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Edited by
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Diana Forker

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CHAPTER 1

Word hunters

Unsung heroes of linguistics

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It reflects poorly on our societies that the contributors to this volume are not household names. In fact, these career-long linguistic fieldworkers are true heroes. Over decades, each has repeatedly stood up to physical, intellectual, interpersonal, intercultural, and sometimes political challenges in the pursuit of scientific knowledge. These scholar-explorers have enlightened the world to the inner workings of complex languages in remote communities of Africa (West, East, and South), Amazonia, the Arctic, Australia, the Caucasus, Oceania, Siberia, and East Asia.

In our discipline, as in others, scholars are lauded for research outputs, and very occasionally for their methodology. There is no public reward for heroism. The degree to which a linguist undergoes personal and professional risks to contribute to our field goes unacknowledged in journals. Only friends, students, and close colleagues marvel at the grit with which fieldworkers and their families suffer through malaria and other field-related maladies or attempt to balance personal lives with drawn-out travel (Macaulay 2012; Newman 2009; Newman & Ratliff 2001; Robinson 2013). Discussion of the intercultural skills of the linguist, usually honed over many years and with much effort, is relegated to the introductions of Festschrifts, if mentioned publicly at all. And the extra hours many fieldworkers devote to learning to speak the languages of their research win them few points in academic job hunts.

Fieldwork – taken here as *in situ* language research – is surely the ultimate all-around challenge that a linguist can voluntarily undergo. Intellectual challenge and puzzle-solving are compounded with the effort of taking copious, detailed notes from observations of language in context. On top of these challenges are the sometimes overwhelming ones that come from immersion in other cultures, relating to other people, and staying healthy and sane in unfamiliar, remote environments. Personal resourcefulness and creativity are called for in the highest quantities.

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Extended periods of linguistic fieldwork make one likely to be a *mensch*. We learn patience through adjusting our work hours to the needs of the field community; we learn forbearance by living in their houses. There is always someone who ridicules our best efforts at getting along or speaking the language, or helpfully points out our physical, mental, or personal shortcomings relative to the local standard. We ourselves are only too aware of these shortcomings at low points in the field, when the puzzle pieces seem not to fit together, or an analysis is just out of reach. We return to our universities from the field with tempered egos, lowered expectations, and renewed empathy.

Although linguists have, here and there, recounted their professional journeys, sometimes including fieldwork (Dixon 1983, 2011; Everett 2008; collections in: Davis & O'Cain 1980; Ellis 2016; Fernández et al. 2009; and Koerner 1991, 1998), never before have long-term linguistic fieldworkers reflected on their fieldwork careers all in one volume. We thus asked eleven of these linguistic heroes to recount their fieldwork career autobiographies. We stipulated that each chapter give both an overview of the linguist's fieldwork, and details of some linguistic and extra-linguistic experiences in the field. Beyond this and a word limit, however, contributors had *carte blanche*. It is not easy to sum up a lifetime of eureka moments and intercultural lessons in a single chapter; each contributor has chosen a slightly different strategy. Throughout the volume, these scholars' tenacity in pursuing linguistic revelations and love for the intricacies of language and culture are palpable. The tone of the autobiographies confirms their *mensch*-hood; these authors of scores of important books and journal articles do not crow. They recount mistakes and embarrassments alongside (and sometimes instead of) the discoveries that have cemented their scientific legacies.

Alexandra Y. Aikhenvald, an authority on both languages of the Amazon and of New Guinea (and on Modern Hebrew), writes about her fieldwork experiences through the frame of naming practices. When the Tariana and Manambu communities adopted Aikhenvald into their classificatory kin systems, they also bestowed her with names in their languages. Names happen to be highly salient in Tariana and especially Manambu cultures; the Tariana used special 'blessing names' in healing rituals, while for the Manambu (and nearby peoples) names are possessed by particular clans, and 'naming debates' are held to discuss rights to use them.

For Austronesian authority and historical linguist Robert Blust, linguistic fieldwork has been a means to acquire trustworthy data for historical linguistic reconstructions. Blust has collected data firsthand on 100 languages, both through fieldwork and *ex situ* consultant work. His chapter shows how a historically-motivated field linguist follows clues from one language to another to solve whodunits of language change.

African language scholar Matthias Brenzinger has done extensive fieldwork on numerous East and South African languages. In his chapter, he highlights special ways in which language correlates with worldview and with the particulars of lifestyle and topography. His notes on navigation in a vast savanna environment show his deep familiarity with the way of life of the communities with which he has worked. Brenzinger also discusses the importance of language activism as an extension of the field linguist's involvement with a language community.

Expert on Atlantic languages G. Tucker Childs describes his fieldwork career with a series of self-deprecating misadventure stories. Childs's chapter also reveals much of the flavor of the languages on which he worked, through, for instance, an alliterative language game he used to practice Kisi in the field. Childs's story of hearing clicks in !Xóõ conversation fade from audibility in a precise order as he walked away from a campfire gathering illustrates how serendipitous important linguistic findings can be in fieldwork.

Caucasianists Nina Dobrushina and Michael Daniel's chapter gives vivid insights into the Soviet collective linguistic field trips institutionalized by Aleksandr Kibrik. The couple met on a field trip to Daghestan and later led trips there themselves. They write candidly about bringing their children to the field, and about the emotional ties formed with their consultants. Like Blust's, their chapter also touches on a special subtype of linguistic fieldwork, in their case sociolinguistic documentation.

Expert on Arctic and sub-Arctic languages Michael Fortescue has pursued both in-depth description of individual languages and comparative projects. He writes of the joys of conducting fieldwork in Greenland and other parts of the Arctic, and some of the adventures involved in reaching remote communities. Fortescue's fieldwork directions, like those of Aikhenvald, Blust, Childs, de Vries, and Wise, have been shaped partly by the aim of genetic reconstruction and comparison within language families.

Knut Olawsky's career is unusual in that he not only pursued linguistic fieldwork on three continents, but also wound up working as chief linguist for a speech community in remote Australia. Olawsky began his fieldwork career in northern Ghana, continued in a very different community in the Peruvian Amazon, and has now worked for years in northern Australia, where his linguistic agenda is explicitly set by the speech community themselves.

Dargic languages specialist Nina Sumbatova, like Nina Dobrushina and Michael Daniel, was trained in and later led collective field trips to the Caucasus in the Soviet/Russian tradition. Sumbatova outlines some of the practical burdens of planning these field trips, including the issue of funding; she notes that many linguists in Russia today must self-fund some or all of their fieldwork. Sumbatova later worked to document dying Tungusic languages, and discusses how

depressing it can be to work with the last speakers or semi-speakers of a language. Finally, Sumbatova is now conducting fieldwork in Guinea, West Africa, meaning that she joins Aikhenvald and Olawsky in having worked on unrelated languages on separate continents.

Host communities have their own takes on field linguists. Pioneering scholar on languages of remote northwestern New Guinea Lourens de Vries frames his own chapter with the epithet used to describe outsiders in those communities: 'after-death demon'. De Vries sensitively describes his relationships with communities through the communities' own belief system.

Of all contributors, Mary Ruth Wise started in linguistic fieldwork the earliest. In 1951, after graduating from college, Wise recalls driving over 1,000 miles across the southern United States with "a carload of classmates" to begin linguistics training at the Summer Institute of Linguistics in Norman, Oklahoma. Less than two years later, Wise landed in a float plane at a village on the Palcazu River, Peru, learned her first Yánesha' word, 'buzzard', and began what would be a distinguished career in South American linguistics.

No one understands the challenges – and joys – of linguistic fieldwork like those who have also attempted it. This is likely one of the reasons that the sacrifices of career fieldworkers go unappreciated by much of the greater linguistics community, and beyond. We proposed this volume out of our own love for linguistic fieldwork and respect for those who have kept at it for their entire careers. We offer here our own mini-autobiographies as snapshots of younger Word Hunters.

Sarvasy

If I'd known about linguistic fieldwork as a child, I likely would have gravitated to it sooner. I loved languages from early elementary school, writing journal entries in Runic script and eliciting Indonesian words from a babysitter. I chose to go to Harvard partly because of the number of obscure languages offered there (Hittite being prominent among them).

I was a serious cellist into my twenties, but by the middle of my undergraduate years felt restive learning melodies from sheet music. My first experience with linguistic fieldwork coincided with my first experiences playing Turkish classical music by ear (in the Harvard Middle East Music Ensemble). I had studied Modern Standard Arabic and a bit of Moroccan Colloquial Arabic, and went to Morocco, I thought, to research urban Arabic women's folk songs. I ended up recording rural folktales in Tashelhit Berber instead, and learning some Berber grammar by working through the transcribed stories and consultants' explanation of them in Arabic. I continued analyzing the tales with the help of French colonial tomes in

Harvard's Widener Library. I found that learning a language through recording and transcribing texts, and working out the grammar for myself, was satisfying and effective in the same way as learning music by ear, rather than first reading it from sheet music, then memorizing it.

After graduating from university, I sought out a new type of challenge: physical survival in the wilderness. Indeed, my work for various conservation corps programs in the western United States was so all-around challenging (between running chainsaws and wielding picks, hoes, hammers, and wheelbarrows, and managing crews of either Spanish-speaking illegal immigrants or 'disadvantaged' young adults) that I barely had time to ponder other career paths. But when G. Tucker Childs advertised for a research assistant to document two endangered languages in the "mangrove swamps" of Sierra Leone, I leapt at the prospect of getting paid to learn languages and hike and canoe from village to village. I am grateful for my fieldwork apprenticeship to Tucker; as with the Soviet/Russian collective field trips (see Dobrushina & Daniel, this volume and Sumbatova, this volume), I was able to both contribute to the project and learn from an expert.

Working with the last elderly speakers of Kim and Bom was inspiring in many ways, and the whole experience was memorable. My routine on entering a new village was to set a time to interview all the purported speakers of Kim or Bom there, in turn. These ended up being very public displays of linguistic ability, attended by onlookers of all ages. I would elicit nouns and descriptions of actions through mime and in Mende, noting down both new words and how well the interviewee seemed to know the language. Early in our acquaintance, the late, great Fasia Kohlia breezed through such an interview; the assembled crowd applauded her more than once when she produced an obscure Kim term, such as 'Adam's apple'. Another time, in Bom country, I interviewed a leper without knowing she was a leper. When I reached the numbers, indicating that she should count (on her fingers), bystanders called out in Mende that she couldn't, since her fingers were knotted already from the illness. She followed me when I left the village, begging loudly for leprosy medicine. For years when I remembered touching the leper I examined the soles of my feet, for I had read that that was where the early signs of leprosy began.

In 2011, I began research on the Papuan language Nungon for my PhD dissertation. Papua New Guinea is a very different field site from both rural Morocco and Sierra Leone. In this case, I felt a responsibility to expand on my grammatical description of Nungon with a study of child acquisition of Nungon and now processing experiments on it. Nungon itself may face imminent language shift, and we have a narrow window in which to investigate everything possible about it.

Linguistic fieldwork combines many things I thrive on: independent language learning, puzzle solving, interacting with smart people of other backgrounds, immersion in other cultures, and living off the land in the most authentic ways.

Forker

My love for languages began at the age of eight when my mother proposed to send me to a special "Russian school" where one could learn Russian intensively starting in grade three. My grandmother was a Russian teacher and I was always fascinated by the books and brochures that she kept. I never did *in situ* fieldwork on Russian itself, but it would serve as my key to other languages spoken in the influence sphere of the former Soviet Union or nowadays the Russian Federation. During my undergraduate studies I focused on philosophy and logic. Linguistics, mainly in the guise of formal semantics, was only a side topic. I had not even heard of language documentation nor taken an introductory course to fieldwork.

During an exchange year that I spent at the Moscow State University with the primary aim of improving my Russian, I was introduced to Aleksandr Kibrik and his vibrant and thriving linguistics department. I learned about the field trips they organized in which students participated and about the Max Planck Institute in Leipzig with its own linguistics department. I had not known of its existence even though I was studying in Leipzig! At the same time I met by chance a Daghestanian girl with whom I share a name and age, among other things, and she invited me to come to Daghestan. I bought a train ticket from Moscow to Makhachkala although all my Russian friends had warned me that the Caucasus was a dangerous place and that I might be kidnapped. When I arrived in Daghestan and got off the train, I searched around and noticed that my friend was not there. But Caucasian hospitality is something you can really count on. So she had sent her sister-in-law to meet me and the four days I spent with her parents in the Caucasian mountains were enough to get me deeply fascinated with the Caucasus and its people and languages. After returning from Moscow I changed the focus of my studies to linguistics, in particular to everything related to language documentation and fieldwork. Until now I have mainly worked with two Daghestanian communities, Hinuq and Sanzhi Dargwa, each time with the goal to document the language and to produce a description, an electronic corpus and, for Sanzhi Dargwa, also a dictionary.

The first time I came to the field for my dissertation I asked around for people who knew traditional stories and fairy tales. I was brought to a man in his seventies who lived alone in an old house. He spent his nights watching TV and seemed to be happy about company. First, he tested my ability to write his language (Hinuq) with a short story. Apparently my knowledge of Hinuq – albeit rather scanty at that point – convinced him that it was worth the time, and during the next weeks I spent almost every late evening at his house, sitting next to him on an old bed and writing down Hinuq tales. We had to work at night because the local power station underwent reconstruction and electricity was only available during the hours of darkness. That worked perfectly because he did not have to get up early and

the quietness of the night fit the atmosphere in his house and our activity well. He turned out to be an impressively talented storyteller who was able to talk for hours. He wanted me to write down the stories immediately, so he told every sentence slowly, waited for me to write it down on my laptop and then corrected it. After a couple of paragraphs he provided a Russian translation. One night he said that he had made a mistake and that I had to cancel a part of what we had written down that night and the previous night. I regretted the lost work and lost time and decided to save the texts in another document. After three years of working on the language I took out the original text again to see what the differences were from his second version. It turned out that the first half of the canceled text was entirely identical to what he had told me later. Identical not only in the events portrayed, but identical in every word and sentence. So he not only knew the story, but had it word for word in his head.

Fieldwork has many aspects, many of them rewarding, others not only challenging but not very appealing. Fieldwork means breaking into what at the beginning seems like an endless stream of incomprehensible sounds and solving a never-ending number of puzzles. One advantage of being in the field is that you can come up with a hypothesis and test it immediately. If it is rejected, you try to modify it or develop a new one, and so it goes on. You search for patterns, rules in apparently messy data that explain the different constructions that you come across. At the same time it is also a personal challenge. It means to be divided from your usual environment, from your family and your friends. This is especially difficult if you have children or other dependents. You have to adapt to different food and probably lower hygienic conditions and will perhaps have difficulties explaining to people why you need some time to yourself. The confrontation with a different way of living and different views on life is something that I find particularly enjoyable and gratifying. It gives me the possibility to see my own life from a different angle. What I normally consider to be difficult problems and unresolved questions (such as "Will the paper that I submitted to a journal be accepted or rejected?" or "Will I ever have a secure position that allows me to feed my family?") lose a lot of their menace.

We now turn to the stories of those with many more decades of Word Hunting than ourselves.

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CHAPTER 2

The magic of names

A fieldworker's perspective

Alexandra Y. Aikhenvald
James Cook University

For Olga Kazakevich, a very dear friend who enticed me into linguistics

Alexandra Aikhenvald and her adopted family after the official launch of the Manambu grammar in Avatip Primary school (from left to right): Josephine Kamgau with her son, Lindsay Kamibau, the late Joe Apikuñ Luma, Paul Badaybag Kat, Alexandra Aikhenvald, Lucy Luma and Pauline Yuaneng Agnes Luma Laki

That firsthand knowledge of diverse languages is the backbone of any respectable linguistic work – and a *sine qua non* for trying to understand how languages work – has always been obvious for me. And what better way to acquire this

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knowledge than to do linguistic fieldwork – venturing into a new community, facing an unknown world, embracing difficult living conditions and trying to come to terms with new and unexpected linguistic structures. Working with a minority language, spoken out of the way of what we know as 'civilization', may be physically taxing – no running water (except in a river or a waterfall), no electricity, plenty of insects and diseases, tiring travel and further challenges. But what you learn is well worth the effort. In many ways minority 'out-of-the way' languages can be more expressive, more efficient and perhaps overall 'better' than familiar European ones (see Dixon 2016).

Ideally, linguistic fieldwork involves observing the language as it is used, on a day-to-day basis, becoming a member of the community and being treated like a family member. Such 'immersion fieldwork' allows the researcher to experience the language in its spontaneous use.[1] Participant-observation and what people say to you on a day-to-day basis may reveal something about the language that would have been easy to miss, should one concentrate just on recording and transcribing carefully planned narratives. During my first visit to the Avatip village in Papua New Guinea, I realized that a suffix which marks simple plural on some common nouns is used in a different meaning with personal names (we return to this in § 2.2). And there are many more similar examples – like serendipitous sparks which set you off on a path of further discoveries.

Immersion fieldwork means being there to observe and to learn, to be part of the community, and to be integrated into a kinship system with its network of obligations and reciprocity. And to be given a new name – one then gets a feeling of belonging, a status, and even extra protection. Names can be special in terms of their grammar. The intricacies of naming patterns, and the features of names – as experienced throughout my life as a fieldworker – are the main topic of this paper. I start with Amazonia.

1. The Amazonian names

'Sacred' names among the Tariana of north-west Amazonia have healing powers. What does it mean to obtain a traditional sacred name, and what is it used for? Before we turn to this, let me briefly introduce the Tariana, and the traditional

1. 'Immersion' is not always possible: as many minority languages fade into slow extinction, there may simply not be a community where they are used. I was not able to undertake any immersion fieldwork with Baré – I was lucky enough to work with Candelário da Silva, the last fluent speaker of that language. As a result, my grammar of Baré contains just the 'bare bones' of linguistic structure and little on interaction.

linguistic situation of the Vaupés River Basin linguistic area where the language is still spoken.

1.1 The Tariana of the Vaupés River Basin

In 1991, I started fieldwork with Tariana, a previously hardly described and endangered language of north-west Amazonia. Tariana is mainly spoken in two villages, Santa Rosa and Periquitos, and the neighboring mission center Iauaretê, in a remote area of the state of Amazonas in Brazil on the banks of the Vaupés river (across from Colombia). This is one of the major blackwater tributaries of the Upper Rio Negro, which flows into the River Amazon. (Blackwater looks like dirty dishwater, and is not very pretty, but blackwater regions do not have as much malaria and cholera as do whitewater areas). Out of a couple of thousand who consider themselves ethnic Tariana, only about 100 people still speak Tariana. Tariana used to be a continuum of dialects belonging to sibs. These were organized in a hierarchical order in agreement with the emergence of the ancestor of each sib from a hole around the Uapuí rapids (see Aikhenvald 2013). The speakers of Tariana with whom I have been working for more than 25 years belong to one of the lowest ranking sibs, the Wamiarikune.

Back then (and even more so now) language loss was a worry to many of the speakers – and this must have been the reason why the late Graciliano Sanchez Brito welcomed me to his family and his community. Since he was two years older than me, he adopted me as his younger sister. I immediately acquired a huge family – an asset to someone who is an only child and whose parents were only children. This included a plethora of Graciliano's brothers and sisters, and also his natural father and mother together with his father's elder brothers (his classificatory fathers) and his mother's sisters (his classificatory mothers). The Tariana have a classificatory kinship system (see Keesing 1975 for a general discussion, and Hugh-Jones 1979, for a snapshot of an East Tucanoan group in the Colombian Vaupés). So every Tariana is in a kinship relationship to every other Tariana, and to every other Indian within the Vaupés area.

The kinship system of the Tariana (similar to that of other indigenous groups of the Vaupés linguistic area) makes a distinction between cross-cousins and parallel cousins. Parallel cousins (children of parents' same sex siblings, e.g. father's brothers) are regarded as one's siblings, and cannot marry each other. Cross-cousins (children of parents' different sex siblings, e.g. mother's brothers) are preferential marriage partners. Just like in many indigenous societies, calculating the varied ways in which people are related is an intricate task, and a topic of many conversations. Establishing relationships and marrying the 'right' person has interesting repercussions for language – the main badge of identity in the Vaupés River Basin context.

The Vaupés River Basin is known for its language-based exogamy – one can only marry someone who speaks a different language and who belongs to a different tribe (this is called exogamy). People usually say: 'My brothers are those who share a language with me' and 'We don't marry our sisters'. That is, in the Vaupés area one's ethnic identity is inextricably linked to one's linguistic identity inherited through one's father. Incidentally, in this context it makes no sense to talk about 'mother tongue' – one identifies with the 'father's tongue'. As a consequence, the loss of one's language is a pitiful and, ultimately, a shameful thing. People who have lost their language run the risk of losing their group affiliation and of becoming 'like dogs' (*tsinu kayu-peni* (dog like-ANIMATE.PLURAL), in the words of Leonardo Brito, one of the three surviving Tariana-speaking elders) – that is, marrying their brothers and sisters. They are pitied as those who 'speak a borrowed language' (*na-sawãya na-sape* (3PL-borrow 3PL-speak), lit. 'they borrow they speak'). Hence the desire on the part of all the Tariana not to lose their language – or, nowadays, to 'learn it back'.

The other languages in this area belong to the Tucanoan family. Some are still spoken by a fair number of people. What is special about Tariana is that it is the only language from the Arawak language family in the Vaupés region. Tariana is related to the famous Taino, the first Indian language heard by Christopher Columbus when he arrived in 1492 at the central American island of Hispaniola (it became extinct less than 150 years later: see Aikhenvald 2002, 2003 about the family). The marriage network (discussed in numerous anthropological publications) only involves the Tariana and the East Tucanoan speakers. It is, however, not fully straightforward: for instance, the Tariana are not allowed to marry the East Tucanoan-speaking Desana, who are considered their 'younger siblings' – perhaps as a memory of distant historical relations now forgotten.

The Vaupés Basin is perhaps the most multilingual area in the world. In traditional times, each person knew several languages: their father's (which is the language they identify with), their mother's, their spouse's, and languages of other relatives and other members of the community. 'Mixing' languages and borrowing forms from other languages is not an acceptable practice. But the rampant multilingualism has resulted in the spread of grammatical structures which are more difficult to control.

Nowadays, what used to be a situation of stable multilingualism without dominance of one language group over another is rapidly changing: Tucano is the dominant language spoken by most indigenous people.[2] And Portuguese, the

2. See, for instance, Aikhenvald (2002, 2013, 2014a); the pioneering work on the Colombian Vaupés, by Arthur Sorensen, is only partly relevant since the situation in Colombia – where only East Tucanoan languages are spoken – is different from that in Brazil.

national language, is gradually gaining ground not 'just' as a lingua franca but also as a main means of communication in environments associated with 'white people' – including schools and local government. There are hardly any children who acquire Tariana as their father's language (see Aikhenvald 2013, for an up-to-date study). In a situation of encroaching language obsolescence every scrap of knowledge is valuable, especially that which has healing power.

1.2 The naming systems

Salesian missionaries have been established in the Tariana-speaking areas since c. 1925 (see Aikhenvald 2003; Cabrera Becerra 2002; and Brüzzi 1977 for a brief history). Every Tariana is a practicing Catholic, and was baptized and given a Portuguese first and last name. All the Tariana of the village of Santa Rosa have the family name Brito, and those of Periquitos are Muniz. The Portuguese given names are used to refer to people when they are absent, but hardly ever to address them. The given names are usually shortened. Most of these shortenings are at least partly predictable (and I had already known what to expect thanks to a mention in Brüzzi's 1977 ethnographical description of the Vaupés Indians). Jovíno becomes Joví, Rafaél – Rafá, Jacínto – Jací, Leonárdo – Leó, and Olívia – Óli. Graciliano becomes Gará, Glória – Goró, and Margarida – Magá, to avoid clusters (non-existent in Tariana).

Members of one's community – that is, one's family, by blood or by marriage – are addressed using kinship terms. In many languages of the world, kinship terms form a special grammatical subclass of nouns. One of the features of kinship terms in Tariana is the existence of vocative forms (a full list and a discussion are in Aikhenvald 2003: 69–70). Some are suppletive or involve irregular phonological changes: *nu-we-ri* (1SG-younger.sibling-MASC.SG) 'my younger brother' has the vocative form *nu-erí!* (1SG-younger.brother.VOC), and *nu-we-do* (1SG-younger.sibling-FEM.SG) 'my younger sister' has the form *nu-edú!* (1SG-younger.sister.VOC). The most regular pattern involves omitting the last syllable. This may lead to losing gender distinction – the vocative form of both *nu-phé-ri* (1SG-elder.sibling-MASC.SG) 'my elder brother' and *nu-phé-ru* (1SG-elder.sibling-FEM.SG) 'my elder sister' is *nu-phé!*

But one may have many elder brothers, or sisters. To make sure one addresses the right person, it is handy to have a vocative form of a Portuguese name – by either using the shortened form if it has word-final stress (Gará for Graciliano, Emí for Emílio and so on), or shifting stress to the last syllable (Olí for Óli, a shortened form for Olívia), or by omitting the last syllable of the full name – Leoná for Leonárdo (with the shortened form Leó used just for third person reference).

These irregularities were easy to learn, after I heard people talking to each other and mentioning others by name. They alerted me to the fact that person names are special in the language – and that they indeed do form a distinct subclass of nouns. Unlike other nouns, they do not take a plural marker (one simply cannot say **Maga-pe*, ?'Margaritas', despite the fact that there a few women sharing this same name), and cannot be possessed. Portuguese names are referred to as just *dipitana* (3MASC.SG+name) 'his name', or *dupitana* (3FEM.SG+name) 'her name' (the word 'name' *-ipitana* is obligatorily possessed and cannot occur without a possessive prefix). This is but one aspect of the Tariana naming system.

