the *assertion*, the telling of what is new; *focus*, the term of the assertion that is what the assertion is about; and the *focus domain*, that structural part of the sentence that contains the focus. In Role and Reference Grammar, this focus domain is known as the *actual focus domain* and a further distinction is made of the *potential focus domain*, i.e. where the language allows actual focus domains to occur.

To get a feel for focus domains in English and other languages, for example, consider the following, extracted from Van Valin's work on these matters in Role and Reference Grammar (and sent to me for use here by Van Valin in email, March 2006):

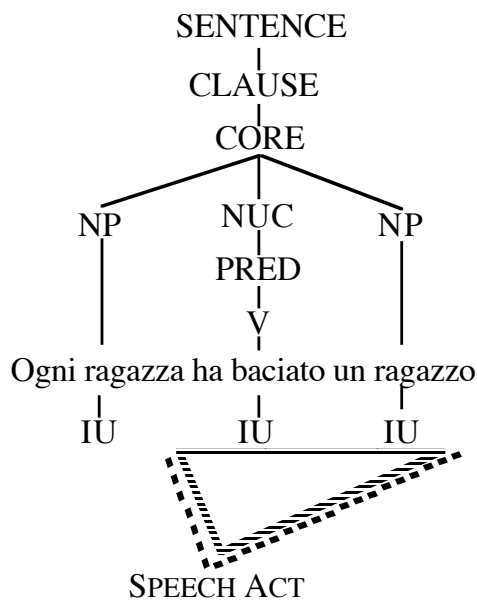
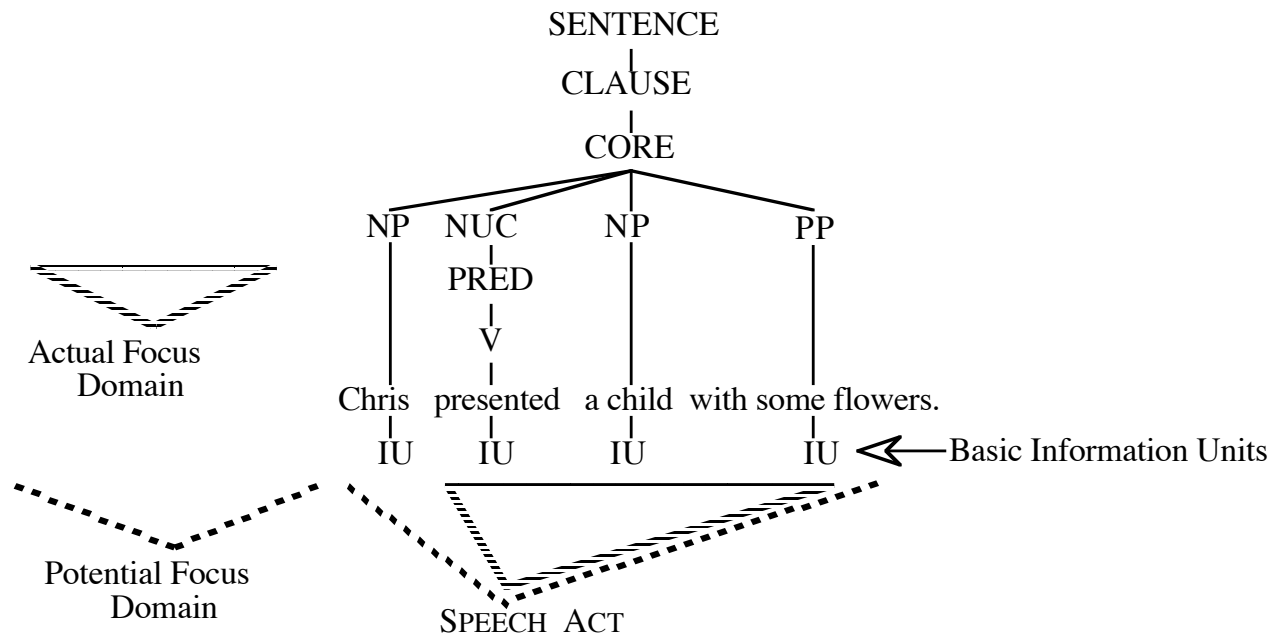
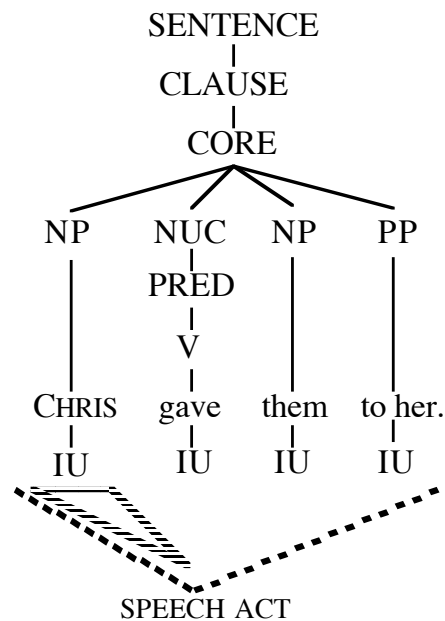


Figure : Predicate focus in Italian
English



Now let us consider other types of focus. If the entire proposition is the assertion, then the focus is sentential. For example:

(7.56) Sentence focus

Q: What happened?

- | | |
|--------------------------------------|----------|
| A: a. My CAR broke down. | English |
| b. Mi si è rotta la MACCHINA. | Italian |
| c. Pokvario mi se AUTO. | Croatian |
| d. J'ai ma VOITURE qui est en PANNE. | French |

e. KURUMA ga KOSYOO-si-ta.	Japanese
(7.57) Sentence:	<i>My CAR broke down.</i>
Presupposition:	none
Assertion:	‘Speaker’s car broke down’
Focus:	‘Speaker’s car broke down’
Focus domain:	Clause

Focus can also take as its scope only a single phrasal constituent:

(7.58) Narrow focus (what Lambrecht calls ‘argument’ focus)	
Q: I heard your motorcycle broke down.	
A: a. My CAR broke down.	English
b. Si è rotta la mia MACCHINA.	Italian (Lit: ‘broke down my car’/
it’s	
È la mia MACCHINA che si è rotta.	my car which broke down’)
c. AUTO mi se pokvario.	Croatian
Pokvario mi se AUTO.	
d. C’est ma VOITURE qui est en panne.	French (‘it is my car which broke down’)
e. KURUMA GA kosyoo-si-ta.	Japanese

- (7.59) Sentence: *My CAR broke down.*
 Presupposition: 'speaker's x broke down'
 Assertion: x = 'car'
 Focus: 'car'

English is relatively remarkable in the degree of flexibility it allows for focus domains:

- (7.60) a. Leslie sent the book to DANA yesterday.
 b. Leslie sent the book to Dana YESTERDAY.
 c. Leslie sent THE BOOK to Dana yesterday.
 d. Leslie SENT the book to Dana yesterday.
 e. LESLIE sent the book to Dana yesterday

As suggested by this example, languages differ (VanValin and LaPolla (1997, ---)) according to the relative rigidity of their focus structures. Some languages allow focus only in narrowly circumscribed syntactic positions, e.g. French and Italian (immediately postverbal position is reserved for focus), whereas other languages allow more flexibility in their potential focus domains, as illustrated in Table __ below:

	Rigid Focus Structure	Flexible Focus Structure
Rigid Syntax	French, Toba Batak	English, Toura
Flexible Syntax	Sesotho, Italian	Russian, Croatian

Table : Typology of the interplay of focus structure and syntax (Van Valin 1999)

Moreover, as Van Valin (1999) has also show, there is an interesting interplay of syntax and morphology with focus structure, as illustrated in (). As () shows, the more marked a particular morphosyntactic object is in a particular information theoretic role, the more phonology it has.

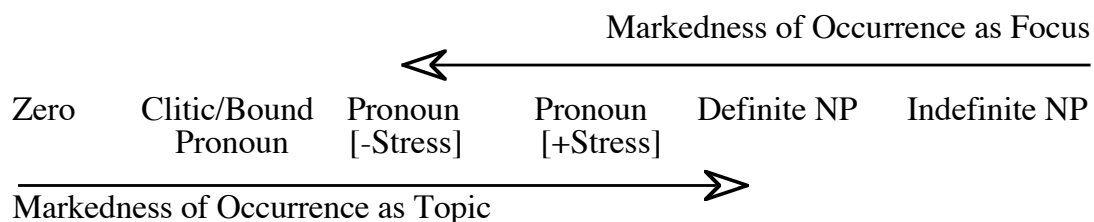


Figure 1: Coding of referents in terms of possible functions

7.10. Argumentation

All syntactic analysis is observation, guessing, belief-formation, and argumentation. Argumentation is understood here as providing warrants for one's own

beliefs while removing warrants from plausible opposing beliefs.⁴⁷ Unfortunately, too many grammars include only [observation + beliefs] or [observation + beliefs + warrants for those beliefs]. And yet, leaving out the final stage, demonstrating a lack of warrant for plausible alternative beliefs, renders a reference grammar or article much less useful to many readers. It is easy to see why. The audience of a reference grammar or professional article will be linguists and specialists from related disciplines. Good readers must be persuaded, not merely informed. They will be constantly thinking of alternative accounts of the facts. And those accounts will have warrants that are only ostensible by your account. You need to say why, in anticipation of these being raised.

Let me give a very simple example. Let's say that you analyze the following sequences as trochaic stress, oriented left-to-right within the word, i.e. 'stress every even-numbered syllable from left-to-right in the word' (where boldface indicates stress).

- (7.62)
- | | | | | | | | | | |
|----|----|-----------|----|-----------|----|-----------|----|-----------|----|
| a. | ba | ba | ba | ba | ba | | | | |
| | 1 | 2 | 3 | 4 | 5 | | | | |
| b. | ba | ba | ba | | | | | | |
| | 1 | 2 | 3 | | | | | | |
| c. | ba | ba | ba | ba | ba | ba | ba | ba | ba |
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |

Your analysis works, quite obviously. But are there other analyses (structural beliefs) you might form about these sequences? One possibility is that the system is instead an iambic system oriented right-to-left, i.e. exactly the mirror-image of the proposed trochaic analysis. Just as obviously, this new proposal works as well for the facts as the trochaic analysis (stress every even-numbered syllable left-to-right). Therefore, parallel to your belief about how stress works in the language, there is a belief that it works in just the opposite fashion. The small amount of data in (7.62) warrants both sets of beliefs. So how to improve the warrant for your belief while removing the warrant for the opposing belief? Well, expand the empirical set under consideration. What does each analysis predict for words with even numbers of syllables (those above have only odd numbers of syllables)? Consider then the further hypothetical examples in (7.63):

- (7.63)
- | | | | | | | |
|----|-----------|----|-----------|----|-----------|----|
| a. | ba | ba | ba | ba | | |
| b. | ba | ba | ba | ba | ba | ba |
| c. | ba | ba | | | | |

If, *ceteris paribus*, words with even numbers of syllables are indeed stressed as shown in (7.63), then the alternative iambic hypothesis fails, its belief is no longer warranted.

Now we need to ask how one comes to have ideas on how to select between alternative hypotheses. Basically, the ability to argue effectively derives from knowing your own analysis well.

⁴⁷ A 'warrant' can be understood informally here as the linguistically ostensible reason for your belief.

Knowing my own analysis well enough to argue for its superiority to plausible alternatives was hard for me in the early days of my career. And the effect was that all of my initial submissions to journals were rejected. It was embarrassing to have referees point out alternative analyses that worked as well or better than my own. I realized that they were able to propose these counteranalyses because they understood my own claims better than I did myself. So I eventually learned to work more slowly and to think and reflect about my own analysis so that I would know its implications at least as well as the referees. This enabled me to discuss the predictions of my analysis and why these were superior to the hypothetical alternative analyses.

To know one's analysis requires that one know well the linguistic subfield in which one is working, including the particular theory or framework one has selected to analyze one's data and present one's research report. This means that the fieldworker must read deeply and widely in linguistic theory, familiarizing themselves with the predictions, constraints, and understanding of the relevant aspects of grammar urged by a particular theory.

A good linguistic argument is based first of all on a good hypothesis and solid knowledge of that hypothesis's predictions and implications. Again, good hypotheses come from knowledge of linguistic theory, knowledge of the language, knowledge of other, especially related, languages, as well as intelligence, aptitude, and luck.

One can approach hypothesis-formation in the field inductively, deductively, or abductively, as in ()-():

(7.64) Inductive methodology

- a. A fact is found. Analyze this fact. What is your understanding of this fact in your theoretical framework and in your larger understanding of the language (etc.)?
- b. Your understanding leads you to believe that future facts encountered in the language will be constrained in specific ways.
- c. You systematize facts according to what you think accounts for them best.

(7.65) Deductive methodology (best exemplar is the syllogism)

- a. A theory is known. The theory constrains the notion of 'possible fact', along a certain range.
- b. Your understanding of the theory leads you to believe that facts encountered will have a certain shape and to so classify the facts you find.
- c. You systematize facts according to your understanding of the theory.

In fieldwork, as in all scientific research, both inductive and deductive hypothesis-formation should be used. Using these as a beginning, it is inevitable that some facts subsequently encountered will not fit the understanding mentioned in () and (). When new facts do not fit our expectations, we are surprised. A great deal of the success of field research is the result of being surprised, i.e. coming up with problems to resolve. But, again, coming up with these problems to resolve comes almost exclusively from the reasoning types in () and () in conjunction with the careful study and knowledge on which they are based.

So when one comes up with a surprising fact, how is it to be dealt with, that is, how is it to be integrated in one's emerging understanding of the language? The American philosopher, Charles Sanders Peirce (1839-1914) suggested a method of reasoning which has become extremely influential in accounts of how scientists reason

about the world. Peirce () labled this new type of reasoning *abduction* or *retroduction*. I summarize this in ():

(7.66) Abduction

- a. You find a surprising fact, A, where by 'surprising' we mean that it fails to conform to or contradicts your predictions.
- b. But if another belief, B, a specific one you have imagined, were held, the A would no longer be surprising.
- c. The ability of B to remove the surprise of A is evidence, C, for B.

Let's consider an example. Consider that you have discovered several clauses where the subject NP precedes the verb which in turn is followed immediately by the object NP. So based on both your knowledge of theory (deduction) and your experience with these examples (induction) you propose ():

- (7.67) Information-theoretic based constituent order hypothesis: old information precedes new information (where subjects carry old information and objects new information).

Next, assume that you find a sentence in which the subject follows the object, e.g. the simple form VOS. You are convinced that in this and other VOS examples, the subject still realizes old or topical information and the object realizes newer or focused information. Therefore, insofar as you believe the hypothesis in (), you are surprised. However, you then have another idea, the hypothesis in ():

- (7.68) Information-theoretic based constituent order hypothesis: new or focused information immediately follows the verb.

Hypothesis () works for both sets of examples. If it were true. then VOS examples would no longer be surprising (because () says nothing about the positioning of the subject NP, only the object NP. Therefore, C, you have reason to believe (), the abductive B.

In a sense, abduction is simply a formalization of guessing. But it is useful because it gives us a clear, explicit visual representation of the nature of our task in hypothesis formation. Peirce (1877) is a principal source on the concept of abduction. Every fieldworker would profit by familiarizing themselves with reasoning models and the notion of warranted belief.

Wood, Linda and Rolf O. Kroger. 2000. *Doing discourse analysis: methods for studying action in talk and text*, London, Sage Publications.

CHAPTER 8. MONOLINGUAL FIELDWORK

8.1. General considerations

Working monolingually can at times be like working on the hieroglyphics of lost languages – except without a Rosetta Stone.

Some researchers (Berg (), Matthewson ()) refer to the importance of a 'metalanguage' in doing linguistics research. That is, a language which is not the target language, but which both the linguist and the language teacher speak. If you have a metalanguage to draw on, many tasks will be easier and perhaps even more effective, e.g. getting translations from the language of study to the metalanguage, examining 'truth conditions' of sentences more easily, etc. Let us refer to situations in which there is a metalanguage available as 'bilingual fieldwork'. If there is no metalanguage available, we can refer to the situation as 'monolingual fieldwork'. In this chapter, I want to provide some suggestions for working monolingually, as well as some reasons that it might actually be superior to bilingual field research for a number of purposes. Just to remind the reader of my philosophy of field methodology, however, I do not believe that there is a 'best' way to do fieldwork. Many ideas and possibilities should be explored, as many methodologies as you can think of or read about should be tested. In fieldwork, the more ideas the better.

I first learned of monolingual fieldwork in 1976, when I took my first course in linguistics from Kenneth Pike, one of the preeminent linguists of the first half of the 20th century. In one of my first linguistic classes, a speaker of a language that Pike had never studied was brought in to the classroom. Pike switched to Mixtec, which the speaker of this other language did not know. Pike then proceeded to speak to the language teacher in Mixtec, showing her a number of natural objects, e.g. fruit, sticks, stones, leaves, etc. and doing things with them, such as dropping them, throwing them, breaking them, using them to hit people, etc. Within 30 minutes he had filled all the available blackboard and overhead projector space with data. He then stopped and thanked the teacher. He proceeded to tell us about the phonemic structure of the language, the grammar (basic word structure and sentence structure), and even was able to classify the language. For a new linguistics student, or even for an old hand, it was most impressive.

To much applause, the speaker left, quite impressed herself.

Since then I myself have done such 'monolingual demonstrations' many times, at the University of Pittsburgh, Presidency College in Madras, India, the University of Campinas, Brazil, the University of Manchester, United Kingdom, and other places. The two that most stand out in my mind, however, are demonstrations that I did at the annual meeting of the Linguistic Society of America, in Manhattan, and a special forum to honor the memory of Kenneth Pike at the University of Michigan. Both of the latter lasted two hours and involved two speakers of the same language in each, man and woman in each, on Kisi and Nepali, respectively, languages I knew absolutely nothing about beforehand. At the LSA meeting, over 110 professional linguists were present, many of whom had more field experience than I did (though not necessarily more monolingual field experience). In fact, there were specialists on the Kisi language in the crowd. In Michigan, there were specialists on Nepali in the audience and Pike's family was present. There is a considerable amount of pressure on you each time you do such a demonstration and many people hope that you will really get something wrong, or at least the possibility that you might adds to the entertainment value and the suspense. But these 'shows', though I consider them legitimate and very important teaching tools, using them now in most of my introductory linguistics classes, do not begin to bring the

linguist under the same amount of pressure to perform as real fieldwork in a community of speakers where they have research objectives crucial to a particular state of her/his career and crucial to the language community (perhaps) or to the relevant funding agency, etc.

Before my demonstration at the LSA meeting, I flew to Dallas, to meet with Ken Pike, not long before he died, as it turned out, and to ask him why he thought monolingual fieldwork was so important. The main reason that stood out to me from our conversations on the subject was that monolingual fieldwork and monolingual demonstrations teach us about language as a holistic experience. They involve making sense out of interdependent facts about communication, e.g. gesture, intonation, body orientation, facial expression, accent, etc. in ways that simple elicitation, discourse studies, or investigating natural corpora, all of these mediated through a metalanguage, simply could never do. Pike's view of the importance of monolingual fieldwork thus follows from his own theory of language as part of human behavior (Pike (1967)).

By being forced to figure out how language, grammar, the body, and the social environment are all integrated in communication, the grammar in a sense (regardless of theoretical perspective) becomes more 'concrete' or more intuitive and more easily learnable. There are other advantages to monolingual fieldwork that I mention directly, but this is very important reason – the 'phenomenology of language'.

There are various reasons why one might work monolingually. Here are some of the more important ones.

First, it could be that you *must* work monolingually. This was my case with the Pirahãs and it has been so for other field researchers. It is very rare today to find an entire people group that speaks no outside language, except for a few phrases and lexemes. But the Pirahã, for important cultural reasons (Everett (2005)) have chosen to not learn Portuguese or any other language and have insisted on remaining monolingual. Therefore, there was no choice for me. Either I conducted monolingual research or I would have been forced to move on to another group. I believe that the field of linguistics has been enriched by what it has learned about the Pirahã language over the years, however, so I am very glad that I made the decision to tough it out and work monolingually with the Pirahãs.

Another reason for working monolingually is that you may want to learn the language better than you might under normal fieldwork conditions, even though you might plan to eventually move to a metalanguage. In this sense, the monolingual method of fieldwork is just a starting point for you.

A reason that some people work monolingually is because they believe that it is superior all around to using a metalanguage, as difficult as it may be for some field researchers to believe. That is, working through texts, semantics, pragmatics, and so on, in only the language of study, is seen by some to give ultimately a better grasp of the language, culture, grammar, and people than the 'cheat' of working through a metalanguage. I do not believe this. But it can be a very well-reasoned position and it is not one to set aside lightly. Usually, it is the view of people (such as missionaries) who have much longer-term goals than the average linguist. But if you want to work intensively to understand one language and people, and are willing to commit many years to the effort, a case can be made that working monolingually is better.

If Pike was right in his own writings and lectures on the subject, a researcher might adopt the monolingual method in order to develop a deeper intuitive grasp of the language. How does monolingual research deepen intuitions about a language? Well, it does this by, as I stated at the outset, summarizing Pike, forcing the researcher to

approach the language, the grammar, and the people holistically, learning all simultaneously. It does this because when there is no metalanguage, the researcher must pay attention to every gesture, every expression, every outsider remark, every response, all nuances of the utterance as communicative and cultural event (see again Everett (2005)) in order to begin to make sense of what is said and begin to make an inroad into the understanding of this new language and culture. To put this in terms that Pike invented, but that have been quite influential, especially in the anthropological literature, the fieldworker is trying to move from an *etic* perspective to an *emic* perspective, i.e. from seeing only the surface, physical character of events and states, to understanding the meaning of what is heard and seen, as someone on the inside of the system (though, realistically, of course, one never is going to become an insider).

Yet another reason for working monolingually is to demonstrate greater respect for the people whose language/grammar you aim to study. This respect emerges as the people see that you are avoiding what may be to them 'languages of domination'. That is, the national language of the country in which they are found, usually the language of the 'conquerors', perhaps still (as with many groups I have worked with) having very negative connotations. Moreover, by forcing yourself to learn the people's language and use it as the (reflexive) medium for studying itself, the field researcher demonstrates very clearly and publicly their willingness to subordinate themselves to these people. One way, of course, that this is shown, is that the linguist willingly subjects her/himself to becoming a laughing stock, at least temporarily during language-learning, as a by-product of a genuine effort to learn, and thus, attribute value the people.

An additional reason for working monolingually that I will give here is one that I have argued for elsewhere (Everett (2005)) but which is admittedly very controversial, namely, that it is not always possible to translate between languages, i.e. that not all languages have the same expressive power. In other words, a metalanguage may simply fail you in two ways: (i) by not having the wherewithal to talk about concepts in the target language and (ii) by misleading the linguist into putting concepts that are in fact not understood in terms of the concepts expressed in another language and culture, which are roughly, but too roughly, equivalent or in fact very different.

Of course, there are reasons why a field researcher might legitimately choose not to work monolingually. These include at least the following. First, time is always limited and it may be that in a particular research project there is simply not time to work monolingually, that the researcher simply must use a metalanguage to get at the data they need in the amount of time available. This is fine. But then, of course, all the other advantages of monolingual field research discussed above will be forfeited. And the fieldworker will have to avoid monolingual communities. There is nothing new at all, though, in the idea that researchers cannot do everything but can only do what they have time to do and nothing more. Another reason to avoid working monolingually is to reduce the risks that the field research will fail to turn up anything useful. This is a legitimate concern. If your objective is detailed semantic analysis then, as Matthewson (2004) makes clear, the absence of a metalanguage could adversely affect your research or, at least, require much more time for the same level of analysis. The benefits of monolingual research in this case could be 'outranked' by the disadvantages of working without a metalanguage towards certain goals, especially semantic ones. There is a risk, then, that working monolingually with these goals could result in spending a lot of time and coming away with nothing to show for it theoretically. I am skeptical that this would happen, but it is certainly a reasonable concern for someone contemplating a fieldwork methodology.

A final reason I will mention here that might lead someone to avoid monolingual field research is when the field situation could require moral, ethical, or political understanding from the outset of the field research. If you are working in an area where people are particularly suspicious of people from your home country, for example, it will likely be to your advantage to use a trade language, the national language, or some other language shared by you and the local community in order to more effectively explain your purposes and to understand the relationship that the community expects with you and the constraints that it expects you to operate under.

Choosing whether to work monolingually, therefore, is a complex decision that will have numerous implications for the fieldworker's research success, trust from the community, overall effectiveness, and, also important, enjoyment of her/his field situation. There is no really 'correct' choice to make. Each person has to make the choice that think best. The important thing is that people do field research.

Now I would like to include three anecdotes, with important lessons to teach the potential fieldworker, from linguists who have worked monolingually. The first anecdote is from my own experience.⁴⁸

In early December 1977, when I was but a lad of 26, I flew in a Cessna 206 aircraft (which I fondly refer to as the 'pukemobile', given my problems with motion sickness) for one hour and forty-five minutes Northeast of Porto Velho, Brazil, to make my initial contact with the Pirahãs. Before I got on the missionary plane that was to fly me to the village, I was full of thoughts about whether I had indeed chosen a good way to spend this time of my life. Was I really up to the task? I had heard about the Pirahãs. In fact, SIL had asked me to work with the Pirahãs because their language had stymied two previous SIL teams and I had done very well in my linguistics training. As the plane took off and we were flying low over the Amazon jungle (so much of it has been cut down since that flight), my concerns intensified, aggravated by my growing motion sickness. What would the people think of me? What would I think of them? Would the monolingual methodology Pike urged me actually work in real life, apart from the 'shows' I had seen in classrooms? (At this time I had not done any such 'shows' myself.)