Talking and gossiping with my Tariana relatives, I soon discovered that each Tariana man has a nickname, called *na-pekaru-nipe ipitana* (3PL-mock/play-NOM INDEF+name), literally, playful names. This is usually the name of an animal, an insect, or of a part of one. So, Ricardo Brito (one of the three older speakers of Tariana still alive now) is known as *Ñamuritu* 'wild peccary'. The late José Manoel Brito, a knowledgeable elder, was *Ñewi* 'otter'. Names for younger speakers include *Hema* 'tapir' for Rafael Brito, *Hi:ri isipi* 'rat's tail' for Jacinto Brito, *Paitsi* 'frog' for the late Ismael Brito, *Kumada* 'duck' for José Luis Brito, and *Wirikaru* 'flycatcher' for Emílio Brito.

Some of these names are considered slightly comical, and some slightly offensive. Jovino Brito, my closest younger brother who has always accompanied me on my trips, was reluctant to reveal (for the satisfaction of others) his nickname *Kapatu* 'bodó-fish' (a fish that sticks underneath a canoe). I was strongly discouraged from using Crispiliano Brito's nickname *Ñaki* 'evil spirit', the late Juvenal's *Ka:siri* 'crocodile', and Marino Muniz's nickname *Inari* 'mucura rat' which they were said to dislike. Cândido Brito never revealed his nickname to me. Only some white men are given nicknames. Father Giacone was a Salesian missionary who did some work with a group of Tariana in the 1930s and then in the 1950. In the 1990s, a few old people still remembered him and told me that he was referred to as *Maliapa* 'whitehaired monkey' (Portuguese *macaco loiro*), because of his white hair. Giving him a nickname was described as *na-pia-nipe* (3PL-hide-NOM) 'secrecy, lit. their hiding' – as the late Ismael explained to me, this was a trick so that Giacone shouldn't understand when people were talking about him.

The nicknames are not used to address people – only to talk about them. They differ from personal names in that they have no vocative forms. Like personal names (and unlike their common noun homonyms), they cannot be possessed or take plural markers. Women have no nicknames – so I didn't get one.

All the Tariana have a further set of names – traditional personal sacred names called 'names of blessing' (Tariana *pa-ñapa-nipe ipitana* (IMP-bless-NOM INDEF+name) 'blessing name', Tucano *basé'ke wame* 'spirit name'). I knew of their existence among the peoples of the Vaupés from Father Brüzzi's anthropological

study (1977: 378–380), and then from Father Casimiro Bekšta. This highly knowledgeable Salesian missionary – moved away from the Vaupés area to Manaus in the 1970s because of his extensive interest in the indigenous culture and languages – also alerted me to the existence of a special set of 'blessing' names for each clan group of the Vaupés people. I asked Graciliano, his father Cândido and his brothers about the 'blessing names'. They proudly confirmed that Brüzzi was right, and that the Tariana – both men and women – still have blessing names. So do many other peoples of the area, including the Yuhupde (Cácio Silva, p.c.).

The blessing names are not secret – though people tend not to reveal them to outsiders (this was noticed by Brüzzi 1977: 379). I was given the full list of Tariana names with a comment by Graciliano: 'you are a Tariana woman, you need to know'. This covers four generations – Cândido, his grandfather, father and father's brothers (there were no sisters), and all his biological children and grandchildren. (Children of Tariana women are not 'named' in Tariana, because they belong to a different language group and do not count as Tariana.) This was as far as they could remember (no one could tell me the name of their great-grandfather, referring to 'generation beyond grandfather' as *payape-seni* (old.time-HUMAN.PLURAL, 'old-timers'). And Graciliano and others encouraged me to publish the list – which I did, in Aikhenvald (1999: 36–37), 'so that our children should know'. They were dismissive of those who didn't know their blessing names.

As befits a Tariana woman (*Taria i-sado*), I was given a blessing name, *Kumatharo* 'female duck'. Jovino said that it was a good name for me – since I fly so much (it is indeed a long flight from Australia to Amazonia). And then something strange happened: Graciliano, Jovino and Cândido decided to bestow blessing names on my son (who cannot be a Tariana) and on my partner, Bob Dixon. My son Michael was given the name of *Tuiri* 'pied crested oropendola', and Bob was named *Serewhari* 'lilac-tailed parrotlet'. These names will protect you all, you all fly a lot, said Graciliano.

Blessing names are given to children when they reach the age of about three. They are not used to address people, nor to talk about them. Their main function is for spells and blessings, especially when a person is sick. I experienced this myself. In 1999, I developed an ugly boil (due to exposure to the merciless Amazonian sun on our lengthy boat trips to the village of Santa Rosa), and Cândido, who had some shamanic powers, decided to 'bless' it. An alternative term for 'blessing name' is *na-phya-nipe ipitana* (3PL-breath-NOM INDEF+name) 'name of breathing'. This reflects the way in which blessing is done – by breathing onto the person, or the wound. As Cândido was breathing onto my wound, he was whispering some words – the only one I could make out was my blessing name, Kumatharo. The blessing must have worked (aided by a generous dose of antibiotic powder) – the wound faded, leaving a hard-to-notice scar.

Blessing names are a separate subclass of nouns, and in many ways are similar to personal names: they have vocative forms, they cannot be modified, and do not form plurals (see the relevant section of my Tariana grammar: Aikhenvald 2003: 70–71). Their vocative forms are typically derived by omitting the last syllable of the full name and shifting the stress to the new last syllable, e.g. referential masculine name *Túiri* – vocative *Tuí!,* referential female name *Bálida* – vocative *Balí!, Anasádo* – *Anasá!* This technique is similar to the most productive way of forming vocatives on kinship terms, and on Portuguese personal names.

Blessing names differ from Portuguese personal names and from male nicknames in that they can occur in just one kind of possessive construction. Spouses address each other, as 'father or mother of X', X being the blessing name of the couple's first-born son (but apparently, not daughter). (This practice is known as 'tecnonymy'). The first-born son's blessing name would then appear without the last syllable but with no stress shift. The late Ismael Brito was Cândido's and Maria's first-born. His blessing name was Túiri. Cândido would address Maria as *Túi hado!* 'mother of Tui(ri)'. She would address him as *Tui haniri* 'father of Tui(ri)'. Note that the vocative address form for 'father' is *paí!* and for mother *na!* (Aikhenvald 2003: 71) so these tecnonymic combinations are quite unusual. I am still not sure as to how spouses would address each other if there had been only daughters in the family, or if they were childless – when I asked, Graciliano curtly remarked that 'we do not have such people in our village'.

No one, including the late Américo Brito, the oldest speaker of the language who remembers eyewitnessing the Offering Rituals before the advent of the Salesians in 1925, knew anything of the name-giving rites (described in some detail for all the Vaupés peoples by Brüzzi 1977: 378–379, and partly based on the remarkable work by Ermanno Stradelli 1929: 537). I was just told that blessing names were given by the father or by a shaman. But on what principles? Graciliano and with him Cândido repeatedly said that it was just what the father would have chosen. On my way back to Australia, passing through Manaus, I asked the same question of Father Casimiro Bekšta. He said, 'no, they don't know, the names are given by the order of birth'. It was not until 2012 – after both Graciliano and Cândido had passed away – that Cândido's younger brother Leonardo, now one of the three remaining elders among the Tariana, said to me, as a matter of fact, that many blessing names were indeed given by the order of birth. For instance, Tuiri refers to a first-born male, Balida to a first-born female, and so on. However, a number of unexplained exceptions remain.

Most of my brothers' children do not know their blessing names. None of them speak the language, nor understand it. But they did show interest in looking up their own blessing name in the list in my book *Tariana texts and cultural context* (Aikhenvald 1999). And they are happy to learn them. Graciliano's eldest son,

Rosimar, exclaimed (in Portuguese): 'Oh, my name is Tuiri! I went to see a shaman (*pajé*) when I was sick, and he used another name, so this is why what the shaman did to me didn't work (*não pegou*)'. The power of names is still there, even if the language is on its way out. I am grateful to my Tariana family for allowing me to document this knowledge now appreciated by their descendants.

2. The Manambu of the East Sepik Province, Papua New Guinea

I moved to Australia (as an Australian Research Council Senior Research Fellow) in 1994. My project was to work on the typology of genders and classifiers. As I went deeper and deeper into the types of systems, I realized that I needed to learn more about shape-based genders in the languages of the Sepik region of Papua New Guinea. Fortunately, Alan Rumsey was running a Field Methods course at the Australian National University (where I was then based). The consultant was Pauline Agnes Yuaneng Luma Laki, a most competent expert in the language and the lore of the Manambu of the East Sepik province.

Manambu didn't have a comprehensive grammar or detailed documentation of any sort – and so I embraced this new and exciting enterprise (see Aikhenvald 2008: 27–28 on previous attempts at describing the language). At that stage, Pauline's husband James Laki, then a lieutenant-colonel in the PNG army, was doing a degree at a military academy in Canberra. Pauline – a professional journalist who had taken time off her job in the public service in Moresby to be with her husband and look after their three children – started teaching me Manambu, recording stories, and telling me the ways of the people and their behavior. Pauline adopted me as a younger sister (*ñamus*). Similar to the Tariana, the Manambu have a classificatory kinship system, and so I immediately acquired a plethora of relatives across the Middle Sepik – not only the Manambu, but also the Iatmul, the Yalaku and many others. Pauline has an impressive grasp of every aspect of Manambu – knowing more than a woman is expected to (men in the village accept this, and often refer to Pauline if they do not know something themselves).

The importance of personal names to Manambu people is a well-documented fact in the anthropological literature: the main source on Manambu, by Simon Harrison, is entitled *Stealing people's names* (1990). Personal names are a key to the culture and the everyday life of many peoples of the Middle Sepik. They are the source of power, and tokens of personal wealth. One's claim to land ownership is traditionally linked to knowledge of names of people, of landmarks, and of objects associated with different clans. Harrison (1990: 59) estimates that a knowledgeable initiated man in the Middle Sepik area would master thousands of names

(resonating with Gregory Bateson's 1958: 22 estimate that an erudite Iatmul man 'carries in his head between ten and twenty thousand names').

Pauline's status within the community and her spiritual wealth are reflected in the number of traditional personal names she has (Manambu *təp-a-sə* (village-LINKER-name) 'village name') – Yuaneg, Pakeneber, Iramanjau, Nyam, Bebeywagurakineber, and Bebeypasekineber. She then gave me a name, *Ñamamayrata:kw*, a 'strong' name of an ancestral totemic woman from her paternal clan called Maliau. This sealed our relationship – from then on she became my name giver and protector, in the Manambu context.

2.1 A multitude of names

Manambu is a member of the Ndu family (together with Iatmul, Yalaku and a few others). The language is spoken by about 3000 people in five villages in the Ambunti district in the East Sepik province: Avatip, Malu, Yambon (or Yuanab) are the three major ones which appear on many tourist maps of Papua New Guinea; two smaller ones are Yawabak and Apa:n. Pauline comes from Avatip (*Ap-a-təp,* bone-LINKER-village, or 'main village'). In 2001 Pauline and I traveled to Avatip together for the first time (after I had developed some fluency in the language).

This was not my first visit to Papua New Guinea. Even so, I was overwhelmed, from the moment we disembarked in Avatip. Everything was so different from what I'd been used to in north-west Amazonia. Even the canoe we traveled in was different. The Tariana canoes have benches. The dugout canoe with an outboard motor which met us in Ambunti (a local mission center) to take us to Avatip had nothing of the sort. Women are supposed to sit on the bottom of the canoe, while men sit on its rim, or stand, steering. And then came tropical rain – it was the beginning of the wet season. Pauline cheerfully remarked that we would dry up very quickly, after the rain had stopped. My first thought was – we will, but what about my recorder, the tapes and the notebooks? I wrapped them up in my rain poncho – hoping they would survive. They did (and I spent the next month coughing – this allowed me to quickly learn many names for diseases).

And so started my new life in a new language. A major shock came with the names – how to call people? The first person I was introduced to was Jacklyn, my classificatory younger sister. Jacklyn was to take care of me. I stayed in the same house and made friends with her little girls. I knew that I was to address her as *ñamus* 'younger sibling', and she would address me as *ma:m* 'older sibling'. But things were not that simple. Besides kinship terms, people freely use personal names. My newly acquired younger sister was constantly called *Maliyooo!* No one called Jacklyn Jacklyn. I ran to Pauline for an explanation. Pauline replied that

Jacklyn was her *wali-sə,* a 'white person name'.[3] And that Jacklyn has a *təpa-se,* a village name, *Yuamalimæg* or *Yuamaliwæg* which I was to use. But nobody else did – all I heard was *Mali* or *Yuamali*. I asked Jacklyn herself – and was told that Yuamali was a shortened version, and Mali was an even shorter one. And *Mali-yooo!* was a vocative form shouted from some distance.

Having a smorgasbord of names to choose from should not have been so surprising to me, with my background. My first name is Alexandra. Back in the Soviet Union, some people would respectfully address me as Alexandra Yurievna, using my patronymic (so everyone knows I am the daughter of Yuri Aikhenvald). I am known as Sasha to most colleagues and friends; some endearingly call me Sashka or Sashenjka or even Sanja; a dear friend and colleague from Peru calls me Sachita, and for a friend with Georgian background I am Sashiko. Some linguists in the former Leningrad (now St. Petersburg) used to refer to me as Shura (a legitimate shortening from Alexandra); my father would occasionally use an abbreviation Sushka, or Susha. And I was aware of potential confusion. Once, at the height of Brezhnev's oppressive regime, a Danish Semitologist came to see Alexandra Aikhenvald at the Institute of Oriental Studies in Moscow, to talk to me about Hebrew studies (my then speciality). The Institute was a highly politicized entity where we, purely linguistic scholars, formed a little niche community. At the end of our conversation (in English), he asked me if I, by any chance, knew where to find Sasha Aikhenvald for whom he had brought some prohibited literature from Boris Vail, a dissident exiled from the USSR a few years earlier. I couldn't help but giggle – what a stupid foreigner! And now, in the Manambu context, I was one, too.

Slowly I started taking in the ways people use their names, and their individual preferences. Mali (Yuamali, Yuamaliwæg, Yuamalimæg, or Jacklyn) is always addressed and spoken about as Mali – but only by the Manambu people. Since 2001, Mali has been the tower of strength in the village and my main point of reference. Despite having two, and then three, children, she has always been there to help with everything – including telling stories, transcribing them, and explaining grammatical points. In 2014, she made a point of accompanying me to my new field language, Yalaku – spoken away from the Sepik river, firmly insisting that those 'dry-land people' (*nebe-du*) will not know how to look after a white woman, especially a Manambu one. For the Yalaku she was just Jacklyn, same as for the few white visitors and the Iatmul relatives of her husband Luc. The village name was like an in-group token.

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3. The term *wali* is fascinating in itself – it refers to 'eastern wind', the totemic territory (*wa:gw*) owned by representatives of the Sun-moon clan where white people are also adopted.

It was handy for me to have a village name – everyone came to know me not as *Sasa,* but as *Ñamamayrata:kw*, or *Ñama*, for short. My relatives would come in and tell me what relations we were to each other, and how 'strong' my name was. A few times, when we traveled in a canoe along the Sepik and its smaller tributaries, someone from Avatip would sail by and ask Mali: 'Who is this white woman?' Mali would reply, 'She is not a white woman, she is Ñama'. 'Oh, Ñamamayrata:kw!' was their reply – and then followed a brief summary of how we are related. Having a name was like a naturalization certificate – my white woman status faded into the background, now that I had a real village name.

The traditional names are pretty long, and they would often be shortened. So Pauline can be referred to and addressed with her full name Yuaneng, or shortened to Neng. The late Yuawalup was my classificatory elder sister. We were particularly close because she had developed vitialgo, a disease which leaves one's skin discolored and almost white – we were effectively the only two white-skin women in the village, and could offer each other sympathy as to how hard it is to protect our white skin from the merciless Sepik sun. Many people called her simply Walup. When I first met her, she introduced herself as Patricia.

Not every name can be shortened. Yuakalu, or Joel, Pauline's (and my) younger brother, and a former minister of road services under Sir Michael Somare's government, is rarely if ever referred to as Kalu. Simon Harrison, the anthropologist of the Manambu, was given the name of *Yuaseseng* but no one ever referred to him as *Seseng*.

People have preferences as to how others call them. Yuawalup didn't mind being referred to as Patricia. My maternal uncle Badaybæg is referred to as Paul (with his white name) or as Baday. But my sister-in-law, Damel (or Dameliway) – the one who taught me the rules of a joking interaction between sisters-in-law – resents her 'white name', Leona, saying 'it is not me'. Many children and young people prefer their white names and do not even know their village names.

The village names are precious: they are a property of each subclan. This is what we turn to now.

2.2 The Manambu clans and naming system

The Manambu divide into three clan groups: the Wulwi-Nyawi, associated with sun and moon, and everything bright; the Gla:gw (the 'blacks'), associated with earth and jungle, and everything dark; and the Nabul-Sablap, the in-between clan group. Clans are exogamous, with Gla:gw and Wulwi-Nyawi marrying each other, and each marrying the Nabul-Sablap (see Aikhenvald 2008: 11; Harrison 1990: 70–73). Clan allegiance is inherited through one's father. Every subclan owns a set of personal names: the knowledge of names and of one's ancestors who

bore it is considered tantamount to material wealth: for instance, ownership of a piece of land can be proved or disproved on the basis of knowing the name of the ancestor who lived there. Competent orators would know names more than fourteen generations back.

Using a name which belongs to a different clan or subclan is akin to theft. When this happens, the elders organize a name debate (*saki*), with orators from each subclan arguing for the subclan's totemic ownership of the name. In October 2004, I witnessed such a name debate, and was allowed to sit with men, take pictures and record (for this purpose, a white woman was different from other women). The object of dispute between the Sarak and the Wagau subclans of the Wulwi-Ñawi clan group was the name *Kiginəbək*; this name and its feminine equivalent *Kiginəbəkəbər* was won by the Wagau clan. The Sarak clan was awarded another name, *Kəgidəmi* and its feminine counterpart *Kəgidəminəbər*, as a 'compensation' (see a description in Aikhenvald 2008: 14–15; and Harrison 1990 on earlier name debates).

I was told by Pauline Laki and other Manambu that a child is given a name, from their paternal subclan, after it is a few months old and is expected to live (confirming Harrison 1990: 60). The first given name is given by the father (if the father is knowledgeable enough) or by senior initiated men in the community. This is how a child acquires their *ap-a-sə* (bone-LINKER-name) 'the main name (lit. bone name)'. This can also be referred to as *ta:y-sə* (first-name), and is the main name the person is known by. Throughout their life, a child may receive more names known as *səgliak* 'non-main name'. Some of these will be given by their father's family; some by matrilineal relatives – especially maternal uncle – as a sign of bereavement: these used to be called *kəpa-kur sə* (dirt-take name), associated with the traditional custom of smearing oneself with dirt during the mourning. The more names a person has, the richer they feel. A special relationship exists between those who share the same name (*wasali*): they are like siblings and are expected to help each other.

I was happy enough to have my *apa-sə,* the main name, from the Maliau clan (Pauline's father's clan). Then, in September 2013, Pauline and I organized a launch of the Manambu grammar, texts and dictionary at the Avatip Primary school. The launch attracted crowds. I made my first public speech in Manambu and was, perhaps, visibly exhausted by the strain and the roaring noise of children and adults. And then Paul Baday, my classificatory maternal uncle, came up to me and gave me the name *Ap-a-ga:j* (bone-LINKER-rooster), literally, bony rooster ('bony' in Manambu is equivalent to 'very thin'), adding that I needed it because I looked sick. The name was very appropriate because my classificatory mother (that is, Pauline's mother) belonged to the Nabul clan, and chickens and roosters are their totems (we turn to this in § 2.3). I felt very rich – I now had two village names!

Personal names in Manambu form a special subclass of nouns. Similarly to what we saw in Tariana, they cannot be pluralized or possessed. They can occur as postnominal modifiers in appositional noun phrases, as in *wun-a ma:m Yuanəng* (1-LINKER+FEM.SG elder.sibling Yuaneng) 'my elder sister Yuaneng'. But unlike any other noun in the language, they can form the associative plural. If the suffix *-bər* is attached to a personal name, it then refers to that person plus their family or associates, for instance, *Mali-bər* is 'Mali and her family', and *Leo-bər* 'Leo and others'.

That associative plural can only be formed on personal names is not that unusual: this was predicted by Moravcsik (2003) in her typological study. I came upon associative plural in Manambu by chance. I knew that the suffix *-bər* in Manambu has another meaning: it is used as a plural marker on a handful of kinship nouns (Aikhenvald 2008: 132–133): for instance, *amæy* means 'mother', and *amæy-bər* means 'mothers, classificatory mothers'.

During the mortuary ritual ceremony, we were sitting in Damel's house in Avatip waiting for Leo Yabwi Luma (Pauline's elder brother) and his family to come and bring some drinks. One of the mourners asked: *Leo-bər*? This is a normal Manambu way to ask about the whereabouts of a person, by just using the name. But why plural? We had only one Leo in the community. I incredulously repeated the question. Then Damel explained: she is asking about the whereabouts of Leo, his wives (he had two), and his children. I pointed at Mali's daughter Tanina who had just come in in the company of her younger sister and her cousin, and asked: *Tanina-bər*? Damel nodded in agreement. From then on, more and more examples came up – confirming that *-bər* has different meanings with kinship terms and with personal names (no matter what their origin is). All of the examples come from participant-observation; there is not one example in my corpus of Manambu stories (more than 30 hours of recordings).

Personal names of Manambu origin stand apart from other nouns in one further way. Manambu has two genders, feminine and masculine. They are covert: the form of the noun tells us nothing about the gender it belongs to. It is the form of an adjective, a demonstrative or a verb that does. Manambu names are an exception: they may contain formatives which identify them as feminine or masculine. Male names often contain *-du* 'man' as their last component, as in *Duamakway-du, Kawi-du*; or they may contain *-bædi* 'youngster', as in *Saun-bædi, Wali-bædi; -dəmi* (meaning not known), as in *Kasa-dəmi* or *Kigidəmi*; or *-nəbək* 'hill?', as in *Kiginəbək* (the name debated in the 2004 Debate I was privy to). Female names may contain *-ta:kw* 'woman', as in *Ñamamayra-ta:kw, Saun-ta:kw, kay*, as in *Kwarawijəba-kay, Wakənaw-kay, -mæg* or *-wæg* as in *Yuamali-mæg, Yuamali-mæg, Gabal-mæg*, or *(n)əbər*, as in *Kiginəbəkəbər* or *Kəgidəminəbər* (see Aikhenvald 2008: 128–129).

All the names that a person had are recited at mortuary festivals, within the context of myths associated with the ancestors bearing them. They are an integral part of *gra-kudi* (cry-talk) 'mourning songs' sung just by women, and also laments about foiled marriages (*namay)* and nostalgic songs (*sui*) sung by women and men (Harrison 1983, 1990; Aikhenvald 2014a). The knowledge of how to compose the songs is on the way out. But the names remain, and they acquire new uses. In a Catholic eulogy about a young Manambu man, tragically killed on the streets of Madang (where he was studying), his father listed all his Manambu names within the text written in English.

Oftentimes, one can tell what clan a person belongs to from their main name. Many names of the Maliau subclan, into which I was adopted, begin with *Yu*, the term for green snail shell, a totem of the clan. This is what we turn to next.

2.3 The totems, and the terms of address

Within the Manambu tradition, every important object is owned by a subclan: e.g. *sawn* 'white pelican', *wapwi* 'clothing', *wali* 'east wind' are owned by the Wulwi-Nyawi (and so is writing and goods associated with 'white people' who are supposed to have come from the East: see note 3). For this reason, white people – including myself and Simon Harrison – were adopted into this clan group. This was one of the very first lessons I learned from Pauline.

In addition to a set of personal names, every subclan owns a further set of specific ancestral names (one or more) for important objects and natural phenomena. These include crocodile (generic term *mu)*, stone (*kabak)*, dog (*a:s*), slit drum (*ra:b*), totemic haze (*mali*), house (*wi)*, ceremonial house (*kara:b, sa:y)*, residential area (*yarəg*), and many more. So, *ra:b* 'slit drum' will be referred to as *Yuadəmag, Dəmay, Kəgi,* and *Yabæn*, for the Maliau subclan; *Kwa:j* and *Sajan* for the Sarak subclan; *Walikw* and *Walitaguru*, for the Ñakau subclan; *Nabakapi* for the Nawik subclan, *Kapiy*, for the Wankau subclan; *Wulakəkay*, for the Ñagudaw subclan (all within the *Wulwi-Ñawi,* or Sun-Moon clan group), and as *Təpabtəpab* for the Makem subclan; *Takanau*, for the Yimal subclan; *Kudikway*, for the Valik clan; *Maymal, Təmayaman*, and *Kabukuliy* for the Gabak subclan, and *Wapimi* for Wapanab and Wargab subclans (all within the *Gla:gw,* or the 'dark' clan group). These are known as *gwalugwu-gwalugwu ja:p* (clan-clan thing).

Only a few truly knowledgeable men and women would know the full set of names (but the allegiance of some names may be debatable). This creates a problem for a full dictionary of Manambu: do we list all the names? And what if we are accused of misappropriating one? Our draft Manambu dictionary (Laki & Aikhenvald 2013, and still in the making) contains only a handful of sets of names, and is far from complete. Whether it can ever be is an open question.

Each of these ancestral names, and each totem of a clan, can be used as an address term (called *wayəpi* or *wa:y*) – meeting someone, or farewelling them, or simply trying to attract their attention. Someone belonging to the Wapanab clan can be addressed as *makati* (a general address term for the *Gla:gw* group), as *ma-nəbi* 'bow and arrow', or as *yayib* 'tree kangaroo' (all totems of that subclan). Once, in 2004, I went to the Port Moresby Botanical gardens with Pauline and her family. When she saw a tree kangaroo – a totem of the Wapanab clan, and thus a member of the *Gla:gw* clan-group – she lovingly addressed the animal as *makati.*

Besides ancestral names and names of totems used as address terms, each subclan has a set of terms which are used as address terms only. Some distinguish masculine and feminine forms, e.g. *yabənay* 'an address form to a Maliau woman', *yabən* 'an address form to a Maliau man'. Others do not, e.g. *makajəwi* for a Maliau, or *apwi* for a Sarak.