Within the first thirty minutes I was feeling queasy. By the end of the first hour, I was trying to find a happy place and not look out the window. Then suddenly, the pilot offered me a tuna sandwich with onions. I had to talk and turn off my olfactory system simultaneously. Half an hour later we were circling the Pirahã village, where the previous SIL team had, at great expense and effort, built an airstrip (at the cost of about 1,500 trees or so). Just as I was beginning to hope that we would crash, anything to relieve my misery, we touched down and came to a quick stop at the end of the airstrip. We turned around and taxied back the 450 meters or so to the village path where a large number of Pirahãs were waiting, yelling, and gesticulating. I was happy not to have thrown up on the plane, though the temptation to do so was still present. Out of the high altitude and cool air above the Amazon, my sickness was compounded by a temperature of approximately 90 degrees (Fahrenheit) and humidity of about 99%. The plane stopped and the door opened. I got out. I didn't yet speak either Pirahã or Portuguese, so I was literally speechless. As the pilot and two friends of his who had flown along started to look around, I heard the pilot say that this was the weirdest place

⁴⁸ I also used these anecdotes in Everett (), from which this chapter borrows heavily and freely.

he ever flew into. Walking along the path to the village, overgrown with grass up to my knees and with river water up to my mid-calf, I smelled hair being singed. My head was throbbing, I was perspiring heavily, and I was thinking that in a few minutes the plane was going to leave me there. (One of the passengers who had visited the Pirahãs several times, but spoke none of the language, had offered to spend the next ten days – the length of this first, exploratory stay – with me. So I would not be completely alone.) The Pirahãs were clearly asking me things, but I was paying little attention, just thinking that this language sounded like a greater challenge than my brains were up to. The fellow singeing the hair gestured for me to come over and have a look. He squatted by a fire, in the sun, with no shade, and had a large rodent (a paca, I later learned) that he had just thrown whole onto the fire. Blood was coming out of its mouth, dripping from its protruding tongue, and the smoke from its hair was, let us say, pungent. I just managed to control my gag reflex. But now I was beginning to recover. I remembered that I had a notebook and a pen. I pulled out my notebook and pointed to the animal. He looked at it and said something. I wrote it down and said it back. He smiled and everyone else seemed pleased. So then I tried to refer to the whole process, the smoke, the fire, the animal, in an effort to get 'singeing the hair'. He said something back. This time I didn't bother to try to write it. It exceeded my short-term memory's capacity for strange syllables. I stopped there in the hot sun and picked up a stick. I got the word for stick, repeated by six or so Pirahã onlookers. Then I let the stick drop to the ground and got that phrase. And so on. Within an hour after beginning my own private monolingual demonstration, I was pretty sure that the language had only three or four vowels and a small number of consonants, some very strange sounds among the latter, and that it had two or three tones. I had also learned about twenty words. Over the next ten days, I learned a number of expressions, none of them particularly useful for normal conversation, but was coming to think that this job might actually be doable. I had promised myself not to read anything about the Pirahã language until I tried to figure some things out for myself first (this was not a good move and I strongly recommend against this – you should read everything you can on the language before ever going to the community). When I emerged from these ten days and started to read what Arlo Heinrichs and Steven Sheldon had written on the Pirahã language, I was pleased to learn that my ideas formed in those ten days were not so bizarre. This was going to take a long time, but I could do it.

When I first went to the village of the Banawa people (Arawan), my expectations for the first day were based, naturally enough, on my previous experience with the Pirahãs. But when I got off the plane, I knew things were very different. The jungle, the heat, humidity, and sounds were roughly the same. Yet men came up to me and addressed me in very good Portuguese. By the next morning, I was working with Sabatao Banawa, perhaps the best language teacher I have ever had. Though he had almost no formal schooling, he not only gave me very natural texts, but as we went through them to translate them together, his comments went something like this: 'This word means that the words here were spoken long ago, by a woman. This part of the word means that the one who is speaking is not sure. This part means that the pig was on a log, just above the ground.' And so forth. In other words, Sabatao was able to give me nearly morpheme-by-morpheme glosses. In my first three weeks among the Banawas, I felt I had learned more about their language than in my first six months with the Pirahãs. So even within a similar geographical area, field conditions can be radically different.

Let's turn to another part of the world now, for a final anecdote. This one comes from Loving (1975, 268) about her experience at the beginning of her career, working monolingually with the Awas of New Guinea.

"We were especially on the lookout to learn to say 'What is this?' After two weeks we were tired of pointing and we wondered if the Awas were not equally tired of seeing us point. Evidently they were not, for they continued to be gracious enough to give us new words as we continued to point. One day, we were cooking some greens around an open fire. I pointed to the food, directing my 'question' to an elderly man standing around looking into the pot. He turned to the man next to him and said 'anepomo'. I repeated this thinking this was the name of the greens. He and several others smiled and then leaning towards me, he said 'tura'..."

What Loving had learned here was not what she had asked, but something much better – the precious phrase, 'What is this'. It is difficult to overemphasize how important this phrase is in learning a language. A companion phrase 'What is it/she/he doing?' is also extremely important to learn and Loving's anecdote provides a useful clue as to how both phrases could be learned in a monolingual field situation.

OK. We have seen some stories of how other people initiated their monolingual field research. Now it is time to move on to consider the methodology in more detail.

8.2. The *gavagai* residue

It is crucial from the outset here to remember that Postal's Maxims outrank any method. Moreover, it is important to recognize that all suggested methods for scientific research are heuristic procedures, not algorithms. Unlike baking a cake, following a set of methodological suggestions in science will not always give the same results. There are too many variables. The best that one can hope for is that the methods will be very useful. But the *art of fieldwork* (see Walcott ()) results from the fact that it is the individual fieldworker's resourcefulness, resilience, and creativity that will also be more responsible for success than any set of automated procedures. With these caveats, I turn to outline a method that has worked for me in my own monolingual field research.

Since this method involves beginning with simple words and working your way 'up' the grammar, progress assumes that you are actually understanding what they people are telling you. That is, it assumes there is a reasonable semantic match between the linguist's translation and the teacher's intended meanings. First, begin with individual words. Speakers will, in every language and culture I have ever heard about, almost immediately recognize when you indicate an object that you want the name of the object and they will give it to you. On the other hand, monolingual field research is always haunted by the spectre of Quine's (1960) 'indeterminacy of translation' problem, brought out most clearly in his famous *gavagai* example. The fact that this issue does not seem to be a tremendous problem in actual field experience is confidence-inspiring, yet it should still be sobering to fieldworkers, because it does arise. Moreover, the problem is such that it can arise without anyone noticing at first.

Summarizing loosely, here is the famous *gavagai* problem and how it emerges from Quine's philosophy. This problem is extremely relevant to fieldwork, because speakers may not always give the answers expected. How does the field linguist recover from bad information? Is it even possible to do so?

In *Word and Object* (1960) and *Ontological Relativism* (1977), Quine considers, for its philosophical lessons, a hypothetical problem for field linguists attempting to translate a hitherto unstudied language that the linguist does not speak. Here is how the problem goes:

One asks the question of a native speaker, by gesture or by metalanguage, 'What is that?', pointing, say, at a rabbit or some other object, and the native speaker utters *gavagai*. How can the linguist match the utterance of *gavagai* to a specific meaning? Does it mean 'rabbit'? Does it mean 'rabbit fur'? Does it mean 'token of a platonic rabbit type'? And so forth. Even if the native speaker were to mean 'rabbit', the linguist might never encounter a crucial counterexample to doubt their translation (unless they look hard, which they only would do if they had reason to doubt their initial translation which, as we have just seen, they would not have). How could you tell if you have the right meaning? Quine answers that in principle you could not know and therefore there is an 'indeterminacy of translation' problem. To some degree, this problem is a real and sobering one for the fieldworker. And it is never completely solvable. However, there is a bright side to the issue.

First, the *gavagai* problem never happens in exactly this way in fieldwork. Native speakers, however naive linguistically, will almost always give you the name of an object as a whole, e.g. 'rabbit', 'tree', etc. and will not intend by their answers parts or salient features of objects.⁴⁹ Of course, Quine would counter that you cannot ascertain the exact meaning of an elicited word, in principle. But the fieldworker's reply is that in the course of grammar writing and analysis of the language wrong interpretations 'come out in the wash', especially following the methodology of this book. That is, the issue might be important for philosophers, but it has perhaps no significant impact on fieldwork.

On the other hand, in a monolingual field situation one can never be sure, initially at least, that one has gotten the answer that one thinks they have gotten. Some 'false' answers can be easily discovered, e.g. when one points at a tree and the speaker, focusing on what is pointing, rather than what is being pointed at, says 'finger', instead of 'tree'. Other meanings are not so easy to determine in any case, e.g. verbal mood, aspect, etc.

Overall, of course, it is almost certain that no matter how long a fieldworker conducts research on a given language, there will be many words where the meaning the linguist has recorded is not quite right. And the same goes for many grammatical constructions. Some of these will, as I said, be resolved in the course of the grammar writing. Other errors will be recognized and resolved as the linguist learns to speak the language better. On the other hand, some mistranslations will never be recognized. There will always be a 'gavagai residue' in fieldwork between any two languages.⁵⁰

It is simply vital for the linguist working monolingually (and, to a lesser degree, linguists working with a metalanguage) to always remember Quine's lesson, namely,

⁴⁹ What does this tell us about human nature? Well, as Searle (to appear) notes, *Homo sapiens* share a strong, evolutionary attention to objects. I doubt that anyone could function well thinking of 'rabbit essences' when they saw a rabbit in most circumstances, instead of just 'rabbit', since only the latter is edible.

⁵⁰ QUOTE Grace on indeterminacy of translation.

that you might not be getting what you think you are getting. For this reason, Postal's Maxims and careful reasoning, checking, and re-checking become ever more important.

Let's consider some suggestions for monolingual fieldwork that might enable you to avoid or at least not be completely misled by the gavagai problem. The procedure below is very simply. Clearly the resourcefulness of the linguist is the main ingredient.

I usually begin my monolingual elicitation, whether in a monolingual community or during a demonstration, with objects from nature. So pick up a leaf, stick, rock, or some such to begin with. Get what you think its name is by pointing at it and saying what it is in your language. Don't just grunt. Use your language freely. This is natural. Then repeat back what you were given for correction. Now say the word again as you let the rock, whatever, drop to the ground. Write down (don't use a taperecorder yet. Just you and your ears and paper) what you get. Now say it back again. Now pick up another object of the same type and roughly same size and color. Show two fingers while letting both drop. Imitate. Write this down. Did the form of the verb change? Can you recognize any differences in the form of the nouns or noun phrase? You likely now have a form for plural or dual. Now pick up two other objects (same type, size, color) and repeat the process from the beginning. Now do it all over again with three, then four objects. You should be getting numerals and grammatical number, articles, etc. Always build up slowly, so that you can feel in control of what you are getting. Look at how much you are learning! And this is all with just a few natural objects and a single verb.

Now work with colors, sizes, and conjunctions (which you can get by mixing the object types, e.g. 'a rock and a stick fell to the ground'). After exploring these aspects of noun phrases for a bit, you can try some transitive verbs. Begin perhaps by having your language teacher hit you. You can do this by taking his hand (if appropriate) and hitting it lightly on your shoulder. Then you pretend to hit him. Work with this for a while until you feel fairly secure that you are getting a transitive construction. Now take a biggish stick. Hit yourself with this. Now pretend to hit your language teacher with it. Now have him hit you with it. Now have him hit someone else. For every single action, get a description by your language teacher. Repeat after him and make sure that you watch reactions. It is very easy to confuse 'I' and 'you' in these circumstances. But as you repeat as you perform the action, if you describe what you are doing with the wrong pronoun, you will almost certainly be corrected. But if you don't repeat and make sure you are following, you could easily confuse the pronouns and confuse yourself for a while.

You are prepared, if you haven't already gotten this, to get the paradigm for pronouns (at least those corresponding to interlocutors in your environment). You can get at this or add to your knowledge by now switching to intransitive verbs, e.g. 'jump', 'stand up', 'walk away', 'crouch down', 'sit down', and so on.

These ideas should be enough for you to start your monolingual fieldwork well! And it is so much fun! You are learning things no one maybe ever has. And it is all your brain and creativity and the friendship you are building with your language teachers that is responsible for this! Organize your future sessions. Each day you will be building, making huge steps in your understanding. Work simultaneously on vocabulary memorization and greater fluency. Your comprehension, as is usual, will exceed your ability to produce. But it will all come, slowly but eventually and surely. Just takes a lot of hard work. But no secret to that.

From these data, that you should be collecting for two-three hours per day, you will begin your phonetic, phonological, morphological, semantic, and syntactic

analyses. After perhaps 12 weeks of hard work of this type, and accompanying language-learning, you should be in a position to begin collection of simple texts, following the methodology of chapter ____.

Finally, let's review the implications of the fact that working in a monolingual situation there is no 'metalanguage' (Matthewson (2004)) to use. So how does one come to understand verbal or other meanings? One thing is certain – you cannot tease apart or even discover the full range of verb meanings (including affixal meanings), e.g. tense, aspect, valence, aktionsart, argument structure, case, etc. simply going through texts. In a monolingual situation you cannot get translations for texts, for one thing. The best you can hope for are paraphrases. But since analysis of verbs requires subtle and accurate distinctions of verb meanings, you must come up with a method to help you do this.

Here are some suggestions:

First, act out scenarios. Let's say you're following a guide, e.g. the Lingua Descriptive Questionnaire, and you want to try to distinguish directional actions or to see whether such distinctions are made on the verb in the language. Try this. Assemble a few objects from the local environment, e.g. sticks, rocks, leaves, bones, tools, necklaces, etc., things that are all in regular use and seen as normal objects that one would handle within the community. Next, begin work initially with individual teachers, moving later to work with multiple teachers (see ____). It is important to use culturally relevant objects and tasks at the beginning of the research to build on people's confidence in their knowledge of their culture (there is so much opportunity for uncertainty on all sides in any case).

So take, say, a necklace, some beads, and the string (or whatever) used to make necklaces. Place the necklace on yourself. Get the description of what you did. *Record and film all of this*. Place the necklace on your language teacher, on a child, on a man, on a woman, etc. That is, as you have time and opportunity in this session, try to act out paradigms. (Always be careful to obey cultural constraints.)

Place the necklace on the ground. Place it on a table. Hang it from the roof. Drop it. Throw it. Toss it up and catch it. Take the beads and place one on the string. String several in succession. Let one bead fall off the string to the ground. Let another fall off onto a table or chair. Let all of them fall off on the table, chair, ground, etc. simultaneously. Get various (3-6) speakers to describe each one of your attempts.

Filming and recording all of this will give you good data, help you find directionals, numbers, positions, aspect, valency, transitivity, etc. And this is just a trivial example. Tremendous amounts of high-quality data can be collected in this way. But you need a plan. There are various questionnaires and aids to fieldworkers (see the LDQ in appendix ____ or visit the website for the Max Planck Institute for Evolutionary Anthropology: <http://lingweb.eva.mpg.de/fieldtools/tools.htm>).

You can also use films, of the type prepared by the Max Planck Institute for Psycholinguistics in Nijmegen. Using films, rather than acting things out, has both advantages and severe disadvantages. The advantages include the fact that film can do things you cannot (like make a bowl of beans suddenly appear on a table where there was nothing before).

There are, however, a number of disadvantages to using films instead of acting out scenes for elicitation. These are largely cultural. First, some language teachers may have a difficult time following two-dimensional electronic images. Second, they may not recognize the gender of the person on the film (many peoples consider long hair a sign of femininity or flat-chestedness a sign of masculinity, to cite two problems I have

encountered) and this is surprisingly distracting. Finally, many of the activities make little sense to some language teachers watching the films. Therefore, though I certainly recommend that films be used in elicitation, as well as other media, where applicable, they must be used very judiciously.

Say it back to several speakers.

Many times I have received crucial corrections on my third or fourth repetition to different native speakers. Inexperienced linguistic teachers often find it hard to know what the linguist is after (so does the linguist at times!) and find it even harder at times to correct the linguist (who is, after all, paying their salary in many cases and should have the 'right' to say whatever they want, or at least this is the impression I get sometimes). So checking the data with speakers other than the one who provided the original data is crucial to processing the data. Get these other speakers to paraphrase what the previous speaker said, as well as saying it back identically. This way, you have pronunciations of the same data by different speakers, as well as various alternative syntactic expressions of the same or similar (usually, but not always) content. If you can work via a metalanguage, ask each speaker for a translation. But in all cases, make sure you get paraphrases of difficult to understand data. Almost always, translations and paraphrases will vary from the original's meaning in subtle, yet significant and revealing ways.

If you overhear something and speak no language in common with the speech community, describe the context in a few lines next to the transcription (e.g. time of day, who was speaking, what they were doing, what you think they were saying, etc.). Then test your understanding by trying to use the expression, as you understand it, when and where you think it would be appropriate to do so. Almost certainly you will discover that your initial guess was wrong (and if it wasn't, congratulations, but you might not know one way or the other at this stage anyway) and people will laugh at you. (I am laughing at you now just thinking about the mistakes you are going to make. Ha Ha.)

Generating Paradigms

A word about paradigms: lists or theoretical constructs? Wari' (Everett 2005)

The next thing you must do is to take the expressions you want to study and generate paradigms from them. Let's take our **squeat** example from the outset of the morphology chapter. What does it mean? Well, to get at this we write its context as uttered by someone, say, as they rise to leave, about 12ish, and as they walk towards the cafeteria. I ask someone else to paraphrase it and they say 'he's hungry and he thinks you are too'. Then I ask someone to say the phrase slowly and they something like **Let's go eat**. I ask someone else to repeat this and they also say **Let's go eat** or, perhaps, **Let us go eat**. Now I can try to build some paradigms from this expression. To do this, I divide the utterance into positions or 'slots' and try to put other words I have learned in each of these slots. An example of what someone studying English for the first might do is given in (8.1):

- (8.1) a. Let us go eat.
b. Try us go eat.
c. Make us go eat.
d. Help us go eat.

- e. See us go eat.
 - f. Believe us go eat.
 - g. Run us go eat.
- (8.2) a. Let me go eat.
 b. Let you go eat.
 c. Let him go eat.
 d. Let them go eat. etc.
- (8.3) a. Let us do eat.
 b. Let us try eat.
 c. Let us make eat.
 d. Let us can eat.
 e. Let us want eat.
 f. Let us be eat. etc.
- (8.4) a. Let us go run.
 b. Let us go fish.
 c. Let us go please.
 d. Let us go work. etc.

Next, I work through all of these, asking whether each is acceptable/pretty/crooked/etc. and how they compare to one another, i.e. which would the native speaker think that they are most likely to use. Never be satisfied with an answer that simply says a form is 'good'. Get the native speaker to say it themselves. If they will not, then the form is almost certainly not 'good'. If they do, do they say it with natural speed and intonation and a solid sense that they understand it?

We can now ask what the linguist is supposed to learn from these paradigms? First, they learn that the verbs that can go in the first slot are limited. **Try** and **believe**, for example, are not allowed. Why not? What makes these verbs different from **let**, **make**, and the others? Let's say you hypothesize that **try** and **believe** are excluded because they are interpreted as second-person imperatives by native speakers. How could you test this hypothesis? That is, what data would you need to refute it, do you have such data, and is it refuted?

The point, of course, is not whether this initial hypothesis is right or wrong. All your initial hypotheses will likely be off to one degree or another. The point is that by developing these paradigms, you generate grammaticality differences and different hypotheses about the grammatical structure of the language, a way of implementing Postal's Maxims.

The development of paradigms must be restricted so that the initial data used to generate them come from *natural texts*, not elicitation. Using elicited data, which itself can be artificial, can *propagate artificiality* throughout the examples.

Next, test each paradigm with several speakers. If you see little or no disagreement in judgements between speakers, then you can move much more quickly to the next paradigm. If you do encounter disagreements (say, for example, that some speakers allow **try** and **believe** in the sample paradigms above) then ask them about this, howbeit *indirectly*, never directly. For example, what does the controversial example mean to the speakers who claim that it is acceptable to them? Can they be

gotten to say it with intelligibility? What does the same example mean to those who reject it?

Now let's assume that you have worked your way through a paradigm and that you now believe that you have a reasonable understanding of what a morpheme means. What next? One example of what to do next comes from the beautiful study by Ivan Lowe (), in which he shows how to 'track' the morpheme through texts, as reported above.

Can the morpheme be isolated? In fairly mechanical terms, does the same sequence of segments occur in different positions in the utterance? Can it occur with other segments, e.g. in answer to a question? Does this sequence correlate with a constant meaning in the utterances in which it occurs? Consider in this regard another example, from English:

(8.5) They were running but stopped suddenly.

(8.6) When I am done eating, I'll talk to you.

(8.7) I would rather be playing my guitar.

(8.8) Q: Are you done?

A: *No, I am **ing**.

Does it appear in some parts of texts more than other parts (e.g. introduction vs. conclusion; denouement vs. setting vs. build up, main theme 'line' vs subsidiary information lines, etc.)?

Deliberately show people things from your baggage that you do not think that they will have had previous experience with. If they allow it, you should have a video camera, preferably, or an audio recorder (keep it running as you unpack, settle, etc, and keep it pointed at the people speaking). Listen as you unpack, try to imitate, go back over your tapes later and try to figure out what was being said. Test your hypotheses by trying out phrases, based on your understanding of what they mean, with the people. Do you see or hear question-like behavior? Look for things like hand-gestures, eyebrow-raising, intonational changes, and question-like actions that might provide clues. Do any of these seem to be focused on potentially novel items among your possessions? In particular, you after things like 'What is that?', 'What is he doing?', etc. These are vitally useful phrases for your research, for getting along with the people, for negotiating your way through the community and the language. These are not usually easily to get by direct elicitation, yet they are uttered spontaneously in exactly the kind of situation your initial arrival in the community will create. Pay attention. Linguistically, such phrases are vital even if the linguist otherwise plans to work bilingually (i.e. using a trade language for lab work).

CHAPTER 9. INTEGRATING ETHNOGRAPHY AND FIELD LINGUISTICS

9.1. The neglect of ethnography

In this chapter I discuss ways in which culture and language can interact that go beyond standard sociolinguistics and in which the ethnography of communication can affect the formal grammar of the language, as per the Boas quote in __ above. The point is that the linguistic field researcher must be an observer of culture, not just language, and she must keep a careful record of the connection between cultural contexts and uses of different constructions. Let's get a feel for what is at stake here by considering a classic example from Sapir:

Sapir (1921, 172) writes of the need to understand the 'genius' of each language. By this Sapir refers to that which makes each language unique, the essential core of a language, that part less subject to historical change (a sort of Heraclitus-inspired question of what changes vs. what remains). Judging by his intellectual output, Sapir was always concerned with what Everett (2004) refers to as 'coherent fieldwork', i.e. fieldwork that integrates specific phenomena in the larger cultural context and the background of the researcher. Sapir's concern was with difference, the *relative* value of a given language, as opposed to seeing it merely as an exemplar of one variant of an absolute Universal Grammar. One good example of what I mean is found in a study he undertook of Nuuchah-Nulth (then known as Nootka, Wakashan, Canada) consonant alternations. In this language, as Sapir (1915, 181) observes, there are extremely interesting consonantal alternations that cannot be explained grammar-internally.:

"It is possible and often customary in Nootka to imply in speech some physical characteristic of the person addressed or spoken of, partly by means of suffixed elements, partly by means of 'consonantal play'. Consonantal play consists either in altering certain consonants of a word, in this case sibilants, to other consonants that are phonetically related to them, or in inserting meaningless consonants or consonant clusters in the body of the word. The physical classes indicated by these methods are children, unusually fat or heavy people, unusually short adults, those suffering from some defect of the eye, hunchbacks, those that are lame, left-handed persons, and circumcised males."

Sapir exemplifies this 'consonantal play', concluding that to understand the grammar of a language, we must therefore understand the culture in which that grammar is found. Sapir's study of Nuuchah-Nulth is well-known, of course. This is the kind of study that illustrates the solid connection between culture and language, though it is only a single connection between one rule of phonology and culture. Perhaps it has failed to exert modern influence because it is considered to be a marginal example.

But the main reason that Sapir's and others' studies of these connections have failed to influence theoretical grammar as they might have because in the mid-50s, as discussed in chapter 1, linguistics underwent a conceptual change, from a discipline concerned with documenting, describing, and explaining observed language behaviors in terms of linguistic and social structures and dynamics (e.g. variation and change), to one in which the primary focus was on inferred mental structures and abstract principles supposedly common to all human languages. This is a priori neither good nor bad, simply a change of interest. The change, as stated in __ above, has produced some of the most exciting scientific results in the history of the study of language.

However, *a posteriori*, there have also been nontrivial implications to this change in focus. One is a reduced emphasis on fieldwork as the natural activity of linguists. But another is the near complete lack of attention given to culture and its relation to language, a full rejection in practice of the views of Boas and Sapir cited above. And with this change has come a loss of any truly evolutionary perspective on language, in particular, a loss of the idea that individual languages might follow different evolutionary paths, adapting to different cultural-social niches.

9.2. Ethnography, Cognition, and Grammar

The purpose of this chapter is, therefore, to equip the fieldworker for beginning field research on the culture-language nexus. As a conceptual point of departure, let's consider Table 1 below, which lays out some of the potential connections between language, cognition, and culture, all of which are ultimately necessary to an understanding of language (which one could, but need not, equate with Chomsky's () E-language) and grammar (Chomsky's () I-language):

Table 1

CONSTRAINT RELATIONSHIP	SAMPLE RESEARCH PROGRAMME
1. <i>cognition</i> → <i>grammar</i>	Chomsky's Universal Grammar
2. <i>grammar</i> → <i>cognition</i>	Linguistic Relativity (Whorf, Lucy, etc.)
3. <i>cognition</i> → <i>culture</i>	Berlin & Kay (1969) on colour terms
4. <i>grammar</i> → <i>culture</i>	Urban's (1991) work on discourse-centred culture
5. <i>culture</i> → <i>cognition</i>	Anthropological research on semantic fields
6. <i>culture</i> → <i>grammar</i>	ETHNOGRAMMAR; individual forms structured by culture (e.g. evidentials or Sapir 1915)

This table is clearly inadequate because, among other things, it is two-dimensional. A three-dimensional display would indicate connections between all three domains (cognition, culture, and grammar) simultaneously. Nevertheless, it provides a useful basis for discussion, so let's consider each of the rows in Table 1 in turn.

We can situate a good deal of modern linguistics and anthropology relative to this table. For example, simplifying tremendously, the shift in linguistic research focus that Chomsky was rightfully credited with moving the focus from Cell 6 to Cell 1. And the questions raised by the relationship in Row 1 are indeed fascinating. How *does* cognition constrain language (whereby 'cognition' in Chomskyan theory has traditionally focused on the properties required of the initial state of the part of the mind responsible for language-learning). This is a vital question, obviously.

Row 2 highlights research which examines how linguistic structures, lexical structures, constructions, etc. can affect thinking. The idea is old (much older than Whorf, for example) and in some senses obviously true. There are many tasks we engage in that are affected by language, as numerous researchers have shown. Nevertheless, I think that the usefulness of linguistic relativity has been overstated in much current research. Research on this relationship is exemplified today in the work by Lucy (), Levinson (), and their colleagues, among others.

Row 3 again interprets 'cognition' as static, the built-in mental parameters for distinguishing colors which affect how cultures categorize their world. So the way that cultures classify and identify the colors of the world around them is seriously constrained by color-perception biology, i.e. static cognition (what is known or given in the brain and that underlies behavior).

Row 4 indicates the research on the effects of language on cultural values. So, for example, Urban (1991, --) has argued that a high percentage of passives in the discourse of a language, themselves resulting from independent structural constraints, can affect the cultural concept of 'hero', producing heroes that are better known for what happens to them (they are the Undergoer subjects of passive clauses) rather than for what they do (if they were instead Actors of active clauses). This is fascinating research that linguists by and large show no interest in – a result, in my opinion, of the shift in interest in most linguistic training and research since the mid-50s.

Row 5 indexes research on how cultural values can affect how members of the culture think about the world. An example of this kind of research can be found in Seeger () and related research where, say, types of body adornment are at once a reflection of cultural concepts (communication in Seeger's () ground-breaking study of Suya/Kisedje)) and how members of society come to value or use the relevant body parts (ears, lips, tongue, etc) in communication. Another example is Bradd Shore's excellent 1996 study, *Culture in mind: cognition, culture, and the problem of meaning*.

Row 6 indicates the kind of research that I have been doing for the past few years, namely, the effect that the culture can exercise on grammar. My interest is in whether culture can exercise an architectonic effect on the grammar as a whole (and I conclude that it can, Everett (2005)). A different, but very important, approach to this problem is found in Enfield (2003), Sapir (1915), and other studies where convincing evidence is adduced that culture is responsible for particular structures, constraints, or rules in a grammar.

Rows 3, 4, and 6 are areas of potentially rewarding research by the linguistic fieldworker with a background in ethnology. But these of course do not exhaust the possible domains of research in linguistics, they simply list some salient interdisciplinary connections.

9.3. Content of an ethnography of communication

Perhaps the single most important broad topic of relevance for linguistic-cultural connections is the 'ethnography of communication'. References in this regard include Hymes (1974), Saville-Troike (2003), Gumperz (1986), and various others. Specific research topics in the ethnography of communication are many, but some of the more prominent ones are:

(9.1) Topics in the ethnography of communication (see Saville-Troike 1982, 51ff)

- a. Communication and social structure
- b. Language and culture
- c. Rituals and communication
- d. Phatic language and cultural relationships
- e. Varieties of language
 - Registers
 - Social and regional dialects
 - Different languages
 - Channels of communication

Let's consider these briefly, as examples of some of the kinds of connections between language and culture have been shown to be productively studied by linguists interested in the causal-connection between the two. First, under communication and social structure we may consider such topics as the nature and types of discourses associated with different social roles (e.g. those in political power, those without power, those with specialized roles, e.g. shaman, physician, pastor, official spokesperson, etc.). A simpler way of putting this issue is 'Who says what and when do they say it?' This can impinge on studies of pragmatics, semantics, and even morphology and syntax (use of different moods, evidentiality markers, etc. according to type of discourse and discourse utterer), but clearly to study these manifestations of language requires cultural observation and understanding. Ultimately, the ethnography of communication, like all studies involving a member of one group studying members of another group, involves attempting to move from an *Etic* perspective to an *Emic* perspective (Pike (1967,); see also Headland ()). That is, if we understand a particular ethnography of communication we shift from the perspective of the outsider to an understanding of the perspective of those within the system, group, culture, etc. Item (--b), language and culture, is a more specialized type of study, one that I have come to refer to as Ethnogrammar, and I offer a case study of such a study from Everett (2005) in ____ below. Other areas of () are less difficult to see the point of, so let us close this introductory discussion by considering what it means to study 'varieties of language'. Recall the accounts in chapter ____ above about my monolingual demonstrations with speakers of Kisi and Nepali. Both revealed the need to understand varieties of language. In the Kisi demonstration I conducted at the annual meeting of the Linguistic Society of America in (), I noticed that most of the word endings from the female language teacher differed from those of the male language teacher. During the demonstration I noticed this, but of course I had no way of knowing why the forms of her words were different. It might have been a women's dialect; it might have been social register (a woman addressing an unknown and older male – me), it might have been something else. It turned out to be something else – Kisi was not her native language! She simply didn't know the endings. That can be a trivial observation, but it requires understanding of who is who in the community and under what circumstances people learn or use different languages. In the Nepali case, the differences in the woman's responses to my questions, as opposed to the male Nepali language teacher, were in fact a matter of register. I was older and a stranger, so she used the formal register with me. To understand different choices of languages and registers (and by 'understand' I mean more than simply observing that they exist as I have done here) requires observation of the culture. The importance of understanding different varieties of language in analyzing the grammar of a given language can perhaps best be seen via a case study of discourse channels in Pirahã, to which I now turn.

As pointed out in Everett (1982) Pirahã phonology cannot be fully described or understood without a knowledge of how it interacts with culture. There are other examples from Pirahã phonology. Let me present two of the strongest, in ascending order of importance for coherence.

Imagine that a language could have various systems/modalities of sound structure, beyond its phonetics and phonology. And then consider the possibility that one modality can affect another, but not necessarily via constraint-rankings or rules, the standard devices of phonological theory proper. If so, then to understand the sound system of language, *L*, at any level (e.g. 'what happens' or 'what native speakers know

when they know the sound system of their language') we must look carefully at the modalities of expression made available via an ethnography of communication and not merely at a supposed universal formal apparatus. Corollaries of this scenario might include, e.g. the appearance of new roles for old constraints (e.g. mode-faithfulness of segments being highly ranked to mark syllable types; syllables are maintained, a form of prosodic faithfulness, in order to parse the larger speech stream, not merely to enhance the perception of segments; and thus arguments for syllables may go beyond phonotactics and segmental enhancement and the syllable may have roles not envisioned by the so-called 'phonological hierarchy'). If this were true, the coherent fieldwork would evolve from a curiosity or desideratum to an imperative. Is there such a case? Indeed. Consider the following facts about Pirahã phonology, beginning with its phonemes.

Table One
Pirahã Phonemes

Consonants () = missing from women's speech

p	t	k	?
b		g	
	(s)		h

Vowels

i			
			o
	a		

The first thing to notice about Table One is that the segmental inventory is one of the smallest in the world. The next is to recall that it includes allophonic sounds found in no other language, subject to cultural constraints. The third is that the /s/ is in ()s because it is not found in women's speech, but only in men's.

Though this is one of the simplest segmental phonemic inventories in the world (the women's inventory does seem to be the simplest known), we should juxtapose alongside this simplicity, the complexity of Pirahã's prosodies. Pirahã's stress rule is a good place to begin, since it is well-known.

This rule, from Everett & Everett (1984), is considered one of the more complex and unusual stress rules in the literature, mainly for its phonological consequences (rather than, say, any difficulty in stating or recognizing it):

(9.2) Pirahã stress rule: stress the rightmost token of the heaviest syllable type in the last three syllables of the word.

The phonetic basis of 'heaviness' in (1) is just this: Voiceless consonants are always longer than voiced consonants and there are five syllable weights based partially on this contrast:

(9.3) Pirahã 's five syllable weights: CVV>GVV>VV>CV>GV

Pirahã is a tonal language, as well. But stress, tone, and syllable weight vary independently in the language. To see this, I will just review one simple set of examples, in (3), from Keren Everett (1998). In the examples in (3), tone is independent

of stress. ' = high tone; no mark over vowel = low tone. The stressed syllable is marked by !. There are no secondary stresses (7=glottal stop).

- (9.4) a. !tígí 'small parrot'
 b. !pìgì 'swift'
 c. !sàbí 'mean, wild'
 d. !7ábì 'to stay'
 e. tíí!híí 'bamboo'
 f. 7ì!tì 'forehead'
 g. tì!7í 'honey bee'
 h. tí!hì 'tobacco'

Thus alongside Pirahã's extremely simple segmental phonology, it manifests an extremely rich set of prosodies. This leads us to ask a reasonable question, namely, does the language exploit this differential complexity in any way? Indeed, as Everett (1985) describes it, Pirahã communication makes crucial use of the CHANNELS in (4), below, where Hymes (1974) defines a channel as 'sociolinguistically constrained physical medium used to carry the message from the source to the receiver'. The four principal modalities or channels in Pirahã after 'normal' speech are:

(9.5) CHANNEL	FUNCTIONS
a. HUM SPEECH	Disguise Privacy Intimacy Talk when mouth is full Child language acquisition relation
b. YELL SPEECH	Long distance Rainy days Most frequent use – between huts & across river
c. MUSICAL SPEECH ('big jaw')	New information Spiritual communication Dancing, flirtation Women produce this in language teacher sessions more naturally than men. Women's musical speech shows much greater separation of high and low tones, greater volume.
d. WHISTLE SPEECH (sour or 'pucker' mouth' – same root as 'to kiss' or shape of mouth after eating lemon)	Hunting Men-only (as in ALL whistle speeches!) One unusual melody used for aggressive play

Example (9.5) illustrates how prosodic information in Pirahã is exploited to create these channels. The inventory in Table One above, also partially shows how little

b.

Pirahã allows a tremendous amount of variation among consonants, though not for the features [continuant] or [voice]. This can be accounted for, but only if we refer to Pirahã's channels. The ungrammatical examples in (6)-(8) show that the features [continuant] and [voice] are linked in the sense that they may never vary in the effect. Only place features may vary. With no reference to channels this is without explanation. But in light of the channels this follows because [continuant] and [voice] are necessary for stress placement (Everett (1988)) which in turn must be preserved in every discourse channel, or the constraint in (9) is violated:

(9.10) *Constraint on functional load and necessary contrast* (Everett (1985)):

- a. Greater Dependence on the Channel → Greater Contrast Required
- b. Lesser Dependence on the Channel → Less Contrast Required

Notice that I am not claiming that the absence of variation for different values of [continuant] is predicted by 'channels' alone. This case in fact demands that we further investigate the connection between [continuant] [voice]. There is no claim that ethnography replaces phonology! But I am claiming that without the study of channels and their role in Pirahã culture, not even an understanding of Pirahã's segmental phonology is possible.

The lesson for the field researcher and theoretical linguist to be drawn from these examples is just this: first, language and culture should be studied together; second, as a modality-dependent channel, phonology may be subject to constraints that are (i) language specific and (ii) grounded not only in the physical properties of the instantiating modality (the phonetics) but also or alternatively on the culture-specific channels of discourse employed. This is a very important result because it shows that the 'interface conditions' of the HUMAN COMPUTATIONAL SYSTEM, in Chomsky's (1995) terms, may range beyond PF and LF, if we define an interface system as a system setting bounds on interpretability for HC_L . Such examples also show how coherent fieldwork can be useful for theory. Thus not only the fieldworker, but also the phonologist must engage the language as forming a coherent whole with culture. And this in turn entails more culturally informed fieldwork.

Before turning to another case study from Pirahã, on Ethnogrammar, it would be worthwhile to conclude this particular section with a consideration of some methodological suggestions for studying ethnography of communication.

9.4. Methodology for ethnogrammatical studies

Some suggestions in Saville-Troike (1982, 108ff) include the following:

(9.11) Methodological suggestions for the ethnography of communication:

- a. identify recurrent events
- b. analyze these events, examining their function, form, and relationships between different constituents.
- c. examine the relationship between these events to other speech events and to the society and culture in which they occur.

For example, one might study the use of whistle speech on the Canary Islands. One variety, Silbo Gomero is used in and around La Gomera. In relation to (), each use of whistle speech is thus an event. Some questions that might be asked about these events are: When is it used? Who uses it? What are the constraints on its intelligibility? (e.g. Can two people understand Silbo under any circumstances or does a topic of conversation need to be established first to provide context?) How many other channels of discourse are there among speakers who use Silbo? Are there contents or types of discourse in which the people prefer to use Silbo? Are the contents or types of discourse in which the people prefer not to use Silbo? What are the phonetic details of Silbo and how is it possible (since the language it is based on is not tonal, does it use inherent segmental frequencies as a basis, intonation, etc.)? How does it relate to the consonant and vowel channel (i.e. normal speech)? etc.

We now move to consider a different type of connection between language and culture, namely, Ethnogrammar.

Ethnogrammar

Enfield (2002,3) makes the important observation that "Grammar is thick with cultural meaning". Enfield defines ethnosyntax (part of ethnogrammar, in my conception of the latter as including ethnosyntax, ethnophonology, ethnomorphology, and ethnosemantics, among other possibilities) as "... the study of connections between the cultural knowledge, attitudes, and practices of speakers and the morphosyntactic resources they employ in speech..." And "This field of research asks not just how culture and grammar may be connected, but also how they may be interconstitutive, through overlap and interplay between people's cultural practices and preoccupations and the grammatical structures they habitually employ." To better appreciate the nature of what is meant by 'Ethnogrammar' let's consider another case study from Pirahã, one that has received a good deal of attention from linguists, anthropologists, and the popular media, the connection between 'immediacy of experience' and the group's cultural and grammatical structures. Following this, I conclude the chapter with methodological suggestions on the investigation of Ethnogrammar.

Everett (2005) notices the following facts about Pirahã:

(9.12) Pirahã facts in need of explanation by anyone's account:

- a. Pirahã lacks number, numerals, or a concept of counting.
- b. Pirahã lacks color terms.
- c. Pirahã lacks syntactic recursion.
- d. Pirahã has the simplest pronoun inventory known and evidence suggests that Pirahã's entire pronominal inventory may have been borrowed (see Appendix Two).
- e. Pirahã has no perfect tense.
- f. Pirahã has perhaps the simplest kinship system ever documented.
- g. Pirahã has no creation myths – its texts are almost always descriptions of immediate experience or interpretations of experience; it has some stories about the past, but only of one or two generations back.
- h. The Pirahã in general have no individual or collective memory of more than two generations past.
- i. Pirahã people do not draw, except for extremely crude stick figures representing the spirit world that they (claim to) have directly experienced.
- j. Pirahã has no terms for quantification, e.g. 'all', 'each', 'every', 'most', 'some', etc.
- k. The Pirahã are monolingual after over 300 years of regular contact with Brazilians.

Some examples of this follow:

LACK OF NUMBER, NUMERALS, AND COUNTING

(9.16) **hiaitíhí hi kaoáibogi bai -aagá**

Pirahã people he evil spirit fear -be

'The Pirahã are afraid of evil spirits.' OR *'A Pirahã is afraid of an evil spirit.'*

OR *'The Pirahã are afraid of an evil spirit.'* OR *'A Pirahã is afraid of evil spirits.'*

- (9.17) **kó'oi, kóhoibiíhai, hi píai, 'aáibígaí, hi**
 namename he also, name 3

píai, hi koabáipí
 also, he die

'Kó'oi, Kóhoibiíhai, and 'aáibígaí died.'

- (9.18) **báigipóhoaá 'i 'óooí kobai -baaí**
 name:f she tarantula watch -intensely
'Báigipóhoaá watched the tarantula(s) closely.' (this can refer to one woman
 named 'Báigipóhoaá or several)

LACK OF NUMERALS

There are three words in Pirahã that are easy to confuse with numerals, because they can be translated as numerals in some of their uses.⁵¹ These are listed in (9)-(11):

- (9.18) a. **hói** 'small size or amount'
 b. **hoí** 'somewhat larger size or amount'
 c. **bá a gi so** 'lit: cause to come together (loosely
 'many')
 touch-causative associa nominalizer
 -tive

Some examples which show how Pirahã expresses what in other cultures would be numerical concepts:

- (9.20) a. **tí 'ítí'isi hói hii 'aba'áigio 'oogabagaí**
 I fish smallpred. only want
'I only want {one/a couple/a small} fish.' (NB: This could not be used
 to express a desire for one fish that was very large, except as a joke.)
 b. **tiobáhai hói hii** 'small child/child is small/one child'
- (9.21) a. **tí 'ítí'isi hoí hii 'oogabagaí**
 I fish larger pred. want
'I want {a few/larger/several} fish.'
 b. **tí 'ítí'isi báagiso 'oogabagaí**
 I fish many/group want
'I want {a group of/many} fish.'
 c. **tí 'ítí'isi 'ogí 'oogabagaí**
 I fish big want
'I want {a big/big pile of /many} fish.'

⁵¹ The 'translation fallacy' is well-known, but field linguists in particular must be ever-vigilant not to be confused by it. Bruner, Brockmeier, and Harré (2001, 39) describe it as the supposition that there is only one human reality to which all 'narratives' must in effect conform – be they fiction or linguistic theories, say. Throughout this paper, I will urge the reader to be on guard against this – the mistake of concluding that language x shares a category with language y if the categories overlap in reference.

There are likewise no ordinal numbers in Pirahã, e.g. 'first', 'second', etc. Some of the functions of ordinals are expressed via body parts, in a way familiar to many languages:

- (9.22) **ti** **'apaíkáobü** **'ahaigí** **hi** **tíohió'ío/gaaba** **káobü**
 1 head fall same generation he towards me/there stay fall
 'I was born first then my sibling was born.' (lit: *'I head fall sibling to me/there at fall.'*)

The two expressions in (14), **tíohió'ío/gaaba**, are interchangeable in most contexts. They refer to both intermediate points in a succession of participants, events, etc. or to the final position. But we need to be clear on one thing, namely, that the word 'head' does not really mean 'first', not if we assume that 'first' derives its meaning partially in opposition to 'second', 'third', etc. but overlaps with 'first' in referring to something 'at the beginning of a spatial or temporal sequence'.⁵²

The Pirahã language has no words for individual fingers, e.g. 'ring finger', 'index finger', 'thumb', etc. They occasionally refer to their fingers collectively as 'hand sticks', but only when asked by an insistent linguist. By the same reasoning, there is no word for 'last'. Moreover, they do not point with individual fingers. If they use any part of their arms for pointing, they tend to extend a flat hand, turned sideways, or an open palm facing up or down. More often, they point, as is common around the world, with their lower lip or jaw, or a motion of the head. When discussing a large quantity/number of objects, they do not make tallying motions on individual appendages, etc. If they use gestures, they hold the flat hand out, palm down, varying the distance between hand and ground to indicate the size of the 'pile' or amount under discussion. However, a seated Pirahã man or woman (though women rarely do this) occasionally will extend both feet and hands, with toes and fingers also extended to indicate a large number of individual items (they would not do this in my experience for a non-individuated quantity, such as manioc flour, but rather for bags of manioc flour, etc.). Other than these gestures, there is no other use of body parts, objects, or anything to indicate a concept of 'tallying'.

Very suprisingly (see Davidson () and Wierzbicka () for the predictions that such quantifiers will be found in all languages), there are no quantifier terms like 'all', 'each', 'every', 'most', 'few' in Pirahã. There are also no 'WH-quantifiers' per se.⁵³ To appreciate this, let us consider the examples in (15)-(18), to see the closest expressions Pirahã can muster to these quantifiers:

ALL

- (9.23) **hiaitíhí** **hi** **'ogi** **-áaga** **-ó** **pi** **-ó**
 Pirahã people he big -be (permanence) -direction water
 -ó **kaobü**

⁵² Part of the conclusion of this paper, agreeing with Gordon (2003), is that much of Pirahã is largely incommensurate with English and so translation is simply a poor approximation of Pirahã intentions and meaning, but we do as well as we can do.

⁵³ One reviewer suggests that these Pirahã words *are* quantifier words, but have different truth conditions from their English counterparts. But having different truth conditions just means have different meanings in this context so if it could be shown, as I do here, that they have different truth conditions then they are different words. Period.

-direction entered

'All the people went to swim/went swimming/are swimming/bathing, etc.'

MOST

(9.24) **ti 'ogi -'áaga -ó 'ítii'isi 'ogi -ó**
I big -be(perm) -direction fish big -direction

'i kohoai -baaí,
she eat -inten.

koga hói hi hi -i kohoi -hiaba
nevertheless small amount intens. intens. -be eat -not

'We ate most of the fish.' (lit: 'My bigness ate (at) a bigness of fish, nevertheless there was a smallness we did not eat.'

Example (9.25) is the closest I have ever been able to get to a sentence that would substitute for a quantifier like 'each', e.g. 'each man went to the field'.

EACH

(9.25) **'igihíhi 'ogiáagaó 'oga hápií; 'aikáibaísi, 'ahoáápati**
man he bigness field went name, name

pío,

also,

tíigi hi pío, 'ogiáagaó

namehe also bigness

'The men all went to the field, 'aikáibaísi, 'ahoáápati, tíigi all went.'

FEW

(9.26) **gáta -hai hói hi -i**
can -foreign object small intens. -be
'aba -'á -ígi -o 'ao -aagá
remain -temp -associative location possession -be
(temporary)

('aba'áigio can often be translated as 'only', though I give its full morphological breakdown here to show that it is not really equivalent in meaning to 'only'. Nor does it share the full range of meanings of 'only')

'agaoa ko -ó
canoe gut -direction

'There were (a) few cans in the foreigner's canoe.' (lit: smallness of cans remaining associated was in the gut of the canoe')

There are two words, usually occurring in reference to an amount eaten or desired, which by their closest translation equivalents, 'whole' **báaiso** and 'part' **gúái** might seem to be quantifiers:

(9.27) a. **tíobáhai hi bá -a -i -so**
child he touch-causative -connective -nominalizer

'whole'

kohoai -sóog-ab -agai
eat -desiderative -stay -thus

'The child wanted/s to eat the whole thing.' (lit: 'Child muchness/fullness eat is desiring.')

- b. **tíobáhai hi gíi -ái kohoai -sóog**
child he that -there eat -desiderative
'part' (in the appropriate context)

-ab -agai
-stay -thus

'The child wanted/s to eat a piece of the thing.' (lit: 'Child that there eat is desiring.')

In (19) **báaiso** and **gííái** are used as nouns. But they can also appear as postnominal modifiers:

- (9.28) a. **tíobáhai hi poogáhiaíbáaiso kohoai**
child he banana whole eat

-sóog -ab -agai
- desiderative -stay -thus

'The child wanted/s to eat the whole banana.' (lit: 'Child banana muchness/fullness eat is desiring.')

- b. **tíobáhai hi poogáhiaígííái kohoai -sóog**
child he banana piece eat -desiderative

-ab -agai
-stay -thus

'The child wanted/s to eat part of the banana.' (lit: 'Child banana piece eat is desiring.')

Aside from their literal meanings, there are important reasons for not interpreting these two words as quantifiers. First, their Truth Conditions (see chapter ____ above) are not equivalent to those of real quantifiers. For example, consider the contrast in (21) vs. (22):

Context: Someone has just killed an anaconda. Upon seeing it, (21a) below is uttered. Someone takes a piece of it. After the purchase of the remainder, the content of (21a) is reaffirmed as (21b):

- (9.29) a. **'áoói hi paóhoa'aí 'isoí báaiso**
foreigner he anaconda skin 'whole'

'oaboi -haí
buy -relative certainty

'The foreigner will likely buy the entire anaconda skin.'