From listening to my Avatip family and trying to understand how they addressed each other, I soon realized that, besides a patrilineal totem or name, one can address someone with a matrilineal one. I was adopted into the Maliau clan as my patrilineal clan (the same as Pauline's). My classificatory mother – Pauline's mother – belonged to the Sablap clan: this is why in 2013 Paul Baday gave me my matrilineal name 'bony rooster' (see § 2.2). As Pauline and I were leaving Avatip to visit Malu, another Manambu-speaking village, one of my sisters farewelled me as follows:

(1) yara ma:y yabənay makajəwi ga:j tapwuk
well go.IMPFV Maliau.ADDR.FEM Maliau.ADDR small.pelican hen
'Off you go (good bye), you woman of Maliau clan, daughter of a woman of Sablap clan.'

The address forms here reflect the fact that I belonged to the Maliau clan, but that my mother's clan is Sablap, since *ga:j* 'small pelican-like white bird, rooster' and *tapwuk* 'chicken' are the totems of this clan. James Sesu Laki, Pauline's husband (and my older brother), once said to me that that using a name and an address term referring to one's mother's clan is 'nicer' than using a patriname. This resonates with Harrison's (1990: 61) statement that matrilineal names are given for protection during the person's lifetime; once the person dies, the name 'returns' to the subclan it belongs to.

When we arrived in Malu, I made enquiries about the clan allegiance for every person I encountered before I greeted them. The best compliment I ever received was a spontaneous remark by an old lady who exclaimed *Wayəpi reka:rək sə-na!* (address.term correctly put/call-ACTION.FOCUS.3SG.FEM) 'She calls names correctly'.

Once again I was overwhelmed by the amount of knowledge one has to have about each person: you need to remember their father's names and thus clan

allegiance, and also their mother's. I have always appreciated the Russian tradition of patronymics – at least we know what the person's father was called, even if we know little about him. But having to keep a multitude of genealogies, on both parents' sides, in one's head? I had to write everything down and learn and relearn by rote.

Remembering which term to use for which person was one thing. Dealing with them in the grammar of the language was quite another. Address terms are noun-like, but have limited syntactic functions. They can be used as vocatives, and be heads of intransitive predicate and copula complements. For instance, I can say *wun yabənay-awun* (I address.term.Maliau.subclan.woman-1FEM.SG) 'I am Yabenay', meaning that I belong to the Maliau subclan, and am to be addressed as *yabənay*. But they cannot be used as core arguments or obliques. Nor can they take case markers, be modified, or occur with number markers (including the associative plural, common with personal names). I opted for considering them a special, hitherto unattested, subclass of nouns (distinct from personal names) – discovered only thanks to learning how to communicate in the village context.

The advantage of long-term immersion fieldwork is constant learning, and watching the language change. When I went to Avatip in 2013, I realized that people about my age are finding new uses for their sub-clan's totemic names. The totemic name for a canoe (*val*), belonging to the Ñakau clan, is *ñalabankay*. Members of the Ñakau clan are using this term to refer to the Missionary Air Fellowship (MAF) planes (a major means of relatively quick transportation from Ambunti to Wewak). My brother Kawidu explained that this is a secret term most people won't know – and so they won't be privy to discussions about plane journeys in which they are not involved. Totemic knowledge is there to serve new purposes.

3. New language, new name

The quality of grammars we write depends on the language materials we gather, and the types of interaction we are exposed to. Having been adopted into the Tariana and the Manambu speech communities and exposed to the networks of kinship relationships, naming systems and other terms of address was instrumental to identifying new grammatical subclasses of nouns, which I probably wouldn't have noticed had I stuck to only analyzing narratives.

A village name has many advantages. Having a blessing name in Tariana makes me a proper Tariana woman (*Taria i-sado*). This has eased my way into new, yet-to-be-fully-described varieties of the language, spoken off the Vaupés River (see Aikhenvald 2014b, on the Kumandene Tariana of Santa Terezinha and their blended language).

In 2013, I ventured into new territory. I started working with the Yalaku language, spoken by just over 300 people in the mountainous region north of the

Sepik river. Since I was an adopted Manambu, I was immediately assigned my place in the kinship system: as Pauline's younger sister, I was elder sister to Joel Ukaia, the local councillor and the key person in the village. I wasn't given a village name (*kaitepa-na yi* (village-GENITIVE name)) until my third trip. Then, Joel told me that Mark Tsupandu, one of the oldest speakers and my classificatory son, believes that I am his mother come back from the spirit world. The name for dead person's spirit, *kaba*, is also used to refer to Europeans (similar to the Greater Awyu communities in West Papua discussed by de Vries, this volume). So I, *kaba-takwa* (spirit-woman), was, by general consensus, given the name of Mark's mother's elder sister, *Holegitakwa, Holegi* for short. This made my bond with Mark (the best storyteller in the village) and my obligations to him truly special. My future grammar of Yalaku will be another journey of discovery, written by a named participant rather than a complete outsider.

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Abbreviations

ADDR	address form
FEM	feminine
IMP	impersonal
IMPV	imperative
INDEF	indefinite person marker
LOC	locative
MASC	masculine
NOM	nominalization
PL	plural
SG	singular
VOC	vocative

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CHAPTER 3

Historical linguistics in the raw

My life as diachronic fieldworker

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Robert Blust with Paiwan speakers in Taiwan

1. Introduction

Although I have collected field data for over 100 languages, in many ways I am not a typical field linguist. First, nearly all of the languages that I have worked on are genetically related. As a result both of student experience in Field Methods

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courses, and of faculty experience in teaching such courses, I have had classroom contact with speakers of Armenian (Indo-European), Skagit (Salishan), and Sentani ('Papuan'). However, the great bulk of my fieldwork experience is with Austronesian, and this has been mainly concentrated in three areas where I have spent time in the field: (1) Sarawak, Malaysian Borneo (April-November, 1971), (2) Manus, Admiralty islands, Papua New Guinea (February-May, 1975), and (3) Taiwan (multiple trips from 1994–1999).

In addition to classroom fieldwork, and fieldwork in the field, I have collected data one-on-one by meeting privately with speakers of several languages in Hawai'i, Australia and Holland, either while I was a student, or later while employed in those three locations.

I am a historical linguist working in a language family that reportedly has over 1,200 members (Lewis, Simons & Fennig 2016), many of which have additional dialects that differ in interesting ways. Since many of these speech communities are not well-described, from the beginning of my career I have felt that the best way to make progress in understanding the history of this language family is to combine the traditional work of the historical linguist with that of the field linguist. For this reason, my typical field experience is to record novel data for dozens of languages in a given geographical region, with a focus on determining their interrelations, reconstructing a common ancestor where that is feasible, and determining major features of change that have led to the modern language situation.

In what follows I will first briefly survey my field experience in Sarawak, Manus and Taiwan, drawing attention to similarities and differences in the approach adopted as a result of different practical considerations that constrained my options. I will then mention my one-on-one and classroom contacts outside the home environment of the speaker, and conclude with an overview of the role that fieldwork has played in my overall career as a historical linguist specializing in the study of the Austronesian languages.

2. Sarawak

My interest in Sarawak began accidentally. I had previously learned Indonesian in an Army Language School that existed for a short time in Hawai'i as a consequence of the Vietnam War, and I then took a local discharge and entered the University of Hawai'i, where I completed a bachelor's degree in anthropology in 1967. In the Fall of 1967 I entered graduate school in the Department of Linguistics, studying comparative Austronesian linguistics under the guidance of the late Professor George W. Grace. Given my background, I was interested in contacting students from Indonesia or Malaysia to practice my Indonesian language skills,

and as a result I soon discovered that there were speakers of a number of minority languages housed in the dormitories of the adjacent federally-funded East-West Center, including speakers of several Austronesian languages for which very little published data then existed. One of these was Kelabit, a language I had not previously known to exist.

I met Gerawat Nulun Tuan, the lone speaker of Kelabit in Hawai'i, on June 28, 1969, explained to him that I would like to learn about his language, and began to work at once.

We started with counting, and so naturally began with 'one'. With the very first word I recorded I knew that there was something typologically odd about this language, as the transcription in my notebook read [ʔadtah], with what appeared to be a consonant cluster of a type generally unknown in languages from this part of the world. I soon learned that a more accurate transcription of this form is [ʔədt^{h}əh], with two schwas and a medial consonant that begins voiced, ends voiceless, and is lightly aspirated. Before long, in addition to refining my phonetic transcriptions, it became clear that the apparent consonant cluster in such words was a unit phoneme. By the time Ladefoged (1971: 9) described the phonetic properties of a true voiced aspirate (as opposed to a murmured stop), and declared such sounds to be unattested, I realized that this was what I had been describing for the previous two years as Kelabit b^{h}, d^{h}, g^{h} (Blust 1969).

Although the etymology of the Kelabit word for 'one' was unknown, other words with similar consonant types soon appeared, and raised questions about the adequacy of the established reconstructions for many forms, as with *qabu > *abuh* 'ashes', but *təbu > *təb^{h}uh* 'sugarcane', *bulu > *buluh* 'body hair, feathers', but *buhək > *əb^{h}uk* 'head hair', or *daŋdaŋ > *dadaŋ* 'heat from a fire', but *dakdak > *dəd^{h}ak* 'tamp down earth'. Being a historical linguist I had been trained to abhor unconditioned phonemic splits, and at first glance some of these pairs of words showed splits of proto-phonemes for which it was difficult to state conditions. In an effort to make progress with this problem I turned to the only survey of the languages of Borneo available at that time, namely Ray (1913). What I found intrigued me. Where Kelabit had qabu > *abuh* 'ashes', but *təbu > *təb^{h}uh* 'sugarcane', for example, Ray gave Bintulu (a coastal language at some distance from Kelabit) *avo* and *tebau*, again with different reflexes of the intervocalic *b. Ray's data was not collected by linguists, and it was clear that it was phonetically inadequate, so I knew that to advance my understanding of these sound correspondences I had to gather my own data. After waiting nearly eleven months to obtain a research permit to conduct dissertation fieldwork in Sarawak I finally got my visa and was off to the land of the orangutan and tattooed headhunter.

I found Sarawak an incredibly beautiful place, with major stands of primal forest still intact in 1971. And the people simply added to the charm of the place,

which was amazingly healthy for a rainforest environment that straddles the Equator. Because my goal was comparative I aimed to collect data for several dozen languages, and in order to do that in the eight months I would be there it was necessary to make arrangements to work at high schools, where there were students from many different language groups.

My first location was Tanjong Lobang College, a locally prestigious high school located in the coastal town of Miri. Here the school headmaster, who was Chinese, helped me to locate students and to explain the reason for my presence among them. There were plenty of students eager to help, most of them 15 or 16 years old, with a knowledge of English, Malay, their own native language and usually some other language of Sarawak, most commonly Iban or Sarawak Malay. Working through English, Malay, or a mixture of the two, I did a preliminary set of interviews to screen for potentially good speakers, and with my first Bintulu speaker a hunch that I had carried with me from Hawai'i was confirmed: the Bintulu word for 'sugarcane' (*təɓəw*) had a medial bilabial implosive corresponding to Kelabit b^h.

I designed a work schedule in which I typically collected data for four languages in parallel each week, meeting with one speaker from say, 3:00–4:00 and another from 4:00–5:00 on MWF, with two others at similar hours on TTh and Saturday mornings. This meant that even though I rarely spent more than 18 hours of collection time for a given language, it was spread over a period of up to six weeks, and so gave ample opportunity to think about features of the language for which I needed more data. Because my primary aim was to work out the historical development and interrelations of these languages, my data collection was heavily focused on the lexicon, but with morphology as an important secondary target. For most languages, I collected vocabularies of 700–800 words, with specific reference to reconstructed forms that provided information on the development of particular proto-phonemes, and 15–20 pages of sentence material which was elicited by asking for an affixed form of a verb base to be used in sentence context.

Once this routine was established (which took very little time), it went very smoothly, and I was able to collect what I needed in order to determine whether a given language was like Kelabit in showing an unexplained split of earlier voiced obstruents. During this period (April–July, 1971) my wife and I rented a ramshackle backyard house on the property of a well-to-do Malay family, and we became close to their three children. In addition, we began to appreciate the food, culture, and ethnic complexity of Sarawak, since the mother in the Malay family was ethnically Chinese and the father was Malay on his father's side but Kenyah (one of the indigenous interior groups of Sarawak) on his mother's side – yet they were 'Malay', which basically meant 'Muslim'.

After three wonderful months in Miri, we had to pull up stakes and catch a Chinese launch upriver to the bazaar town of Marudi, some 50 miles inland. Here we met another helpful school headmaster, who arranged accommodations in a newly-constructed but still unoccupied house raised high on stilts above the tall grass of the yard, and he naturally introduced me to the students at the Marudi Government Secondary School as well. The principal difference between Tanjong Lobang College and MGSS was that there was a higher percentage of speakers of coastal languages (Melanau, Bintulu, Miri, Sarawak Malay) in Miri, and a higher percentage of interior languages (Kelabit, Kayan, Kenyah, Sa'ban, Murik, Berawan, Kiput) in Marudi, although some students from both coastal and interior areas were found in both schools. In addition, being further from the coast we were able to see more of the culture of the indigenous interior peoples: older Kayan men sported pointed leopard's teeth inserted through the pierced shell of their ears, and some of the older Kayan women were tattooed with such fine filigree designs that it appeared they were wearing black lace stockings and armlets. Even many of the younger girls wore heavy brass earrings in earlobes that were distended to their shoulders – earlobes that they had to grip with their fists when running, but which swayed gracefully in the supple movements of traditional dances.

In Marudi I continued with the program of elicitation I had established in Miri, and by early November I had collected descriptive and comparative data for 41 language communities, representing perhaps 15–16 distinct languages. My principal achievements from this trip were: 1. a strongly justified classification of the languages of northern Sarawak, most of which were assigned to a North Sarawak group defined by, among other things, a sound change that produced such startling sound correspondences as Kelabit b^h, Kiput *s*, Bintulu, Lowland Kenyah *ɓ*, Highland Kenyah *p* (Blust 1974a, 2013, Section 10.3.2.4.), 2. several sketches of languages that had previously received little if any descriptive attention (Blust 1977, 1988, 2003a), and 3. the first scientifically reliable evidence of true voiced aspirates, evidence that I continued to support with arguments in later years (Blust 1974b, 1993, 2006, 2016) against the puzzling resistance of some phoneticians who insist that these must be consonant clusters (Ladefoged & Maddieson 1996: 80) even though they exactly match the definition of voiced aspirates as single segments in Ladefoged (1971: 9).

My Bornean fieldwork was shared with students at the University of Hawai'i over the past several decades through a graduate seminar titled 'Ling. 770: The languages of Borneo' in which I made photocopies of my field notes and each student 'adopted' two languages for the semester, using these notes in lieu of a speaker to work up reports first on the phonology, then the morphosyntax, and finally a comprehensive sketch grammar.

An optional project for this course that went undeveloped for many years because of lack of long-term student commitment was to work with me to develop a new bibliography of the languages of Borneo. Many students were willing to work on this in a desultory way until the semester ended, along with their interest. Finally an exceptional student came along who was committed to seeing the project through to the end, and this resulted in Blust and Smith (2014).

With regard to Bornean linguistics, the torch has now been passed to my students Jason Lobel, who has produced the first comprehensive survey of the languages of Sabah (Lobel 2016), and Alex Smith, whose nearly 700-page dissertation based on field data from 78 language communities across the island of Borneo has reset the standards of the field.

3. Manus

My second field trip was to Manus, the principal member of the Admiralty islands, sometimes called 'a miniature Melanesia'. This is an area that is perhaps best known from the anthropological work of Margaret Mead (Mead 1930). Manus is about 60 miles in length and 18 miles in average width, with a heavily forested hilly interior rising to about 2,400 feet. Some 6–8 small islands lie off the north coast, each spaced a few miles from the next, and each harboring a population with a distinct language. Other islands lie off the south coast, and to the east are Baluan, Lou (one of the two major obsidian sites in Melanesia), and some smaller islands. West of Manus are Bipi, several smaller islands, then the large Ninigo lagoon where Seimat is spoken, and some 180 miles to the west, the isolated islands of Wuvulu and Aua.

Like my Sarawak trip, my trip to Manus was prompted by a comparative observation. Little reliable information was available for the languages of the Admiralties when I became interested in them, but it appeared from some limited published lexical data that these languages were unusual for their location. All of the Austronesian languages east of a north-south line that can be drawn just east of Cenderawasih Bay in Papua (Indonesian New Guinea) are members of the large Oceanic subgroup of Austronesian, containing about 460 languages (Lynch, Ross and Crowley 2002). Oceanic is defined by several important phonemic mergers, including the merger of *b and *p as POC *p, and of the whole palatal series of consonants, written *s, *c, *z, and *j as POC *s.[1] The wholesale merger

1. These symbols are attributed to Proto-Malayo-Polynesian, the hypothetical ancestor of all Austronesian languages outside the island of Taiwan (this includes Yami, spoken on a small

of the palatals in Oceanic languages had been established by Otto Dempwolff, the German comparativist who first proposed a 'complete' phonological reconstruction for Proto-Austronesian (Dempwolff 1924–1925, 1934–1938), and this was widely accepted as a given when I entered the field as a student. However, it appeared that in the few Admiralty languages for which published data was available words reconstructed with *j did not show the same reflex as those with *s or *z (*c is rare and hard to test). Instead, *j appeared to have merged with PMP *d as POC *r. Although a seemingly small point in itself, this observation, if confirmed, would have major implications for the structure of the Oceanic subgroup, as it would suggest a bifurcate split into Admiralties vs. the rest. This in turn would imply a POC homeland in the Bismarck archipelago.

Again, in order to test my hunch it was necessary to get to the field and collect data relevant to this question. At this point I had recently finished my doctorate, and I had a two-year postdoctoral research position in the Department of Linguistics within what was then the Research School of Pacific Studies at the Australian National University in Canberra. Stephen Wurm, the department head, was very supportive, and on February 1, 1975 I was in a plane heading north over the enormous pancake that is Australia (flat, brown, dry), toward its long-lost sister on the Sahul Shelf, New Guinea. Once we crossed the comparatively narrow Torres Strait New Guinea came into view – a radically different world of brilliant green mountain ridges one after the other, fading into the distance. A couple of days in the capital of Port Moresby helped me acclimatize, and then I was on a small plane on my way to Lorengau, the tiny capital of Manus.

Manus was a very different experience than Sarawak. It wasn't just differences in the physical anthropology of the two regions (Asia vs. Melanesia), but culturally they were radically distinct. In Sarawak I had encountered resistance from at least one speaker of a Kenyah language to accepting payment for her work with me, since in her words her language was precious, and should not be equated with monetary value. The people of Manus, by contrast, were eager for monetary gain, and everyone readily accepted the payment given for their time.

As in Sarawak, I was able to arrange a working schedule with students from many parts of the Admiralties who were attending Lorengau High School. The headmaster of the school was an Australian national whose wife, Nahau, was a Nali speaker from eastern Manus. As it happened, they had a spare house that

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island southeast of Taiwan, but genetically a Philippine language). *s may have been an affricate (reflexes include /s/, /h/, /c/ and /t/ in various languages), *c and *z probably were voiceless and voiced palatal affricates respectively, and *j was very likely a palatalized voiced velar stop (reflexes include *d, *r, *y, *s, *g, and sometimes other segments).

they were looking to rent out, and it fit me perfectly – less than 100 yards from the beach, near some relatives of Nahau, and within easy commuting distance of the school via my trusty moped. As it turned out, I had a rather unforgettable adventure in this house, since one morning as I sat on a bench at a picnic table facing the wall working on the data I had collected the previous day I heard a shuffling sound behind me. At first I ignored it, since I left my front door open to let in what breeze there was, and small neighborhood children would sometimes drop by, wander in and then out again. But then it happened again, so I glanced over my shoulder to check. It couldn't have been more than a second before I was on top of the bench staring in shock at a monitor lizard nearly as long as I was tall as it flicked its forked tongue in and out, not more than two feet from me. To make a long story short, the lizard started when I jumped, and ran into my tiny bathroom, formed mostly by a tin-sided shower stall. I peered through the crude wire-and-hole system used to lock the door, and could see the creature standing full length, forked tongue working furiously, as it clawed at the tin in a vain attempt to climb to safety. I was about to congratulate myself on my quick thinking when it occurred to me that I might need to use that bathroom later, and this would be difficult unless my new housemate was evicted.

A quick visit to Nahau's relatives' with the wild exclamation 'Hey! Yupela laik lukim wanpela bikpela palai? Em i stap long haus bilong mi!' produced little more than yawns. They were eating lunch, and couldn't be bothered by a crazy white man who must have been freaking out over a gecko. I went back to sweat in my suddenly uncomfortable house, and to my relief in another 15–20 minutes several of my neighbors came over, foremost among them a grown man holding a machete, accompanied by a little brown dog. My Tok Pisin was limited, but when he walked into my bedroom to look for the invader I managed to let him know that it was behind the closed door of the bathroom. He pulled the door open, and in a flash was running backward, cutting Z's in the air with his machete, faster than the speed of light. Strategy no. 2 was to send in his 18″ long hunting dog. It started with a lot of canine sound and fury, punctuated by a loud reptilian hiss, and then a pathetic little pup running out whining, tail between its legs. We were stymied.

Nahau's uncle (as I found out, this is who he was) then tried the spiritual approach, standing a few feet back of the open bathroom door expostulating to the lizard in Nali that this was a human house, and lizards had no business taking up residence there. In my somewhat fractured Tok Pisin I tried to tell him that his eviction notice to the lizard wasn't working because the poor creature was terrified (by now half the neighborhood was in my living room). He didn't get my point, so I pulled up a chair close to where he was, stood on it, and with a long stick poked at 'Lizzie' until s/he came running out under the chair into the crowd, which instantly scattered amid panicky shrieks. Lizzie dived into a pile of lumber

against one living room wall, and probably would have stayed there until nightfall if I hadn't secured the assistance of a brave woman who took one stick while I took another, and between the two of us we guided the unwelcome guest through the living room and out the back door. At that spot a true Manus entrepreneur was waiting with a club, and as soon as the lizard's head crossed the wooden block used for scraping mud off one's shoes before entering the house he flattened it, and claimed the carcass as his property.

They went home, and hung the body of the lizard over a low tree branch. In a little while I dropped by, pulled it off and measured it vertically with the nose touching the ground. It was about four and a half feet long. When I asked them if they planned to eat it, they said 'No' – that would happen only during famines. But the skin was valued as material for the heads of the hourglass-shaped *kundu* drums in local use, and the man who had flattened its head was planning to take it to market the next day to sell for that purpose.

During my comparatively short stay on Manus I had other memorable adventures in addition to this one, but they are peripheral to my story. Because my wife back in Canberra was pregnant with our first child and was expecting early in June, my trip, which began on February 1, was limited to three months. Rather than the open-ended questionnaire format that I had used with success in Sarawak, my Manus data collection was more fixed on a particular set of questions in Tok Pisin. Nonetheless, I was able to collect vocabularies of 600–700 words and limited sentence material for 27 communities throughout the Admiralties, as well as five from other parts of western Melanesia. This resulted in sketches of Lou from the southeast Admiralties (Blust 1998a), and Mussau, from the St. Mathias archipelago off the northern tip of New Ireland (Blust (1984).

The results of this data collection confirmed my initial suspicion that PMP *j had not merged with *s, *c and *z as had been assumed by comparative Oceanic scholars, and it further reframed the definition of 'Oceanic': should this term apply to all languages previously called 'Oceanic', and the non-Admiralty languages be called 'Narrow Oceanic' (still containing over 400 languages!), or should the term apply only to the non-Admiralty languages, with the larger collection called 'Broad Oceanic'? In addition to recognizing *j as a phoneme that had not previously been assigned to POC, I pointed out that most languages of the Admiralties also distinguish PMP *n (alveolar) and *ñ (palatal), a contrast that scholarly consensus until then had regarded as lost in this large Austronesian subgroup. The result was a monograph integrating the new comparative material from the Admiralties with that from other parts of the Pacific (Blust 1978). As a bonus, my field notes from the Admiralties made it possible to resolve a longstanding debate in Polynesian linguistics. Briefly, although PMP *b and *p merged as POC *p, the prenasalized forms of these consonants, PMP *mb and *mp, merged as POC *mp (now written *b).

Each place of articulation had its own 'oral grade' (OG) and 'nasal grade' (NG) reflexes, but the OG/NG contrast for the palatal series in Proto-Polynesian had exercised the best minds in Oceanic linguistics and even brought in well-known outsiders like Eric Hamp and Charles Hockett, with their own views on whether PPP *s was the nasal grade and *h the oral grade of PMP *s, or vice-versa. Thanks to data from the faraway Admiralties, I was able to demonstrate that the NG of PMP *s in PPN was actually *t, showing that Dempwolff's insistence that languages can show no more than two consonant 'grade' reflexes of any PMP obstruent was wrong (Blust 1976). The observation that some Oceanic languages have not two, but three distinct reflexes of the PMP stops set the stage for the landmark study of Ross (1988), who showed that in addition to the NG reflex the OG of PMP stops shows 'fortis' and 'lenis' distinctions in many languages, an observation that has governed all subsequent research on Oceanic comparative phonology.

4. Taiwan

My field trips to Sarawak and Manus happened in fairly close succession (1971 and 1975 respectively), but it was nearly two decades before I went to the field again. In the meantime I remarried, this time to a Taiwan national, and the frequent trips to Taiwan because of family connections led me into the Austronesian homeland. From January 1 to December 31, 1994 I was on sabbatical leave in Taiwan, with an appointment at the Academia Sinica in Taipei. My research goals during this year were to collect primary field data for four of what were then the least well-described Formosan aboriginal languages: 1. Kavalan, 2. Thao, 3. Pazeh, and 4. Saisiyat. This offered promising returns, since Taiwan seemed clearly to be the region of greatest genetic diversity within the Austronesian family, and the subgrouping of these languages was in a state of total chaos at the time, with different scholars making diverse claims, few of which were supported by sound evidence of exclusively shared innovations. An adequate subgrouping of the Formosan languages promised to shed important light on the primary center of dispersal of the Austronesian languages – no mean feat, given that Austronesian spreads over 206 degrees of longitude from Madagascar to Rapa Nui (Easter Island), and about 75 degrees of latitude from northern Taiwan to southern New Zealand.