- b. **'aió** **hi** **báaiso** **'oaob** **-áhá;** **hi** **'ogió**
 affirmativehe whole buy -complete certainty 3 bigness
- 'oaob** **-áhá**
 buy complete certainty
'Yes, he bought the whole thing.'

Now, compare this with the English equivalent, where the same context is assumed:

- (9.30) a. STATEMENT: He will likely buy the whole anaconda skin.
 b. OCCURRENCE: Piece is removed (in full view of interlocutors).
 c. STATEMENT: %He bought the whole anaconda skin.

It simply would be dishonest and a violation of the meaning of 'whole' to utter it in (22b). But this is not the case in Pirahã, (21b).

Next, there is no truly quantificational-abstraction usage of **báaiso** 'whole':

- (9.31) ***Ti 'isi báaiso 'ogabagai, gíái 'ogi -hiaba.**
 1 animal 'whole' want, piece want -negative
'I prefer whole animals to portions of animals.' (lit: 'I desire (a) whole animal(s), not piece(s).')

Sentences like (9.32) cannot be uttered acceptably in the absence of a particular pair of animals or instructions about a specific animal to a specific hunter. That is, when such sentences are used, they are describing specific experiences, not generalizing across experiences.

It is of course more difficult to say that something does not exist than to show that it does exist, since in the former instance a skeptic can always reply that you have not looked hard enough. Nevertheless facts like those discussed in this discussion, in the context of my nearly three decades of regular research on Pirahã, lead me to the conclusion that there is no strong evidence for the existence of quantifiers in Pirahã.

ABSENCE OF RECURSION⁵⁴

With regard to recursion, as mentioned in ____ above, there are two varieties, where I claim that Pirahã lacks either type. I repeat these here:

- (9.32) $A \rightarrow AB$
 (9.33) a. $A \rightarrow BC$
 b. $B \rightarrow DE$
 c. $C \rightarrow AF$

The lack of rules like (1) explains why Pirahã lacks the following:

⁵⁴ Everett (2005) was not always clear on which of these was in focus. Due to space limitations, I only gave evidence against (1), even though (2) is the principal claim. In work in progress, I tighten up the arguments (thanks to suggestions from David Pesetsky and David Adger) so as to rule out system recursion as well, thus strengthening the case *against* the view of 'creativity in human language' advocated in Hauser, Chomsky, and Fitch (2002).

(9.34) Coordination:

- a. [NP John and Bill] came to town yesterday.
- b. [S [S I saw [NP Mary, Sue, and Willy] [PP in town and at the mall] and [S I saw some other people too]].

(9.35) Disjunction:

- a. Either [NP Bob or Bill] will come.
- b. I had [NP chicken or pork], some white meat.

(9.36) Embedding:

- a. I doubt [S whether they will come].
- b. John says [S that Bill thinks [S that Mary will agree [S that Sue should come too]]].

(9.37) Basic phrase structures:

- a. I came [PP with the Pope].
- b. [NP The big man] got sick.
- c. The [AP very big] man got sick.

(9.38) Semantic scope requiring embedding:

- a. He didn't say it was raining.
- b. He will not say if he is coming.

Everett (2005) argues that these facts follow from the constraint on 'immediacy of experience' in ():

(9.39) *Immediacy of experience expression in Pirahã*: Declarative Pirahã utterances contain only assertions related directly to the moment of speech, either experienced (i.e. seen, overheard, deduced, etc. – as per the range of Pirahã evidentials, as in Everett (1986, 289)) by the speaker or as witnessed by someone alive during the lifetime of the speaker).⁵⁵

⁵⁵ The original formulation was: "Grammar and other ways of living are restricted to concrete, immediate experience (where an experience is immediate in Pirahã if it has been seen or recounted as seen by a person alive at the time of telling), and immediacy of experience is reflected in immediacy of information encoding—one event per utterance." David Adger (personal communication) rightly points out that I will need to

To use an old term of anthropology in a new framework, the constraint in () is in effect a cultural *taboo*. If this proposed taboo were the correct way of looking at what is going on, it would automatically and exactly capture the facts that were listed in Everett (2005), i.e. the *lack of*:

- (9.40) **Embedding**: since embedded sentences are not assertions (Cristofaro (2003)), they cannot be used. To avoid these, the grammar of Pirahã will not have rules of the type in (2) above. This will explain all the anti-recursion effects in the paper.
- (9.45) **Number & numerals**: These are skills that have both immediate application and wider application, ranging beyond immediate experience. Since the latter uses would violate the cultural principle in (3), however, these are not available in the grammar (interestingly, counting and numerals involve recursion, which could be taken as additional evidence that Pirahã lacks recursion).
- (9.46) **Relative tenses**: These involve assertions defined in terms other than the moment of speech. So when I say in Pirahã 'When you arrive, I will go', as I show in Everett (1993), both 'arriving' and 'going' are defined relative to the moment of speech, (however, one could argue that relative tenses involve recursion and so are for this reason unavailable). More complex tenses would violate (2).
- (9.47) **Kinship terms**: All kinship terms are related directly to the one speaking (the controller of the 'moment of speech', i.e. ego) and none are defined in terms of other relations (i.e. no kinship terms involve recursion, e.g. grandfather, grandson, etc.).
- (9.48) **Color terms and quantifiers**: Color terms and quantifiers can identify immediate experiences, as can numbers, but, like numbers, are avoided by the grammar because they also entail a significant component of ranging beyond immediate experience.
- (9.49) **Myths and fiction**: These violate the evidentiality constraint in (3).

One reader of Everett (2005) wondered why, if the above is correct, Pirahã has nouns, since there could be, for example, abstract nouns, i.e. ranging beyond immediate experience. This objection doesn't follow, though I see the point. First, Pirahã does lack abstract nouns. Second, it cannot do away with the (semantic) category of nouns, because all languages must have terms that represent entities and terms that provide information about them. Pirahã is a language, so cannot get by without noun-like elements.

9.6. Linking Ethnography and Grammar

Whether or not Everett () is right about capturing all these facts under the single generalization in (), the Pirahã facts nevertheless provide strong evidence for the importance of investigating the culture-language nexus as part of fieldwork. Let us therefore consider some methodological suggestions for investigating this connection. First, I suggest a few questions that might lead to a methodology.

- (9.50) Pre-methodological ethnogrammatical questions:

discuss how this applies to modality.

- a. Are there irregularities that have no obvious structural explanation?
- b. Are there examples 'free variation', i.e. where there are choices between two structures which are not constrained by the structures or the grammar, in so far as can be determined?
- c. Are there unusual facts about the cultural events, values, or explanations that involve principles or phenomena that at any level look similar to principles operative in the grammar?

As to the methodology that follows from such questions, Enfield (2002, 14ff) offers some cogent and very important considerations and suggestions for the study of ethnogrammar. First, he recommends that the fieldworker "Examine specific morphosyntactic structures and/or resources and make explicit hypotheses as to their meaning." Second, following development of this and related methodological considerations, he raises the crucial issue of 'linkage', namely, how can we establish a causal connection between facts of culture and facts of grammar? I turn to this directly. Before doing this, however, I want to point out what seems to be the biggest lacuna in the study of ethnogrammar, whether in the studies in Enfield (2002) or elsewhere. This is the effect of values, especially cultural taboos like () above, in restricting both culture and grammar. That is, previous studies, like those in Enfield (2002), while reasonably focusing on meaning, which is after all a principal contribution of culture (i.e. guiding its members in finding meaning in the world), fail to consider cultural prohibitions or injunctions, however deeply or shallowly embedded in the community system of values. The Pirahã example of this section is evidence that such values should also be considered in ethnogrammatical studies. However, before we can draw any conclusions at all about ethnogrammar in a given language, we need to consider the vital issue that Enfield refers to as 'linkage', i.e. the establishment of a causal connection between culture and language. That is, how can we convince someone or, at least, effectively argue that property *p* of culture *C* causally determines feature *f* of grammar *G*? According to Clark & Malt (1984), cited by Enfield (2002, 18ff) there are four prerequisites to establishing linkage between culture and language:

(9.51) Culture – grammar linkage prerequisites

- a. Empirical grounding
- b. Structure independence
- c. Theoretical coherence

And also (9.52):

(9.52) Avoid circularity

A circular argument in ethnogrammatical studies would be to claim that a particular linguistic feature is simultaneously determined by an aspect of culture and evidence for that aspect of culture (so, for example, 'The language has evidentials because the culture values empirically-based reasoning.' And then 'We know that the culture values empirically-valued reasoning because it has evidentials'). The way to avoid this is to first establish, using *nonlinguistic evidence*, particular values or meanings in a certain culture. Next, using *noncultural evidence*, establish the meaning and structure of the relevant linguistic examples. Finally, show how linking the two

provides a conceptually and empirically (in terms of predictions where possible, or explaining independent domains such as historical change) superior account of the facts that leaving them unconnected.

Ethnogrammatical studies thus range from showing that, say, a language has honorifics because of a severe social structure, or a particular set of kinship terms because of its restrictions on marriage, to (what most researchers have overlooked), the kinds of global, architectonic constraints on grammar from, e.g. *taboos* like () above.

Chapter 10. The written record: grammars, dictionaries, and text collections

10.1. Grammars

10.1.1. Why write a grammar?

I once heard a colleague remark that no one should write an article about a language until they have written a grammar of the language. I think he was serious. It is good advice, even though it is exceedingly stringent. For one thing, writing a grammar deepens and broadens one's knowledge of a language. Also, writing a grammar helps a linguist get the 'big picture', seeing how things fit together – kind of like helping to understand each tree better by understanding its place in the forest. This is what Pike (19--) would have called the 'field' view of language.

To my way of thinking, writing an article about a single construction or rule without a good grasp of the grammar as a whole is like writing about the function and form of a puzzle piece without knowing what the whole puzzle looks like. Of course, many languages already have grammars. So one could read a grammar instead of writing one. Certainly, no one should write about a language without at least reading entire grammars of that language when available. I would, on the other hand, tend to avoid working on languages where a grammar has been written, simply because so many other languages need to have descriptive grammars written.

In any case, reading a grammar is a poor substitute for writing one, if you really want to figure out how a language works. Writing a grammar teaches you that every part of a grammar is a series of decisions on how best to bring order from the chaos of data that the linguist has collected. There is more or less security in the usefulness of these decisions in different parts of the grammar. Often only the grammar-writer knows this, even when they are not at all trying to sweep recalcitrant facts under the rug. I can think of no more challenging, rewarding, important or urgent task for individual linguists than writing a grammar of a language based on their own field research. Ultimately, a grammar is the linguist's theory of how a specific language works, atomistically and holistically, i.e. what are its 'bits' and how do these 'bits' fit together. A grammar is the result of careful methodology, lots of hard thinking, innumerable conversations with native speakers, artistic flair, boldness (to propose connections or to say that 'x' does not exist in language 'y', etc.), lots of luck, and huge amounts of reading, planning, testing, and interpreting. A grammar may even identify a 'theme' of a particular language – recurring properties, semantic or formal or cultural or societal – that are found in multiple constructions, text types, social patterns, etc. throughout the language. A grammar should ideally include an ethnography of communication, for reasons presented in chapter __ above.⁵⁶

So how does one do fieldwork with writing a grammar in mind? First, have a basic outline of a grammar in mind from the start of your fieldwork. One such outline is the *Lingua Descriptive Questionnaire* by Norval Smith and Bernard Comrie (this can be found on the Department of Linguistics website for the Max Planck Institute for Evolutionary Anthropology: <http://lingweb.eva.mpg.de/fieldtools/linguaQ.html>). Another is my own phonology questionnaire (see appendix __, also on the MPI site above) or the phonology outlines of one of the volumes in the Oxford *Phonology of the World's Languages* series.

Another way to do fieldwork guided by questions relevant to writing a grammar is to invest time learning the craft of grammar writing by reading grammars that are

⁵⁶ Give sources on writing ethnographies of communication.

considered exemplary by other linguists, either for a given region of the world or in general. Linguists should read grammars for fun and for professional development. If you do not like to read grammars, you may be in the wrong profession.

However, ultimately, the questions you ask will reflect your own interests and background. What attracts your attention most in this grammar or language-culture pair? What kinds of things do you believe are in most need of being said about this language? What have you learned from native speakers about their view of their own language? What would they liked highlighted? (Alternatively, would they want anything omitted from public discussion, even grammatical forms, certain kinds of texts or semantic domains, etc.?)

First I begin with a discussion of the different kinds of grammars, including the following three main types: Reference, Descriptive, and Pedagogical Grammars, as well as the usefulness of 'grammar fragments'. I will emphasize that quality is more important than quantity, but that a certain coverage is required to get a 'feel' for the 'genius' of the language and knowing how and where to 'hang' each piece in the grammar. I discuss grammar-writing as a literature genre in relation to the remarks on its fallibility in Chapter One and the implications of this for different methods and attitudes towards grammar-writing. I also discuss ways of testing the grammar as a whole with native speakers, e.g. reading it to groups of them as it is written and after the entire grammar is in draft. I next turn to consider the task of dictionary-making, comparing and contrasting different kinds of dictionaries. I then discuss the importance and methodology of compiling representative collections of texts, framing the discussion in terms of at least the following parameters:

10.1.2. Grammars are the nucleus of documentation and description

The goals of a project of documentation and description are to provide a record of the sounds and meanings of a language and how these are associated with one another and with the culture in which they are embedded. Sound files of high quality, texts, and a useful and fairly extensive dictionary form the core of such documentation. But the grammar provides not only the description (the analytical account of how the pieces of the structure of the language fit together) but also is the tie that binds together the various components of the documentary evidence of the language's sounds, texts, and words. A complete documentary and descriptive project for a language will include the following components:

- (10.1) a. Descriptive grammar
- b. Sound files (all prosodies and segments exemplified)
- c. Dictionary
- d. Texts
- e. Ethnography of communication
- f. Pedagogical grammar portions

So for most fieldworkers, even if their own objectives are not a full documentation and description of a language, contributing to the construction of a grammar of the language under study is at least an important goal to which their research should contribute. In other words, if the linguist is doing research on the phonetics of a language's segments, on information questions and dislocation, or on voice alternations, etc., the research should be written up in such a way that it can be

incorporated into a grammar of the language eventually. This is done by providing careful glosses, high-quality sound files, and relative jargon-free descriptions.

10.1.3. Types of grammars

10.1.3.1. Pedagogical vs. Reference Grammars

There are at least three types of grammars – pedagogical, descriptive or reference, and theoretical. A comparison between pedagogical and reference grammars is given in Table 1 below (see also the SIL website

(<http://www.sil.org/linguistics/GlossaryOfLinguisticTerms/WhatIsAReferenceGrammar.htm>)

Table One: Reference vs. Pedagogical Grammars

Type of Grammar	Audience	Purpose	Organization	Style of Presentation
Pedagogical	Native speakers looking for a tool to preserve and teach their language	To provide lessons in how to speak and write the language	In terms of short lessons on specific topics of pronunciation, word usage, etc.	Textbook like, for learners, to inform and to develop skills.
Reference	Linguists who want specific kinds of technical information and who will not believe everything you say.	To persuade and to inform. To provide a cohesive picture of the language that allows for both comparisons with other languages and understanding the 'genius' of the language at hand.	Things are presented in the way the linguist considers most effective to understand the language, but will usually be in the order phonology-phonetics-morphology-syntax-semantics-discourse	Clear for a wide readership, but technical when necessary and with sufficient argumentation to convince well-informed readers.

A pedagogical grammar is an educational tool and as such requires different skills and training from a reference grammar. Also, since the pedagogical grammar is largely, if not exclusively developed for the needs and uses of the language community, it must be built up slowly, based on experience in using it as a teaching tool, discussing it with community teachers, getting advice from educational experts, and so on. Moreover, a pedagogical grammar should be based carefully and solidly on a comprehensive

reference grammar. It should not be developed in isolation from the larger effort of describing, arguing, and reasoning about the structures of the language.⁵⁷

Pedagogical grammar development can require skills in motivation as well. For example, if the community leadership wants the linguist to lead an effort for language revitalization, where a pedagogical grammar is to play a major role, the linguist will have to engage the active participation of native speakers and those trying to learn the traditional language of their people, to help shape and improve the pedagogical and analytical aspects of this specialized grammar.

A reference grammar, on the other hand, is designed to provide analyses, descriptions, and data in a cohesive, relatively standardized form to a general audience of professional linguists and other linguistically sophisticated readers. These can follow rigid guidelines (e.g. the Croom Helm descriptive series, see Everett & Kern (1997)) or allow greater freedom for the linguist to describe the grammar as they think best (e.g. the new Cambridge series of descriptive grammars (see Dixon (2004))). The former model, when part of a series, has the advantage of producing a range of grammars to serve as a comparative reference set, an encyclopedia of language structures appropriate for rapid comparisons. This is also its disadvantage, however, because its detailed, meticulous, and highly constraining outline can be too confusing or force the linguist to distribute across various subpoints of a detailed outline a discussion that would have been best presented as a whole, a single portion of the grammar. Reference grammars have the general advantage of being directed towards all linguists and thus do not generally require any special jargon or theoretical background to read.

10.1.3.2. Theoretical grammars

The final type of grammar I want to discuss in this section is the theoretical grammar. The first in-depth application of generative grammar was G.H. Matthews's grammar, *Hidatsa Syntax* (Matthews 1964). Hale (1967, 341) concludes his review of this book by claiming that this grammar is "... an outstanding landmark in American Indian studies as well as a highly significant contribution to general linguistic inquiry. This work is very much a credit to modern linguistics." Chomsky further situates the theoretical grammar in modern theory:

"LSLT [Logical Structure of Linguistic Theory, Chomsky (1975), DLE] and other detailed work of the 1950s (particularly G.H. Matthews, Hidatsa Syntax) at once revealed a tension between descriptive and explanatory adequacy. As soon as serious descriptive work was undertaken, it was discovered that available accounts of language, however extensive, barely scratched the surface; even the most comprehensive grammar provided little more than hints that sufficed for the intelligent reader; the language faculty was tacitly presupposed (without awareness, of course). The same is true of the most comprehensive dictionary. To attain descriptive adequacy, it seemed necessary to construct extremely intricate and complex grammars, radically different for different languages. On the other hand, to approach explanatory adequacy

⁵⁷ In my Arts and Humanities Research Council project for the documentation and description of the Kisedje language, one of my research associates was an expert in indigenous education with more than fourteen years of experience in education among indigenous groups of the Xingu Park, where the Kisedje live.

it was necessary to assume that the states attained are determined to an overwhelming extent by the initial state, which is language-invariant. Thus languages must all be cast to the same mold, differing only superficially. The major research project was aimed at overcoming this tension by showing that the apparent complexity and variety of language was only superficial, the result of minor changes in a fixed and invariant system." Chomsky (1994)

Moreover, as Hale's review shows in considerable detail, Matthews's book provides detailed argumentation and explicit structures for most major aspects of Hidatsa syntax. In addition, for researchers working within Generative Grammar in the early to mid 1960s, *Hidatsa Syntax* provided a model for how to write a grammar. Judged when it appeared, therefore, this volume was a pioneering, extremely important study, almost universally accepted by theoreticians as a significant improvement over previous grammars.

The problem is that today, forty-one years on from the publication of *Hidatsa Syntax*, this grammar is almost universally lamented as being of very little use to anyone wanting to learn about either Hidatsa or Generative Grammar (the latter because the field has changed so dramatically in the intervening years). So this once proud grammar is today relatively useless, exercising almost no influence at all, whereas the so-called 'taxonomic' (i.e. reference or descriptive) grammars it was once so favorably compared to continue to be read and serve to enlighten new generations of linguists about the languages they describe. And in my opinion and experience the same fate has befallen every theoretical grammar written since.

This is *not* to say that theoretical grammars should be avoided. But we need to draw at least two lessons from the fate of *Hidatsa Syntax* and its ilk. First, if one does write a theoretical grammar, the theoretical discussion contributes to very short-term (howbeit potentially important) goals, a sort of rock in the stream that can divert the flow around it, though it itself is long forgotten downstream. Another lesson is that it is useful to reconsider the organization of a theoretical grammar. My PhD on Pirahã, for example (Everett (1990 [1983])), was divided into two major sections, a reference grammar (also published in English as Everett (1986) and a theoretical discussion. The theoretical discussion is no longer particularly relevant, but the reference grammar portion can still be used by linguists regardless of theoretical orientation. To sum up, the ideal grammar should be useful for present and future generations; it should be clear; it should anticipate reader questions and objections and deal with these by detailed argumentation (except in the case of a pedagogical grammar).

Let us conclude by considering a couple of ways to enhance the usefulness of the grammar. First, the grammar must be tested. This can be done by re-checking the entire grammar, or at least the parts that are controversial, theoretically significant, or for which the linguist feels less secure, with native speakers in groups and with individuals. Second, be sure that the grammar includes argumentation for every major analytical assertion. If the linguist says that 'Stress goes on every other syllable from right to left within the word', then they should explicitly alternative analyses (e.g. stress first picks out the rightmost syllable and then goes from left to right), by considering hypothetical or ungrammatical forms, by spectrographic analysis, etc. (see ___ above). Argumentation is at once a display of erudition and reasoning and tremendously enhances the reader's opinion of the grammar-writer, the usefulness of the grammar, and the likely reliability of the grammar as a whole. In a reference grammar, argumentation will be less theory-internal than in a theoretical article, but it is

nonetheless crucial. Finally, tell the reader something about the empirical basis for the grammar. What size is the corpus? What is the corpus like? How long was the linguist in the field? What was the role of the community in the analysis (including how language teachers were involved)? How many hours a day did the linguist work and how did they work (e.g. 'I was in the community for six months and I worked three hours per day with language teachers and five hours a day filing, analyzing, and retesting examples by walking around the village trying out my knowledge'). And so on.

I turn now to consider the corpus upon which the grammar is based.

10.1.4. The corpus for the grammar

To write a grammar of any type requires a significant corpus of data. What is the nature and size of an 'adequate' corpus for a grammar? The answer is simple: an adequate corpus must be varied, natural, and big enough. Let's consider each of these terms.

First the corpus should be varied. That is, it should contain the greatest possible number of distinct form, meaning, and construction types. It should vary for ages of language teachers, emotional states, content, social class of teacher, genders, speeds of utterance, topics of discourses, genres of discourses, styles of discourses, and channels of discourse (see ___ for the concept of 'channel').

The corpus should be natural, providing data that native speakers utter (in the appropriate context) or naturally reject (That is, a good corpus contains (tested) ungrammatical examples as well). This latter point is very important theoretically and methodologically. Theoretically, the presence of ungrammatical utterances recognizes that there are many facts about speaker's knowledge of their language that cannot be learned simply by observing what they say or do not say. Methodologically, however, this raises the issue of how to collect grammaticality judgments. So before considering the final question, i.e. when is the corpus 'big enough', we need to take up more carefully the implications of ungrammatical examples in field research.

Ungrammaticality is a difficult notion, as Schutze () and Cowart () demonstrate at length. The reason that ungrammaticality is problematic is that utterances can be rejected by native speakers for a variety of reasons. Let me give an example from my fieldwork on the Wari' language. As Barbara Kern and I were working on the grammar of Wari' (Everett & Kern (1997)), a particular construction (see Everett (2005c)) type struck me as extremely weird. I could not believe that the facts were exactly as Barbara had described them to me. Even though Kern's knowledge of the language seemed impeccable and even though I had never had reason to doubt her before, I just found these facts hard to take in. When I raised this concern, Kern's response was typical of her – "Then go to the village and check them out with the Wari' for yourself." Since we were working only a day's travel from the village, I took her suggestion and departed the next morning. I met thirty or so Wari' speakers on the banks of the Mamoré river on the Brazil-Bolivia border and we sat down to consider the examples.

As I read a couple of examples out loud, the people seemed mystified. "That makes no sense at all", said the man on my right. "Who told you we say that?" asked the man on my left. So just as I thought, these example are ungrammatical! Then I answered the man on my left, "Barbara told me you say these things." Surprised looks all around. Someone then asked, "Barbara said that? Hmm. Then we must say that. Let's think." So people began discussing the example and then they began to smile. "Oh, yeah, we *do* say that. If we are talking about something and want to say something about it like this, then we say it just like you said there. Just like Barbara told you." The

utterances were now perfectly fine, we discussed them, they corrected my pronunciation (segmental and prosodic) and I was satisfied that they were indeed grammatical utterances. But this was clearly a nonstandard way of getting at their grammaticality. In the normal course of events in, say, English, linguists simply ask individual language teachers if this or that utterance is grammatical. Yet the Wari' example shows that speaker's initial reactions can be quite misleading.

Why and when is an utterance accepted or rejected? The Chomskyan distinctions between competence (grammatical factors) and performance (cognitive factors, among others), as well as the Saussurean distinction of *parole* vs. *language* (social factors), when applied to such judgments predict that speakers can accept or reject utterances for a variety of reasons. For example, the sentences in () and () are grammatical but unacceptable, both famous examples in the literature:

(10.2) Buffalo buffaloes Buffalo buffaloes buffalo buffalo Buffalo buffaloes.

Meaning: Buffaloes from Buffalo, NY who hoodwink buffaloes from Buffalo, NY hoodwink buffaloes from Buffalo, NY – i.e. this is a 'center-embedded' tautology.

(10.3) Oysters oysters eat eat oysters.

Meaning: Oysters that oysters eat (also) eat Oysters.

Both examples are hard to process but are grammatical, as linguists have known for decades. The traditional distinction, based on the competence-performance dichotomy, is to say that both are grammatical but unacceptable. Sometimes one hears too that an utterance can be acceptable but ungrammatical, as in the fairly trivial example I have constructed in ():

(10.4) Q. Who would do such a childish thing? (Wife to husband)
A. Me do it. (Husband replies sarcastically to wife.)

The answer in () is not grammatical, but it is an acceptable utterance in that circumstance for the purpose of expressing sarcasm. Although we can thus make the distinction between 'acceptable' and 'grammatical' conceptually clear, separating them practically in fieldwork is nontrivial (see also Schutze []). Moreover, the difficulty of distinguishing these vital concepts, in a Whorfian twist, becomes even more intense in the lack of any terms for these concepts. That is, there are relatively few languages in the world that actually have metalinguistic terms like this such that the linguist can actually ask "Is this grammatical?" or "Is this acceptable?" And circumlocutions such as "Is this 'pretty'?", "Is this OK?", "Can I say this?", "Is this good English?", etc. are all extremely unreliable.

Consider "Can I say this?" (or "Can you/one/a Pirahã/a speaker of your language say this?"). In my experience language teachers often say "Yes, you can." without the slightest regard for the acceptability or grammaticality of the utterance. This is partially because the linguist is paying to talk so, to some language teachers, the linguist can say anything they damn well please. None of these expressions in fact can be relied upon to give clear evidence of either the acceptability of an utterance or, a fortiori, its grammaticality. There are simply too many variables for the fieldworker to know why a particular answer has been given – does the speaker's judgment reflect pragmatics, semantics, syntax, phonology, lack of concentration, desire to please, or feelings of different status? There is no foolproof way to know the answer to these questions.

So what can the fieldworker do? Well, one thing is to get the speaker to confirm his or her broad assessment (e.g. "Yeah, that is OK.") by uttering the sentence themselves. Then the linguist can repeat it slowly and ask for yet another repetition. In my experience, the native speaker will not repeatedly utter an ungrammatical sentence. They will 'edit' it, i.e. change it slightly to make it grammatical. Or they will refuse to say it. (If the utterance is grammatical there often will be no problem for the speaker to repeat it.) The kind of correction made should identify what was wrong with the example structurally. On the other hand, if the example is *pragmatically* bad (roughly, if it is inappropriate in the environment of the lab session), it is quite likely that the speaker will still not say it. In this case, the linguist cannot tease apart the ungrammatical from the inappropriate.

One way around this latter problem is to work with multiple language teachers simultaneously when checking for grammaticality. Recall from the earlier example from Wari' that I only discovered that a particular kind of construction was possible by working with a group of speakers who discussed the example among themselves. Usually language teachers attempt to be helpful and when several are working together they tend not to accept snap judgments by one of their group. They discuss and refine. Therefore, checking grammaticality judgments with a group of speakers has benefits. On the other hand, none of this guarantees the field linguist that they know exactly, precisely why a given utterance is rejected. Moreover, even utterances that are acceptable could be ungrammatical, as we have seen. So what is a poor linguist to do?

They must carry out grammaticality tests with multiple speakers (serial language teachers and groups, see ____ above). Where the results of these various interviews produce a unanimous judgment, one way or the other, the linguist can have *moderate* confidence in the result. But it is still necessary to test and interview for *truth conditions* (see ____ above), for naturalness (in speakers' opinions), look for related examples in natural texts, etc. But in fact it is likely that the results of all these interviews will not produce a unanimous judgment but, rather, a mixed judgment. Speakers are quite likely to disagree, especially when they are not in the room together as they offer their judgments. In this case, the linguist could go with his or her best judgment (using their theoretical predictions for example), advising the reader of their grammar or article that this is what they have done and why. (Always, always be honest with your reader.) Or a more scientifically respectable approach can be adopted and the linguist can employ a statistical approach to grammaticality, following suggestions in Cowart (). After all, other social scientists deal with conflicting judgments and accounts of behavior all the time. Their reports, therefore, present statistical analysis of these different judgments, etc. and distinguish statistically significant vs. insignificant results. But using statistics and standard social science methodology requires more and different training of linguists in general and fieldworkers in particular.

The upshot is that determining grammaticality is never a straightforward matter. Judgments, testing, statistical analysis, text tracking, eavesdropping, etc. are all crucial. As a consumer of grammars, the linguist reader must always take all crucial grammaticality judgments with a huge grain of salt, in the absence of the corroborating studies just mentioned. All grammars ever done, past, present, or future, need retesting and can never be understood as a 'God's eye view' of the language in question. Once again, grammars are stories we tell about a language as we see it.

Let us turn now to the final question concerning the corpus for writing a grammar, namely, 'When will I have enough data?' Samarin (1967, ---) gives a number of useful

suggestions in this regard. I have borrowed some of Samarin's suggestions and added some of my own to come up with the list in ():

(10.5) A complete corpus is obtained when:

- a. All the closed classes of linguistic elements are fully accounted for
- b. When there are no 'holes' yet in the data needed for analysis (partially, therefore, the answer depends on theory)
- c. When there are multiple tokens of all types.
- d. When it is maximally useful for other disciplines, as well as linguistics
- e. All new material collected only contains structures and meanings already found in the corpus collected.

Let's consider each of these points in more detail. First, what does it mean to say that all the closed classes are fully accounted for? This simply means that when you have all the prepositions, all of the adjectives, all of the verbs, i.e. classes with a small number of members that do not expand in membership. In some languages verbs will be in the open classes of lexical items (e.g. English), while in others they will be among the closed classes (e.g. Moseetén, Sakel []). How do you next determine that there are no 'holes' in the data? Well, you have to have a view of how language works, partially based on general principles shared by most linguists and partially based on the particular theory that you are most influenced by. And you must be able to argue for your conclusions. On the latter, see ____.

Table –

The phonetic segments of hypotheticala

p	t	k
b	d	
m	n	ŋ

There is a missing segment in Table -, i.e. a voiced velar, [g]. Is this an accidental gap or an actual asymmetry in the segmental inventory? The linguist will need to look for examples of [g]. At some point, they might conclude that the system is indeed asymmetrical, certainly not all that uncommon. But until they can say with confidence that this is the case, the corpus is incomplete.

It is also important to ensure that for every segment, prosodic pattern, syntactic construction, suffix, etc. in the language, that the corpus includes multiple tokens of each. And the linguist's analysis must be the guide as to when there are enough tokens of each. One useful criterion in answering this question is 'Are all tokens I am now recording simply repeating the patterns that I already have?' If so, then there are probably sufficient tokens in the corpus. However, one cannot simply rely on texts to magically produce all the tokens and their distributions that are necessary for a complete corpus. The fieldworker must *think*, based on his or her analysis and ask questions like the following: 'If my analysis is correct, then there will be forms of interpretation/shape *x* but never forms of interpretation/shape *y*.' Then the linguist must look for the missing forms, both those they predict to be missing (no matter how long they search) and those which they predict to be found eventually, but which are currently absent (i.e. accidentally) from the corpus. The linguist must be able to assure the readers of the grammar that the corpus is complete by this metric.

The corpus should also be maximally useful for other disciplines. The fieldworker may be working on a rare language that few people are likely to have access to. In this case especially, but in all cases ultimately, the linguist should collect texts and data relevant to other disciplines insofar as they have time and knowledge to do so. Text collections should include all important cultural values, to the degree that the community is willing to allow access to these. Claims about numerals or counting should be accompanied by experimental evidence corroborating the claims (even if this means bringing in an expert consultant). And so on. Finally, once all new data appears to contain no new structures, etc. then the linguist can consider that, with respect to his or her current working hypotheses and purposes, the corpus is complete. But, as we have been saying, the 'complete corpus' is a relative, never an absolute concept.

We move now to another core component of language documentation, the dictionary.

10.2. Dictionaries

The traditional view of the dictionary in linguistic theory up until twenty-five years ago, and still widespread, is that the dictionary is an asylum for the misbehaved, i.e. where we put forms that are not derivable by regular rules of syntax or phonology. People who work under this view may be tempted to produce trivial dictionaries that are little more than lists of words, idioms, and morphemes. But this would be a mistake, even for those with the 'asylum' (or 'jail' – see Williams and DiSciullo ()) view of the dictionary, because it renders the dictionary less useful. A dictionary is formed by a view of its potential users, not merely by a particular theoretical perspective.

In their volume, *Making Dictionaries: Preserving Indigenous Languages of the Americas*, the editors address the purpose of making a dictionary:

"A reasonable person might ... ask, Why do it? One way to read the contributions to this volume is as personal answers to this question. But a more general response can be discerned in all the chapters and, indeed, in the work of every lexicographer. There is something at once both marvelous and practical about producing a guide to the mind, world, and behavior of a group of people. The benefits that accrue from such a handbook – literacy, preservation, history, discovery – only add to the excitement of seeing the published dictionary standing upright on the shelf." (Frawley, Hill, and Munro (2002, 2-3)).

The editors go on to suggest that the ten most important issues in compiling a dictionary, with immediate application to languages of the Americas, but with a clear pan-geographic relevance are as follows:

(10.6) Ten crucial issues in dictionary compilation:

- a. Entries
- b. Theory
- c. Literacy
- d. Graphics
- e. Role of the community
- f. Types and numbers of dictionaries
- g. Historical information
- h. Technology
- i. Presence or absence of dictionary-making tradition
- j. Handling exceptions

Let's consider these in turn:

Entries: how is the *headword* (the objects of definitions, possibly with subentries) determined? Should the headword be a 'basic form' (e.g. citation form)? How can the fieldworker decide on a basic form?

According to the editors, ultimately the choice of headword for a dictionary will result from a "... tradeoff between the pressures for maximal explicitness and the desire to match the users' minds to facilitate their inferences as they fill in what must be left implicit." And further, "In the end, entries are a wager that the tension between the way the dictionary ought to look to the compiler and the way it feels to the user will not be too great." (Frawley, Hill, and Munro (2002, 5)).

Theory: How much information in each entry should be there for theoretical vs. applied reasons? How much should linguistic theory affect the overall form of the dictionary? Each entry should contain as a minimum sufficient phonetic, phonological, morphological, syntactic, semantic, and pragmatic information for the reader to know how to use and pronounce the entry and where it fits in the grammar and culture of the language. Some linguists build large amounts of additional theory into entries, while others see the dictionary more as a service to the language community and prioritize its utility rather than writing a lexicographic treatise on each entry. I favor the user-friendly view, though it is conceivable that linguist-only dictionaries can be done, in addition, if the linguist has inclination and time to do so.

Literacy: The dictionary may be the first or one of the first documents ever produced in the language under study. In this case its impact on literacy and discussions of representations of the language will be massive. But regardless of when the dictionary appears in the literary history of the the people, it will be or can be an important part of their self-identification and 'represents' their language to themselves and to people outside the community, the latter especially if the dictionary is bilingual (see ____ below).

An issue that arises in this regard is the extent to which the national or other major language(s) should influence the orthographic representation of the vernacular. For example, in Romance languages, as in many other languages, vestiges of history are included in the national orthography. So, consider the words in () from Brazilian Portuguese:

- (10.7) a. casa 'house'
 b. cicatriz 'scar'

As is well-known, in Romance languages, the old palatalization rule from Latin, whereby the voiceless velar occlusive and the voiceless coronal affricate are in complementary distribution such that the latter occurs before high front vowels and the latter elsewhere, continues to be encoded in the orthography by using a single letter, 'c', for both. (Also imported to some degree into the representation for loan words in English, e.g. 'electric' vs. 'electricity'.) So in some cases 'c' is [ts], in others it is [k]. And to complicate matters further, [k] is represented in still other cases, i.e. to represent the voiceless velar before a high vowel, by [qu], as in (10.8):

- (10.8) a. quinze 'fifteen'

This representational inefficiency is OK for a language with a long history of literacy and in which it is apparently desirable to continue to maintain some of the history of the language (and avoid 'imperialist' symbols, e.g. 'k!'). But should it be imported into a language that does not share its history, a minority indigenous language, say? Should the language of field study be obliged to share the inefficiencies of the national language? The questions become more radical and urgent when considering whether to use national scripts, such as Chinese, Devanagari, etc. or Western symbols. These are just some of the reasons why the dictionary is at once an important milestone in the literacy of a community and why it must be subject to community discussion and approval. The linguist cannot simply present a dictionary as a 'gift' to the community, but must develop it in conjunction with them.

Graphics: Will your dictionary include illustrations, photos, different colors, etc? These will add to its expense (in print media in particular), but funding for well-planned dictionaries is readily available from a number of sources, so the expense should not be allowed to become overly discouraging. Well-designed and utilized graphics can enhance the dictionary's usefulness as a tool for literacy since beginning readers can use different graphics to figure out on their own the meanings and form of certain words. Of course, graphics can get out of hand and create complications out of all proportion to their benefits. So once again the use of graphics has to be constrained via discussions between the linguist and the speech community.

Expense of graphics can be tremendously reduced of course if the dictionary is electronic, rather than (or complementary to) print media. See ___ below for more on this.

Role of Community: This is discussed throughout this section.

Number of dictionaries: By and large the average documentation project could be rightly proud of producing a single high-quality dictionary. But other types of dictionary are possible, e.g. dictionaries of place names, flora and fauna, thesauri, cultural concepts and artifacts, etc. More specialized additional dictionaries will not be something the average fieldworker expects to produce, though communities could change the linguist's mind and plans by requesting these. In such a case, the linguist needs to determine what the community wants in terms of specialized dictionaries and figure out how to produce it.

History: How many dictionaries and how much linguistic and, particularly, lexicographic work has been done? To what extent should dictionaries track changes in lexical meaning? How much, if any, space should be dedicated to etymologies in the dictionary? The answers to such questions will depend partially on the linguist's training and interests, partially on community wishes, and partially on the view of the readership most likely to use the dictionary outside of the community. It is obvious that the more historical information the better. This enriches the dictionary and its role in the cultural heritage of the people. However, at the same time, it is nontrivially complex to provide etymologies and such an endeavor can get out of hand rapidly in the hands of someone with little training in etymology.

Technology: There are a number of advantages to constructing an electronic dictionary, whether as a complement to a print dictionary (recommended) or as the sole dictionary (not recommended). Just a few of the things that an electronic dictionary can do better and more cheaply than a print dictionary are: better graphics, audio files (providing pronunciations of each entry and many examples), easier dissemination, easier connections and cross-references from entries to the text or portion of field data from which they come, much greater and easier searchability, permanence (if done properly, an electronic dictionary can last forever), accessibility (more people have access to the dictionary), lower cost, easier to look for and predict trends in semantic change of lexical entries, and much greater interactivity – changes, corrections, and updating the dictionary become much easier when it is in electronic form. Anyone who has consulted an on-line dictionary source, e.g. one finds on internet sites such as Yourdictionary.com (<http://www.yourdictionary.com/>) knows that being able to listen to the pronunciation of entries, being able to find entries almost instantly, etc. are tremendous advantages of electronic media dictionaries.

Ultimately, however, the crucial thing to remember once again is akin to Postal's Maxims – the linguist and the community must see dictionary making as a fluid process, subject to negotiation, constrained only by imagination and budget.

We conclude the chapter now with a discussion of the nature and importance of text collections.

10.3. Text Collections

What is the purpose of collecting texts? Well, for some linguists texts should be the core component of the corpus upon which analysis and the grammar are based. To some degree, I agree with this view, although as has been pointed out numerous times in this and other chapters, texts are but one type of data that is essential for a good corpus. They are important because they are revealing with respect to the interaction of language and culture and they provide natural data, often providing examples and structures that the linguist would never have discovered on his or her own. It ought not to be difficult for any fieldworker to understand why text collections are important to the enterprise of linguistics in general and fieldwork in particular. Accepting the importance of texts, then we come to questions about the nature of textual documentation. First, what kinds of texts should be collected? Second how many texts are needed? As to the kinds of texts, my suggestions are in ():

(10.9) Kinds of texts needed:

- a. All genres (narrative, procedural, hortatory, expository, and any other type that the fieldworker identifies as relevant for the language).
- b. Texts that cover all significant cultural topics, e.g. life, death, harvest, hunting, dealing with the outside world, creation, fiction, history, etc.

Different genres will reveal different kinds of linguistic information. Different moods, aspects, tenses, participant coding and tracking, different kinds of logical connectives, etc. all are associated to a greater or lesser degree with different text genres. And anthropologists, ethnographers of communication, and linguists will all benefit from a rich array of text topics, linked to the culture (which implies that the linguist must understand the culture more than superficially, in order to know which topics to ask for texts on).

Next how many texts does the fieldworker need to collect? This gets us back to the issue of the 'complete corpus' and is answered by the considerations given in the previous section. However, as a rule of thumb, I would suggest at least two hundred pages of transcribed texts with morpheme by morpheme glosses, single spaced, no greater than 11 point font.

Chapter 11: Modeling and Managing Field Research

11.1. *Individual Fieldwork*

As we consider some of the issues in individual field work, it is useful to ask what it means to work alone in the field or, indeed, even if it is possible to work alone. To work completely alone is a fiction; it is not possible. If it were, this would imply, for example, that there is no speech community you are working on – you are obviously not working alone if you are learning from another person (just as there is no such thing logically as a 'self-help' book. If the author of this book helps you, it is not 'self-help').⁵⁸ As Mithun puts it, 'The product of fieldwork will ultimately be shaped not only by the nature of the language, but also the methodologies chosen, by the roles assumed by the speakers, and by the preparation and sensitivity of the linguist.' (Mithun, p34, in Newman and Ratcliff). As experienced fieldworkers know, and as Mithun goes on to point out in her useful discussion, the linguistic record is a product of the collaboration between linguists and speakers. This means in practice and in theory that there is no way to really work alone. But let's consider this a bit more, because there is some intuitive appeal to the idea that some people are loners, while others work in groups.

If you go to the field with your partner or family, then you clearly are not going alone. On the other hand, if you travel into the community without any companion from your own culture, background, etc. can't you say that you are alone in the sense that there is no one to help you with your work? No, once again, because the language teachers are in fact your partners in the crucial senses (as Mithun aptly states it above). And you have, if you follow this guide, books with you that will help you, so you are still not working 'alone' in the strictest sense.

So let's try to be clear: field linguists do not work alone, not ever. In his excellent book, **The Art of Fieldwork**, the anthropologist Harry Walcott (2001, 37) puts it this way:

'A moment's reflection reveals how extensive are the strands that link, and important ways bind, seemingly lone and independent researchers, in the field or at their desks, to larger and more embracing social systems.'

As I write this book, for example, I have potential readers, students, and reviewers looking over my shoulder. Still, though, if a fieldworker can be alone professionally, if they are the only linguist around. In this latter, special sense, they are alone. Working alone in this sense of 'individual' field research has both advantages and disadvantages. These are not frequently considered, however, because the 'individual' model is the default, usually unquestioned way to do fieldwork for most linguists. In any case, let's consider the pros and cons of working alone in this sense, beginning with the advantages.

Advantages of the 'Individual' model

Less Pressure and more flexibility

When you work apart from other linguists, you can experience less pressure, depending on your individual psyche, by not having people 'looking over your

⁵⁸ This is another bit filched from George Carlin's HBO special at the Beacon Theatre in about 2000.

shoulder'. When you work alone you are not immediately accountable to anyone else for how you spend your time. Your language teacher will put time constraints on you to some degree, but nothing like members of your own culture would if they were there to work with you towards a common research objective. Nobody in the field, when you are there without a linguistic research team, is going to watch you to see if what you are doing daily conforms to official project objectives. This is simultaneously an advantage and a disadvantage. It is a disadvantage because it removes the field researcher from some potentially useful and immediate quality control. However, it is an advantage in living in the field because it reduces strain, pressure, and potential conflict. If you get up in the morning, you can in principle change your schedule. Let's say you were planning to analyse the previous day's data and you have no lab sessions planned this day. But now that you think about it, it would be nice to spend the day baking cookies, going for a walk in the jungle, paddling a canoe, or reading the trashy crime novel you brought with you. If there are other linguists working with you in the field, you will probably feel constrained by them, to make the day useful. But psychological well-being in the field is often based on feeling like you are in control of your time (another illusion, but a helpful one). I do not like routine, to be frank. I do not like feeling that I need to do something because of others' expectations. When I am the only linguist around it is easier for me to give time to myself than when I have colleagues with me, especially if I am the senior colleague and the one the others are looking to for guidance and example.

Also, there is less temptation when you are alone to sit around and talk with friends from your culture about topics unrelated to the research. This kind of camaraderie can become a detriment to research by siphoning time and by making the linguist less dependent on the culture and language under study (see chapter ____ below for why this dependency is crucial).

More learning (since you have to do more on your own)

I hate mechanical things when they break down. And in the field they break down a lot. For my field research among the Pirahãs, I needed a motorboat, provided for me by the National Science Foundation. One morning, after a heavy rain, I woke up to the smell of gasoline. I went to the river, over thirty meters deep in the peak of the rainy season, which this was, and looked. There, about six meters down, held only by a very taut nylon rope, was my motorboat, my only transportation to the road (the Transamazon) where my car was stored, 100 miles upriver. One night of hard rain had sunk it. With the help of many Pirahãs and a Brazilian missionary, I was able to raise the boat, just until the edges were about one inch above water. Pirahã women came running with gourds and bailed the water out. But even though the boat was up, I had water mixed with my gasoline and the cylinders of my boat motor were full of water. I had to fix the motor and get the water out of the gasoline and cylinders on my own, or I would not get out of there. I did. And in the process I learned more about motors.

As a linguistic example of forced learning when working alone, consider my attempt to discover comparative structures in Pirahã. First, I gathered a number of sticks and leaves of varying sizes. Then working first with the sticks I got the words for 'long' and 'short' (all work with the Pirahãs is monolingual, as described in Everett (2001) and chapter ____ below). Then I would hold up a short stick and a shorter stick from my carefully size-arranged array in front of the language teacher. Then I would hold up an even shorter stick and try to get 'shorter/more short', the comparative form. What I got were answers like the following:

- (11.1) a. /íí **tio/óihii**
 woodshort
 b. /íí **píi/isái**
 woodlong
 c. /íí **tio/óihii** /**iahá**
 woodshort go

I had to conclude, after hours and hours of this, that either (i) Pirahã had no comparatives or that (ii) it used /**iahá** 'go' to indicate that one stick was particularly short, though with no specialized morphosyntactic comparative marker, or (iii) I was incompetent (and the latter feeling rarely goes away entirely). Before this experience (and countless others like it), I thought every language had certain structures, e.g. comparatives. Since then, I have read more, but as a graduate student my ignorance was, perhaps, even greater than it is now. So I had to struggle with this and, in the struggle, learn more than I might have otherwise have learned had others been with me. A partner in the field might simply have told me that some lack comparative structures. But finding this out on my own made the learning more effective.

It can also be an advantage to work alone because this simplifies interpersonal and intercultural interactions between linguist and speakers. The more outsiders that are present at one time in a community, the more complex the social dynamics. Working alone avoids many of these complexities (see Chapter ____ for a discussion of some of the ethical issues that can arise).

Few barriers between the researcher and the speech community

The more people present with the linguist who do not belong to the speech community, whether government officials, fellow linguists or friends – i.e. anyone except family (see ____ below on how children and family can be of tremendous help in field research), the more insulated from the target language culture the fieldworker becomes. And insulation slows, impedes, and halts learning. If there are multiple linguists present in the community at a time, for example, the people can feel less comfortable approaching them, especially if they stay together in the same house or location, for example, reducing chance for observing and engaging in spontaneous conversations, etc.⁵⁹ But a 'lone' field researcher will have a more direct and constant connection to the community and, an extremely important correlate, will *need* members of the community socially more if they have no colleague or partner or family member along.

Getting more credit (or more blame)

The final advantages of working alone that I will mention have to do with money and fame. The more people working on a project, the more money needed and the less control over the project's funds will the fieldworker exercise. And the wider the credit will have to be shared for discoveries and the work of the project. So if you work alone, you have the advantage of keeping the money and fame to yourself. OK. Those are some of the advantages of working alone. Let's now consider a *disadvantage* of

⁵⁹ This is only true, of course, if the entire team goes to the community at the same time, which is not at all a necessary correlation of team research.

working alone. We will then see how team work need not have the disadvantages associated with it above and can have the advantage of multiple individual field research.

A disadvantage

My favourite quote on fieldwork is the following from Margaret Mead, the distinguished American anthropologist on her first field trip. It is found in a letter to her thesis supervisor, Franz Boas:

'I have no idea whether I am doing the right thing or not, or how valuable my results will be. It all weighs rather heavily on my mind.' (January 16, 1926)

This is honesty. And just about anyone who has done fieldwork alone has felt somewhat like this at some point. But notice how this is expressed – in a letter to a colleague thousands of miles away. Alone in Samoa, Mead had to grapple with the responsibility of her (thesis) research without local academic interlocutors or any local anthropologist to share her field responsibility with her. The advantage of this for the researcher is that it makes them think harder and learn more than they might otherwise have learned. But the disadvantage is obvious – working alone can at times nearly overwhelm the field researcher with self-doubt and emotional pressure. I will take up other disadvantages when we move to the section on team research.