Since the promised support personnel for my work never materialized, I had to scale back, starting with some limited contacts with Kavalan speakers, and then a long wait before I was able to make contact with Thao speakers in September, 1994. My Mandarin is minimal, and I had been warned by colleagues never to work through interpreters, as this would surely result in serious misunderstandings and major errors. However, what I discovered working in Taiwan was entirely

different. Both in a handful of days working with the Kavalan in February 1994, and in my much longer and more intensive work with Thao, I was accompanied to the field by students who spoke Mandarin, Taiwanese and English. As it happened, most of the older Formosan aboriginal population at this time spoke Taiwanese or Japanese, but not Mandarin.

Because I had already collected primary data from around 90 Austronesian languages when I began work in Taiwan, I had a good sense of what to expect and what not to expect in an Austronesian language. The result was that I caught my interpreters making mistakes in what they offered as the meaning of Thao sentences much more often than I misunderstood the informant. To cite two representative examples, the sentence *yaku ya mun-saháy farukuz makuyu, ya m-riqaz qlhuran sh-ug-kash* (1sg if cross bridge queasy, if see snake afraid) was offered by my interpreter as meaning 'I was crossing a bridge and saw a snake and was afraid'. However, structurally the sentence did not agree with this gloss, and after I insisted that the interpreter had misunderstood the meaning further inquiry revealed that the speaker was trying to illustrate the semantic contrast between *ma-kuyu* 'queasy', and *sh-ug-kash* 'afraid' by providing a hypothetical context for each: 'When I cross a bridge (and look down) I feel queasy, (but) when I see a snake I feel fear'. On another occasion, a well-known Taiwan linguist who has worked with other Formosan languages was working as my interpreter, and began to express wonder and even satisfaction at how similar the structure of some Thao sentences was to Mandarin. I suggested that this was a reason for concern about the speaker producing not genuine Thao, but rather word-for-word translations of the elicitation sentence in Taiwanese. The sample sentence that brought this out was (in English) 'He is 75 years old already, but doesn't yet have gray hair'. As any good fieldworker knows, part of data elicitation is reading body language and tone of voice. In this case, the speaker asked for the Taiwanese sentence to be repeated several times, each time contemplating how to word the Thao equivalent. It was evident to me that he was translating the Taiwanese word-by-word, as the structure of the sentence we got was very similar to that of the English (or Taiwanese), yet in many Austronesian languages an expression like 'to get gray hair' is expressed by a suffix reflecting *-ən, a construction that is sometimes humorously called 'the paranoid passive'. I then asked whether *qutash* 'gray hair' could be verbalized as *qutash-in* (be 'struck' with gray hair), and the informant's face positively lit up as he said (in Taiwanese) 'Yes – that is even better!' (= native). When asked why he had given us the more Chinese-like sentence structure earlier he said matter-of-factly 'I wanted it to sound like beautiful Taiwanese'.

Surprisingly, my most productive days in eliciting Thao data were when I worked through *double* interpreters. Although my wife is from Taiwan she is a Mandarin speaker and never learned the language of the majority of the Chinese

population, namely Minnan Chinese, or Taiwanese. For a week or two she accompanied me to the field along with the Taiwanese speaker who was interpreting for me at that time (and who herself is a linguist). The sessions then went through the following connections: 1. Thao data was elicited via Taiwanese, 2. this was relayed to my wife via Mandarin, and 3. it was then relayed to me through English. My passive knowledge of Mandarin was enough to let me follow much of what went on in stage 2, but I was naturally following the Thao itself as it was produced. The result was not the chaos I had been led to expect, but rather an almost constant back-and-forth flow of information, with no moments of idly sitting and waiting for something to happen. My best days resulted in fourteen 11.5 x 8.5 inch pages of transcribed data – more than I was ever able to record when working through a single interpreter.

In the end I published an 1,106-page dictionary in double columns with 12-point type for a language which at that time had 15 named speakers, and was the last surviving member of a primary branch of the Austronesian family (Blust 2003b). I say 'dictionary', but I think some coinage like 'grammaticon' might be more appropriate, as there are 277 pages of introductory material (Background, Phonology, Historical Phonology, Morphology, Subsystems, Grammatical Miscellany, Texts), and the lexicon itself contains thousands of affixed forms, with about 4,000 sentences illustrating usage. Unlike most grammars or dictionaries, which are like language supermarkets, the Thao dictionary is a Walmart, with everything in one place (where 'everything' means all the field notes I collected, including those that illustrate variation between speakers or styles).

Some people ask me what kinds of software programs I use for lexicography, and the answer is 'none': I entered everything by hand, simply toggling back and forth with search commands to alphabetize the entries. In large part this is a product of my temperament: I work exceptionally fast without most technological support, and I have always felt that I learn far more by doing things 'the hard way'.

While my contact time with speakers of languages in Sarawak rarely exceeded 18 hours, and was even shorter in Manus, the Thao work gave me an opportunity to concentrate intensively on one language over an extended period, totaling in all about 451 hours over 51 working days from 1994–1999. In addition, I had shorter contacts with several other Formosan languages, including 38 contact hours with the last fluent speaker of Pazeh, Mrs. Pan Jin-yu, before she passed away on October 24, 2010 at the age of 96, and with her the Pazeh language.

My fieldwork achievements in Taiwan were, first and foremost, the *Thao dictionary*, and the expanded concept of what a dictionary can be that I tried to exemplify through it, together with original field notes on Kavalan, Pazeh, Northern Bunun, and Central Amis. This primary data collection resulted in a number of publications, including the role of sibilant assimilation in the history of the

Austronesian numeral system (Blust 1995), a determination of the linguistic position of Thao as the last surviving member of the Western Plains subgroup (Blust 1996), the central role of the previously unrecognized but now widely accepted process of Ca-reduplication in Proto-Austronesian grammar (1998b), the fullest published account of Pazeh morphology (Blust 1999a), and the existence of a previously overlooked East Formosan subgroup that includes the extinct Siraya (Blust 1999b).

All of this happened between 1994 and 1999, when my youngest daughter was born, and my life as a fieldworker effectively came to an end.

5. Fieldwork 'at home'

In addition to the three field sites where I collected original language data I have had brief stints of data collection with speakers of various languages in Hawai'i, Australia and Holland. This includes both one-on-one contacts and work in Field Methods courses. One-on-one contacts in Hawai'i include about six hours of collection time with Bimanese of eastern Sumbawa, Indonesia (January, 1972), Atoni (aka Dawan) of west Timor (July, 1973), Roro of New Guinea (1980s), and Kemak of central Timor (October, 2002), in Australia about six or seven hours of collection time with Selau of the western Solomons in (1975), and in Holland about ten hours of collection time with the Soppeng dialect of Buginese (1982). In addition to these one-on-one meetings I have taught Field Methods courses with speakers of the following languages, all of which are Austronesian except Sentani (Papuan): 1. Palauan (Spring, 1997), 2. Balinese (Fall, 2000), 3. Central Amis (Spring, 2002), 4. Galeya (Spring, 2003), 5. Tindal Dusun (Spring, 2004), 6. Sentani (Spring, 2007), 7. Gabadi (Spring, 2011), 8. Truku Seediq (Spring, 2010), 9. Eastern Cham (Fall, 2013), 10. Tetun (Spring, 2014). Together with data collection while sitting in on Field Methods courses taught by other instructors, all in all I have collected field notes for exactly 100 Austronesian languages.

6. Conclusion

It is easy to think that every linguist goes into the field with the same types of expectations and goals, but this is not the case. My goals in doing fieldwork have perhaps not been typical for the field as a whole, but they are shared with some others, as my colleague Lyle Campbell, who has also provided much of the raw data for his work as a historical linguist through his own field notes. In terms of the impact that fieldwork has had on my career as a historical linguist, I cannot

imagine that many of the insights I have had into the history of Austronesian languages would have been possible without access to my own field materials. In effect, my work as a historical linguist and a fieldworker evolved in parallel, each enriching the other and reinforcing the timeless insight that bridges are ultimately more productive than walls.

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CHAPTER 4

Sharing thoughts, concepts and experiences

Fieldwork on African languages

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Matthias Brenzinger with the late David Naude, a Khwe language enthusiast, Namibia, 1997

1. Introduction

The following accounts of selected events at various field sites reflect 35 years of linguistic fieldwork on the African continent. They offer personal insights from my research on languages spoken in Eastern and Southern Africa, primarily by small communities in rural settings. Their languages are generally understudied and under-documented. For this reason, my research always began

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with the compiling of empirical data on basic linguistic features. Beyond this, I have worked on a wide range of specific topics, such as spatial orientation, perception, metaphorical use of body part terms, traditional beekeeping, place names, and ethnobotany. References are included to allow the interested reader to connect the personal stories here to works I have published.

Interacting with speakers of different languages is one of the most rewarding experiences one can have; I am thrilled at the challenge of being confronted with other ways of perceiving our physical and social environments (Brenzinger 2003a). My preferred kind of fieldwork involves sharing experiences and knowledge with people of different backgrounds and fundamentally different living contexts. Most of the communities I have worked with live in areas with poor physical infrastructure. They are marginalized and often neglected in national policies and suffer from being discriminated against by dominant neighbors (Brenzinger 1992c).

The work with and for the communities has become an important aspect of my fieldwork. Developing and establishing orthographies for previously unwritten languages, for example, is to me much more than a purely linguistic service for communities; writing their languages can in fact make a difference in people's lives. My linguistic fieldwork experiences also led to work as a consultant in the implementation of mother tongue based educational policies in Ethiopia and in the development of AIDS/HIV awareness programs in Namibia. In 2012 I founded CALDi, the Centre for African Language Diversity at the University of Cape Town, through which I support fieldwork of African students and scholars on the African continent.

2. Languages of former hunter-gatherers in Southern Africa

One of the most important phases in my academic life began when I started to work on Khwe, an indigenous click language (formerly wrongly subsumed under a non-existing Khoisan language family) spoken by approximately 8,000 people in Namibia, Botswana, South Africa, Angola and Zambia. From 1996 to 1999 I spent several months every year among Khwe in the Caprivi Strip of the Zambesi Region of Namibia, an area which became the Bwabwata National Park in 2007. I also visited and worked with ǁAnikhwe (or ǁAni), who speak a variety of the same language, in the Okavango panhandle of Botswana. I have recorded Khwe and ǁAnikhwe speakers in almost all of their approximately 50 settlements (Brenzinger 1998, 2010).

During the first years of my research in Caprivi, I was still able to visit remote Khwe settlements in the bush, despite the fact that the guerrillas from Jonas Savimbi's UNITA (National Union for the Total Independence of Angola) regularly

crossed the border from Angola into the western Caprivi Strip. Later the mines they planted became a serious problem and killed people, even though I always felt safe when I traveled with Khwe, as they were excellent trackers and could easily detect the mines. In those early years, I could still go on extended bush hikes with Thaddeus Chedau and other Khwe friends. We slept on our blankets next to a fire under the open sky. They showed me how to survive in the wild not far from lions and other dangerous animals, without guns. These trips gave me the chance to record empirical data on natural communication in the bush.

On these trips I regularly got lost when I left my friends to walk on my own into the thick bush. At times, I would get lost after only a very short distance and then couldn't find my way back. I would have to shout to my friends to direct me back on the track, which always provoked astonishment and laughter about my bush-blindness. They couldn't understand that I was unable to read the obvious tracks I had left: the grass bent to one side, the pebbles I had dislodged, etc.

In 1997, my family came to visit me in the Caprivi and Thaddeus became interested in seeing our home and our friends in Germany. That year, Thaddeus accompanied me to Cologne and after more than a month he still didn't recognize our house; it looked to him like all others to the left and right. We discussed how Khwe find their way around and he explained to me that trees and water pans are the most important landmarks. Fathers teach their sons, and mothers their daughters, hundreds of individual trees. Khwe memorize large numbers of trees by heart, which then guide them safely through the bush. The sun, to the contrary, is considered to be dangerous when used for orientation; evil powers and unhappy ancestors often misplace the sun in order to misguide hunters and gathering women. Several experienced hunters told me how this had happened to them and how they luckily survived these attacks. In 1998, our friend Saronge got lost in an area he knew very well. He obviously didn't realize that the sun was misplaced. His footprints later showed that he went in circles till he finally collapsed and died of thirst, not very far from his homestead.

When I started documenting Khwe and ǁAnikhwe place names, one Khwe elder explained to me that "there is no named place, where there is no water". Knowing the location of water sources in the semi-arid West Caprivi Strip is crucial for surviving in the bush. More than 600 Khwe place names in the Strip refer to wells: both traditional dug wells and modern ones that were drilled by rural development projects. Individual names are also given to pans in which rain water gathers. These can hold surface water for some weeks or even for several months depending on their size and depths. Such surface expanses of water are often referred to as *khyanicica* or as *yiceca*, both meaning 'God's water'. In the West Caprivi Strip with no mountains – not even hills – fossil drainage lines (in Khwe, *dom-oro* 'throat-depression') are the most striking physical features of the

landscape. Dug wells and pans only exist in or next to these fossil drainage lines, each of which is called by the name of its major pan or well.

In societies without written records such as Khwe and ǁAni, place names play an important role not only in remembering history, but also in conveying cultural wisdom and knowledge of the environment. The place names evoke memories of events that happened at the water places, and most often recall encounters with animals. With their limited exposure to the bush, knowledge of place names is rapidly fading among the younger generation. Girls and young women, accompanied by experienced women, still collect a variety of veldfood and for that reason still learn names for places in the vicinity of their settlements. In contrast, few young Khwe men of today have gone on extensive bush trips. If they do so they risk being imprisoned or even shot to death by nature conservation guards or soldiers; any man in the bush is considered to be a potential poacher. Elderly Khwe of both sexes can easily name more than a hundred pans and wells and locate them with reference to their mental maps. Most young women know between 30 to 40 place names and their locations, while young men know much less (Brenzinger 2003b; Letloa Trust 2015).

When I recorded stories on migration and conversations on spatial topics, I soon realized that I didn't understand the concepts underlying the use of the Khwe terms *ǀám ǂxóá ànì* 'sun come.out side' and *ǀám ǂxǎã̀ ànì* 'sun enter side'. I first translated these commonly-used terms as the cardinal directions East and West, as this is what is found in many languages of the world. I was however puzzled by the fact that Khwe elders often pointed to the North or South and also called these directions 'sun come.out side' or 'sun enter side'. When asked to indicate sunrise and sunset, they pointed exactly to what for me were cardinal East and West, thus completely different directions from what they had shown me before. Furthermore, Khwe of the older generation insisted that they had no terms for North and South, and that they used the term *ǁgàrɛ́ná* instead which refers to the North-South axis.

Sometime later, I heard Khwe parents telling their children that the Okavango River runs from the 'sun enter side' to the 'sun come.out side' while the Kwando flows in the opposite direction, from the 'sun come.out side' to the 'sun enter side'. I started to argue with them and showed them on a map that both rivers run in the same direction, namely from West to East. It took again some time and more discussions before a Khwe friend came and told me that the Kwando River in the section on the Caprivi Strip in fact runs from East to West. I didn't realize this before and saw that he was right. I had not thought about the obvious fact that without largescale maps one considers only the local portions of rivers and other landforms. I was ashamed of my ignorance and the premature way I had reacted.

This incident, however, helped me to finally understand that the North-South axis has to be taken as a precise line which bisects the horizontal plane. Thus, Khwe of the older generation don't use cardinal directions but split the world into an eastern and a western half. They use the cardinal terms 'sun come.out side' and 'sun enter side' for the western and eastern bisects in all spatial contexts, even for locating objects on tabletop space. With the latter, younger Khwe employ terms for left and right, terms which they have recently invented and which therefore still vary significantly among speakers. The young generation also introduced terms for North and South, and reinterpreted the terms 'sun come.out' and 'sun enter' as the cardinal directions East and West (Brenzinger 2006, 2007, 2008).

Khwe and ǁAni, like members of marginalized communities on the African continent in general, have much lower life expectancies than the national averages. Poverty and hazardous environments result in malnutrition and high rates of tropical diseases, as well as AIDS. Several of my Khwe friends died young and when Kiepie George, the late chief of the Khwe, asked me to do so, I initiated a HIV/AIDS awareness campaign for the Khwe, who had still very little understanding of HIV/AIDS at that time. Together with Namibian health care professionals and supported by medical doctors, we developed materials and strategies for their dissemination with a focus on the illiterate majority of the Khwe community (Brenzinger & Harms 1997, 2001).

On my first field trip, the Khwe asked me to teach them how to write their language. I facilitated a community workshop in which we laid the grounds for a community orthography of Khwe-ǁAni. On 15th September 1996, Khwe became a written language when the first community members wrote their language. On that memorable event, one of the Khwe participants writing his first words stated, seemingly impressed about his achievement: "So we actually speak a real language". He, like many speakers of minority languages, had accepted the negative connotations neighboring people associate with them and their languages. He realized that Khwe could be written and that it has grammatical rules like any other language. I expected that the writing of Khwe would remain largely on a symbolic level with no real practical relevance for the community. I was proven wrong. A second workshop a year later, which I organized with my late colleague Mathias Schladt, was already partly conducted by young Khwe themselves. The late David Naude and Bothas Marinada became the driving forces for spreading the knowledge and use of the Khwe orthography. They conducted writing classes beyond the national borders of Namibia and taught Khwe in Botswana and South Africa. Since then, numerous booklets in and on Khwe have been produced by community members and were published by the Letloa Trust, such as on the traditional use of plants for food and medicine (Letloa Trust 2007), and on Khwe place names (Letloa Trust 2015).

Petra Ociepka and Bothas Marinda wrote *Xam nu Yeu: Learning Khwedam is Your Future Life* (Ociepka & Marinda 2009).

In January 2017, while traveling in the Caprivi Strip, I saw a newly founded Khwe settlement which the inhabitants call *Kyarena* 'returning'. I visited the preschool and was very surprised to see Khwe written in the community orthography on the blackboard. When I asked the teacher, John Mbeleko, where he had learned to write Khwedam, he answered that he was taught by Matthias Brenzinger. After 20 years, neither he nor I had recognized each other. It will remain one of the most unforgettable moments of my life. The teaching of the alphabet to these children even before they attend government schools has been kept up all these years, despite the difficult living conditions with severe poverty and hunger, political discrimination and social marginalization. The pride in their language had become an important source of empowerment. Writing their language has increased their self esteem and countered the negative stereotypes assigned to them by dominant neighbors.

3. Arrival at the African continent

As soon as I had registered for African linguistics at the University of Cologne, Germany, fieldwork became a central aspect of my research activities and an important part of my life. In 1982, my first field trip took me to the Marakwet in the Cherangany hills of Kenya, who speak a Southern Nilotic language. A number of trips to the Mbugu people in the Usambara Mountains of Tanzania followed between 1984 and 1986. I wanted to learn more about Ma'a, a language some members of the Mbugu community speak in addition to their own mother tongue Mbugu. The settlements of the Mbugu are scattered in several valleys of the Usambara Mountains. I mapped their location and interviewed Mbugu elders on the history of their migrations as well as on their own personal life stories.

Prior to my trip, I had reviewed the few sources that were available on Ma'a, a language that is often referred to as a mixed language as it doesn't allow for easy genetic classification. Some scholars, such as Thomason and Kaufman (1988) postulated that speakers of a Cushitic language borrowed their entire grammar from a Pare-related Bantu language, while at the same time maintaining their original vocabulary. But the historical accounts provided by the community members led me to the assumption that speakers of a presumably Cushitic language had rather shifted completely to the Bantu language which the Mbugu people speak today. Mbugu had become their new mother tongue before some community members relexified their language with lexemes from their former, abandoned language. This might have been happened by consulting elderly community members who

still had retained knowledge of the vocabulary of their former heritage language. This language or "register" as Maarten Mous prefers to call it, became known as Ma'a. In my Master's thesis (Brenzinger 1987a) I claim a "U-turn" in the lexicon after a completed language shift as the most plausible explanation for the genesis of Ma'a. More recent research by Maarten Mous published in his comprehensive monograph on Mbugu/Ma'a supports this interpretation (Mous 2003).

4. Plants and their use in East Africa

In 1990, along with Bernd Heine and Ingo Heine, I conducted fieldwork near Mount Kenya with the Mukogodo Maasai. The community had lost their former East Cushitic language known as Yaaku and had adopted the Eastern Nilotic language of the Maasai. Along with the pastoral mode of production these former hunter-gatherers also had taken on the culture of the pastoralists.

In the early 1970s, Bernd Heine had worked with Yaaku speakers, but even at that time, none of the language consultants was fluent in Yaaku anymore (Heine 1973, 1975). Most community members had shifted to Mukogodo Maasai (Heine & Brenzinger 1988) decades before, in the 1930s, when the community had decided to abandon their language (Brenzinger 1992).

Nevertheless, an impressive number of plant names from their Cushitic heritage language had survived in their new Nilotic language, most likely because many of the plants found in their mountainous environment do not exist in the lowlands, where the Ma'a-speaking pastoralists graze their cattle. We therefore focused on plants, collected plant specimens for botanical identification in the herbarium in Nairobi, noted their Yaaku and Mukogodo names and complied information on folk taxonomies as well as on the use of the plants (Brenzinger, Heine & Heine 1994).

When I tried, I failed to collect hunting vocabularies from these former hunter-gatherers because they feared they would be prosecuted for poaching. Honey and bees had become for them what milk and cattle are for pastoralists: precious commodities that are extremely important in their daily lives and thinking. The special vocabularies that deal with traditional beekeeping and honey hunting became one of my central research interests for several years. Together with Mukogodo hunters, I followed honey guides, birds that are known throughout the African continent for leading humans, but also honey badgers to bee nests. After the people have harvested the honey, the birds receive their share as reward for their service. This has been often described in the literature as the only collaboration between human beings and untamed animals (Brenzinger, Heine & Heine 1994). My participation in such economic and cultural activities has been vital for me to fully

understand technical terms, proverbs and stories, and most importantly, to get an idea of people's conceptions of their environment (Brenzinger 1987b).

On our visit in 1990, only a few elderly people recalled the Yaaku greetings and some other phrases, and young community members had not even heard the language name 'Yaaku' for their own heritage tongue. Lee Cronk, an American social anthropologist, studied the change in ethnicity and culture among the Mukogodo for several years. In his monograph on the Mukogodo (Cronk 2004: 83) he captured an experience I had shared with him some years before.

> One day while conducting an interview with an elderly Mukogodo man about the old language [Yaaku, M. B.], Brenzinger played back some Yaaku phrases that the man had spoken earlier into a tape recorder. At first, the man was delighted to hear the old language, even if it was coming from a box rather than from a person. Not really understanding the principle of the tape recorder, he tried engaging it in conversation, giving the customary cheery "Eiuwuo!" response to the recorded greeting "Aichee!" But soon it became obvious that the tape recorder did not really know how to carry on a conversation in Yaaku. The man first became frustrated, then very angry, and finally he began to weep.

This affection for the Yaaku language and grief at its loss were not shared by younger community members in the early 1990s. At that time young Mukogodo were not interested in the language and culture of their grandparents, whom they thought of as "primitive" cave people. The community had assumed Maasai values and lifestyle, along with a high appreciation for cattle. They practiced Maasai customs, such as female circumcision, adopted the Maasai age-set system, and performed Maasai marriage ceremonies. In more recent years, however, Yaaku language activists are trying to revive their lost heritage language by consulting the very limited written documentation of it. Community members often realize the loss of their languages and cultures only when it is too late, and the last speakers have already passed on (Brenzinger 1992a,b).

5. Languages spoken on islands in lakes of Ethiopia

In the early 1990s, while teaching at the University of Addis Ababa for several months, I conducted fieldwork in Southern Ethiopia. There, I recorded data from little-known languages spoken by small communities living on islands in the lakes of the Great Rift Valley. Bayso and Harro communities on Giddicho Island of Lake Abaya maintained their distinct East Cushitic languages due to their geographic isolation on the island. In 1993 and 1994, however, most Bayso and Harro had already left the island because of a lack of food. They had moved to the western shores of the lake and settled among Gamo- and Gofa-speaking people. There

children of these former islanders now grow up speaking the Omotic languages of the dominant groups of the area, and these languages become their new mother tongues (Brenzinger 1995, 1999).

In 1993, when the new transitional government of Ethiopia approved its liberal language policy, I was more than happy to support the implementation of mother-tongue education (Brenzinger 1997). As a language consultant I interviewed teachers, students and parents in hundreds of schools all over Southern Ethiopia and in the Oromia region. On numerous field trips I collected information on language repertoires, on languages use patterns, on attitudes towards the various languages of the area as well as on the positions towards the introduction of multilingual classrooms. It was wonderful to see that my findings had a direct impact on the implementation of the media of instruction in the schools that I had visited. To be involved in decisions made on official language use is very rare for academic linguists, but a great privilege.

6. A N|uu reader for the ǂKhomani community

In 2012, I founded the Centre for African Language Diversity (CALDi) within Linguistics at the University of Cape Town. With financial support from the University of Cape Town and substantial funding from the AW Mellon Foundation, CALDi was able to grant scholarships and support fieldwork on African languages. The principal aim behind CALDi is to foster the study, research and documentation of African languages. CALDi also seeks to establish "African Linguistics" as a discipline emphasizing the study of African languages by African scholars, on the African continent. Fieldwork on African languages by African linguists and local language experts will open new dimensions in our understanding of these languages.

After arriving in Cape Town, my first trip took me to Upington, a town in the Northern Cape, to visit Katrina Esau, alias Ouma Geelmeid, one of the few remaining speakers of N|uu, the heritage language of the ǂKhomani community in the Northern Cape, South Africa. All community members speak Afrikaans as their mother tongue, with the exception of roughly 50 ǂKhomani, who in addition to Afrikaans also use Nama (Khoekhoegowab). From the 1970s on, linguists considered N|uu to be extinct. This was widely accepted until the late 1990s, when some 20 speakers made their language competence in N|uu public. Katrina was one of them and today she is the last speaker of N|uu who actively teaches her language to children and adults. One year after the founding of CALDi, Sheena Shah joined CALDi as a post-doctoral research fellow. In the following three years, we traveled numerous times to Upington to work with Katrina.

Over the preceding 20 years, several linguists had studied N|uu, and using their work as well as consultation with Katrina, we established a practical orthography for N|uu. This language holds an exceptional rich phoneme inventory, with 45 clicks, 30 non-click consonants, and 37 vowels. The most striking phonetic feature of the language is the sets of bilabial clicks, also known as "kiss clicks". In 2014, after several run-throughs with children, we printed the alphabet charts which have been used in N|uu teaching efforts since then (Shah & Brenzinger 2017). On our field trips we also compiled sentences and wordlists for the production of a N|uu reader that we tailored to Katrina's teaching needs. This project was completed in 2015 and we returned 300 hardcopies of the reader to the community (Shah & Brenzinger 2016). It is accessible as a free download at OpenUCT, <https://open.uct.ac.za/handle/11427/17432>.

N|uu is of outstanding importance for linguistics and has strong symbolic value not only for the ǂKhomani community but South Africa as a whole. On Freedom Day 2014, Katrina received the Order of the Baobab in silver: "For her excellent contribution in the preservation of a language that is facing a threat of extinction. Her determination to make the project successful has inspired young generations to learn". <http://www.thepresidency.gov.za/>.

7. A Luruuli-Lunyala dictionary project in Central Uganda

Invited to participate in a corpus-based dictionary project on the Bantu language Luruuli-Lunyala, funded by the German Volkswagen Foundation, I joined a team of scholars on a field trip to central Uganda, where these two varieties of one language are still spoken. Children generally no longer acquire this language; dominant neighboring languages are taking over as new mother tongues. Thus, while thousands of adults know and use Luruuli-Lunyala to some extent in their daily lives, the language must be considered seriously endangered. Over the last years, concerned Baruuli-Banyala community members have engaged in revitalization activities, but seemingly – at least up to now – with very little impact on the youth.

The project was under the guidance of Saudah Namyalo from Makerere University, Uganda and German partner Alena Witzlack and further comprised five project members. We worked in teams and compiled video and audio recordings of narratives, interviews, free speech, songs, etc. in different parts of the language area. The speech by the King of the Bunyala Kingdom, Major Baker Kimeze Mpagi Byarufu II, in which he welcomed us and expressed his support for the dictionary project, was among the most memorable texts we recorded. Others were on fishing, on agriculture, carpentry, but most turned out to be on local politics of the kingdom.

In several respects, this fieldwork was very different from my previous experiences compiling empirical data on languages. I had never conducted fieldwork with such a big team and with so many language workers and language consultants involved. Workshops with more than 20 participants in which team members trained Luruuli-Lunyala speakers in transcribing the video and audio files were on a new scale for me. Saudah and the other team members distributed digital audio files among the transcribers and assisted them with their transcriptions by hand or on the computer. To organize food, accommodation and transport for so many people was quite challenging, but everything at the end worked out well, despite regular power cuts, heavy rains, etc.

An impressive number of audio files were transcribed by a large group of local transcribers, producing a huge amount of language data in a very short time. With a lot of patience and great competence in various data management systems, Alena labelled all the sound files and linked them with the transcripts. I was happy to elicit word lists and sentences with a highly motivated and competent speaker. I was even happier to be able to start working on a grammatical sketch of the Luruuli-Lunyala language with Ruth Mukama, a renowned linguist from Makerere University. Ruth discovered that her own mother tongue, Lugwere, is closely related to Luruuli-Lunyala, which made our work much easier. At the same time we both realized that what was happening around us was a new way of conducting fieldwork requiring skills in data management that neither of us had and which we wouldn't be able to acquire easily.

Over the past few decades, linguistic fieldwork saw great changes with regard to the methods as well as the technology employed in language documentation. Very few scholars still use pens to write in notebooks when interviewing speakers or eliciting wordlists and questionnaires. Computers and tailor-made software programs allow for the processing of large quantities of language data from natural conversations. This kind of data reflects the spoken languages much better than the limited elicited language samples on which the old, prescriptive types of dictionaries and grammars are based. High-tech audio recording devices have become affordable and even video coverage of conversations has become relatively easy to handle.

The foregoing sections having offered a sense of my own fieldwork experiences, I would like to place them in context by briefly reviewing fieldwork conducted in the past on African languages.

8. Contextualizing "fieldwork" on African languages

African languages in sub-Saharan Africa were first recorded by early travelers, explorers, colonizers, traders, hunters, and missionaries, and most of them

came to Africa with transparent agendas, namely colonial conquests, exploitation of natural and human resources (including slavery), as well as religious conversion. They collected data on African languages for different reasons; for enabling communication in trade and in colonial administrations, as well as for translating the Bible.

These first tries by amateurs to document languages spoken on the African continent were followed by more solid linguistic fieldwork conducted by scholars and missionary linguists. Especially the latter often lived substantial parts of their lives in speech communities, wrote grammars and dictionaries, and translated hymns, prayers and the Bible into African languages. Some of the missionaries married local women, who then played crucial roles in their linguistic work. Like language consultants and language teachers in most other cases, these wives were hardly ever mentioned. For example, the first Nama Bible, published in 1831, is usually assigned to Johann Heinrich Schmelen, a German missionary who moved to Namibia in 1812. In 1814 he married Zara Hendrichs, a Nama woman. In his more than 30 years among the Nama, Schmelen never acquired fluency in their language. Thus, it is clear that the Bible translation into Nama was in fact the work of his wife Zara Schmelen (Trüper 2006).

In the 1850s, professional linguists began to dedicate themselves to long-term studies of African languages, and subsequently scholarly works on African languages were published. Wilhelm Heinrich Immanuel Bleek (1827–1875) moved from his German home to Durban in 1854 and, 1.5 years later, to Cape Town. Bleek was one of the most outstanding intellectuals and scholars of his time. He invented the term "Bantu languages", when he analyzed the basic structures of the languages of this group and initiated comparative historical linguistics on African languages. His most important contribution to science and humanity, however, is his work on !Xam, which he began in 1870, together with his sister-in-law, Lucy Lloyd. Among the 6 !Xam language consultants, ||Kabbo played an exceptional role in this collaborative project on documenting the language and folklore of these former hunter-gatherers. ||Kabbo explicitly stated that he wanted the stories to be written and published in books, so that they wouldn't get lost but kept for the generations to come. Bleek and Lloyd discussed the "Bushmen folklore" told by these prisoners and former hunter-gatherers with great admiration. Bleek referred to ||Kabbo as his "main language instructor" and with obvious confidence ||Kabbo called himself "Bleek's language teacher" (Pippa Skotnes, p.c.). As a woman, Lucy Lloyd's work has been underappreciated in our field, as she actually compiled, analyzed and published most of the data from the 12,000 hand-written pages that can be accessed in the Digital Bleek and Lloyd Archive <http://lloydbleekcollection.cs.uct.ac.za/>. The fieldwork which led to this enormous collection of documents of empirical language data started

at Bleek's home in Mowbray, Cape Town. Bleek obtained permission to work with San prisoners from the Breakwater Convict Station at his private residence. The respectful way Bleek and Lloyd interacted with their language consultants is remarkable for that time, when settlers were still hunting and killing bushmen like animals.

While ||Kabbo was aware of and actually demanded the type of work done on this documentation project, until quite recently local communities had neither a voice on the content as well as design of these academic studies nor on the ways the research was conducted. Over the past 20 years, ethical codes of conduct have also been developed for linguistic fieldwork. Foundations, institutions, NGOs, universities, etc. demand ethics clearance from researchers before they are allowed to start their fieldwork. Ethical research emphasizes respect and aims at ensuring interaction between speakers and linguists on the same level.

The language communities approached by linguists often request support for their language activities and programs. The development of practical orthographies for oral languages or the design and production of teaching and learning materials are some of their demands. This kind of fieldwork requires a collaborative approach which involves and respects community's interests and their rights. Local capacity building is a common request by founding agencies with an emphasis on the training of co-workers and involved community members as part of linguistic fieldwork. Making outcomes of projects accessible to the community as well as archiving the data for them to be used in further research are other crucial principles for ethical research and good practice in linguistic fieldwork (Peters et al. 2015).

Linguists in more recent years have rediscovered the value of empirical language data and work increasingly with data from natural conversations. With that, language documentation in fieldwork, i.e. the description of spoken languages in social contexts, has seen its reappraisal. Modern dictionaries and grammars are based on large corpora of oral texts and no longer represent reduced "standard languages". Variation and linguistic diversity more generally is recognized and respected. The corpora-based research has redefined the role of speakers, who in traditional linguistic fieldwork were mainly language consultants who provided examples from their languages. The new focus on spoken natural conversation demands a participatory approach to the study of African languages in which speakers are instrumental in carrying out the fieldwork as well as in the processing and analysis of the language data.

Language diversity is one of Africa's most essential intellectual treasures, and many of the more than 2,000 African languages are spoken by small communities. Children in smaller language communities increasingly acquire dominant languages, which often become the languages used at home. Prestigious

African languages such as Setswana in Botswana and Kiswahili in Tanzania are replacing many of the heritage languages in those countries by spreading as new mother tongues. In Uganda, Luganda is one of the replacing languages among the Baruuli-Banyala communities. The fact that a Luganda-speaking scholar from the University of Kampala launched a major documentation project on their endangered languages made a strong impression on the community. While most community members speak and appreciate Luganda, which is the dominant language of the Bunyala kingdom, there is a growing number of Baruuli-Banyala who want their own language being taught at school and revived among the younger generations.

Earlier this year, I visited Gustav Mbeha, one of my Master's students, in a small village in south-eastern Zambia where he conducted fieldwork on his mother tongue SiLozi. When I arrived, Gustav had spent already several weeks with his grandmother. The intimacy of their interaction and the ease of communication would hardly be possible for non-family members and not possible at all for foreigners. English and the other languages of the former colonial powers are spreading at the expense of African languages. Discussions on empowering African languages and their intellectualization have seemed to get louder in the past few years and might lead to also an increase of academic interest among their speakers. The speakers of African languages themselves have to be qualified to conduct fieldwork on their own languages. Throughout my career and especially since founding CALDi, it has been my privilege and challenge to work toward contributing to this goal.

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CHAPTER 5

Forty-plus years before the mast

My experiences as a field linguist

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Tucker Childs with Mani speaker Ma Hawa Camara, Benty, Guinea

1. Introduction

My work as a fieldworker began before I knew it, certainly well before I knew that I wanted to be a (field) linguist. It began when I was a Peace Corps volunteer (PCV) in Liberia (West Africa) and had to figure things out there in order to get along. I went without a whole lot of preparation (not unlike field linguists today, e.g. Newman 2009; Macaulay 2012) – Peace Corps was notorious for its inadequate

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and misguided training, e.g., teaching volunteers the wrong language for the place they were going, training in outdated and inappropriate teaching methods. I had some language training in Kisi, the local language (not nearly enough to carry on a conversation), but bravely joined the teaching staff (a somewhat elusive entity) at Tamba Taylor Junior High School in Shelloe, Liberia, and began teaching on arrival. No one spoke English and only a few people spoke the resident pidgin, Liberian English, which I learned very quickly out of necessity (as I did a functional Kisi).

My pre-Peace Corps preparation was minimal. I came from a small (all-white) town in the Midwest, albeit with some pretty stark class differences. Ethnicity was totally WASP; there were no Jews, of course, and even Catholics were few and far between. Before joining Peace Corps, I had seen the population sign in my home town of Wayne, Illinois, change twice as I was growing up: once from 350 souls to 400, then later from 400 to 500. The change was not complicated; it required only repainting parts of one sign on the east side of town and the same parts on its counterpart on the west side (same road, the only road through town). My linguistic sophistication was non-existent, despite a somewhat disastrous trip to France.

Due to a number of misguided beliefs including a love for anything Gallic, my mother sent me off to France when I was fourteen, supposedly to learn French. I spoke no French before I left, as was not the case with my fellow students, all of whom had one or more years of schooling under their belts. I was sent to a large country farmhouse catering to Americans and Europeans who were studying French near the small town of Solignac in Haute-Vienne. Unfortunately my instructor embodied all of the negative features of the French stereotype (she was a farm girl), and I learned very little, being horribly distracted by non-linguistic factors including some deficiencies in (her) personal hygiene. My French accent was good, but I was poorly equipped for the Peace Corps experience and my future as a field linguist.

Serving as a PCV in Shelloe, Liberia, I was able to taste the highlife of a larger and definitely more international town than Wayne, Illinois, much more so even than Solignac – it was bracing, to say the least. What I experienced and learned during my first two years in Africa stood me in good stead, however, for the following decades, driven by the desire to properly acquit oneself and not prove to be too much of a fool in front of what Peace Corps called "host country nationals". Wanting to learn about the culture and how to speak the language were two driving forces, the compelling needs of the field linguist.

Because the compulsory draft and the Viet Nam War were still going on when I finished Peace Corps, I decided to stay out of the country for a while longer. Fatigue and ill health eventually brought me back to the States where I soon suffered a malaria relapse and became the first recorded case of malaria in Stevens

County, Washington. After teaching for a few years, I discovered linguistics as part of an education degree and promptly jumped ship. At Georgetown I began my study of linguistics and African languages, which continued in earnest when I transferred to Berkeley after a year. Continuing that work at Berkeley and Stanford (my undergraduate school), I eventually earned my degree. Several visiting positions later I ended up in South Africa where family unhappiness put me on the job market once again back in North America. After two years at Toronto I found a permanent job at Portland State where I have been ever since. Somewhere over those years I had various field expeditions and visiting positions in Europe and Africa. For the last eighteen years I have been documenting the dying languages of Guinea and Sierra Leone.

What I have tried to do in the vignettes that follow is not only characterize some significant events in my field-working life, but also entertain and edify. The events described here represent only a few of the many "learning experiences" I have benefited from. Note that I have included several Peace Corps experiences because it is basically the same thing done by field linguists, figure out the language and see how the society works. Recounting one's blunders is embarrassing but may have some value for the reader, and I hope that is the case here. A caveat: At the time I went through graduate school, field linguists were not trained to be sensitive or reflective, despite the contribution of anthropology to the founding principles of the department where I was trained.[1] It was something you learned on the job. We were trained to collect the facts and analyze (and publish!).

2. Clueless in Africa

The first posting that is relevant occurred during my service as a PCV. The incident serves well to illustrate the naïveté (cluelessness?) of what people (Americans) like me have exhibited in our first encounters with the rest of the world, especially those with the sort of background I outlined above. An applicable term is "babes in the woods" (Parcell 1985 p.c.), originally used to characterize the Greene mission in Liberia (Greene 1938 [1991]; Greene 1936), a spying effort led by the author Graham Greene. Bolahun, where the Greenes holed out, was something of a clean

1. The first Department of Linguistics in America was founded at the University of California, Berkeley, in 1901. Despite its founding being led by an Indo-Europeanist (Benjamin Ide Wheeler), the Linguistics Department has had, since its very inception, close links to the Department of Anthropology and to fieldwork, especially on Native American languages, e.g., Goddard 1905 (the first PhD thesis).

and peaceful sanctuary, a world away from where I lived as a PCV. The mission had cisterns to collect water, electricity, a basketball court, and some very good students. The contribution of PCVs to Foya and Shelloe, where I worked, was, as I wrote rather cynically to a friend at the time, to introduce peanut-butter-and-jelly sandwiches and new sexual practices, both markers of civilization as I then knew it.

Because of my isolation, I was perforce deeply immersed in Kisi culture. The Bolahun Mission was located in the Kisi area, but the language and culture had little impact on the mission work. Back where I lived in Shelloe, the involvement was compulsory. I was given the Kisi name "Saa Chakporma" by Fayia McCarthy, my "father" (*Kɛ̀kɛ́*) (eventually the school principal at the school where I taught). "Saa" signified that I was the first-born male child in my family.[2] The Chakporma part of the name ([ʧakpɔmɑ]) also had an explanation. It was the town in which it was claimed I was born, my "born-town", as it is called in Liberian English; it was actually the born-town of my father, Fayia McCarthy.

Because I was born there and because my father insisted, Chakporma was a destination. I agreed that the least I could do was salute my born-town, a visit much urged on me by *Kɛ̀kɛ́*. Visiting a friend's family and town was a high honor and gave the host a chance to show some hospitality, a much valued act among the Kisi. I initially resisted the invitation but finally succumbed and scheduled a visit for an upcoming weekend, a visit which I anticipated with some unease.

Finally the day arrived. *Kɛ̀kɛ́* had gone ahead to Chakporma to make arrangements, and I was left, somewhat exceptionally, to navigate the route on my own. In such situations someone will usually accompany a "stranger" (Liberian English for 'outsider, guest') on such a journey, but Chakporma staffing was short. I was carefully instructed as to which direction I should head for the two-hour walk to the village and assured that there would be people along the way from whom I could ask directions. I was carefully instructed on how to ask for directions, e.g., drilled on the words for 'right' and 'left'. My Kisi, I felt, was pretty good at this point and sufficient for the task. I felt totally prepared. The hubris of youth urged me on. I set off alone at a brisk and confident pace.

Pride goeth before a fall. Unfortunately the language instruction, etc., was not quite enough preparation for the journey. I got lost and didn't meet that many people from whom I could ask directions. I later learned that directions are problematic in Kisi land, especially with regard to distances and times but also as to

2. "Fayia" [faayɛ] was the name for the third-born male; the names covered the first six males or females and were then recycled. A seventh-born male child, for example was known as "Saa Pormbor", or 'Small Saa', sometimes as "Saa Korli", 'Saa Behind'.

descriptors. I did eventually reach Chakporma, albeit some hours later than was expected, near sunset – I had left home in the early afternoon for a projected two-hour walk. This close to the equator all days were of equal length; the projected two-hour jaunt had extended to four hours on into the dusk.

That I had become disoriented or misdirected on the way became evident as soon as I reached Chakporma. I entered the village from the wrong end of town! This inappropriate entry was something of a disaster insofar as the townspeople were concerned. The correct end of town was festooned with arching palm fronds and seemed very gay once I saw it; nothing similar existed at the other end of town, where I had appeared. This wrong-ended entry caused immediate problems being totally inauspicious and against expectations. I was asked to rest easy until some resolution could be reached. In fact, I learned later I had been initially barred from entering the village.

A different resolution could be reached only by talking to the ancestors, who were housed in a miniature palaver hut,[3] a downsized round house with no walls. It had only posts supporting a thatched roof. It was located on the auspicious side of town and its floor consisted of round stones and perhaps *pìɔ̀mndó* (*nomoli* in Lamp 1979), small statuettes that represented one's parents or grandparents or even more distant ancestors. I wasn't allowed to look that closely at what was happening as the town elders crowded into the small hut, which was somewhat unusually set off from the town proper in Chakporma. They are usually found right in the center of town. Whatever or whoever was there constituted was what was variously called the 'grandparents' or 'ancestors', who were regularly consulted on matters of importance.

The village elders (all male) of Chakporma were seated on the outside edge facing inwards and began their discussion with the grandparents. Their first conversation produced an unsatisfactory result insofar as I was concerned: I was not allowed to stay in the village. My father interceded and begged them to try again. This time the decision was more favorable – I was to be welcomed and I was indeed welcomed royally. The first event was to be bathed by two young women in the village, and the hospitality only got better after that. There was food, music, drink, and dancing all night long; I retired long before everyone else. The party continued as I was well aware since the musicians were playing in the next room.

Because of my fecklessness (getting lost and entering town from the wrong side), I was exposed to a much broader and deeper display of Kisi behaviors

3. A palaver hut is something like an open-air town hall where public meetings took place and often where the town chief heard cases and adjudicated. It is much larger than the hut I speak of here.

and culture than would have normally occurred were I better organized. I learned quite a bit, much of which I would not have learned had I successfully navigated the way to Chakporma. Stephen Daedalus in *Ulysses* says that errors on the part of genius are "volitional" and "the portals of discovery". Though the typical fieldworker hardly qualifies as a genius, much can be learned from one's mistakes.

The next topic, language learning, is a challenge that confronts every fieldworker, one that I've confronted many times with varying degrees of success.

Language learning

One tends to adopt many different strategies when learning a language. The best strategy is to go out into the community and engage its members in conversation, though these exchanges are often tedious for both learner and teacher, especially in the early stages. Ideally the interactions are meaningful but at the beginning there is little the novice can talk about, lacking vocabulary and any idea about how the words should be strung together.

Games are a relief to the tedium of memorization and can be played in the privacy of one's compound. When re-learning Kisi as part of my PhD research in Liberia, I invented a game that relied on the "relentless rhythm" (E. Sapir, as quoted in Greenberg 1978: 53) or "alliterative concord" of noun class agreement. The game featured an "Ill-assorted Guards" marching chant, where I stressed every noun class (agreement) marker, which occurred at regular intervals as shown in the languages below. (The first example comes from Latin and E. Sapir.) Kisi is not so regular but still exhibits agreement in the same way as Swahili and Limba, which have less complicated allomorphy.

(1) The "relentless rhythm" of Latin and the "alliterative concord" of Bantu and Atlantic

Latin: *ill-ōrum saev-ōrum vī-ōrum*
those savage men
'of those savage men'

Swahili: *ki-tu ki-kubwa hi-ki ki-lianguka*
thing large this fell
'This large thing fell.'

Limba: *ŋa-yen ŋa-sɔnwunthe ŋa-lɔhɔi ŋa, ŋa thimo-yii ŋa …*
boards six good the which look-you the
'the six good boards that you are seeking …'

ba-yen ba-sɔnwunthe ba-lɔhɔi ba, ba thimo-yii ba …
'the six good beams that you are seeking …' (Berry 1958: 172)

Kisi: *mèŋ-ndáŋ mà cò mà ní.*
water it is it Foc
'This is water.'

Nouns and their dependent elements share a marker exhibiting concord. Stressing the concord markers in my learning game drove home the facts of agreement. It was fortunate that all Kisi suffixes have a high tone (though the pronouns do not), something similar to stress in English, as opposed to the patterns in the other African languages featured above. Thus the prosody was not too far off. Swahili, on the other hand, has stress on the penultimate mora far away from the prefixed noun class marker. Limba would also complicate the learning of prosody for it follows the majority pattern of prefixing Atlantic languages where the tone on the marker is always low.

A second anecdote is also relevant to language learning and perhaps undercuts some of the advantages of community engagement advocated above. As part of my dissertation fieldwork, to learn the language I regularly visited several old people in the village who were something of a captive teaching corps. They were either immobile or just didn't like to travel. One old man I had to pass every day at least twice as I headed into and out of town, Pa Kpakio, teased me mercilessly every time I walked by with the same question, "Did I know [tàkpàá]?" I would always give the same response, "Yes, I know [tàkpàá]". The inevitable follow-up question was, "Well, what is it?" I'd begin my reply which was inevitably responded to by a grave, pitying shake of the head. "No, no, no, white man" he'd say. "You have learned nothing the whole time you've been here. You are so stupid. Even children know [tàkpàá]. Etc.". The string [tàkpàá] is ambiguous; it could either be the compound 'ant bridge'[4] (*tàl-kpàá*, two morphemes) or 'chest' *tàkpàá*. Whatever I answered, he would say he meant the other word. The teasing was demeaning but not totally useless. It taught me something about the language's phonotactics (and word play): syllables closed with [l] were disallowed word-internally, as well as something about the domains of compensatory lengthening and morphological processes.

These two brief anecdotes reveal two different approaches to learning a language, particularly languages so different from English, a task that every field linguist must undertake. Learning about a culture can also involve different strategies. It can be fun but also dangerous. Returning to the theme of learning from one's mistakes, a second mistake led less to some cultural insights but more to the wisdom of heeding an admonition from local sources.

4. An 'ant bridge' is the ant-body bridge that driver ants form over streams of running water so that other ants can cross.

Mr. Wade's farm, in search of the pygmy hippopotamus

In addition to involving myself in Kisi culture, I also wanted to learn something about the non-local African population, Americo-Liberians and the like. Americo-Liberians and Krios were repatriated slaves in Liberia and Sierra Leone respectively, roughly speaking, who had (been) returned to Africa and who dominated the political stage in the early histories of the two countries in parallel ways (e.g., Clapham 1976). In addition to dominating the political scene in the capital cities, the two groups sought to extend their influence up-country, along with other non-auchtochtones, typically to make money. This extension usually took the form of a country estate, usually an oil palm plantation that produced some outside income, and a country wife (all the people of this type that I knew were men and polygamy was widely practiced).

The fertile land of the Kisi people was coveted by many outsiders. In addition, there were diamonds and gold to be found. The place where I drew water during the dry season was a former diamond mine, and a nearby inselberg was a party hill with a fire pit and ladders providing access to the top, where the view was quite stunning. It was called "Belcher's Hill" after the diamond miner Mr. Belcher, an African American from North Carolina, who had prospected in the area.[5] Our well was a place where he had looked for diamonds. Belcher's Hill had been the site of several Peace Corps bacchanalia.

Another prominent individual in the neighborhood (broadly speaking) was Mr. Wade, whose first name I never learned but whose surname was known further north. It was rumored that he had governmental connections in Guinea that had sometime gone sour. Mr. Wade's son James was something of an outsider to the local culture and in the school where I taught; he hadn't been initiated (gone through Poro, known in Kisi as *tɔ̀mndó*, the boys' initiation society). Though he could speak Kisi, James was not generally accepted locally and sought to curry favor with other outsiders, especially white people.

When I told James I was quite keen on seeing the locally common pygmy hippopotamus, he said he would arrange for such a sighting. (The pygmy hippopotamus was really an excuse to do some exploring.) His father had a large plantation on the Makona River, the boundary between Liberia and Guinea, where the hippos were known to be found. He and his father had invited me for a visit several times before, and this time I decided to take him up on the offer.