Before closing this section, I want to urge prospective field researcher to read others' accounts of their own field research on the individual model. Such exercises remind us of our commonalities, that no one's experience, however good or bad, is truly unique. In general ethnographies contain accounts of the ethnologist's own field experience. The lessons our predecessors have learned and their very experiences can teach important lessons before going to the field and save hours of wasted effort, as well as providing an emotional vaccine against some of the hardships of different field experiences. Here is a list of some of my favorites, though this is only the tip of the iceberg: Dixon (); Chagnon (); Macaulay (); Conklin (); Malinowski (); Mead (); Darnell (); and Darnell ().

These all tell stories of independent, resilient, and resourceful fieldworkers who entered 'alien' environments alone and thrived as scientists and as human beings. They discuss their research and, often, explicitly discuss their personal challenges as lone field researchers. I recommend that all fieldworkers read at least the chapter by Macaulay before going to the field, especially women.

11.2. *Team fieldwork*

Fieldwork has long been considered the domain of the individualist, the strong personality who is self-motivating, able to withstand loneliness, well-rounded, etc. A sort of Indiana Jones. That is, it is not normally thought of as an environment for teamwork (at least not with members of one's own culture), even though, as I have shown above, this is a fallacious concept. What I want to do in this section is to argue that a research team has a better chance of success than a lone researcher.

Composition of the team

The team is composed of at least the speech community, the professional linguist or graduate student who is responsible for the written form of the results to a host institution or funding agency, the program director of any relevant funding agency, the

government officials or local academic representatives who must monitor the research project for local government agencies, academic officers at the linguist's home institution who must authorize his or her absence and activities, as well as his or her host institution's ethics committee (and any other ethics committee, e.g. funding agency, government of host country, etc.), consultants, PhD students, co-Principal Investigators, secretarial staff, and postdoctoral research associates, depending on each individual research situation. When the team contains more than one designated fieldworker, management becomes even more crucial.

Advantages

The advantages of team research are best framed in terms of the tasks the linguist takes on in the field. One of the most important tasks facing linguistics today (though it has always been important in spite of recent more general awareness) is the documentation and description of endangered languages. At one time 'documentation' was seen as little more than collecting word lists. Early explorers, naturalists, and missionaries, among others, often seem to have collected almost random lists of words.⁶⁰ But to document and describe a language in the modern sense draws on a number of specialized skills that may go beyond those possessed by any one linguist.

More help, more specializations present

When you work in a team, there is more knowledge available to you. If your team is balanced in terms of specialties and experiences, you could have solid knowledge of most of the field of linguistics with just, say, three people, along with different kinds of practical and information-processing knowledge. For example, if the team leader is a phonologist, working alone they would struggle a bit more with syntax analysis, perhaps, than if they had a syntactician on the team. Likewise for other subdisciplines.

Ever since tape recorders began to make their impact on field research, we have come to expect more from documentation than mere word lists. Today, the documentation of a language requires a wide variety of skills. Photography, videography, sound recording and storage, web design, computer programming, and other specialized knowledges may all be called upon in documenting a language. Even if one person had all the requisite skills, to use them all in a particular project would take so long that it is unlikely that a full (e.g. dictionary, text collection, and grammar – see ___ below) project would be completed. But people do not commonly have all the skills and training necessary to fully document and describe the parts of a language considered crucial in a documentation project. And, I say this from raw experience, people change as they age. I no longer feel inclined to learn agricultural vocabulary, for example, by working all day chopping trees in the tropical sun. But I do know how to collect and analyze data better than ever before in my career. And there could be graduate students on my team who would jump at the chance to swing an axe among flies and mosquitoes in 99% humidity, in exchange for an array of chopping/cutting verb paradigms and plant (and insect) names.

These are advantages which are difficult to overcome for any one working as an individual linguist in the field.

⁶⁰ In spite of their obvious shortcomings, these early efforts have been much-appreciated by posterity because they are often the only record left of lost languages. In many cases these lists provide bases for historical reconstruction and understanding of language change. One ought not to look a gift horse in the mouth after all.

More work can be done (in theory)

Just as there are more specialities available within a team than to an individual worker, a team can do more work. The task of, say, writing a grammar, can be distributed and each member of the team can work on a section of the grammar. To be sure, everyone should read everyone else's work on the grammar and comment on it, but there is no way that a single individual can do the same amount of work in the same amount of time as a well-organized, well-selected team. One member can take, say, primary responsibility for a dictionary, another for a pedagogical grammar, another for the reference grammar. There are many ways the task could be divided so as to increase both its scope and quality.

Replicability

Another important advantage of team research is that it can help make results more replicable for non-team members, as well as facilitating replication of results prior to publication. This is so because working as a team enables all results, methodologies, and analytical decisions to be subjected to discussion, criticism, testing, refinement, and development by the group as a whole. If the research team is well-managed, with each team member contributing a needed speciality, then each member will teach the other members, challenge the other members, and strengthen the project as a whole, beyond what any single field research could accomplish working alone.

The team concept is hardly new, of course. Linguistics, like other sciences, has always benefited from cooperation and group-based research, whether in small local groups such as particular labs, or in international theoretical ventures, e.g. chomskyan linguistics and functionalism. But in field linguistics the common assumption has been that research will be conducted by a solitary field researcher 'figuring out' how a language works on their own. If I am correct, however, it is important to challenge this assumption.

For the sake of completeness, however, let us briefly review the disadvantages of team fieldwork and how to counter them.

Disadvantages

The advantages listed for the individual model earlier would be disadvantages for the team model if everyone on the team went to the same community location at the same time. For this reason, I recommend instead that members of the team with a responsibility for team work only go to the field as individuals, trying to cover the entire year in the village, at least in the first year of a multiyear project. (Another reason for this is that many aspects of culture and language are seasonal and only by being there for the entire year can the team hope to observe and learn these seasonal aspects.)

The principal disadvantage of the team model is that it entails group dynamics. People might not get along, they might be different in practice than they are on their CV, they might turn out to do terribly in the field situation, they might decide that they do not like you, they might complain about their salaries, they might not want to follow the team objectives after all, etc. There is no way around the issue of 'group dynamics', which is why I have added the following section on management. Management is vital in any project, team or individual, but it becomes especially critical in a team situation. The principal ways of overcoming the disadvantages of team research are selecting the right team members and managing the team well. Whether you are a team manager/Principal Investigator on a grant or a team member (postdoctoral research

associate, PhD student, or language teacher) solid management principles can help you contribute to building and understanding what makes a successful team for field research.

11.3. *MANAGEMENT*

I want to review a few principle management principles in directing and working together in teams, though many of them apply to individual workers as well. Not everyone who is part of a team will perceive themselves as directly concerned with its management, but all members of a team should be familiar with some of the issues involved in getting the best out of team work.

No algorithm for success

First, it is important to realize that in team fieldwork, just as in individual fieldwork, there is *no algorithm for success*. Good judgment is always more important than lists of priorities and principles. What follows are points that have been important to me. Since this is not a management manual, it is likely that I am omitting principles that business administrators would consider vital. But this reflects my experience as a fieldworker.

Mission statement

The team must have a clear statement of what their objectives are, some might refer to this as a '*mission*' statement or goal statement, or some such. There should be no ambiguity as to what you as a team are trying to accomplish. If you have a funded project, the written project can offer guidance, of course, but it is not sufficiently succinct to serve as a mission statement. It is very important that you be able to summarize in a sentence or two what your entire project is about and what it hopes to contribute to the world. The entire team, secretaries, PhD students, postdoctoral research associates, and co-Principal Investigators should all know and agree to this.

Criteria for success

All members of the team also need to know and agree on what the *criteria for success* are. How will the team know that it each individual and the team as a whole has finished its job and finished it successfully? These criteria must be explicit, specific, and, above all, measurable. 'Finishing a dictionary', for example, is not a well-stated criterion for success. 'Publishing a 3000 entry, tri-lingual dictionary, with publisher ___ is', however. The criteria can and should be refined and reassessed as research goes along, in discussion involving all team members.

Expectations

It is important that *expectations for all team members be reasonable*. If, say, the team leader is an experienced researcher and regularly works at a particular rate of accomplishment, clearly measurable in terms of project goals, it must be recognized that other team members with less experience may not work as fast or as well, initially at least. Expectations must therefore be negotiated at the outset between the team leader and team members, on an individual basis, and then evaluated and reset as necessary as time goes on. The team leader should be very hands on in the beginning of a research project to ensure that all members are using their time well, working with a clear sense of mission, a commitment to the success of the project, and that they are

able to meet the expectations that the team has of them. The project must be seen as belonging to the entire team, not merely the Principal Investigator.

Records

Teams must keep records of team administration as well as team intellectual output. Therefore, a procedure of record-keeping and filing is essential to the well-being of the team. Some types of records that need to be kept are given below.

DATA: How are you going to record the data that you collect? Are you going to use proprietary software, e.g. *Shoobox* (see http://www.ethnologue.com/tools_docs/shoobox.asp), or software developed to serve as a long-term record, universally usable, e.g. *Elan* (<http://www.mpi.nl/tools/elan.html>)? How will you store backups of data? On a server, on DVDs, or some other means? These decisions will no doubt be required from the early days of writing your proposal for funding, since most funding agencies these days are very careful about data-storage and data-ownership. Most field researchers these days will be required to turn over copies of all of their data in an XML-compatible format. (For further information, the reader is urged to consult, as an entry point into the general area, the website of EMELD (<http://emeld.org/>), a project developed in connection with the LinguistList (<http://linguistlist.org/>)). There are many linguists working in various places around the world to maintain long-term storage facilities and standards for the preservation of linguistic data. Before going to the field, all researchers should be familiar with the basic standards for data-storage, an issue discussed briefly in Chapter ____ below.

WORK MEETINGS: Team members should meet to discuss the project face-to-face on a regular basis. meetings can be less frequent perhaps as the project goes on and people know each other better, but they should not stop. Regular contact, discussion, and assessment must be a vital part of the project from start to finish. Minutes must be kept of all these meetings, in the language of the funding agency, so that if any problems develop or if conflicts arise, it may be determined whether such issues were foreseen, previously discussed, etc. Was any action taken to avert such consequences? Why not? If your project has a secretary, they can be appointed to keep records of regularly scheduled project meetings. Otherwise appoint someone at each meeting to keep the minutes and to distribute them for approval and then filing within forty-eight hours of each meeting.

CORRESPONDENCE: All project correspondence must also be stored in an easily accessible manner. Correspondence with the speech community, between team members, with the funding agency, with government officials, etc. must all be carefully stored. If it is email, then it can be stored as electronic media using the filing system on your mail program, though I also recommend that hard copies be kept in an old-fashioned drawer filing cabinet.

FINANCES: Your project will also generate financial records which must be stored. If you are ever audited (an unpleasant experience), you must have careful records of project expenditures. In most cases, your home institution will keep records of financial transactions. But the Principal Investigator, and team members to a less degree, ultimately bears responsibility for all financial decisions and use of funds in the project, so I suggest that the project office keep duplicate records of all transactions.

AGREEMENTS: Everyone on the project, as well as the relevant members of your home institution's personnel department should receive copies of the agreed-upon structure for project accountability and administration. This is important. Even the most reliable team members can forget or selectively remember what the agreements are in

this regard. It is vital that everyone be clear on how the project is administered and who is accountable to whom. And this must all be written down and read and approved by all. The originals of these agreements should be filed in the project office (which may simply be the Principal Investigator's university office). Documents on accountability and administration should also include a record of time commitments, job descriptions for the overall project, job descriptions for each individual field trip, and any other expectation of individual team members that could affect the overall results of the project. It should also be made clear in these administrative documents what the publication and presentation guidelines and constraints are for the project. For example, how many papers should the project produce per year according to its funding commitments? Who should write the bulk of each paper? Does everyone know that the Principal Investigator (and this is very important!) has veto power and editorial control over every project publication or presentation? These matters must be spelled out clearly in advance and a copy of the agreements filed in the project office and with each individual team member.

SUCCESS CULTURE: Aside from filing and documents, there are even more important considerations for team management. First, a team is responsible for creating a 'culture of success'. What does this mean? It means that each member on the team is committed to the success of the project, that she knows what the criteria for success are, that she feels that her contribution will be a success – because she feels well-trained, well-equipped, well-guided, and well-supported for this success – and that the contributions of the other team members will also be successful. People should enjoy the challenges of the project and be excited about the likely scientific and career benefits of successfully concluding the project.

PROJECT CALENDAR: All research projects must have a (somewhat but not completely fluid) project calendar. A project calendar should lay out the responsibilities of the entire team, the timing of its deliverables for the funding agency and the language community and a detailed breakdown of each team member's responsibilities in the field and out of the field. Ideally, each team member should provide the team with a breakdown of how she plans to spend each week in the field, giving the amount of time estimated for each task. This is done by breaking the principal task(s) assigned for each field trip into subtasks. These can then be translated into daily, weekly, and monthly 'work quotas', based on project duration. So, for example, if it is my responsibility to analyse the segmental phonology, then how much time should I give to that task (I suggest one month of regular fieldwork), what are the subtasks (e.g. consonants, vowels, syllable structure), how much time should I give to each? And so on. It is important, however, that you realize that you will be regularly interrupted in the field, by visitors, by the community, by your health, by many unforeseeable factors. Use every minute. Field time is precious. Don't waste it. Therefore, I recommend that you *double your estimates for all project tasks* and schedule your time accordingly. For example, if you believe that you can collect five new words per day, learn them, and analyze them, then you need to shoot for ten words per day or double the amount of days that you have dedicated to this task, such will be the unforeseeable variables that affect your time.

TRUST & RESPECT: Nothing can kill a project, ruin its chances for success, and embitter all participants more easily or more permanently than a lack of trust or respect by team members for one another. The project participants must trust one another. The PI must have the trust of all subordinate members of the project. And, perhaps even more importantly, the PI must give all project members trust and respect. Do not try to

micromanage their assigned responsibilities. If there is evidence that someone is not fulfilling their responsibilities, this will emerge in the regularly scheduled reports. But micromanagement can be/is a symptom of distrust and paternalism (or maternalism). Everyone should be trusted as a highly-qualified professional. If appointments to the project are made carefully, this will be true.

FINANCIAL MANAGEMENT: Research projects also require financial management. If an organization gives you money to complete a research task then you must have a process in place for managing the income. In general, your home institution will have procedures already in place. If you follow these procedures, your home institution will support you in your financial accounting to the funding organization. If you do not follow these procedures, you will not receive the support you need. And you also open yourself up to potentially very unpleasant consequences if and when you are audited. Therefore, each team member must be fully aware of and careful to follow, the financial regulations (for receipts, advances, equipment purchases, etc.) of the home institution and the funding organization. Managing money is not something that all academics are good at or want to be bothered with. But these days getting external funding is a strong expectation that universities and many research institutions place on their staff and therefore all academics must attempt to get funding and, when successful, must know the operative regulations for managing this money. One very sensitive issue, for example, that arises is the use of per diem funds, i.e. funds that are used to pay the living expenses of team members in the field and at conferences. Each team member must, again, be very clear on how these funds are to be used. They must also be very clear on how much is to be paid to language teachers, what amount of money is available for gifts to the community as a whole, and they must be able to resist pressure to increase such payments and gift amounts. The project cannot invent money. It must live within its budget.

CONFLICT RESOLUTION: A project must be prepared for conflicts to develop. Someone is going to get upset at someone else or disagree strongly with some aspect of the project's management, etc. This is unavoidable. Therefore, a project must have some way of resolving conflicts. In my own projects, I have taken advantage of services already available from the University of Manchester, which allow members of my own research team to discuss their role in the project regularly with other faculty in the department or members of the personnel department. I also maintain regular and informal contact with other team members, in the field when possible (certainly with language teachers and the language community in the field), and also at the home institution.

ROUTINE: In the field, it is also vital that you have some way to avoid boredom. I offer a number of suggestions in Chapter __ below. But here, let me say that the most important component of mental health, in my experience, in field research is the sense that you are making solid, measurable progress towards your goals, and that you have a workable routine established that you follow daily.

Among the Pirahãs, in my early years, I fell into a routine quickly. I would get up about 545AM, just as it was starting to get light, get my flip-flops and two 25-liter containers and walk down the path to the river. I would then make four trips, hauling up roughly 200 liters of water to a large barrel mounted above our kitchen sink (which I built) and then pour the water into the barrel, add chlorine, and then recover the barrel (looking for frogs that often got into the barrel). Then I would help my wife get breakfast for our three children and us, most often oatmeal, powdered milk, and coffee. After that, usually about 815AM, I would work with a language teacher (arranged the

day before usually, but sometimes just opportunistic, depending on who was in my house) for two hours. After that, I would help with our children's correspondence courses, usually teaching my oldest daughter language and history. Then I would visit Pirahã in the village, doing more elicitation, but mainly just visiting and practicing the language. I helped with lunch. We always took a brief nap after lunch, to help cope with the jungle heat. After lunch, I worked most of the afternoon on processing and clarifying (with an additional language teacher) data that I had collected in the morning. In the evenings, we went to the river to bathe, after exercise (jogging when trails were wide enough, skipping rope, lifting weights, etc.). Our home, completely open as it was, with no doors, no full walls, etc. would fill up with Pirahãs at night. They often slept in the house with us. After a time with the Pirahãs, we would go to the back part of the house, the one part that had a door, walls, and screen, and spend time as a family, usually with me reading to the children. The Pirahãs respected this space usually and would not come back to that part of the house, though of course they could have done and occasionally did. As you can see, my day was hardly a morning-to-night linguistic marathon. This is partially because I had gone to the village with my entire family, but also partly because I could not absorb more than that. I don't think it is reasonable to plan long days and seven-day weeks in the village. You can 'burn out' that way. I recommend a much easier pace, one that allows you to develop an enjoyment of village living, at least in the first months of your research.

And time for recreation is crucial. So weekends, or at least one day per week, should be reserved for doing nothing whatsoever on the project. Fieldworkers differ about this. But I need at least one day a week in the field to myself, to read novels, to play my guitar, etc.

Case study: the Suyá project

If field researchers were to attempt to develop more team-centered approaches, what would be needed to achieve success with this type of model? Let me provide a case study of team-based field research, my own projects for the study of the Suyá language (the people prefer the autodenomination, Kisedje). Kisedje is a Ge language spoken by approximately 378 people in the Xingu Park region of Brazil. Although their culture has been studied extensively, especially by Seeger (), documentation and description of their language is very sparse (see dos Santos () and Guedes ()). An apparently related language, Tapaiuna, is spoken near the Kisedje villages. In 2004 I received grants from the Arts and Humanities Research Council and the Economics and Social Research Council, both of the UK, to prepare a reference grammar, dictionary, pedagogical grammar, and theoretical & descriptive articles on the language, along with a comparative study of Tapaiuna. For this research, I received two postdoctoral research associates, a PhD student, and a secretary (also a PhD student in Linguistics, but not doing research on Kisedje). This was a huge change for me. All of my previous research followed the 'Indiana Jones' model.⁶¹

My first tasks were to hire the postdoctoral research associates. What qualities should I look for in my partners? Analytical ability? Theoretical interest and insight?

⁶¹ The project also began with a Brazilian consultant who later dropped out, citing my bad management, though I think that there were other motives. Still, if I had been a better manager, perhaps this would not have happened.

Field experience? Data-base experience? Knowledge of Brazil? Knowledge of Portuguese? In the end I hired two Brazilian PhDs who turned out to be a marvelous combination of all these various desiderata and more. But they did not always find me all that easy to work with. Apparently, my management style can be at once dictatorial and laissez-faire, which naturally led to misunderstandings and occasional hurt feelings. That is, after explaining the project objectives and my view of what everyone should do and could do on the research, I tended to leave people on their own, only getting back to team members when they failed to meet my expectations, an experience we all found unpleasant. But my team wanted regular meetings, clear feedback, assessment, encouragement, and linguistic help. All reasonable items. And they pointed out my problematic management style. Without management and leadership from me, therefore, the project would slow down. I could no longer just do my own thing, as it were, on analysis and elicitation, as per the Indiana Jones model, but needed to be concerned about others. And I needed to give thought to project management principles.

CHAPTER 12. FIELDWORK ETHICS

12.1 INTRODUCTION

Establishing a 'best practice' for ethics crossculturally can be complex. On the one hand, there are universals or near universals of conscience such that the fieldworker can go a long ways towards establishing a sound ethical basis for his or her fieldwork by following his or her conscience and 'doing what seems right'. But regardless of how well-developed one's conscience is, this subjective rule of thumb is far from an adequate basis for governing the ethical aspects of any given field project. Ethical fieldwork is not simply avoiding gaffes or the giving or taking of offense, or failure to commit criminal acts. Just as peace is more than the absence of war, so ethics involves a positive, pro-active code of behavior and right-thinking intended to leave the field situation and language community better off than when the linguist first 'found' them. Moreover, no researcher can avoid explicit consideration of ethical issues because all major universities and funding agencies in the Western world require that all research projects associated with them receive pass rigorous ethical review that requires that the researcher(s) deal with the issues discussed in this chapter.

There are several sources available on ethics and fieldwork on the internet.⁶² I provide the URLs to several of them in the footnote above. This chapter is unable to provide more than the barest of overviews of ethical considerations. I therefore strongly recommend that the prospective fieldworker also consult the web sources in the footnote and their references. Nevertheless, this chapter does attempt to cover the basic issues of the major ethical considerations in fieldwork. We begin with a discussion of issues affecting the language community and then move to issues concerning government relations and then to the very sensitive issue of relating to missionaries, an issue that few fieldworkers will be able to avoid.

12.2. THE LANGUAGE COMMUNITY

CONSENT

The first issue that arises in any fieldwork session is the consent of the language community to allow and participate in the research and the subsequent dissemination of the research results and data. The key issue here is that the community and each individual participating in the research give *informed* consent to participate and that this consent be registered in such a way that it is accessible and clear to all that the consent was voluntary and informed. One way of doing this, when working with a literate society, would be to spell out the details of the research in writing and ask the community leadership and/or all language teachers to sign an agreement to work on this project as it is presented in the document. The details of the research that should be made clear include:

62

<http://www.lsadc.org/>

<http://www.als.asn.au/>

<http://www.stanford.edu/dept/linguistics/fieldwork/info/ethics.html>

<http://www.eva.mpg.de/langua/files/ethics.html>

<http://www.aaanet.org/committees/ethics/ch1.htm>

<http://www.chass.utoronto.ca/linguistics/LIN458H1F/04.458.ethics.pdf>

<http://www.geneva.quno.info/pdf/tkmono1.pdf> (very useful paper, perhaps the best I have seen on Traditional Knowledge and Property Rights).

() Research details for discussion with language community (all of these will ideally be made clear in writing with written consent from the community leadership):

a. The objectives of the research – what are your scientific reasons for being in the community? How did you make this clear to whoever is funding you? Work hard to ensure that the community really understands why you are there. Do not be satisfied with giving them vague ideas about 'studying your language' or some such.

b. The methodology of the research – how do you plan to go about finding out the things you said you wanted to know under (a) above? How do you expect the members of the community to be involved? What will they do? What are the possible risks to them in terms of physical safety, group or individual prestige or loss of face, etc?

c. The funding of the research: who is funding it; what are the categories of expenses; for how long is it funded; what percentage of the funding is for the community? People are rightly suspicious about where the money is coming from. And they may have heard all sorts of rumors about the linguist's nationality and its activities in the world. This has the potential downside of confusing people.⁶³ Why, for example, might the linguist have a grant for \$100,000.00 if their share is only \$8,000.00? Why is the linguist staying in a hotel, spending *their* money instead of staying with a community member and paying the savings to the community? This is a very sensitive issue. It is not always a good idea to tell everyone everything about the finances. On the other hand, it is not a good idea to ignore the issue either.

An interesting example of the kinds of unexpected issues that can arise in this regard comes from a recent research project of mine. As is common nowadays, the funding agencies listed my projects along with the total amount of funding in each on their public webpages. The language community found these pages and read about the projects. During my next visit, the people were all concerned that I was profiting from the research because the total funds listed by the funding agencies greatly exceeded the amount to be received by the community. I had thought before this that it made little sense to explain things like indirect costs, overheads, postdoc salaries, secretaries, and university office supplies to the community. But suddenly, I needed to do this. So I did. Along the way, I explained to the community that I received nothing in salary at all for the research and that I would earn exactly the same if I never returned to their community or indeed if I never did field research again. This was important news to them, because they thought that I was able to make a living only because of their consent to work with me and that therefore they should be seen as my employers. This was a very new experience for me, because previous groups I had conducted research on, in the Amazon, would never have looked at the internet or questioned me in this way. This is why it is essential to consider each group anew and not to lecture the

⁶³ Many PhD dissertations in the US, like much of the early work on Transformational Grammar, was funded by branches of the US Military and the US Department of Defense. This is common knowledge. Although there apparently were no strings attached to these grants and though there was no connection I am aware of between the linguistics research of that time (1960s by and large), still, people in countries suspicious of the USA might want assurances that the funding behind the project have no connection with the US military. This is an extreme example, but not implausible.

community but to dialog with it, giving plenty of time and opportunity about issues of finance, etc.

d. The outside participants in the research: The community will want to know if there are any 'silent' partners in the research, i.e. postdoctoral research associates, professorial colleagues, graduate students, etc. that are involved in the research, but who never come to the community.

e. The potential for profit of the research: Some anthropological studies make a profit. Anthony Seeger's book, *Why Suyá Sing*, (Seeger ()) has sold thousands of copies and makes a reasonable profit, a large part of it (perhaps all, I am not sure) goes to the Suyá community. As we discussed my linguistics research and the publication of a grammar and dictionary on Suyá, I assured that any profits at all would go to the community but that it was unlikely that this would amount to much (ergativity is a less likely marketing tool than exotic music). (Barbara Kern and I gave all the profits of Everett & Kern (1997) to the Wari' community. I think that this amounted to a few thousand dollars. Not a lot, but at least something.)

f. Payment to the community and to individual language teachers: It is crucial that everyone involved in the research understands how much they can be paid and what the parameters are, if any, for salary increments, non-budgeted requests, etc. Agreements should be reached and put in writing or on video about all finances. There is a serious issue that occasionally arises when a community wants to renegotiate financial terms as the project goes along, but that is an issue that each fieldworker will have to confront on their own. The only solution to that is to reason with people and stand firm within the parameters of the research budget.