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5. I met Mr. Belcher in 1969 when he had driven from Monrovia to Foya, Liberia, in a VW bug. The vehicle was not the perfect car for the roads of Liberia, but I expect he had covered the route before. His grandson Max Belcher is a prominent American photographer who has documented much Liberian life, especially vernacular architecture and portraiture on the Americo-Liberian side. His work is now housed at Duke University (Belcher 2014).

James and I arranged for transportation to Mr. Wade's farm, and after I had been shown my room I was introduced to Kromah, one of Mr. Wade's servants who had been instructed to show us around and be our general dogsbody. The first item of business was to predict our futures. Kromah, as it turns out, was a sand-caster. He threw some small sticks on to a pile of sand and was able to foretell the future. I don't remember what he foretold for me, but James was warned that he would meet his death on water and should therefore never travel in a boat. Kromah's predictions had some authenticity and even credibility because of his walleye, which forever seemed to be looking somewhere else. He was also very drunk at the time of the sand-casting, having partaken of some cane juice, a raw rum that he had purchased immediately after we had set up the fortune-telling (and given him an advance).

Kromah was also our river guide. Although generally nocturnal and reclusive, the pygmy hippopotamus, he confidently assured us, could be observed following well-worn paths that he knew down to the Makona River, that formed a border of Mr. Wade's property. He took us to the river, where a moored dugout canoe awaited us. James, of course, did not want to board because of Kromah's prediction; I had to do some convincing and eventually prevailed. James was still very nervous, holding on to the gunwales for dear life.

The Makona was high at this time of the year. It was not the peak of rainy season but rainfall had been plentiful and the river was above normal in height and volume. Once we got out into the main part of the river the current was quite swift. I was in the bow and James was in the middle. Kromah had the only paddle, which was a small flat board nailed on to a fairly stout stick. He maneuvered us out to the middle of the river and we moved quite rapidly downstream with the current.

He showed us several of the hippo pathways down to the river, but we did not see any of their denizens. After some fruitless searching we decided to head back. Kromah, however, found that he could not paddle hard enough to move us upstream: the current was too strong. Our strategy then changed to moving to the side of the river and pulling ourselves upstream using the low-hanging branches that now dipped into the river. That worked fine for a while until we encountered an army of driver ants that swarmed our boat from one of these boughs. They saw us as fresh meat and quickly began chomping. In our frantic effort to rid ourselves of their painful bites, we lost control of the boat and we moved out into the main part of the river where the current was swiftest, and we started swirling downstream.

Somehow Kromah maneuvered us again to one of the banks where we began again our laborious task of making our way upstream. Soon, however, disaster struck more formidably. With a loud plop a snake dropped into the stern of our boat where Kromah still manned his paddle. He abandoned his post and retreated,

while James Wade ran, if that's possible in a dugout, from his position in the middle of the boat past me to the very front of the boat. James was terrified and shaking so hard that the whole boat was vibrating. The canoe drifted out into the current once again.

When encountering a snake, West Africans (males) know only one mode of reaction – try to kill the snake. Kromah bravely confronted the snake. He raised the paddle above his head and WHAM, totally missed the snake and indeed the boat interior, splitting off part of the paddle on the edge of the canoe, which he met with force. The cane juice he had imbibed before the sand-casting had likely dulled his aim. His next strike was closer to the snake but still ineffective. He totally missed the snake and broke off the rest of the paddle, leaving himself with nothing but a short stick. At this time, it must be remembered, we were twirling around in the current with no control of the boat, especially now that our one paddle was shattered. I started calculating the possibility of a quick swim to the river's edge, however disastrous that might be and perhaps unfair to my fellow travelers, especially after the snake began moving toward us.

The three of us were now cowering in the bow as the snake advanced. Who knows why but fortunately for us it all of a sudden decided to leave and slid quite gracefully over the edge of the dugout and into the water. The immediate threat was over but we were still spinning down the river in a dugout canoe without a paddle. I will not recount our long and laborious trip upriver. James Wade and I jumped out onto the land as soon as we could, and poor drunken Kromah was left dragging the canoe upstream.

All the way back to the house, James Wade remonstrated with me. We almost died! How could you make me get in a boat right after I was told I would die on water. He was angry and never let me forget, even well after the incident, how important it was to listen to such warnings. He was right – lesson learned.

The next story recounts some of the difficulties of doing research in post-apartheid South Africa but also some of the excitement and the serendipity of fieldwork.

South Africa: The good, the bad, and the serendipitous

I had been on the job market for three years, two with a PhD, having ended up on the (final) short list at a number of universities and offered several one-year positions but never a tenure-track job. After being rejected at one such short-listing university, I was asked by a member of the search committee if I would consider teaching in South Africa at the University of the Witwatersrand (Wits) in Johannesburg. The boycott had officially ended after Mandela was freed and elections promised, so that political obstacle had been removed. My wife acquiesced with a spirit of adventure

but would join me only after I had gotten settled and could assure her of the safety of the place for her and our young daughter. I thought about the possibility for a bit, talked to some Africanist friends (North American, African, and European), who like me had participated in the boycott, and after consulting with an official at the South African Embassy in Washington, I decided to give it a go.

Without too much ado I was offered a (tenure-track) lectureship in the Department of Linguistics, but a suspicious passport with too many exotic stamps prevented the government from allowing me to immediately assume the job. I cooled my heels with an hourly job at Educational Testing Services in Princeton, New Jersey, writing analytical reasoning questions about the sex lives of wasps for the GREs. Finally the official invitation arrived and I packed my bags.

South Africa probably presented more challenges and more opportunities than most other countries in Africa, especially during the time I would spend there.[6] Because of its development and mixed population, South Africa initially appeared more appealing than other places, especially in the post-apartheid era and especially for a sociolinguist. It may have been naïve to harbor hopes of reconciliation, harmony, and peace, especially on the part of an American who knew the legacy of slavery, in expecting that the ravages and injustices of apartheid would be healed overnight. They were not.

I went there on my own at first without my wife and daughter. Because the former was actively involved in a Silicon Valley software company, she required communication facilities up to American standards, which did not really exist, so that was a drawback. Moreover, at the time of her projected arrival in South Africa, she would be pregnant with our second child. Although she had some severe reservations about coming to South Africa for all these reasons, she also decided to give it a go.

Her misgivings were justified. In relatively rapid succession, our second child died soon after birth due to malfeasance at one of the best hospitals in Johannesburg.[7] During the night at our home, our watchdog was poisoned and our house robbed as we slept upstairs, the thieves having to scale a ten-foot wall to get in. Finally our car was stolen. As a final straw, my wife found that communications

6. I was fortunate enough to actually meet Nelson Mandela during my first week in South Africa. He was being awarded an honorary J. D. by Wits, and a colleague casually asked me if I wanted to attend the ceremony, and just as casually after the ceremony asked me if I would like to meet him. I did and shook his hand with a very sweaty palm. I had never met anyone so charismatic and was completely overcome. I didn't wash my hand for a week.

7. Not surprisingly, his death could be attributed to apartheid. The night watch was turned over to Africans who were not so well trained. By dint of generosity and warmth, the night nurse had picked up and cuddled our son and passed on a virus that killed him.

with the States and Europe were inadequate for her needs, and she decided to leave South Africa.

After my family left, I decamped to a mother-in-law apartment in the same part of town. My car was stolen twice there, once after I begged the young thieves not to steal my car. All to no avail. They left after my remonstrations but came back soon after I went inside. Quite surprisingly the car was discovered by the police straddling a railroad track after their joy ride, where it had been abandoned after running out of gas. Despite the richness of research opportunities and the possibility of doing meaningful research, the lack of civil order was challenging.

But the time in South Africa was not all so grim. I had some wonderful (mostly white) South African colleagues, and the research opportunities were virtually unlimited. I reveled in the support provided by the university for research, and conducted several major projects during my time there. One major study was on ideophones and another on the several South African urban varieties tied to my work on pidgins. The major drawback to doing research, however, was that there were so few African colleagues and so few qualified African students. I found nowhere near the warmth and welcome that I had experienced everywhere else in Africa. All perfectly understandable but still not encouraging. One colleague, however, transformed much of my experience there through his research, intelligence, sense of humor, and humanity. Tony Traill, the famed phonetician, was my colleague and hero, not the least because he had mastered a San (click) language, Zulu, Afrikaans, and had some passing familiarity with several other African languages, including the pidgin Fanakalo. His hospitality and mentorship were invaluable, and I greatly admired his research.

Perhaps the closest thing to a linguistic epiphany occurred when I accompanied Tony on a field trip near a bore-hole in Botswana where a small group of !Xóõ speakers lived, with whom Tony had been working for many years. His many publications on !Xóõ stretched back to the 1970s when he was a graduate student at Edinburgh, e.g., Traill 1973. What he had been working on at the time of our trip was the (distinctive) features of the world-famous clicks of !Xóõ, which with their accompaniments had allowed the language to expand its inventory to contain the greatest number of consonants in the world. On this research trip Tony was considering the "noisiness" or perceptual salience of clicks as to their five places of articulation: bilabial [ʘ], dental [ǀ], post-alveolar [!], lateral [ǁ], and palatal [ǂ].

I had played a very minor role (proof-reading) in the production of a paper on the topic before our trip (eventually published as Traill 1995), so that I was quite familiar with its content and the claims that it made. This field trip forever validated those claims and provided a useful anecdote for the phonetics classroom. The relevant details of the paper focused on how loud the different releases were. The labial, dental, and lateral releases are considered "noisy", primarily because

they are somewhat affricated. They are longer and feature turbulent air flow, as is not the case with the more punctual post-alveolar and palatal clicks, which achieve a tighter and more rapidly released closure. Of the first set the three clicks could be ranked again as to noisiness, going from less noisy to more noisy: bilabial, dental, lateral, likely due to the resonating chamber before the front closure. Of the second set the post-alveolar click [!] was louder because the palatal click [ǂ] is dominated by high frequencies. The apical releases [!] and [ǁ] are generally dominated by low frequencies. Thus, the clicks could be ranked in ascending order of loudness: palatal [ǂ], post-alveolar [!], bilabial [ʘ], dental [ǀ], and lateral [ǁ].

What brought these facts home to me was a brief saunter away from the campfire where Tony and our hosts were talking quite volubly about who knows what. Tony spoke !Xóõ fluently but I was at a total loss. As I moved away from the campfire, I could still hear the sounds of their conversation and probably because of my lack of familiarity with the sounds, heard the clicks quite distinctly. !Xóõ has a full complement of clicks, which I could distinguish as I warmed myself by the campfire with the others, but which faded away as I moved discreetly away from the group to relieve myself.

What struck me so forcibly, just as Tony had stated in his paper, was the order in which the clicks disappeared from the full set I heard in the fireside conversation. Their disappearance was exactly in the order he had ranked them. I was elated at the discovery and impressed by the patent obviousness of the facts. After I had finished I walked slowly back to the campfire to see if the noisiness order would be followed in reverse, i.e., the noisiest being heard first and the others following. Exactly so.

This anecdote is one I have used when teaching phonetics, and is sometimes, unfortunately, the only thing that students remember of the course. Nonetheless, the general point that there is discovery in the most trivial field activities is a good one. Let the doors of perception be cleansed and always be open.

Aside from this phonetics lesson, I learned a great deal about the many language varieties that can arise in political systems of great inequality. I had already done work on the three pidgins used in the Kisi-speaking area where the borders of Sierra Leone, Guinea, and Liberia come together, as well as on the many varieties of Swahili in East Africa. In South Africa I researched the various township varieties such as Tsotsitaal, a pidgin of the pre-apartheid era with multiple inputs, and Isicamtho, a Zulu-based urban slang, as well as Fanakalo, a mining pidgin created by the overseers in the mines. The multilingualism of urbanized South Africa never ceased to amaze me. There was no end to the (socio-)linguistic richness, the byproduct of gross inequities and oppression.

Another research site provided some excitement, though not of the sort I would want to repeat since the incident could have taken a very serious turn.

3. Lessons from health crises

Undoubtedly one's health is always a concern, especially when Westerners venture into the wilds of Africa. The concerns are legitimate but are easily allayed with some common sense provisions. Unfortunately there are times when one cannot follow the good advice of physicians and other health providers. In West Africa, for example, a big part of hospitality is providing food for one's "strangers", one's visitors or guests. That food may not be that to which a Westerner is accustomed in terms of either its nature or in the conditions of its preparation. One often has to do the polite thing and eat what is offered despite reservations. Other health threats can arise from the most mundane activities.

It is sometimes the case that an illness leads to knowledge and insight, as was the case in a very minor way in customizing a research site. The site was Kpelloe, Liberia, at the intersection of the borders of Liberia, Guinea, and Sierra Leone, and I was the beneficiary of a Fulbright Dissertation Grant (1983–84). A hammock has always been part of my various research abodes and sometimes several hammocks at a time for myself and others. In the village of Kpelloe, I had identified the perfect spot, behind the house and to the side of the kitchen. (Kitchens were always separate buildings behind the main house.) Two mango trees were perfectly positioned to accept the hammock and provide plenty of shade for an afternoon siesta. Clearing the site required little more than lopping off a few low branches and sweeping the area underneath. It didn't take long – I sawed off the branches myself – and then I was ready to roll or rather ready to snooze once I tied up the hammock. After a short time in the hammock, well before I had fallen asleep, however, my hands and lower arms began to itch, as well as a few areas on my legs (I was wearing shorts) and on my torso. The itchy spots became red and inflamed. It was, I learned later, "allergic contact dermatitis".

Because the rash was unremitting and seemed to be getting worse, I headed down to the capital city of Monrovia, where I was able to see a doctor. The explanation was simple, he explained, after asking me a few questions: the sap of the mango tree, to which I had been exposed while doing my lopping, contained the same substance (urushiol) that had plagued my youth in Illinois (poison ivy and poison sumac) and my young adulthood in California (poison oak). It was poignant to have a reminder of those times so far away from their original provenances. What I learned, then, was not a great deal about language and its uses but something more about environmental toxins, the takeaway being that one cannot be totally prepared but one can learn and one does learn from various contretemps.

As the previous story indicates, very often the wounds of fieldwork are self-inflicted, as was also the case with the second incident in the health category,

but this time with potentially much more serious consequences. The incident took place in Kankan, Guinea, as part of another Fulbright award, where I was assigned to the up-country university (Kankan) in a somewhat unrealistic effort on the part of the American government to de-centralize the state away from Conakry.[8] My specific task was to create an English language program at the university. Kankan and environs constituted the Malinké part of the country in Haute Guinée, near the heart of the former Mali Empire, which today corresponded to no one specific state, though its descendants feel some commonality encouraged by Nko (N'ko), a newly created writing system (Kanté and Jaané 1992 [1962]), used much in Kankan. It was an area that would forever be in conflict with a non-Malinké government (the last Malinké-led government was headed by Sekou Toure, 1958–84).

My welcome was not a happy one. The house was rather dark and unkempt, and the neighborhood was not terribly vibrant. My misgivings were justified by what took place on my first day there. As if waiting for some prey, local thieves pried open the locked shutters on my windows and fished out, through the steel bars preventing complete entry, the solar power system I had planned on using during my stay. The woman who prepared my food and lived one house away could tell me nothing.

The troubles unfortunately continued. The heat in Haute Guinée was daunting; although it was a dry heat, the thermometer regularly read (during the dry season when I was there) 95° F or more the whole time I was there, especially in the middle of the day when I returned home from teaching. A local PCV told me she had given up; she never bothered to change her shirt because it always reeked of perspiration. It was not a strategy I adopted.

These oppressive conditions required paying serious attention to hydration. I followed the precepts and drank water whenever I could, typically in its bottled form. This pursuit included refilling bottles (plastic containers) with certified potable water, which bottles I lined up on my shelves. There was no refrigeration so the water was always warm, but at least it slaked one's thirst after a long day at the office.

Unfortunately these same bottles were used for storing kerosene, a vital component of the substitute for the solar-power system that had been stolen. Doubly unfortunately I had lined up the kerosene-filled water bottle with my water-filled

8. An American diplomat whom I knew in Paris had convinced me to apply for the Fulbright. I had seen it as something of a research opportunity and a justification for staying with my family (now with two young children) in Paris. (I also had a research position at LLACAN of the CNRS.)

bottles. One unfortunate day, after trudging across an unshaded soccer pitch to reach my house and in the process losing countless units of liquid, I lunged to the water shelf and gulped down two or three very large gulps of kerosene before I realized what I was doing. As I was gulping the liquid, I thought to myself, Gee, this water kind of smells like kerosene. I wonder if the bottle was properly washed. Then I recalled that I had filled one bottle with kerosene and began gagging. People often ask me when I tell this story, Didn't you know you were drinking kerosene? Yes, I reply but only too late.

When I realized what I had done, I indulged in some reflection and conversation with self. Is this the way you want to die? Is this the way you want your children to remember you? As an old fool who drank kerosene rather than water? I then sat down and said to myself, Don't panic. The human body is an incredibly resourceful entity. Just don't get excited and let's see if we can wait it out. Drinking water will probably help. So I tried not to get excited and I drank as much water as I could. The stench was ghastly. My body emanated kerosene; everything stank of it! To this day I get nauseous around kerosene. I didn't want to go to the local medicals – I was a bit afraid of what they would prescribe, so I simply decided to wait it out and hope for the best. I was miserable for the first few days, but gradually the kerosene stench got less and less offensive. I was finally able to eat some food.

The time in Kankan made me very aware of security, as did the time in South Africa. Outsiders are particularly vulnerable, especially those who seek to establish a relationship of trust and even sharing. The moral of this last story is pretty specific, Don't drink kerosene! More seriously, the warning is that one should be very careful as to what one takes into one's body and not panic.

Final words

What I have tried to do is both entertain and edify, specifically with regard to my own research projects in Africa. My involvement in West Africa but also in East Africa and certainly southern Africa spans almost fifty years from 1970 on. Longevity is no substitute for insight, but hopefully this selection of anecdotes will be of some interest to other researchers.

The general advice that I would give to someone just starting out is, Don't try to overthink things before arrival. Don't over-prepare and keep your expectations low, and be careful! Most of what I learned of value was *in situ* rather than from guidelines. One reason for going one's own way is that every situation is distinct if not unique. The people with whom one interacts are not the same, the researcher is different, the research questions are not the same, etc. Go with an open mind and be flexible and adaptable. I recognize that operating in this manner is costly in terms of time and effort, resources that especially young researchers may not have at their disposal (see Newman & Ratliff 2001).

Another caveat is not to be too upset by miscellaneous setbacks on the ground. A recent research project was delayed two years because of the Ebola crisis. There was not much I could do about it. My sabbatical ending up not being spent the way I intended, and the many people engaged in the project were seriously disappointed, to say the least.

People ask me why I persist in this research given all the travails. It has become easier than in the past to talk about the satisfaction and the rewards of the job, especially in the past seventeen years when there has been some support for studying languages on the edge. The old people are particularly glad of the attention of the field linguist; they have known a life of marginalization and welcome the interest in their language. They are particularly happy that their descendants will hear their words. People open up, people are friendly, people laugh and they feel valued. That's enough for me.

In summary I have tried to show what some of the challenges and pitfalls of field research are for the naïf in Africa, not nearly so entertainingly as Barley (1983, 1986) but hopefully with some edification. Researchers sent into the field today are much better trained than I was, and there are many more resources for guidance than there once were. That is progress.

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CHAPTER 6

Field linguistics in Daghestan

A very personal account

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Nina Dobrushina and Michael Daniel's field team leaving Mehweb, Daghestan, 2016. Dobrushina sixth from left, Daniel third from right.

1. Introduction

The special fascination of linguistics is the possibility to combine skills which are usually considered to belong to different academic domains. Linguistics belongs

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to the humanities, since it is about a central property of human beings. Linguistics demands formal methods, because languages are structured. Linguistics requires observation, because languages are a property of human behavior. Linguistics invites one to travel, because languages are found all over the world.

For us, field research is one of the most important parts of doing linguistics. Both of us started taking part in linguistic expeditions – this is how field trips are called in Russian – as very young students. Since that time, we have made field trips almost every year, and during the last five years several times a year. Our field sites are in Daghestan, North-Eastern Caucasus, south of Russia. Daghestan is the home of dozens of peoples speaking very distinct languages (mostly belonging to the East Caucasian alias Nakh-Daghestanian language family, with the level of internal divergence comparable to that of Indo-European). Traditionally, people live high in the mountains. Now more and more villagers are involved in downward migration – down to the towns and new settlements, down to the lowlands and the plains that open onto the Caspian Sea – and they lose their native languages in this descent. With more than forty languages in an area of 50,000 square kilometers (less if limited to the original mountain and foothill area), Daghestan is the part of Russia with the highest language density.

Our personal experience of linguistic field research is of course not unprecedented, but uncommon in several ways. First, we are a couple, we have children; we work together and bring our children to the field. Second, we are university professors, and, following our own student experience, we consider expeditions as educational enterprises – we usually go to the field with students. Third, we work almost exclusively in Daghestan. And, finally, since 2011 we have run in Daghestan a large sociolinguistic study along with language documentation and description activities. These four uncommon features of our field experience will provide the structure for this chapter.

2. Field research as family business

We met on a field trip. This is not rare in Moscow: many linguistic couples met during their student summer training in the field, some of them now very well-known names in linguistics. This is not surprising. For a student brought up in a big city, a field trip to the Caucasus is a very romantic environment, as culturally and environmentally different from his life experience as could be. Smiling villagers, mountain peaks, bleating sheep.

Not many (if any) of these couples continued to go to the field together as a family. Either both of them or one (more often the wife) stopped doing fieldwork after their student years. One reason is obvious. Someone has to stay at home with

the children while the other spouse is doing fieldwork. That is where we parted with the tradition: we started going to the field with children. Otherwise, this was rare among our fellow students; one of the exceptions was Aleksandr Kibrik, who sometimes went to the field with his son, and then with his daughter.

Our eldest daughter first visited Daghestan while in the womb. Her second visit was when she was six. At that time, our son was two. Having the daughter with us was fun, having the son left with his grandmother at the dacha was not. Our son embarked on his first field experience when he was three; our third child, another daughter, when she was ten months.

Going to the field with children means that you have less time to do fieldwork. But you can do fieldwork, and this is what matters. Too often women have to choose between doing fieldwork and having small children. Paradoxically, now that our children are older, they go with us less often. They now have interests of their own, enjoy staying with friends or going to summer schools. But when they were small, spending field time together as a family was great, even at the expense of the productivity of the fieldwork.

We proudly declare that we were not only among the few Russian field linguists who started taking children to the field; we in fact were the first to do this regularly, as a logistical solution. When our colleagues and friends Dmitry Ganenkov and Natalya Bogomolova first came to Daghestan with their (then) three sons, they said that they were inspired by our example. Several weeks ago there was a Facebook post seeking a babysitter for two weeks during the winter linguistic expedition to study the Besermyan dialect of Udmurt (trip and board covered). Nice idea, good thinking. The next volume on field research may well contain a chapter about how to hire a field babysitter – recommended fees and what to pay attention to when receiving recommendation letters.

With children afield, more attention needs to be paid to living conditions. When our younger daughter was ten months, we went to a village only three hours from the airport, not high in the mountains. Our close friend Rasul Mutalov helped us to find a spacious two-story house whose owners lived in town. Later we found out that a big pile of potatoes on the first floor was home to a snake, and a bathrobe on the toilet door, to a couple of dormice. We didn't tell the younger children about the snake, just asked them not to visit the kitchen (not a big loss, since there was no water, and the gas stove gave uncontrollable flashes of flame).

> *Misha (Michael)*
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> *It was I who discovered the snake and the dormice. One morning, I went down to get potatoes from the pile on the kitchen floor and found what seemed to be a rat's head looking at me in discontent. I was very angry and stamped my foot to try to chase it away from our food stock. What happened next chased* me *from the kitchen*

> *quickly – at a considerable distance from the head, a tail emerged from the potatoes, revealing it was a snake, not a rat – and I retreated. Apparently, the snake retreated, too, because when I gathered my courage and returned to scare it off, it was not there. While we limited the children's unaccompanied visits downstairs, we could not totally restrain them from going through the kitchen to the toilet. It was from such a sortie that our son returned, reporting that "the bathrobe on the inside of the toilet door was swaying all by itself". I came down, insisting, in a loud voice, that bathrobes are not living creatures – it was simply closing the door that made this one sway for some time. Inside, I was not sure of my own story – what if the snake had resurfaced in this new location? I closed the door with a bang, saying to my son – look, did it sway this way? That was because you closed the door. The bathrobe swayed… and swayed and swayed… until I plucked up my courage to snatch it from the hanger and empty its pockets into the tub. It was then that two small dormice fell out onto the enameled surface.*

Food might be another children-related problem in the field. Children are to be fed on time and properly, and are not always ready for local food. They may be choosy and reject unknown dishes suggested by local people. This was the case with our oldest daughter. She did not want to eat *khinkal* (traditional dumplings), and of all local treats accepted only bread and *smetana* (sort of thick sour cream). To us, her pickiness was a source of everyday discomfort. We did not want to force her to eat foods she did not like, but we were also afraid of hurting the feelings of our welcoming hosts. Hospitality in Daghestan includes the ritual of continuous feeding, and the traditional upbringing entails that children adapt to that environment.

The opposite problem is that children can be offered food which you would rather decline on their behalf – and they rather not. It was in a Kumyk village of Tumenler where our youngest daughter had a chocolate for the first time in her life. She was ten months, and we were firmly against her eating sweets. Our host, however, was determined to treat her to chocolates to her fill.

Another difficulty which inevitably arises is the difference between child-parent relations in Daghestan (or probably in any other rural place) and in urban families as ours. We remember every year preaching to our children, before leaving Moscow: Please, remember that in Daghestan children do not object to their parents, especially to the father. They are silent and obedient, especially girls. They do not jump on their father from an ambush when he is talking to other people… The local behavioral culture is fragile and pervasive. Breaching it may have initially invisible but far-reaching repercussions. The issues of cultural distinctions and how to deal with them are always complicated, especially for children, and we will come back to them in the last section.