On the DVD of the excellent movie 'The Mission', one of the special features – which I have used in my classes on Amazonian languages for some time – includes the documentation of a dispute over pay and working conditions between the production company of the movie and the Waurana people who played the part of the Guarani in the movie. The people refused to work at a crucial juncture for the movie, in the last days of production, because they believed that they had been lied to. Apparently, what had happened with regard to the payment was that the production company had thought that it could pay the community a lump sum which could then be used or divided as the community saw fit. However, the community had not understood things quite this way and individual Waurana were expecting to be paid at the proportionate amount that had been promised to the community. Moreover, the production company seems to have thought that the Waurana were committed to the making of the film, as something benefitting indigenous peoples, when in fact the Waurana were concerned about getting back to their villages, to their fields, etc. all of which were much more important to them than the goals of the movie. Eventually an understanding was reached. Part of the problem is that no one in the production company spoke Waurana. Another problem was, in my opinion as someone who simply watched the DVD, that the production company, like many Westerners, assumed that the community had a socialist rather than individualist view of economy and wealth. Whatever the reasons, it is an excellent example of why it is vital to explain all financial arrangements with the people before more serious problems develop.

g. Personal gain of the linguist: The linguist will get something out of the research, e.g. tenure, a job, promotion, a raise, fame, a book, etc. It is important for the

community to understand these advantages to the linguist as well as possible. But it is equally important that no one have the idea that the linguist is going to get rich off the research or that the linguist's higher standard of living is a result of his or her work with this particular language community. As I said earlier, I have made the effort to explain to groups that I work with that my salary, income, job security, etc. does not depend on my fieldwork, at least not on my fieldwork in their specific community. It is important to explain this at times so that the people do not get the mistaken impression, as I have heard said of me, that 'You live on their backs', i.e. that the linguist is exploiting the people since they live relatively well and can travel, whereas the community that is perceived as paying his or her salary does not.

It is also important that the form of consent from the community to work on the project with the linguist is a form that they have access to. And this can vary quite a bit for field linguists from, say, field psychologists or anthropologists, since linguists often work with languages where there have been few previous studies and where the people are pre-literate. There is little point in getting a headman to 'make his mark' on a contract that no one in the community can read. What I usually do in pre-literate communities is to record their consent on video and then let them edit it and rerecord it until they have explained the conditions as they see fit. I then leave a copy of this with them, whether or not they have the means to play it. At least then they can find someone with the means to play the tape or DVD and make sure that it says what they think it says. And these recordings should include the linguist explaining the goals of the project to the community *prior* to their giving of consent.

TRAINING AND EDUCATION: See section __ above. The project must include a component of training for all community participants. This can, but need not be, spelled out in the agreement with the community. It is important to emphasize again, however, that no one 'size fits all' in this regard. Each different field situation will favor or require different training and education relations between the linguist and the community.

EMPLOYMENT CONDITIONS: The community and all language teachers must understand the conditions under which they are employed. When and for how long are they to work? How much will they be paid? Will this pay exceed the national minimum wage (I recommend that people be paid at least five times the minimum wage in the developing world or an above average wage in the developed world)? Do they have to leave their community? What rights for return do they have if they fail to honor the agreement on time outside the village? Other questions will arise in different local contexts, of course. Much care must be taken to avoid misunderstandings. And all understandings and agreements should be recorded in a community-accessible fashion.

ADVOCACY: The researcher will often be associated with community aspirations, at least in the minds of the community if not in their own mind. The community may want the linguist to help them raise funds for community projects, speak to the government about encroachments on their reservation, travel with them to negotiate with people or groups that impinge on the community's well-being in some way, etc. Pro-active application of ethics will motivate the fieldworker to serve as they are able as an advocate for the group. This can be a very important service as the linguist, a prestigious intellectual, for example, can offer that will be deeply valuable to and appreciated by the community.

INTELLECTUAL PROPERTY RIGHTS & CO-AUTHORSHIP: One issue that is becoming more and more important for all indigenous communities is ensuring that they control the applications of their knowledge and community outputs. Who owns the linguistic data in a text for example? Who has the right to prohibit or constrain the use of all data?

The answer is that the community controls all data. The linguist must not use any texts, examples, etc. without the permission of the community (though blanket permission can be given at the outset of the research, it is still appropriate to seek community permission for data in individual publications, at least data heretofore unpublished). And the community has the right to demand the return of all tapes, videos, transcriptions, and so on – all forms of their language gathered by the linguist – at any time. And they must be apprised of this right and this understanding must enter the written or videoed record of agreement between the fieldworker and the community. For further discussion and contemplation of this issue, I recommend a careful reading of the material in the websites mentioned above, especially the discussion paper by Correa (1999).

MEDICAL: This is discussed in __ above.

GOING NATIVE: Occasionally I meet linguists or anthropologists with the quaint idea that they can become just like any other member of the community. So if they see the people going nude to take their baths, for example, these people cheerfully skip to the river with the rest of the community, in all their European nakedness. Most communities are puzzled by such behavior. The fieldworker should show respect and not give offense. And they should adapt *to an appropriate degree* to the culture and behavior of the community, but they are usually expected to respect the simple fact that they are not one of the community and will never be exactly like them. There can be exceptions, of course, but it is important that the fieldworker enter the community aware that 'going native' can be and usually is both silly and offensive.

The chief of the Kisedje people, Kuiussi, raised this issue with me at the beginning of my project to study their language. He said "I don't want to see you or any of your people naked in our village. We go naked, this is our custom. But it is not your custom and it means something different to you than it does to us. And I don't want any of your group have sex with any of our group because you are not part of our group and we are trying to preserve our language, our culture, and our identity."

PERSONAL MORALITY: All the same ethical standards that would apply to the fieldworker in their home country apply to them in the language community. Moreover, to these can be added the standards of the language community and the laws and expectations of the country in which the community is located. 'Sex, drugs, and rock n' roll' are not what the fieldworker has gone to the community for. Most communities have extremely conservative values concerning personal dignity and morality. Some have more liberal standards. The general rule in this regard that I would like to suggest is that the fieldworker figure out the moral standards of the community and follow them closely, even if some members of the community appear not to do so.

INTERFERING: A final issue that I will raise here has to do with the fieldworker's potential 'temptation' to interfere in moral actions of the community. For example in a recent well-known case in Brazil, a missionary couple interfered with a community's practice of infanticide, by taking the baby that was about to be killed out of the village by emergency flight, causing some offense among some in the community (even though in this particular case the parents of the baby wanted it taken out of the community). This is roughly like an anthropologist studying US culture blocking access to abortion clinics. It may be a deeply felt moral conviction on the part of the fieldworker but it will always have extreme consequences.

Other issues like infanticide abound, though different issues will affect different people differently. For example, I have seen Amazonian Indians torture animals, roasting them alive, plucking out their feathers and chasing them around, giving them to little children to pull apart while they are still alive, etc. I have never interfered. I have been bothered by this many times and deeply. Perhaps another person would have interfered. I did interfere once when a group of men were about to rape a young girl in the village. Should I have done that? I felt that I should have at the time. And there were no bad consequences to anyone, though I had no way of knowing what the outcome would be.

These are hard choices and I have no wisdom to offer. But if practices regularly arise that so deeply offend the fieldworker that they feel compelled to intervene, then I suggest that the fieldworker is in the wrong line of work and should consider leaving at least this community and, likely, fieldwork altogether.

13.3. INTELLECTUAL OUTPUTS AND CO-AUTHORSHIP

The primary results of scientific research are publications, webpages, or other forms of public dissemination of one's findings. The decision as to who should share co-authorship (and thus blame or fame) for a particular article, book, report, etc. is a very important one. Should all language teachers automatically be entitled to co-authorship? Or if the linguist is helped in their living in the village, introduction to the people, or data-gathering by a third-party is this third-party (e.g. local missionary, government official, non-indigenous citizen of the country, etc.) entitled to co-authorship? This is a thorny issue. But I suggest the following rules of thumb:

() Guidelines for co-authorship credit

- a. Was the person essential to the research in whole or in part?
- b. Did the person make a major intellectual contribution to the research?
- c. Was the person actively involved in the writing of the piece?
- d. Was the person responsible for obtaining funding or the PI of the grant?
- e. Does the person want co-authorship?
- f. Does the person believe that they have earned co-authorship?
- g. Would it be intellectually dishonest to award or deny co-authorship?
- h. Did the person collect the data for your study?

Let us say that your entry to a community, your having a place to live in the community, and the people's willingness to help you learn their language are all the results of someone else's efforts. This would be deserving of credit in the acknowledgments to the research, but by themselves do not entitle the person to co-authorship. However, there could be additional factors, e.g. if they spoke the language and ran your experiments for you, served as third-party interpreters, or otherwise became essential to your data-gathering. Further, if during the course of data-gathering they offered suggestions that were crucial to the final shape of the research, then this would also cause re-thinking of whether or not they deserved co-authorship. At this point, the other factors would 'kick in'.

Did this person give you ideas, suggestions, and help intellectually without which the research could not have been conceived or carried out or had anything like its final form? Then co-authorship seems appropriate, subject to the other considerations in ().

If the person was actively involved in the writing up of the research (either by actually contributing original sections or by consulting with the linguist on many major

or subsidiary points), then, subject to the other constraints in (), this person should be considered for co-authorship.

If the person assisting the linguist is the actual PI of the grant then, for some people, this would automatically entitle this person to co-authorship. In some of the natural sciences, this is common. However, although I believe that PIs rightfully demand veto power over any research coming out of their funded projects, I do not believe that PIs are automatically entitled to co-authorship if this would violate (g), e.g. if they made no intellectual contribution to the particular research of the report.

It is also important for the fieldworker to know what a person (language teacher, government official, etc.) wants with respect to co-authorship. Do they believe that they have earned it and do they want it? Then, subject to (g), they should receive it. If it would be intellectually dishonest in the eyes of the fieldworker to award co-authorship, then perhaps it should not be offered. I suggest that in such a case the linguist contact someone from their home institution or at the journal or other outlet they plan to submit their research to for publication, to get advice. On the other hand, if the person feels very strongly, there could be cultural value differences involved, and likely are, and these are likely to have long-term effects on the researchers, the research, and feelings of exploitation. For this reason, I believe that co-authorship should be offered when the person feels they have earned it and they want it.

Occasionally it arises that one researcher collects data and does not analyse it. Then another linguist may find the data (openly and honestly with no violations of any ethical standards), analyse it, and publish the results. In such cases, does the original gatherer of the data deserve co-authorship? Not if the data are published. However, if the data are unpublished, then the linguist should contact them, ask them about the analysis that the linguist is proposing, asking if the collector of the data knows of any counter-examples or other problems in the linguist's analysis and then offer co-authorship, if the original collector of the data feels that they have earned it and if they want it.

It is always better to err, modulo intellectual honesty, on the side of giving too much credit than not enough.

12.4. THE GOVERNMENT

LEGAL ENVIRONMENT: In a new country, the fieldworker must not forget that the legal environment is not the same as in his or her home country. They must be sensitive and aware of local laws and customs and avoid anything illegal insofar as possible. This may require reading on the legal system, major laws, etc. and in discussing potential issues with friends from the country in question. Permissions and visas are discussed in __ above.

RELATIONSHIPS WITH NATIONAL SCHOLARS: The fieldworker should (and may be obliged to) develop close relationships with national scholars. It is important that the local scientific community know about the linguist, about the research being conducted, about the community's response to this, etc. There may be legal requirements to this effect in the country in question (as there are in South American countries, for example), but beyond the legal requirements it is simple professional courtesy to develop and cultivate relationships with linguists or related disciplines in the country of research.

12.5. MISSIONARIES

No matter where you decide to do field research, there is a good chance that in the course of your time in the field, you are going to come into contact with missionaries, especially Christian missionaries. The work of Christian missionaries is very controversial and most field researchers will have to decide how they are going to relate to them professionally and personally. The choice made in this regard could affect your relationship to the language community and to host country intellectuals and partners dramatically. Moreover, the linguist will also likely be forced to consider the ethics of the missionary enterprise whether or not they have ever considered it before and whether or not they want to consider it. So let us consider this here. To do this, for the potential field researcher who knows little of Christian missions, I will need to provide some background. This background will include generalizations based on my many years of experience in a missionary organization. Some of this discussion touches on uncomfortable issues of religious and political beliefs. But the discussion must cover these issues, I believe, in order to fully understand why missionaries and missionary work are controversial.

First, let me say that I began my career in Brazil as a missionary with what was then known as the Summer Institute of Linguistics (SIL). My assignment with SIL included the following responsibilities: (i) produce a solid descriptive study of the grammar of Pirahã; (ii) produce at least two hundred pages of translated, glossed, publicly accessible texts of the language; (iii) produce a dictionary of the language – as large as I could, but certainly one of at least 2,000 or so entries; (iv) develop a literacy programme in the Pirahã communities where every Pirahã man, woman, or child wanting to learn to read and write their own language could do so, subject to the permission and direction of the local community; (v) translate at least the New Testament into the Pirahã language; (vi) share my Christian faith, on an individual basis with Pirahãs, but without any proselytising, pressure, or church-related activities. The aim of the latter was to see an 'indigenous church' (one initiated by, managed by and formed doctrinally by the Pirahãs themselves). Had I been with another missionary agency working with Amazonian Indians, my goals might have been to found a church by preaching, baptizing, and never hesitating to let the people know when their culture violated 'God's law'. If I had been a Catholic missionary, depending on which 'order' or organization I belonged to, my goals might have been to baptize the people and see a chapel established for semi-regular visits to hear confession, provide communion, etc.

Often missionaries from one mission are suspicious of missionaries from other missions and across the Protestant vs. Catholic divide there is a yawning chasm of centuries of mistrust. So if you befriend or associate with one missionary or group, you may automatically, however unintentionally, alienate another group with consequences potentially adverse for your research.

Most Christian missionaries are motivated primarily by verses like the following:

Matthew 28: 19, 20

"Therefore go and make disciples of all nations, baptizing them in the name of the Father and of the Son and of the Holy Spirit, and teaching them to obey everything I have commanded you. And surely I am with you always, to the very end of the age."

Acts 1: 8

"But ye shall receive power, after that the Holy Ghost is come upon you: and ye shall be witnesses unto me both in Jerusalem, and in all Judaea, and in Samaria, and unto the uttermost part of the earth."

No matter how these beliefs emerge practically, they still are there and knowing about them can help understand missionaries better.

Whatever anyone may eventually conclude about missionaries, they are doing what they believe to be right. Many if not most of them are also motivated by love of their god and love of the people with whom they work. Contrary to most stereotypes, including the utterly silly portrayal of missionaries in films, e.g. *At Play in the Fields of the Lord*, missionaries are generally kind, reasonable on many issues, attractive, and loving, often with a deep satisfaction with Life and a sense of security that is many ways enviable. It is important to say this, because most of us find it easy to befriend and enjoy the company of such people, especially when otherwise isolated from our native language, culture, and countrymen. Moreover, in many, many parts of the world, the only medical, educational, economic, and political assistance available to the local community is from missionaries. Many missionaries sacrifice personal comfort, the company of their families and friends, their own career aspirations, their health, and material well-being (contrary to many popular stories, missionaries I know who work with Amazonian Indians are poor by US standards and live very simple lives materially) for the sake of the people to whom they 'minister'. Most missionaries that I know would die for the communities they believe they have been called to serve. This latter may sound melodramatic, but the emotions on this issue can run high and it is worth bringing out some of the depths of feelings that people have. Moreover, the members of the language community are likely to support the continued presence of the missionary because many of them will now be believers and because none of them will want the missionary to leave if this removes all the medical and other material support that the missionary has been providing. Of course, this latter source of support for the missionary is part of the problem. One reason that missionaries do 'get converts' in many tribal areas in my experience is because of this support. The missionary may use this support as both carrot and stick to convince people of their 'need' for the church and its doctrines (though certainly not all missionaries do this). The missionary is a powerful person by local standards and it is almost never the case that members of the local community are able to make fully objective decisions about what role to provide to the missionary in their community in the absence of alternative forms of medical and material support.

In this regard, many intellectuals have concluded that missionaries' 'retrograde' thinking (to use a common expression) is harmful for the peoples where they work, largely forced on them, overtly or covertly, and that the paternalism, condescension, and backwardness of much missionary thinking, should be stopped from reaching the language communities facing severe problems in the modern world, problems serious enough to threaten their very survival. And this fairly natural view is likely to be one that many readers of this book share.

The linguist must therefore be prepared for how they are to relate to missionaries. Because of the close relationship that often obtains between the missionary and the community, open hostility or rudeness is not a good choice. This is divisive for the community in almost every case, leading to potential antagonism between 'believers' vs. 'unbelievers' or, from another perspective, the 'rational' vs. the 'superstitious'. On the other hand, completely unguarded, unreserved friendship and

support of the missionary can lead to the linguist being classified as a missionary, since to many indigenous peoples with very limited experience with other nationalities, for example in Brazil, 'American' can already be a synonym for 'believer'. The default assumption is often that all those who share the missionary's nationality share his beliefs. And this assumption can extend to local government officials as well.

How then does one sort this issue out? First, be careful of generalizations. By and large, attempt to evaluate your potential relationship to people in the field not based on their occupation, religious beliefs, nationality, or culture, but based on your assessment of the person as a specific, *sui generis* whole. Second, maintain your distance and build relations very carefully. I suggest an initial attitude that I often urge on my junior academic colleagues when they first begin teaching, namely, 'maintain polite aloofness'. The practical application of this 'aloofness' will need to be worked out in individual cases. In some cases, for example, the linguist may be forced to choose between paying for space on a missionary plane to the language area, though this is likely to associate the linguist with the missionary, or travelling in by car or boat or on foot. Travelling to some of the peoples I work with, for example, this can mean a plane trip of one hour vs. a boat trip of one week. Alternatively, the linguist may have to choose between a local 'air taxi' vs. a local missionary plane. The former will almost certainly be more expensive and will often be less safe than the latter (missionary pilots and planes tend to be the best trained and maintained, respectively). Whatever the decision, the missionary presence presents the linguist with an ethical issue with stark choices over several issues, some clear, some nebulous.

In my very close connection over a period of more than twenty-five years with so-called missionary linguists, I can think of only a relative handful of missionaries who can in any sense be called linguists by training, interest, or motivation. But the 'handful' is not insignificant and these tend to be professional linguists by anyone's standards and quite unlike most missionary stereotypes I have encountered.

On the other hand, some missionaries do produce excellent linguistics. And no linguist has any ethical justification for either refusing to cite that linguistic research or to acknowledge its importance in their linguistics research (as I have known some more radically anti-missionary linguists to do). Moreover, in the case of SIL, the organization produces some of the best software for linguistics research available anywhere in the world, including fonts, data-management programs, electronic libraries, phonetic analysis, and so on. And some missionary-produced grammars and research is among the best in the world as well. It is unethical for researchers to use such software and intellectual output (including grammars and articles) from missionaries on the one hand, while on the other accusing the missionaries of not being linguists in any sense, as some are wont to do.

The world is complex and people are complex. Simplifications that involve complete vilification (or beatification) of entire groups is not only unethical (since it is the basis of prejudice), this kind of behavior leads to the worst (and most infantile) kinds of intolerance. This is not to say that people should avoid negative conclusions of this or that enterprise or organization and eschew any contact with it. Even so, it may be hard to avoid, say, the work on morphology by Nida () or the work on phonetics by Pike (), simply because they were doing missionary work at the time they wrote these books, although, according to Anderson (), exactly this has been done to some degree, by the way that modern linguistics has failed to acknowledge the debt it owes to these two missionary linguists.

Trying to apply an 'orthodoxy' test to writers before reading their writings on any subjects is akin to book-burning and has a long and sad history in Western culture that we have, hopefully, come to reject in modern society. That said, missionaries are at once people and an issue. There will be times that opposing them or befriending them could either be appropriate in particular circumstances.

APPENDIX ONE: WRITING FUNDING PROPOSALS FOR FIELDWORK

Writing a funding proposal is hard work. Funding is never guaranteed and always highly competitive. The work and long wait involved in a funding proposal will more often than not end in a rejection letter from the funding agency. So why should one subject oneself to this?

The reason is simple. You need money to do field research. And well-funded projects can produce better results than poorly-funded projects. In terms of your career, well-funded projects also bring money and student support to your home institution. They also are necessary to underwrite team research and to enable you to train new fieldworkers.

I have received as Principal Investigator (PI) or co-PI more than six million dollars (US) in funding since 1984, with an overall 'hit rate' of 70%. That is, 70% of all my grant proposals have been funded. I have received funding from the National Science Foundation, the National Endowment for the Humanities, the Fundação de Amparo a Pesquisa do Estado de São Paulo, the Arts and Humanities Research Council, the Economics and Social Research Council, and the European Union. This has not all been for field research, but it has given me a solid basis for the advice I want to offer in this section.

At the same time, as I once heard Peter Ladefoged say, I have been turned down for more money than most people ever ask for. The 30% of proposals that I have had rejected, however, have, due to useful feedback from referees and program officers, made me a better grant writer.

The first step in preparing a proposal is to identify an appropriate funding agency. Agencies differ widely in the kinds of research that they fund, the kinds of methodologies that they favor, the amount of additional personnel, indirect costs, etc. they will fund. And they differ in their constraints on the form of proposals. So before writing a proposal, familiarize yourself with the agencies most likely to be interested in your proposal.

All funding proposals will be evaluated by at least the following criteria:

- (i) Track-record of the PI – do they deliver what they promise? Are they active, publishing researchers? Is their work respected as of high quality?
- (ii) Evidence of preliminary preparation, e.g. contracts and permissions obtained ahead of proposed project start date, etc.
- (iv) Budget – is it reasonable, non-lavish, yet well thought out, covering all likely expenses (from batteries to hotels), etc.?
- (v) Criteria for success – how will the scientific community know when the project is a success? How and when will project personnel, including the PI, know this? How will the funding agency know this? What are the follow-up plans if things do not go exactly according to plans (they will not)?
- (vi) Deliverables – has the PI promised too little or too much from what they expect to learn from this project (publications, websites, blogs, community contributions, etc.)?
- (vii) Intellectual quality of the proposal – the reviewer wants evidence that the PI is fluent in his speciality and the matters to be investigated and that the PI has outlined an interesting problem that the referee would like to know the answer too (ideally) and has contextualized it appropriately within the field of study.

With regard to your track record, if you are a new PhD with few or no publications yet, reviewers will be willing to give you the benefit of the doubt, depending on how long it has been since you received your PhD and how well-written the current proposal is. The larger and more ambitious your project, the more reviewers will require from previous publications of the PI. Your track record, as mentioned, will also be extremely important. If the PI says 'We will publish the results of our findings in **Language**, **NLLT**, and **Linguistic Inquiry**', the readers will ask if they have ever before published in these journals.

It is also important that once the PI has identified a funding agency, that they contact the relevant administrative personnel of the agency to discuss their potential project with them to confirm that this agency is interested and to request advice on what the agency looks for in a successful proposal. Occasionally, it is even worthwhile to travel to the headquarters of the agency to meet the program officer in person or to the relevant professional meetings, e.g. the Linguistic Society of America annual meeting, where linguistic funding agencies and program directors often agree to meet with potential PIs.

It is also useful to bear in mind that for many agencies, at least in the USA, the submitter has the right to request that some individuals *not* be asked to review the proposal or that some individuals be requested to review the proposal. If you know someone hates your type of research (or, worse, hates you), then you should say simply 'I prefer that the following people not be asked to review my work.'

Appendix 2: Ethics statements from the Linguistic society of America and the Max Planck Institute for Evolutionary Anthropology

Linguistic Society of America: (<http://www.lsadc.org/>):

"Human Subjects in Linguistic Research

Studies of a human language often depend upon a continuing relation with speakers of the language. Such a relation comes to be defined as much by the speakers as by the linguist. Their patterns of life govern when work can be done. Their expectations, and those of their community, shape what is to become the results of the work. Understanding of the nature of linguistic inquiry grows in the course of the relationship. Sometimes lifelong friendships are established.

Such work must be conducted with respect for those who participate, with sensitivity as to their well being, and with concern for consequences of publication or sharing of results.