In Daghestan, children are everywhere, even now when there are much fewer traditional families with 5 to 10 children. Coming to the field with a child does not make you exceptional for your consultants. On the contrary, you look much more

human. Although Daghestanians are usually very hospitable, there are locations where traumatic experiences of the recent post-Soviet history have made local people suspicious and tense with strangers. The family-like appearance of the field team may change or at least mitigate this.

Children add new social dimensions to your communication with local people. Being four years old, our son used to leave the house where we stayed alone and go down the street to the house of the teacher Dzhalal, to invite himself to homemade apple jam – never refused. We remember him sitting alone, surrounded by different treats, while the hosts were praying or doing garden-work.

In our experience, grown-up children are often less ready to mix with local children than when they were small. Surprisingly, it is more difficult for teenagers to accept behavior which is completely different from what they are used to. Learning, with age, to defend their inner self, they become both shyer and more resistant.

Yet, teenagers may also enjoy the field experience. They are eager to communicate with their peers, coming from a relatively comparable background – students. At fourteen, our daughter became good friends with one of our eighteen-year-old students, and absorbed every moment of the students' field life. Small children are more difficult to manage in a field trip involving students. It takes either your or the students' time to care for them. When we started to go to Daghestan with students, our small children stayed at home. Students need a lot of your time.

3. Field research with students

The practice of going to the field with students was established in Russia by Aleksandr Kibrik (Plungian & Fedorova 2017, and see Sumbatova, this volume). He started going on field trips in the early '60s, when he was a student himself. As he was both an outstanding linguist and an excellent team-builder, he created a whole educational system aimed at involving students in research through relatively short field trips. Many generations of Russian linguists had their first research experience in these expeditions. Most of those who have launched field projects of their own were trained by Kibrik or by someone who was trained by him.

The combination of research on typologically fascinating languages, communication with different generations of students and linguists and the exotic environment made Daghestanian field trips one of the brightest experiences of our (and many others') student life. Friendships established at that time often hold for a lifetime. The intensity of linguistics in Moscow, noted by visiting students and scholars from abroad, is partially due to the fact that quite a few linguists met in the field. For many of them, the field experience coincided with a period of intense

socialization and, more generally, coming of age. A very well known typologist admits that, when she first came to the field at the age of seventeen, she did not know how to fry eggs.

Kibrik used to bring groups of 15–20 people, mostly students. The team lived in the local school, deserted in summertime. Students slept in the empty classrooms, on mattresses and in sleeping bags. They cooked in turns. Consultants were invited to come to school in the morning. Many of them were teachers at the local school, and the workflow and timing resembled that of the school. There were 45 minute lessons (sessions of work with local people translating for us) with 15 minute breaks between them. We usually started at 9:00 and stopped at 13:00.

Nina

Starting at nine was always excruciatingly difficult for me. I am a late riser generally, plus nights were usually quite busy – time for fun. I remember those mornings when you can hardly keep your eyes opened, but instead of sleeping have to talk to consultants, ask questions, analyze their answers, invent new examples.

We started working on a new language by collecting the dictionary. Each of us obtained a small part of the dictionary – for instance, 10 verbs and 30 nouns. We collected several forms (thought to be basic) and wrote them down – usually on blank punch cards or library cards, very convenient media before the advent of laptops. We still have boxes of these somewhere in the cellar. This task was usually completed in the first three or four days. All cards were then collected by the student who was in charge of the dictionary. The student had to check the cards, correct mistakes, and put them into a large box used by all members of the field team during the trip.

Misha

I do not distinctly remember the punch cards of that era. I was young and inexperienced. But I do recall dictionary collection. I clearly remember myself being fed up with the monotonous work of noun elicitation. Struck with an inspiration, I went to the biology classroom, took an educational board with images of animals from the wall and came back to my consultant to elicit more fauna – whale, seal, crocodile… He first seemed slightly baffled, but then started enjoying it.

Next, we started to work on grammar. Students were grouped in small groups, often pairs, all including one experienced student. These groups were called 'shrubs', referring to the system of one or more newbies connected to the root. There was even a special verb – 'to shrub' – meaning discussing the data within the shrub. Usually each shrub had a topic of their own (e.g. nominal morphology,

tense-aspect system or relative clauses). Their aim was to elaborate a questionnaire, to collect the data, to process it and to write a field report.

At the same time, we worked on analysis of the texts recorded from the consultants – this was supposed to give each of the members of the team a more general view of the language structure that would go beyond one's narrow scope of research. It was during these sessions that we were initiated into the ethics of fieldwork – as when translating a text that described comic situations into which one of our consultants was prone to fall.

In the '90s, Kibrik decided that the student team had grown strong enough to publish. Previously, students' involvement was limited to publishing essays on various topics in grammar. Grammar writing, involving data from student reports, was however done by experienced linguists – Aleksandr Kibrik (grammar) and Sandro Kodzasov (phonetics and phonology). Now, the idea was to write, collectively, a comprehensive grammar, each chapter by a different author (or group of authors). We started with a small grammar of Godoberi (Kibrik & Eulenberg 1996). Then, for several years, we worked on a grammar of Tsakhur (Kibrik 1999), and the grammar of Bagvalal (Kibrik 2001), both thick volumes with many hundreds of pages. As students grew more and more knowledgeable, the descriptions became increasingly more typologically-informed.

Kibrik himself wrote repeatedly about advantages (and, with less enthusiasm, disadvantages) of such collective field research. The detailed grammars of Tsakhur and Bagvalal each resulted from a series of relatively short (from two weeks to one month) field trips. Disadvantages and difficulties are, of course, also many. Working in a team, and on one topic each, some students never arrived at seeing the fascinating typological profile of a Daghestanian language as a whole.

We are, however, convinced that collective field trips and linguistic teamwork were the best way to start one's own life in the field. Collective field trips help a young scholar to experience the fabulous feeling of investigation and discovery, the feeling which never leaves you afterwards. At the same time, you are not lonely in an unknown place. You do not have to struggle with unusual life conditions alone, or go through the painful experience of miscommunication with local people. In the field, you are supported, and sometimes even protected, by more experienced researchers with much greater academic and life experience.

On the other hand, for some of us, the charisma of collective field trips made it unsettling to embark on field research of our own. Getting used to shared (and dispersed) responsibility, to the narrow field of linguistic expertise, and to the everyday presence of those who solve all sorts of problems for the group, one may feel lost when left to face them alone. Nevertheless, on the whole Kibrik's educational project was a grand success – these days, five or six groups of students with

their professors leave Moscow and St. Petersburg every year to document minority languages all over Russia.

From a professional perspective, the advantages of bringing students to the field are not obvious. Supervising them takes a lot of time. Often, you have a feeling that being on your own you would have done things more quickly and efficiently. However, special features and objectives of one aspect of our fieldwork make it feel very different. The sociolinguistic project launched in 2011 is carried out much more efficiently with students than without them.

4. Field research in a multilingual landscape

In most Daghestanian villages, people are extremely hospitable and eager to deal with guests whatever they came for, and wherever from. Traditional folk stories tell about villagers competing between themselves to host a guest. A guest is an honor. Today's guest is a prospective social support when you, in turn, travel to his own habitat. In the last ten years, the situations when we were turned down by a Daghestanian family we wanted to visit can be counted with the fingers on one hand. Usually, the biggest problem is to eat all that is offered.

If your aim is to describe a language, it is not a problem. You choose several friendly translators and work with them for months, and your stomach is relatively safe. A wide coverage is however crucial for the sort of research we have run in Daghestan during the last seven years. After working for several years in the highland village Archib, struck by a multilingual community – whose first language is well known and studied due to Kibrik's comprehensive grammar (Kibrik 1977a, b), and recently as the focus of the research of the Surrey Morphology Group (Bond et al. 2016) – we started our own research program on the multilingualism of Daghestan.

In Daghestan, villages speaking different first languages often lie within 2–5 km from one another. Neighboring villages traditionally had intensive communication between them – villagers exchanged goods, took fields on lease, offered mutual services. Meanwhile, in most villages there was a strong tendency to avoid marrying out, except in second marriages – and an even stricter linguistic endogamy, a phenomenon apparently rare for areas of high linguistic density. The communities were endogamic, and speakers of other languages appeared in the villages infrequently.

As a result, most Daghestanians spoke several languages – their own language, the languages of their neighbors, and sometimes also more distant languages. The command of neighboring languages started rapidly dropping in the 1930s, when Soviet schools were opened all over Daghestan, with lessons in Russian taught by

Russian teachers. Russian has quickly supplanted local languages as a medium of communication with neighbors.

Our research had several aims, primarily to understand how the language of communication between villages was chosen and what the social correlates of multilingualism were. Since neighbor multilingualism is waning, Dobrushina (2013) suggested a method of retrospective family interviews. We ask people not only about their language repertoire but also about the repertoires of their parents and grandparents whom they remember well. By doing this systematically we document receding patterns of multilingualism of the recent past, and have quantitative as well as qualitative data. We document endangered sociolinguistic patterns, just as descriptive linguists document endangered languages.

Practicalities of this research are in many ways different from what we, formed as descriptive linguists, were used to. Describing a language, one stays in one village for years and communicates with the same people (those who are more eager to translate for us and their families). Our sociolinguistic program requires much more moving around, from a family to another family and from one village to another, talking to dozens of people every day. There was a sudden Labovian feeling of a rapid and anonymous kind of research (though it was not truly rapid and by no means anonymous, as compared to Labov's supermarkets; Labov 2006). We work within clusters of two to four villages with different native languages, in order to compare the patterns of mutual bilingualism, usually asymmetrical. Since 2009, we have studied 38 villages with 24 different languages or lects.

> *Nina*
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> *Although I was in Daghestan with Kibrik about ten times, and since that we had been in Archib five times and other villages many times, only in this new project, with our vastly accelerated pace of entering new villages, did I realize how diverse Daghestan is. Our sociolinguistic research makes us travel all over Daghestan, and sometimes when you go three hours by car you feel as if you ended up in another country – in terms of climate and nature.*

Lower villages (less than 1000 meters above sea level) often have dense forests, fertile soils, and rich harvests. People grow fruits, vegetables and melons; they have large houses and have never experienced hunger. Highland villages (more than 1500 meters above sea level) are resource-poor, with little to no forest and little tillable land. Highland people often had to buy corn from their lowland neighbors (more specifically, they exchanged lamb and cheese for corn).

An advantage of our sociolinguistic program is the possibility to see Daghestan in its diversity. A disadvantage is that compared to grammatical studies, sociolinguistic interviews are very monotonous. As linguists, we are used to getting new information quickly. Solving linguistic puzzles with a native speaker is

unbelievable intellectual fun. Sociolinguistic research resembles more research in ethology: one has to spend weeks and months observing the everyday habits of the woodpecker species – many and many of them – in order to accumulate enough data and have some insight.

Nina

At the end of a day spent carrying out sociolinguistic interviews I was often extremely tired. Not only did I have to pose the same questions to dozens of respondents, but also to show interest and surprise when I hear for the tenth time that the respondent's mother could speak Lak. Really?! How did she manage to acquire it? Once, when our son was five, he entered the room when I was interviewing, stopped, listened attentively and told me with indignation: Why! You have already asked all these questions!

Misha

That's how it worked: we arrived at a village, usually in a friend's car. We entered the house of the friends of our friend. The table was quickly covered with food and alcohol. We celebrated the meeting. Some ten minutes later, Nina would pick up her notebook and silently slip away while I played the requisite role of showing appreciation for hospitality, imbibing everything offered. At dusk, Nina would return, our new friends would load me, a willing victim of their abundant hospitality, into the car, and back we'd go, to the village we lived in.

We saw that we enjoyed the process and could trust the result. But the data that could be collected by two (actually, one) researchers in one day was too poor for quantitative analysis. We were only two, we needed help. Luckily, in 2011 the Higher School of Economics launched a BA program in linguistics, and we both were invited to teach. In 2013, we took our own students to the field for the first time.

Involvement of the students made our project vibrant. Coming to a village with a group of six or seven students, we could collect more data during one day than we used to collect in a week. From then on, we needed just three to four days to survey a cluster of several neighboring villages. As a result, we started combining sociolinguistic research with documentation of the languages and working on grammar.

In Kibrik's time, large teams of students and professors were installed in school buildings. We considered it important to be independent from the villagers. We wanted to live according to our own rules, eat our own food, have our own evenings with guitar-playing or dancing. We saw the villagers mostly when they came to the school to work with us or when we went out to the shop or for a walk. In general, these contacts were very limited – not because of any rules, but just

because for us, young students, local life was more of an exotic show than a topic of human interest.

Misha

Of course, your relationships with the villagers also depended on your temperament. You made friends, you went on visits. You laughed, you made jokes, you became included. In the late '80s, I used to have long hair – no-go for highland Daghestan. Our consultants talked me into getting it cut; among laughter, I was sitting in the open air, my hair being cut by the local amateur painter, also the drawing teacher. Next to us, men were watching, their jaws dropped, a local chess-lover losing a match to a young girl – Olga Fedorova. As any competition, chess is a man's domain; it was as if she was winning in arm-wrestling. (They did not know she was a winner of many tournaments, an experienced chess-player). And yet, all these contacts and mixing was nothing in comparison to the immersion in the local life we had starting in the 2000s.

In the '90s, Daghestan changed. It became less safe, gradually more and more criminal. The Moscow team had several unpleasant experiences with thefts, intrusions and even assaults. In the 2000s, when we started our own field trips, we came to Daghestan in smaller groups and were housed in private houses. Immediately we understood that this was safer. Even if the host was not in the village (many people keep their village houses after having moved to towns), the house is still considered private territory, as if protected by his invisible hand. Breaking this seal is an almost unthinkable violation of tradition.

Misha

In the first year of this new experience of integrated fieldwork, still feeling insecure, we traveled from one village to another, in a small group, at dusk. Suddenly, several shadows emerged from the side of the road. Young men were coming towards us. My heart sank. The men approached us: "You are the guests of M., right? Would you join us for a picnic? The lamb is almost done".

From then on, we have always insisted on finding a private house for the group, never accepting invitations to stay at schools. Sometimes we are hosted by a family – not always the best solution. Once we escaped from an extremely hospitable host with a large house, covered by carpets and Wi-Fi, because we could not stand the television which was never turned off. Another problem might be the household: you have to share the kitchen, and our cooking and cleaning procedures differ from those of our Daghestanian friends. With students we became too many to be housed by a family, anyway. The best solution is a separate house that belongs to a villager who is away from the village, suggested by his relatives living next door. It gives a mixed but pleasant feeling of safety and independence.

When we started going to the field with students, we understood that many considerations that seem obvious to us now have to be made explicit. Every year we send a long letter to the students who are going to join us. The letter contains information about the trip and the peculiarities of life and work in Daghestan. Below are some excerpts.

> *From the instruction letter*
>
> *"In Daghestan, today you may end up in very different life conditions. Last year, we first lived in a house with Wi-Fi, a bathroom, a shower and a swimming pool (unfortunately, the pool had no water). And then we ended up in an old shack with holes in the roof, washing mouse shit from the floor. Bats flitted around us in the night. It is unpredictable…"*

In Daghestan of the 1990s, the best hygienic solution was to close the door in one of the classrooms, bar the window, put a large basin on the floor, bring a bucket of hot water and ask a fellow student to stand on guard outside – doors had no locks. Sometimes, it is the same now: finding a corner in an ex-cowshed with a bucket of hot water. With hosts around, it may be more difficult. I remember how the ten of us spent four hot days in a house with a very hospitable landlady but not a slightest prospect of a shower. During the last several years, however, showers in houses are becoming more and more widespread. When we see such a house, we make a mental note and try to get ourselves invited to come for a wash.

> *From the instruction letter*
>
> *"Dressing will be especially problematic for the girls, because by default we women wear skirts. Therefore – sorry for going into this – you need warm underwear – once when the weather was awfully cold I was freezing and fell sick. In Mehweb, one can indulge oneself in trousers, but long skirts are better. Villagers appreciate this, especially elder ones. But how it will be in Balkhar or Rikwani, we do not know, so we'll all be wearing skirts starting from the airport. And a kerchief on your head – this is a must! Most likely, we'll be wearing headscarves all the time."*

Daghestanians are Muslims. More than sixty years under the Soviet regime were a serious test to their devotion to Islam. Schoolchildren were forced to drink water during the fast of Ramadan. School dresses were not as long as traditional dresses. Girls and teachers were not allowed to cover their hair when at school. The participants in Daghestanian field trips in the '60-'80s, safely unaware of this controversy, did not view a Daghestan village as a place where their dressing habits should be changed. Old pictures show groups of students where girls are in jeans, and boys wear shorts, or are happily waving from a mountain lake, all wearing swimming suits.

Nina

Looking at the pictures from my field youth, I can't help but be surprised by myself. How could I wear, in Daghestan, all these T-shirts? Although I remember Kibrik telling us to leave our shorts in Moscow and to wear long skirts, we did not take it seriously. Any excuses were taken to violate the rules. In Kvanada (Bagvalal, Andic) we used to swim wearing swimming costumes. When we started from the school to the lake, village boys ran there and sat down on the slopes around the lake. Exciting show it was, now I understand. Local women ventured to swim after hot days of field labor only in their dresses.

After *perestroika*, attitudes toward religion changed completely, and Daghestanians started reverting to traditional habits, sometimes in a version stricter than ever before in Daghestan. In many highland villages, you will not see local women without a scarf covering their hair, nor in trousers or a short skirt. In some villages, local shops that trade alcohol risk being bombed.

This however differs significantly from one village to another, unpredictably. In July 2016, we started our field season in a highland Rutul village in Southern Daghestan. The residents seemed to be very tolerant towards the appearance of those who came from outside. Guests and relatives coming from towns were not required to cover their heads, and some young girls wore jeans. After two weeks, we moved for a short sociolinguistic survey to a Dargwa village. The village was much lower, quite close to Makhachkala, and we did not expect it to be strict in terms of dress code. We did not even put on our headscarves when our minibus approached the village (as we usually do). However, after a couple of hours spent in the village something went wrong. Our extremely kind and hospitable landlady gave us a hint. "Look," she said, when a cow started running, "she is scared. She has never seen a woman with her head uncovered". We came back into the house and got our headscarves.

From the instruction letter

"Bring house shoes and woolen socks. In Daghestan, you never, never enter someone's house with your shoes on. And the floor may be cold."

The foot code is a headache. Especially with the sociolinguistic project, when you enter a new house every half an hour. Each time you take off your boots, sneakers or sandals and leave them in the entrance.

Misha

A house where there are currently guests is quite visible by a constellation of shoes at its door. A colleague of ours, Yury Lander, insists that in a Daghestanian language he studied, a special numeral classifier for women had evolved: "There came three pairs of slippers of women." True or not, heaps of slippers on a doorstep on a sunny day are a bright imprint in a fieldworker's memory.

You might think that sandals would be useful. If you are a man, think again. Daghestanians are very aware of how a man is dressed. Casual clothes, as when you go to your dacha and put on your favorite old jeans and slippers, may be irritating to them. Some villagers dress as if they spend their whole day in the office: white long-sleeved shirt, brand-new trousers, shiny covered shoes. They expect twice this from people coming from Moscow.

A persistent issue with a field site like Daghestan is that one asks oneself, all the time: To what extent should we follow the rules of the community where we work? Our main aim is to do our research. But we also want our mission to be accepted and ideally approved by the villagers. We are lucky that Daghestanians are usually open to everybody, including even the scholars who come to study their language or their culture. (Not that they show any interest in the topic, but the guest is the guest, however eccentric he is.) But the communities are never fully homogenous. We are invited to the houses of, and cherished by, the people who belong to the most open part of the community. Some of them are not absolutely strict in following religious practices and are more tolerant towards others' ways of life. There is also another part of the community. They might not be involved in co-operation with us, but they watch us passing by, they meet us in the shop, they discuss us with their relatives. They judge us by our looks and behavior. They might be influential. We do our best to fit, but there are certain limits.

Often, we are simply unaware of what can disturb our hosts. After one field trip, we got a very angry email from our recent host, extremely friendly, helpful and apparently very easy-going while we were in the village. He seemed insulted by the fact that we put online pictures of his wife – a portrait of a woman, wrapped in a headscarf. Taking into account that he himself was a public person, and was taking and posting pictures of us all the time, we could never have dreamed of such a reaction.

Some local habits might conflict with our own understanding of what is right. In Archib, as in many other highland villages, men do not carry heavy objects. Women carry heaps of hay weighing many dozens of kilos on their back. Women bring water from springs that are often far from the houses. One of Kibrik's favorite stories was how they arrived at Archib on foot and were surrounded by local women. Women tried to take huge backpacks away from men and glared and shouted at Russian women who had let their men undergo such a humiliation. It was difficult for us to decide whether we should follow this rule or not. At some point, there was a compromise – men went to the spring only at night-time, when the chances to be seen were low. Same with housekeeping. In the field trips, we usually cook in turns; women and men have the same duties. Local people may disapprove of men cooking and washing dishes.

Finally, local habits may interfere with our work. The house where the field team lives often becomes a place of attraction for visitors, most often very nice people. They love communicating, and in some villages communication feeds on alcohol. At a certain point, every evening the field team start having gate-crashers – hosts, friends, consultants – people whose sympathy is crucial for our fieldwork. But these visits usually interfere with our work plans. In the evenings, we schedule discussions of the data and consultations with students. But guests need someone (usually an adult) to talk to them. They also have to be offered a meal and tea.

The only solution is to make the local people aware of and accustomed to the fact that every day at the same time, a consultation takes place. The visitors are welcomed and asked to take part in the seminar. Sometimes it works perfectly, because the visitors can be involved as consultants to clarify emerging questions.

5. Conclusion

As we said in the introduction, field linguistics is a marvelous combination of personal opportunities. But it also exacts a price. The field is part of your life. It is not where you go to work, every day of the week, but where you go to live, every year of your life.

For us, Daghestan has been a place to live from our student years to now, half of our lifespan (or more?), and hopefully to its end. It is a place of our family life, home to our children's early memories, in the same right as Moscow. It is also a place where many of our friendships and loyalties lie. A place where people we have met during our sociolinguistic sojourns live, scattered across mountain villages. Some of these people later came to Moscow to visit us, some of them exchange weekly instant messages with us, and some others probably do not clearly remember our names. All of them opened their doors to us strangers, stopped us on the road to chat, brought us bread and milk, both still warm.

In the discussion of fieldwork ethics, there is a long-standing issue of how you should give back to the community. Indeed, what is the community really interested in? In many if not most cases, present-day speakers of Daghestanian languages do not see any value in language materials prepared by us, let alone purely academic research, partly because they do not consider their languages as endangered, partly because social values and attitudes are all on the side of major languages, such as Avar and of course Russian. In sociolinguistic studies we do in Daghestan, any value of the data obtained is even less obvious – we do not collect any data that can be valuable to our hosts – speakers of local languages, endangered or not.

Misha

My strong belief is that paying back the community is in our case much more of an emotional, and thus metaphysical nature. We enjoy staying with our friends, we love them when we are far, we remember them after they die. Our debts to the many communities with which we have worked are the coming-backs promised but never paid. I sharply remember parting with our Dargwa host in Mallakent. I took my leave and started moving away. His look was so intense and piercing that I felt it with my back and stopped to turn around. He said: You're never coming back, are you. I said: I will, when I'm near. To this day, the promise is pending.

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CHAPTER 7

Drinking of the iceberg

Thirty years of fieldwork on Arctic languages

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Michael Fortescue with Greenlanders, Kapisillit, 1997

While I have never been a full-time field linguist, descriptive work based on fieldwork has played an important role in my career in linguistics. It has strongly influenced my more theoretical work on historical, functional and cognitive linguistics, providing first-hand examples of whatever matter was being discussed. My fascination with living languages started at a traditional English boarding school. Not fitting in well with this quasi-monastic institution, I would often be seen wandering about with some Teach Yourself language book or other in my hands rather

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than what I was meant to be studying. Alas, these years also instilled in me a lifelong dislike of Latin, one language I was forced to learn. A move at age fifteen to join my parents in Switzerland (having had enough of orthodox schooling) rendered me fairly fluent in French and German, which proved sufficient to get me into the University of California at Berkeley when my parents subsequently moved back to California. Here I threw myself into as many language classes as I could manage (including Japanese and Swedish) and ended up with a degree in Slavic languages and literatures, little knowing at the time that Russian in particular (in which I specialized for my MA) would serve me well in later life with my work on Eskimo-Aleut and Chukotko-Kamchatkan.

My first encounter with Eskimo languages was also at Berkeley (unfortunately through a pretty useless 19th century grammar of Alaskan Yupik, the best available at the time), but later I read Bergsland's 1955 structuralist grammar of West Greenlandic, which though extremely compact and difficult to use whetted my appetite for more. Later, following some nomadic years that included teaching in Japan and France, as a PhD student in Edinburgh I felt the need for comparative data in a polysynthetic language to complement the typological spread that I had already incorporated into my cross-disciplinary thesis (Japanese, German, Russian and French). This resulted in a trip to the "Greenlanders' House" in Copenhagen and my first recordings of Greenlanders (the nearest speakers of a polysynthetic language), whom I taught to play "Twenty Questions" (great fun!). Upon completing my degree, with no jobs for linguists going in Edinburgh at the time, I obtained a government exchange grant to study West Greenlandic seriously for a year at the unique Institute of Eskimology of the University of Copenhagen. This decision proved serendipitous, as West Greenlandic was a relatively well documented variety of Eskimo, with the number of speakers actually increasing, yet there was very little in the way of pedagogical material available to English speakers and a great deal yet to be done in studying it from a contemporary linguistic perspective. Of course this necessitated learning Danish pronto and West Greenlandic as soon as possible. To that end I was able to profit from the Institute's excellent teacher of Greenlandic language and literature at the time, Christian Berthelsen.