Certain considerations may make the study of a language different from much research in the sciences and social sciences. One asks many questions in discovering the features of the language, of a kind the collaborator learns to expect and even anticipate. They are seldom of a sort that can be disturbing or injurious. Moreover, fruitful work may depend upon the linguist learning and observing the norms of politeness and friendship expected by those with whom he or she is talking. Those who participate in such a work often do so with pride in their command of their language and may wish to be known for their contribution. Not to disclose their names would do them a disservice. Native Americans sometimes justly criticize earlier work with their language for not having adequately proclaimed the contributions of the Native Americans themselves. Fairness to speakers of a language is very much a matter of understanding their viewpoint, and what is appropriate in one situation may not be in another.

Such considerations make it difficult to apply general rules in a mechanical way."

Max Planck Institute (<http://www.eva.mpg.de/lingua/files/ethics.html>)

"The following guidelines are binding on all members of the department. They cover relations between the fieldworker and native speakers of languages under investigation and / or the communities in which those languages are spoken. They do not cover the following aspects that might nonetheless be relevant to the work of the department but which should be covered elsewhere:

a) Matters relating to scientific plagiarism and falsification. These are covered by the Max Planck Society's publication Rules of Good Scientific Practice / Rules of Procedure in Cases of Suspected Scientific Misconduct.

b) Matters relating to the collection of genetic samples. Where members of the department are involved in the collection of genetic samples, the ethics policies of the Department of Evolutionary Genetics must be followed. Researchers should note, with respect to point 1 below, that additional official permission to collect genetic samples will be needed in many parts of the world, in addition to permission to conduct

linguistic work.

c) Matters relating to work with children. The policies set out below must be followed as far as possible in relation to work with children, but other ethical issues relating specifically to the status of children must be taken into account, including but not restricted to the need to obtain the consent of their parents / guardian(s). Members of the department are required to adhere to the highest ethical standards in their research. In particular, they must show respect for the individuals, communities, and cultures with which they work. The following are designed to implement this general recommendation more specifically.

1. Members of the department must comply with all legal requirements for the conduct of their research in the relevant area. This includes but is not restricted to obtaining necessary visas, residence permits, and research permits, from both national and local authorities, and complying with requirements on the collection and dissemination of materials.

2. Members of the department must ensure that they have the informed consent of the individual(s) and community(ies) concerned to carry out the research in question and to disseminate the results of that research. In this connection, it is essential to note that consent must be informed. In particular, explanation must be given of the uses to which the material will or might be put and of the access that will or might be made available to the material. Under appropriate circumstances, individuals or communities may place restrictions on the use or accessibility of material, and such restrictions must be adhered to. Researchers will often have to rely on their own judgements as to which individuals or

communities must be asked for their consent, but such judgements should be formed in a responsible and accountable manner. Researchers should note that informed consent may need to be obtained not only from the source of material (e.g. the narrator of a story) but also from others who are affected by that material (for instance, persons who are mentioned in the story). Under no circumstances should individuals or communities be subjected to coercion to give their consent; researchers will need to be sensitive to local circumstances in this respect.

3. The agreement between the researcher and the individuals / communities involved in the research must be documentable, i.e. if a question arises as to the validity of the agreement the researcher must be able to produce evidence of that agreement. In some cultures and circumstances a written agreement will be appropriate, whereas in other cases some form other than a written agreement will be needed.

4. Especially given the increasing importance of intellectual and cultural property rights, individuals or communities participating in research should be informed that the institute and the researcher seek the right to store, use, and disseminate (with restrictions where appropriate) the material in question, but do not assert ownership of the intellectual or cultural materials entrusted to the institute or the researcher. When stored and disseminated, such materials should always make due acknowledgement to their authors and performers. Authors / performers should be named explicitly only where their informed consent to this has been obtained; otherwise, an anonymous acknowledgement is appropriate. It is appropriate for the researcher to pay the individuals involved in research for their time and travel and other out-of-pocket expenses. It is not appropriate to make payments that might be construed as payments for the transfer of ownership.

5. Members of the department must, wherever possible, ensure that they contribute to the communities in which they work. Exceptions to this policy can only be considered in

truly unusual circumstances where implementation of the policy is impossible, and such exceptions require detailed justification and the approval of the department director.

Contributions to the community would include but not be restricted to:

a) documentation of the language in a way that is accessible to the community, for instance through the preparation of primers or printed or audio recorded collections of traditional stories;

b) development of a writing system for the language;

c) training of members of the community in appropriate ways, for instance in text transcription, linguistic analysis, literacy, audio and video recording.

Appendix Three: Phonology Questionnaire

Caveat: This questionnaire is not intended to be even nearly exhaustive. It is simply a list of some suggestions from my own fieldwork as to what I have found useful. It is not really for beginners, however. I intend it to serve as a framework, a useful prod, for writing detailed phonologies of languages, something missing from most grammars (though the detailed phonologies of the OUP x series, edited by Jacques Durand is a useful model). Like all questionnaires, as Nikolaus Himmelmann has pointed out to me, this one has the disadvantage of asserting categories when in fact many of the categories themselves need to be argued for and established independently. So it certainly isn't meant to be followed slavishly. Hopefully, however, it will provide useful suggestions for the 'phonologically challenged' fieldworker.

1. Segmental phonology

1.1. List the distinctive segments of the language. Give rules of allophonic distribution. Summarize the (articulatory) phonetic realization of each segment.

1.2. What are the nonallophonic restrictions on the distribution of these segments? For example, do any segments appear exclusively in loan words? Are any subject to sociolinguistic or cultural restrictions (e.g. "Do not use /x/ in the presence of foreigners")? Are there differences in the segmental inventory according to gender (e.g. men use /s/ where women use /h/ or variation in points of articulation between women and men)?

1.3. Are some segments restricted as to which word class they may appear in (e.g. /b/ only in Nouns and Adjectives)?

2. Syllabic structure

2.1. What are the syllable types (e.g. CCV, CV, CVC, etc.)?

2.1.1. Describe any restrictions on syllable distribution. Are some syllables allowed only in word/phrase-final position (or medial or initial)?

2.1.2. Discuss the evidence for these syllables.

2.1.2.1. Phonotactics:

Are consonant sequences allowed? Where? Do allowable consonant clusters vary according to where they appear in the word (e.g. **st** only in word-initial position, but **ts** in word-final position)? Are there any restrictions as to the type of vowel/semivowel which may precede/follow consonant clusters?

Are there word-final consonants?

Are vowel sequences permitted? Where? Do allowable vowel clusters vary according to where they appear in the word (e.g. **ai** only in word-initial position, but **ia** elsewhere)? Are there any restrictions as to the type of consonant or semivowel which may precede or follow specific vowel clusters?

How many vowels or consonants may appear in a single cluster, if clusters are allowed? In adjacent vowels are there restrictions on vowel features (e.g. all the vowels have the same value for height, roundedness, etc.)? Are some sequences banned (e.g. **aa**)?

2.1.2.2. Phonetic evidence

Is there phonetic evidence in favor of syllables (e.g. chest pulses)?

2.1.2.3. Do native speakers segment words into syllables in slow speech?

2.1.2.4. Do phonological rules crucially refer to syllable structure, e.g. stress placement, nasal spreading, tone distribution, etc., as in (i) and (ii):

- (i) Stress the rightmost (C)VC or (C)VV syllable in the word, otherwise stress the penult?
- (ii) Lower tautosyllabic, adjacent high tones to mid tones in (C)VV syllables.

2.2. Interpretation of glides

2.2.1. Do semi-vowels, such as [y] and [w], appear in both or either syllable-initial and syllable-final positions?

2.2.2. If the language allows vowel sequences and semi-vowels, may the first vowel be [i] or [u]?

2.2.3. In vowel or semi-vowel sequences, are any of the following orders prohibited? Preferred? (X and Y are variables and thus may represent any segment type):

- X *iy* Y
- X *yi* Y
- X *uw* Y
- X *wu* Y
- X *yu* Y
- X *uy* Y
- X *wi* Y
- X *iw* Y
- etc.

2.3. What are the allowable sequences of segments within the syllable, according to their articulatory classification or generalizable acoustic properties? For example, are there ordering restrictions such as the following (just as a few suggestions):

(i) The onset of a syllable may begin with any consonant, but the second member of a complex onset must come from a more restricted class of segments (e.g. voiced continuant)?

(ii) In a complex nucleus, the first vowel must be a high vowel.

(iii) In a complex coda, the order of consonants is more (or less) restricted than in complex onset.

(iv) The order of consonants in the coda is the mirror-image of the order in the onset.

(v) etc.

2.4. If the nucleus contains a diphthong, can it also contain another vowel?

2.5. If the language has CVC syllables, can V be a diphthong? If so, are there any restrictions on the following C?

3. Tone

3.1. Does the language have contrastive pitches which distinguish lexical meanings of words?

3.2. Do contrastive pitches have a fairly constant F_0 or does their F_0 rise or fall or 'undulate' significantly?

3.3. If F_0 of pitches varies, yet is significant in distinguishing lexical items, does the variation correlate with position in the word, preceding or following segments, preceding or following pitches, or the word's position in the sentence or discourse?

3.5. Can consonants bear tone or only vowels? Which consonants? Under what circumstances (e.g. 'w and y bear tone following a rule of syllabification').

3.6. Does consonant voicing affect tone? How?

3.7. Does vowel quality affect tone? How?

3.8. Can tone patterns of individual words vary arbitrarily or do there appear to be *tonal melodies* assigned on words or classes of words (e.g. High Low Mid for one class of nouns, HLH for another, LHL, etc.)? Do the tonal melodies change according to the number of syllables?

3.9. What happens to a tone if its associated segment is deleted? For example, does the tone delete or appear on another syllable?

3.10. If the language does not allow contour tones (those with an underivable but constant change in pitch, e.g. rise and fall) on short vowels or sequences can they arise from morphological or phonological processes? Consider the Pirahã example in (i):

- (i) tii /isitoí /ogabagaí → tii /isito(ogabagaí
 I egg want

In the case of (i), in normal speech the direct object and the form form a close phonological unit, deleting a verb-initial glottal stop and the noun-final vowel. Notice that the tone does not disappear, however. In the example in (i) no mark over a vowel indicates low tone and the acute accent marks high tone. The wedge over the first /o/ to the right of the arrow indicates that it bears both a low tone and high tone simultaneously. This is in my analysis the result of the high tone remaining even after its original vowel host, /i/, has been deleted. This is otherwise prohibited in the language.

3.11. Can a tone ever *shift* to the right or the left in a word? Across words? Can one tone ever replace another, e.g. in (i) immediately above?

3.12. Is there complementary distribution among the tones, e.g. H → M/___ L

3.13. Are the frequency distances between tones (especially in a language with three or more tones) fairly constant or are some tones closer in frequency than others (e.g. tone Mid and tone High being closer in average frequency than tone M and tone L, in a three tone system)? Is frequency distance affected by how many different tone levels are present in a given word or phrase?

3.14. Is tone affected by phrasal intonation? How?

3.15. Does the language have other channels of discourse that exploit *linguistic* tone, e.g. whistle speech, drum communication, hum speech, etc. Please describe this in detail, as well as the social/cultural restrictions on its use.

4. Intonation

4.1. What is the most common intonational pattern (e.g. rising, falling, fall-rise, rise-fall, etc.) at the end of utterances?

4.2. How are different intonational patterns distinguished? By end points? By beginning and end points? By relative height of the entire intonational phrase? By beginning, middle, and end points?

4.3. What functions does intonation serve? For example, does it distinguish:

- syntactic phrasal types (e.g. interrogative, declarative)?
- illocutionary acts (e.g. indirect request vs. direct request)?
- other?

4.4. Is intonation affected by tone, stress, syllable patterns, or other phonological phenomena? How?

4.5. Does intonation affect tone, stress, syllable patterns, or other phonological phenomena? How?

4.6. What is the largest grammatical unit for which you can identify a distinct intonational pattern? Phrase? Sentence? Paragraph? Discourse?

- 4.7. Does intonation serve to unite two or more phrases in parataxis?
- 4.8. Can intonation mark subordination/superordination relations between clauses?
- 4.9. Are there *step accents* in the language, i.e. where the highest pitch of one intonational contour appears immediately prior to the stressed syllable, which itself bears a relatively low pitch? Are other correlations between stress and intonation placement observed? Describe these carefully, paying attention to the syntax, semantics, and pragmatics of the utterances as you do so.
- 4.10. What are the quantitative variations allowed in basic intonational contours? That is, can the same contour appear with more or less prominence by manifesting greater pitch distances between its distinctive points? When? What is the F_0 evidence like?
- 4.11. Is it more common for frequency to decline at the end of utterances? How many syllables or words are in the domain of this declination? Is there an accompanying rhythm (slow down, speed up, etc.)?
- 4.13. How can the different intonational contours be affected in their overall ranges of pitch, amplitude, duration, etc. by different ways of speaking, e.g. 'speaking up', whispering, etc.?
- 4.14. How is intonation manifested across different prosodic channels (e.g. whistle speech)?

5. Stress

- 5.1. Are some syllables in the language more prominent (for example, by using more acoustic energy, e.g. louder, higher pitch, longer, etc.) than others?
- 5.2. Do such syllables appear in every word?
- 5.3. Is this prominence predictable? How?
- 5.4. Are there different patterns of prominence on different classes of words, e.g. nouns vs. verbs (if there are, describe them)? Or is it constant across all lexical categories?
- 5.5. Are there secondary (tertiary, quaternary, etc.) stresses?

For example: *multiplication*
2ary 1ary

- 5.5.1. Do n^{ary} stresses occur at regular intervals? How are these intervals determined (e.g. every other syllable in the word from left-to-right, etc.)
- 5.5.2. Can primary or secondary (etc.) stresses ever appear on adjacent syllables in a word or phrase?
- 5.5.3. Does the stress of one word/syllable ever seem to move away from the stress of another word when it would otherwise be adjacent? Which of the otherwise adjacent stresses shifts, the one on the left or the one on the right? (e.g. *Thir'teen* + *'women* → *'Thirteen 'women*)?
- 5.6. Are 'heavy' syllables more frequently stressed than nonheavy syllables (e.g. (C)VC, (C)VV, vs. (C)V)? Under what circumstances, if any, can a lighter syllable bear primary stress if primary stress is normally restricted to heavy syllables?
- 5.7. What are the acoustic correlates of stress (e.g. loudness, pitch, length)? Are the correlates constants or variable across utterances or across speakers?
- 5.8. Do any (morpho)phonological processes interact with stress in a systematic way? What is the nature of this interaction (e.g. segmental lenition, voicing, vowel harmony, vowel reduction, etc.)?
- 5.9. If heavy syllables bear stress, what happens if the syllable-final consonant or vowel of the stressed heavy syllable is deleted?

- 5.10. If stress shifts for any reason, in which direction does it shift, leftwards or rightwards? Is its 'final destination' predictable in such stress shifts? How?
- 5.11. Does stress behave identically in longer and shorter words or utterances?
- 5.12. Is there any evidence of native speaker sensitivity to stress, such as correcting you misplacement of it, tapping on stressed syllables as they say the word (Ladefoged, Ladefoged, and Everett ()), etc.?
- 5.13. How does (or does) stress interact with tone? Does stress shift also cause tone shifts? Does stress placement perturb (raise, lower, metathesize, etc.) tones?

6. Morphophonology

- 6.1. Do affixes affect stress or tone patterns in words?
- 6.2. Do the affixes which do and do not affect stress (if there are such distinctions among affixes) fall into natural semantic, phonological, or morphosyntactic classes (e.g. syllable structure, inherent tone, prefixes vs. suffixes vs. infixes, derivational vs. inflectional, etc.)?
- 6.3. Do segmental rules (e.g. devoicing, assimilation, vowel-harmony, deletion, etc.) affect affix shapes? Which and how? Again, what are the differences between affixes which are affected vs. those which are not?
- 6.5. Does the language have clitics? (Like affixes, these are phonologically dependent on another word, never appearing alone. Unlike affixes, a single clitic can appear on a wide variety of word types, e.g. N, V, A, P).
- 6.6. Do these clitics appear in various locations within the sentence or do they cluster in a given position?
- 6.6.1. If clitics appear in different positions, does their placement depend on phonological (e.g. stress) or syntactic (e.g. a clitic must appear on the word to the immediate left of the word with which it forms a syntactic constituent. Consider English, where ()s = phonological boundary and []s = syntactic boundary: ([I][ʔl] (go)).
- 6.6.2. If clitics cluster in a given position, which clitics may cluster and where this takes place in the phrase or sentence (e.g. 'all clitics expressing tense and mood appear following the first constituent of the sentence).
- 6.7. Are some phonological processes peculiar to particular types of affixation (e.g. prefixation, suffixation, infixation, simulfixation, circumfixation, etc.)?
- 6.8. Is there reduplication?
- 6.8.1. Is reduplication monosyllabic, disyllabic, or larger or smaller (e.g. a single vowel, consonant, or mora)?
- 6.8.2. Do the consonant-vowel sequences in the reduplicated morphemes follow a constant order and shape for all reduplicated affixes?
- 6.8.3. Are there subregularities of CV order (e.g. CVC) for one type of reduplication and others (e.g. CVVC) for others?
- 6.8.4. Does reduplication interact with any other phonological processes, e.g. stress, nasalization, vowel-harmony, etc.?

7. Other prosodies

- 7.1. Do any other phonological elements take a domain larger than individual segments? Some possibilities are: aspiration, nasalization, labialization, voicing, vowel features, and so on.
- 7.2. Do such elements take larger domains only under certain circumstances? That is, can they 'spread' to surrounding phonological material?

7.2.1. In what direction can they spread?

7.2.2. What can trigger this spreading?

7.2.3. What can block this spreading, e.g. 'nasalization spread is blocked when it reaches a voiceless consonant).

Is there a minimal word size (e.g. no word in isolation can be less than two moras in length)?

REFERENCES

- Bickerton, Derek. 1990. *Language and species*. Chicago: University of Chicago Press.
- Chomsky, Noam. 1980. *Rules and representations*. New York: Columbia University Press.
- Chomsky, Noam. 1986. *Knowledge of language: Its nature, origin, and use*. New York: Praeger.
- Chomsky, Noam. 1989. "Some notes on economy of derivation and representation." (MIT Working Papers in Linguistics X, edited by I. Laka and A. Mahajan. 43–75.) Cambridge, MA: MIT Department of Linguistics and Philosophy.
- Darwin, Charles. 1859. *The origin of the species*. (Great Books of the Western World.) New York: Britannica.
- Donald, Merlin. 1991. *Origins of the modern mind: Three stages in the evolution of culture and cognition*. Cambridge, MA: Harvard University Press.
- Everett, Daniel L. 1985. "Syllable weight, sloppy phonemes, and channels in Pirahã discourse." (Berkeley Linguistics Society Proceedings, Volume 11.) Berkeley: University of California, Department of Linguistics.
- Everett, Daniel L. 1989. On floating feature nodes in Pirahã phonology. (Talk presented at the annual colloquium of Generative Linguists of the Old World.) The Netherlands: Utrecht.
- Everett, Dan, and Lucy Seki. 1985. "Reduplication and CV skeleta in Kamaiurá." *Linguistic Inquiry* 16:326–330.
- Fodor, Jerry. 1975. *The language of thought*. New York: Thomas Y. Crowell.
- Fodor, Jerry. 1983. *The modularity of mind*. Cambridge, MA: MIT Press.
- Fodor, Jerry. 1987. *Psychosemantics: The problem of meaning in the philosophy of mind*. Cambridge, MA: MIT Press.
- Gibson, Edward. 1991. *A parallel model of human parsing*. Unpublished dissertation. Department of Philosophy, Carnegie Mellon University.
- Jakobson, Roman. 1990. *On language*. Cambridge, MA: Harvard University Press.
- Longacre, Robert. 1964. *Grammar discovery procedures*. The Hague: Mouton.
- Marantz, Alec. 1982. "Re reduplication." *Linguistic Inquiry* 13:435–482.
- Rodrigues, Aryon. 1953. "Morfologia do verbo Tupí." *Letras* I:121–152. Curitiba, Paraná, Brazil.
- Tarnus, Richard. 1991. *The passion of the Western mind*. New York: Harmony Books.

References (seriously incomplete)

- Beckman, Mary E., 1986, *Stress and non-stress accent*, Dordrecht: Foris
- Chafe, Wallace L., 1994, *Discourse, Consciousness, and Time*, Chicago: The University of Chicago Press
- Croft, William, 1995, "Intonation units and grammatical structure", *Linguistics* 33:839-882
- Dixon, R.M.W., 1972, *The Dyirbal Language of North Queensland*, Cambridge: Cambridge University Press
- Donohue, Mark, 1997, "Tone system in New Guinea", *Linguistic Typology* 1:347-386
- Hayes, Bruce, 1995, *Metrical stress theory*, Chicago: The University of Chicago Press
- Heath, Jeffrey, 1980, *Nunggubuyu myths and ethnographic texts*, Canberra: AIAS
- Laver, John. 1980. *The phonetic description of voice quality*. CUP

Pawley, Andrew and Frances Syder, 2000. "The one-clause-at-a-time hypothesis". In Heidi Rigganbach (ed.). *Perspectives on Fluency*, pp. 163-198. Ann Arbor: U. Michigan Press.

Selkirk, Elisabeth O., 1984, *Phonology and Syntax: The relation between sound and structure*. Cambridge, Mass.: The MIT Press

Steedman, Mark, 2000, *The syntactic process*, Cambridge, MA: The MIT Press.

Trubetzkoy, Nikolaj S., 1939, *Grundzüge der Phonologie*, Prague (Travaux du Cercle Linguistique de Prague 7)

Yip, Moira, 2002, *Tone*, Cambridge: Cambridge University Press

Gussenhoven 2004

Güldemann, 2005 – Shona CHECK

Payne, Doris (ed.). 1990. *Amazonian linguistics: studies in lowland South American languages*. University of Texas Press.

Rappaport, Gilbert C. 2002. Numeral phrases in Russian: A Minimalist Approach, *Journal of Slavic Linguistics* 10: 329-342.

Zwicky and Anderson [from the second edition of the *Oxford International Encyclopedia of Linguistics*, to appear]

Japanese honorifics as emotive definite descriptions

Christopher Potts and Shigeto Kawahara

University of Massachusetts, Amherst

Salt 14. 2004

SALT XIV

Held at Northwestern University May 14-16, 2004

Edited by Robert B. Young 322 pages, Cornell Linguistics Circle (CLC)

The Independence of Syntax and Phonology in Cliticization

Judith L. Klavans

Language, Vol. 61, No. 1. (Mar., 1985), pp. 95-120.

On Linguistics and Politics

Noam Chomsky interviewed by Günther Grewendorf

Protosociology, Vol. 6, 1994, pp. 293-303

Dixon, R.M.W. *Searching for Aboriginal Languages : Memoirs of a Field Worker*

University Of Chicago Press; Reprint edition (July 15, 1989).

Chagnon, Napoleon A. **1996. The Yanomamo (Case Studies in Cultural Anthropology)**, Wadsworth Publishing; 5 edition

Conklin, Beth 2001. *Consuming Grief Compassionate Cannibalism in an Amazonian Society* University of Texas.

Margaret Mead. Blackberry Winter: My Earlier Years, Kodansha America; Reprint edition (April, 1995)

Darnell, Regna. 1990. Edward Sapir: Linguist, Anthropologist, Humanist, Univ of California Pr (February, 1990)

Darnell, Regna. And Along Came Boas: Continuity and Revolution in Americanist Anthropology (Studies in the History of Linguistic Sciences, 86) John Benjamins Pub Co (January, 1998)

References

- Dixon, R.M.W. 1972. *The Dyirbal language of North Queensland*. Cambridge: Cambridge University Press.
- Durie, Mark. 1985. *A grammar of Acehnese*. Dordrecht: Foris.
- 1987. 'Grammatical relations in Acehnese'. *Studies in Language* 11.365-399.
- 1988. 'The so-called passive of Acehnese'. *Language* 64.104-113.
- Lambrecht, Knud. 1986. *Topic, focus and the grammar of spoken French*. Unpublished Ph.D. dissertation, UC Berkeley.
- 1994. *Information structure and sentence form*. Cambridge: Cambridge University Press.
- Rögnvaldsson, Eiríkur. 1982. We need (some kind of a) rule of conjunction reduction. *Linguistic Inquiry* 13.3:557-61.
- Van Valin, Robert D., Jr. 1999. A typology of the interaction of focus structure and syntax. In E. Raxilina & J. Testelec, eds., *Typology and linguistic theory: from description to explanation*, 511-24. Languages of Russian Culture: Moscow. [available on RRG web site]
- Zaenen, Annie, Joan Maling and Hölskuldur Thráinsson. 1985. 'Case and Grammatical Functions: The Icelandic Passive'. *Natural Language and Linguistic Theory* 3.441-483.

RRG web site: linguistics.buffalo.edu/research/rrg.html

Blair, Frank. 1991. *Survey on a Shoestring: A Manual for Small-Scale Language Survey*, Dallas, SIL.

Wackernagel, Jacob (1892). 'Über ein Gesetz der indogermanischen Wortstellung', *Indogermanische Forschungen*, 1: 333-436. Reprinted in vol. 1: 1-104 of id. (1953, 1979). *Kleine Schriften*, eds. Kurt Latte (vols. 1-2) and Bernhard Forssman (vol. 3). Göttingen.

Notes