Towards the end of that year (the summer of 1979) I had the opportunity to go to Greenland for the first time (to the spectacular Disko Bay and Uummannaq districts) where I undertook my own preliminary fieldwork with West Greenlandic *in situ*. This was made possible thanks to intermediary Robert Petersen, the professor of Eskimology, himself a Greenlander, small of stature but extremely knowledgeable of all things Greenlandic and with a marvellous sense of humor. He helped arrange things so that I could hitch a ride to Søndre Strømfjord on an Air Force Hercules that was transporting an archaeological expedition to Greenland. Thereafter I had to find my own way, on a meager budget. The timing

was good since it coincided with the recently revived traditional summer camp "Aasivik", organized by Greenlanders of the left-wing independence party, some of whom I had met in Copenhagen. It was to take place on Disko Island that year. Participation improved my practical knowledge of Greenlandic considerably (and the music was great), though this resulted in no immediate publication – a novel in Danish (Fortescue 1984b) did eventually come out of it, however. I also managed to make a recording of a single 27-month-old child acquiring Greenlandic, which I subsequently transcribed with the help of his parents and published. In doing so I discovered some remarkable aspects of the learning of a polysynthetic language (in particular the lack of a "two word" as opposed to a "two morpheme" stage). This was one of the very first case descriptions of the acquisition of such a language. It was to lead to further work in this area with one of my Greenlandic students (Fortescue & Olsen 1992).

It should be admitted at once that fieldwork of any kind in Greenland is facilitated by the widespread knowledge of Danish among educated Greenlanders (20% of the population are Danes, concentrated mainly in the capitol, Nuuk/ Godthåb). Only in the outer districts amongst traditional hunters is Danish rather shaky. Also the presence at any one time of several thousand Greenlanders in Denmark meant one never lacked 'informants' – at least of central West Greenlandic.

At the end of my first year in Copenhagen I received a Danish Research Council grant to research my first book. This was to be a comparative study of Inuit affixes, in which these languages abound (Fortescue 1983). These were only patchily described outside of West Greenlandic. It would require fieldwork in western Canada, where coverage in the literature was particularly poor – it would take me specifically to Coppermine, NWT, where I had made useful contacts thanks to Mick Mallon, the exuberant Irish teacher of Inuktitut. When I arrived in Coppermine, my task was to find Copper dialect equivalents of all the West Greenlandic derivational affixes I had accumulated (some 400 in common use). This ended up as a publication in the *Meddelelser om Grønland* series that served as an introduction not just to the affixes but to dialect differences in general across the Inuit continuum (again something only rather sketchily carried out until then). The great variety in productive affixes between the dialects (as opposed to the uniformity of grammar) was highlighted for the first time in this study. This was to awaken my interest in the diachronic history of the Eskimo-Aleut family as a whole.

By this time I was already thoroughly smitten by Greenland (the fantastic scenery and friendly people) as well as by the Greenlandic language, and my one-year stay in Copenhagen was to turn into almost thirty-four. One grant was to lead to another, with 'gaps' filled by generous support from the "Magisters" union (membership of which is more or less obligatory in Denmark in order to do research),

until after a few years a vacancy on the full-time teaching staff opened up. It was easier in those days to go from grant to grant in this fashion, but – young researchers take cheer: tenacity pays! This was also the time when I began work on my Croom Helm grammar of West Greenlandic, which was to be the most comprehensive, up-to-date description of an Eskimo language so far (Fortescue 1984a).

In the summer of 1984 I undertook a second, lengthier Greenlandic field trip, this time to the Upernavik district, the choice being largely dictated by the paucity of information on this dialect's lexical deviation from standard central West Greenlandic. The encouragement of professor Jørgen Rischel, who had previously done phonetic fieldwork in the region, also played an important role. His copious notes (and another Danish Research Council grant) were to be of great help. This trip was to prove quite eventful (as well as linguistically fruitful), since it involved finding my way around among a number of small island communities in this northernmost hunting district of West Greenland. There were problems finding places to sleep and boats to take me to destinations I planned to visit. I recall on one occasion sleeping on the floor of the sacristy at the back of a church and waking to hear that a service was in full swing next door. The way out was now blocked by parishioners in their Sunday finery. I needed to relieve myself rather urgently and found the only way out was to climb through a window and down a drainpipe to the rocks below. Having done my business against one of them I turned around to see the entire congregation pressed against the windows of the church grinning and pointing at me, to the consternation of the priest taking the service.

As regards moving round by water, I was almost trapped for another two weeks in an isolated community because the captain of the coastal ship I was counting on to take me back to Upernavik decided he was in too much of a hurry to stop so just kept sailing past. The sons of the catechist I was staying with kindly took me out in their speedboat to intercept the ship but the captain still wouldn't stop as we wove amongst the ice floes alongside it. Eventually one of the sons threw my backpack up onto the deck of the ship and someone on board threw a rope ladder over the side. I had no choice but to scramble for the rope and pull myself up, all at high speed in choppy water, occasioning hoots of laughter from the passengers at the railing. Ah, these foolish *Qallunaat*!

I made it back in one piece though, and the result was a publication in the Institute's publication series (Fortescue 1986). This introduced the dialect in a way that enabled direct comparison with the semantic groupings in Pierre Robbe and Louis-Jacques Dorais' 1986 coverage of East Greenlandic, a format I was also later to use for my Polar Eskimo data. It also analyzed the 'i-dialect' phenomenon characterizing the dialect in terms of 'delayed labialization', which represented a considerable simplification of this complex distributional phenomenon affecting several dialects around the Greenlandic "fringe".

I spent the spring of 1988 as a guest at the Alaska Native Language Center of the University of Alaska, Fairbanks, not conducting fieldwork as such (though ANLC was later to facilitate my fieldwork in Chukotka) but profiting from the unique collection of material on Alaskan languages in its library and collaborating with Larry Kaplan and Steve Jacobson in marshalling data for a large-scale comparative project. This was to result in the first edition of the Comparative Eskimo Dictionary (Fortescue, Kaplan and Jacobson 1994). The presence of speakers of different Alaskan Eskimo languages at Fairbanks certainly proved useful in the process. The CED essentially reconstructed Proto-Eskimo (and made clearer the relationship to Aleut). We benefited greatly from the advice of Knut Bergsland, who had made his life's work the ultimate description of Aleut and had also conducted fieldwork with the "Inland Eskimos" of Alaska.

My next field trip was to Qaanaaq, Thule, in the far north of Greenland in the summer of 1990. I arrived there via the American Air Base, where I as an intruder was not welcome but was allowed to stay the night in order to take the helicopter to Qaanaaq the following day. The hangar-sized canteen with fresh food flown in from the States was in stark contrast with the simplicity of fare in Qaanaaq. The base is a bone of contention with the local Inughuit, as they were moved north against their will (so it is claimed), away from the good hunting area near the base, after World War Two. The Polar Eskimos have ever since the days of Knud Rasmuussen been the subject of great anthropological interest, but curiously enough their dialect (closest to the western Canadian dialects like Copper) had not been the subject of detailed description, although Erik Holtved had collected many original texts, transcribed in a subjective phonetic manner (Holtved 1951). It lacked both a grammar and a dictionary. Its 'compacted' prosody and the muffled effect of its various /h/ sounds make it difficult for other Greenlanders (not to mention other outsiders) to understand. In the short season I stayed in Qaanaaq I was only able to gather enough data to produce a brief description of the grammar plus a dictionary along the lines of my Upernavik sketch, but at least it was a much more accurate reflection of the language than previous attempts to characterize it (Fortescue 1991). This time it was the Carlsberg Foundation that funded the trip – it is not a cheap part of the world to conduct research in. Unlike further south, where gathering linguistic data could be done on a largely informal, *ad hoc* basis, people in Qaanaaq are used to researchers investigating their lives and surroundings and have a very clear idea of what such information is worth!

There were some unforeseen problems. It turned out that there were tensions beneath the surface in the little town – factions split over the question of orthography. The dialect has never been a written language (except in one short-lived local newsletter), but it is different enough from West Greenlandic to warrant its own orthography closer to that of Canadian Inuktitut. However, since all

children in Greenland learn the (new) West Greenlandic orthography in which all school books are written, a Canadian-type orthography was considered (understandably) confusing by the faction that wanted to preserve the West Greenlandic conventions. My reasons for choosing a phonemic orthography that reflected the dialect more closely was understood but never found any practical use – later I heard that all teaching of the dialect in the schools had been abandoned anyway, so the copies of my little book remained largely unused. This was reminiscent of the squabbles over whether to write /h/ with or without a tail in Coppermine. I did make some interesting discoveries about the dialect – notably its morphologically limited vowel harmony and some recent sound changes that have developed since Holtved's time, resulting in divisions between the pronunciation of different generations.

The Inughuit themselves were facing far more serious problems for the future: global warming was already beginning to raise its ugly head, making it more and more difficult to venture out onto the sea ice to hunt. While I was in Qaanaaq there was a visit by dogsled of a group of relatives from the Canadian side of Smith Sound. They camped out on the ice with their dogs, which is something they can no longer risk today. The ultimate fate of the small population (less than 900 individuals) is uncertain, with talk of a move back south being mooted. During my stay I lived in the vacant house of the doctor. My drinking water came from a little iceberg conveniently dragged ashore by a friendly neighbor, chips off of it being left to melt in a tub indoors. On one memorable sled trip through the primitive landscape of ice and stark striated cliffs I saw the recent footprints of a polar bear, but thankfully the dogs did not give chase.

My next field trip, in 1995, took me outside the Eskimo world altogether, to Chukotka in the far east of Siberia. This was fueled in part by my long-standing interest in the relationship (genetic or otherwise) between the so-called Paleo-Siberian languages, and between them and Eskimo-Aleut (culminating in Fortescue 1998). What was needed was a comparative Chukotko-Kamchatkan dictionary comparable to the CED, to which reconstructed forms could be directly compared. Just as the CED reconstructed Proto-Eskimo, the comparative Chukotko-Kamchatkan dictionary (Fortescue 2005) was to reconstruct Proto-Chukotko-Kamchatkan for the first time – including in its coverage distantly related Itelmen, which some researchers still do not believe to be genetically related to the close-knit Chukotian group, although the distance is probably no greater than that between Eskimo and Aleut. To meet the standard of the CED, more data would have to be gleaned on the different dialects of Chukchi, usually glossed over by the standard Russian sources as negligible. There were also lexical holes to be filled in. The format of the CED had proved to be user-friendly and this I followed in my new dictionary. It would appear a few years later following

a sabbatical at the La Trobe Research Centre for Linguistic Typology, Melbourne, during which time I completed it. My final conclusion on the question of genetic affiliation, by the way, is that Chukotko-Kamchatkan is more closely related to Nivkh further south than to Eskimo-Aleut, whose distant relationship to Uralic, as suggested already by Rasmus Rask in the 19th century, remains the most likely deep genetic link for that family.

As to the circumstances of the field trip itself, these were chaotic, in fact downright bizarre. Nothing went according to plan. This was just a couple of years after the collapse of the Soviet Union and Chukotka had been hard hit – most Russians had left, apart from the unfriendly border guards in their military camp at Provideniya. The "natives" had been left largely to their own devices, alcoholism and depression was rife, reindeer herds scattered to the four winds. The first hurdle was to get across the Bering Strait to Provideniya. There were sporadic flights carrying goods and visiting Eskimos from Alaska, but the one I was supposed to take was canceled and the only way across for a while was with the missionary organization that had been used by ANLC researchers in the past to get to remote outposts. I was flown across the Alaska Range (a spectacular flight in a small plane) to a lake inaccessible by road which was run as a kind of private fiefdom by the missionaries under the aegis of a biblical-looking patriarch. It transpired that the organization had been involved in ferrying young Chukchis and Eskimos back and forth across the Bering Strait, teaching them the gospel and how to grow vegetables (naturally all below the government radar). This "window" allowing foreign missionary activity in Chukotka was to close abruptly some time later, after an incident in the settlement of Sireniki when certain evangelicals were caught filming Eskimos collapsing and speaking in tongues. They were at all events friendly towards me because I let them make a copy of the unique Chukchi-Russian phrase book I was carrying, which would be useful in their proselytizing work, and I was deposited (for free) in Provideniya on their next flight across.

The town was a desolate sight, many of the block-like buildings crumbling thanks to the mixture of cement and seawater of which their walls were built. The whole town would give a shudder when the coal power station occasionally burst into life. My attempts to get to the "capital" of Chukotka, Anadyr, where I was to meet my contact, a lady of mixed Russian/Eskimo background working for the local radio, were at first repeatedly frustrated. The first problem was that I was informed I could only purchase a plane ticket onwards in rubles. However, there were no rubles left in the bank. So I was told by one of the missionaries (heading elsewhere) that I should go to the local baker and buy a bread roll, paying with a couple of hundred dollars and getting the change in rubles. Standard procedure. It worked! Further mix-ups to do with who exactly was responsible for letting me travel at all were finally resolved with a phone call from my contact to the

local authorities vouchsafing my passage. I arrived in Anadyr in a rickety plane in which half the seats had no backs and the passengers had to drag their own luggage aboard. I was welcomed by my anxious contact, but my attempt to continue my journey up the river to interior Chukotka was further frustrated when the boat I was supposed to take mysteriously sank overnight. An alternative that I also missed was to board a small boat taking some French anthropologists further down the coast. I learned later that it had capsized, probably hitting a whale or walrus in bad weather, and all on board perished. I was quite happy to have missed it.

Luckily for my project, all manner of dialects were represented in Anadyr itself and I had no great difficulty in eliciting the lexical and phonological data that I needed right there. My method of return to Alaska was as unpredicted as my route out. I found myself again on the tarmac at Provideniya with a group of unhappy missionaries in miserable weather. They had been waiting for a break in the clouds for days so that their plane – any plane – might land (there was no radar). Then suddenly a small private jet swooped down out of nowhere, and out strode a pair of cowboys in Stetson boots and matching hats. It turned out to be Franklin Graham, Billy Graham's son, breezing into town to deliver a check from The Samaritan's Purse to pay for a new medical facility. This would only take half an hour and he and his companion would be off again. The leader of the missionaries knew him and when it became obvious that there was no room in the plane for more than one additional passenger he graciously suggested it should be me, since I was awaited on the other side at a conference in California. I was consequently whisked back to Nome in less than half an hour. The talk on the way back was all about guns and Jesus (not my subjects), so I just kept silent. On the tarmac at Nome I was transferred directly to the connecting flight (no question of having to go through customs or border control – just a wave from Franklin).

After being appointed professor of General Linguistics at Copenhagen I was looking for ways to expand my fieldwork experience in North America a little further southwards. The opportunity finally arose in 2006 with my first field trip to Vancouver Island, to work with the remaining speakers of the Wakashan language Ditidaht. I had been fascinated for some time by these complex languages, similar as they are to Eskimo in their general affixing-only polysynthetic type but also very different in other respects. Most importantly, they appeared to be in a state of readiness for a comparative treatment along the lines of the CED thanks to the excellent pioneering work of no less than Edward Sapir and Morris Swadesh (1952). Of all the Wakashan languages, it was clear that Ditidaht had received the least lexical coverage – there were many "holes" to fill. There was also a sense of some urgency here since it was uncertain whether there would be any speakers left at all in a few years' time (as had in fact been the fate of closely related Makah).

I made the necessary arrangements thanks to Adam Werle, who helped me obtain permission to visit the village of Nitinat (a much easier process than in Chukotka). Adam had worked with the Ditidaht for a year as a teacher while completing his dissertation and had already sorted out most of the extremely complex morphophonemics of this dialect. Though the experience here was quite positive, there were a number of problems within the community that I only gradually came to recognize, some of them similar to those I had encountered in Qaanaaq, others rather different. The Ditidaht had been moved far inland from their traditional sites on the coast to give them access to roads and other infrastructure deemed necessary for modern life. The village is reached by a logging road – a beautiful but hazardous route, marred only by occasional massive industrial timber cuts on the hillsides. Despite valiant efforts to keep the language alive, only a handful of elderly fluent speakers remained, and the problem for me was to know which ones to take as the most reliable. This was the "who is the expert?" problem, exacerbated in the Wakashan world where the chief naturally takes precedence in judging "correct" forms, even when it is clear that some of the older women, for example, are better speakers. One has to step more carefully here than with the Inuit, and even practice a little bit of harmless deceit – for instance by speaking first to informants as a group and then individually with the "best" of them in order to double-check the data. The result of my endeavors – the comparative dictionary published by Lincom quite soon afterwards (Fortescue 2007) – was of course sent to the community with a cover acknowledgement to all who provided data, without specifying who donated what.

My final field trip was to Alert Bay in 2009, where I collected data for my investigation of orientation systems along the North Pacific Rim (extending my earlier work on Eskimo orientation systems). This was to cover all languages between Makah in Washington State and Ainu on Hokkaido, for most of which fairly reliable lexical data was available or obtainable from correspondence with "experts". I was glad of the opportunity to get back to working with Wakashan languages, this time with Kwak'wala, the obvious starting point since Boas had done such useful groundwork on the subject there. But his monograph was based solely on the dialect at Alert Bay, and I had the feeling that given the convoluted nature of the coastline around the north of Vancouver Island this was likely to be much more complicated if extended to other traditional sites. In this I was not mistaken. I soon ascertained that at certain sites the word for 'south', for example, could in fact point in any direction, depending on where one was speaking from and at which scale – a situation reminiscent of the Arctic coast. This was all duly mapped out. Though the Kwakwkawakw people are largely confined to half of a small island today and a few other scattered reserves, attempts at maintaining the language were slightly more encouraging than at Nitinat, but not much. Cultural pride, on

the other hand, was very much in evidence (through traditional art forms, wood carving and dance, etc.).

The continuation of the trip took me further up the northwest coast through some spectacular scenery with a prolonged stop-off at Prince Rupert. Here, with the help of Margaret Anderson of the University of Northern BC, I found informants who could provide the necessary information about orientation terms in the Tsimshianic languages. I ended up finally back in Fairbanks where I was to work on finalizing the new, expanded version of the CED. At the same time I was able to check on the orientation systems of the non-Eskimo-Aleut languages of Alaska. The result of the journey was a monograph which typologized for the first time the different kinds of systems encountered all around the Rim (Fortescue 2011). It also pinpointed anomalous uses of coastal or inland riverine terms which could be seen as evidence of earlier movements of people down to or away from the coast. One particularly surprising discovery was the conflation in many of these languages of terms for 'towards the fire' and 'towards the water' (or similarly away from them), forming a trail that led deep into Siberia.

As to what kept me doing fieldwork during all those years when I could have sat back as a full-time armchair professor, I think it was in part the "eureka" feeling of discovering something "out there" that falls into place as part of a larger pattern you have been struggling with on your own. It was also satisfying to know that I was contributing to the documentation of endangered languages that might soon be lost. If I had stayed in the armchair I would certainly have missed out on experiencing some exciting places and meeting some remarkable people – and the sense of freedom that this kind of work can give you. Would I do it again if I were younger? You bet!

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CHAPTER 8

Reflections on linguistic fieldwork between Sahel, Amazon and Outback

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Knut Olawsky with Urarina consultants Julian Nuribe and Vicente Arahuata, Nueva Unión de Espejo, 2004

1. Introduction

Linguistic fieldwork for me is an adventure that has continued to inspire me professionally and personally over the past two-and-a-half decades. Living and working with people in unfamiliar places has profoundly affected my life and left unforgettable memories. Connecting with people from very unalike cultures who,

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most notably, speak languages that I did not even know existed has enriched my understanding of human nature in its great diversity. It has also helped me understand that my own origin and cultural affiliation are among many fascinating ways by which humans are characterized. I feel privileged having had the opportunity to live and work with people on three continents, in very different landscapes, speaking extremely different language types.

In the early 1990s I ventured into the hot and dusty region of Northern Ghana to study the Dagbani language – the mother tongue of over half a million speakers (at the time), a language with a tone system so complex that I feel I have only understood a minor part of it (Hyman & Olawsky 2004). My move to the Amazon in 2000 to investigate the Urarina language, which has only 3,000 speakers living in a remote part of the Peruvian rainforest, felt extreme. This typologically unusual language with an OVS word order and nearly two dozen morphological slots on its verbs was both fascinating and mind-boggling and made me think more than once that those people must have fallen from the sky. At least the landscape and climate of my current location for the past decade is not too dissimilar from my first language adventure: the land of the Miriwoong people in the Kimberley region of north-western Australia is often described as Australia's last frontier, and the travel distances to reach a major city are significant. This language has only a few fluent speakers left and my focus here is not only on language documentation but also, to a large extent, language revitalization (Olawsky 2010). Its verb system is unlike anything I have seen in my upbringing as a linguist and provides me with yet another set of fieldwork challenges.

But what is it that distinguishes a fieldworker from a "normal" linguist (if there is such thing as normal)? And why has fieldwork become more important to me than other aspects of studying language? After all, my MA was in psycholinguistics, which I found very inspiring indeed. But something in me longed for a more "practical" application of what I had learned during years of formal, theoretical linguistic study. Perhaps an analogy will help to illustrate what I mean.

I see the relationship between a fieldworker and a theoretical linguist as like that between a hunter and a butcher: the former gets the meat and the latter turns it into sausages. A good field linguist may actually combine the skills of both, for example by producing a comprehensive grammar. The idea for this imagery grew out of years of working and living with a hunting culture and I hope that my vegetarian friends will forgive me the comparison. The hunter chooses an area where he will find his quarry, often based on where the good game lives – or based on a need for some hunting to occur.[1] In a way this is like the fieldworker doing the

1. The other disclaimer I should include here that the analogy uses gender-specific terms by referring to the hunter in terms of "he". This was done for simplicity and not to imply that

initial research in determining what languages may require some serious fieldwork, either because they are extremely interesting, or due to their level of endangerment (or both). Finding the right target, traveling to the area, and identifying the appropriate tools for the job may involve some time and effort. Getting to the place could mean crawling through the bush, paddling a canoe or climbing a mountain, which is not unlike some of the trips I remember from my own fieldwork. Virtually all of my field sites required lengthy trips thanks to those sites' remote locations. The hunter may then need some patience and endure spending time in difficult terrain which can be unforgiving in terms of issues with health, food, bugs, and other creepy-crawlies. Yet, this is what the hunter also enjoys as it is a life close to nature with all its beauty. He also faces the tough job of dealing with the game (which is where the analogy falters as the linguist requires more than a single shot to "finish the job"). The hunter then carries home what he has found. He examines his game closely and, being an expert, understands all its parts. He carves up the animal and organizes the pieces to some extent, according to his knowledge. When he delivers it to the butcher, the two discuss the game and exchange some expertise about this particular catch. The butcher now, being an expert in preparing the meat, would skillfully organize the various parts. Just like a linguist who has studied the art of grammar and is familiar with the various aspects of language – he would look at the "raw" data which the fieldworker has prepared. The hunter-fieldworker ideally has not only managed to record the data but also has organized it to some extent by transcribing, interlinearizing, and glossing it, ready for further analysis. The next step of that process will typically involve the major work of analyzing and compiling the data by writing a descriptive grammar, perhaps a dictionary and further useful types of output such as teaching materials. In saying that, many hunters also make great butchers as this step is basically cutting fillets and steaks from the pieces. In fact, the work thus far could be likened to producing half a meal! However, there is more to be done following these indispensable stages of work: the butcher is waiting to turn the meat into sausages, salamis, pâté and other delicious products by smoking or grilling them, mixing them with exotic spices and processing the meat to make it beautiful to eye, nose, and palate. In the academic world there must be an army of linguists waiting for a new piece of game to reach the market which will help them develop theories, find explanations, and share their findings with the broader scientific community. Some of those studies have been utterly useful and have assisted us in understanding language better – which is also helpful to the hunter as he goes out for further

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women cannot be hunters or butchers. As is clearly demonstrated in this volume, women and men make equally strong fieldworkers and, in my experience, it would seem that female linguists may actually outnumber male linguists.

game. While such findings would never even exist without the fieldworker having gathered the data in the first place, they can make a valuable contribution to common knowledge, especially where hunter and butcher work together. We may spin this further by thinking of a cook, who can then turn the result of the butcher's skillful work into culinary creations – which could be likened to the work of linguists who simply synthesize the linguistic analyses that others before them have prepared. They come up with recipes that are meant to make a great meal – and some of those certainly do, turning the linguists behind such work into five-star chefs. However, the effort and scale of work that the hunter-fieldworker has committed in order to make this meal possible should never be undervalued.

Mind you, let's not stretch the analogy too far: while the hunter definitely shoots the animal, the linguist is not meant to kill the language they are documenting! Let's view it as capturing the language for later release and ensuring it will live happily ever after. There is a strong ethics factor in both the world of the responsible hunter and that of a fieldworker who works on a collaborative basis – which may justify the analogy after all.

2. Getting into fieldwork

My entry into the world of field linguistics was unplanned and somewhat circumstantial. Although I had always been an enthusiastic traveler, the step from conducting psycholinguistic experiments by analyzing German participles to documenting a scarcely described language in West Africa seemed a bit steep to some of the people around me. I have to thank my *Doktorvater*, Richard Wiese (now a professor in German linguistics) and the many supportive and willing linguists at Uni Duesseldorf for allowing me do a PhD requiring very extensive travel and fieldwork while being based at a linguistics department with an utterly theoretical approach to the study of language. Chomsky, X-bar syntax, LFG, OT and dual mechanism psycho-models aside – that was fun but somehow my heart was leaning towards doing something "practical" and providing the data that would keep advances in linguistic theory alive. I was intent to become the hunter and get the meat so that the butcher linguists would have something to make their sausages from (not without me trying some butchery first by writing a grammar). I am grateful that Norval Smith in Amsterdam agreed to be my co-supervisor with some insight into the more practical aspects of linguistics.

Choosing a language then turned out to be not all that difficult, which is where the circumstantial factor came into play. I had an old friend who was a missionary in northern Ghana and, as it happened, was back in Germany for a visit at the time. Not a man of many words, he still convinced me that the Dagbani language

could use a grammar and since I was looking for a language to study, why not visit him in the savannah and check it out? I followed up with some investigation of what was actually available on Dagbani and made contact with those who had written something of relevance – an important step in the process of language selection and preparation. One of those people was André Wilson, a linguist who had written a grammatical sketch of Dagbani as a missionary back in the '70s but had never published it widely. He then became a subway conductor in London and later retired. My visit to him was of great help and he encouraged me in many ways to pursue my research idea.

I suppose the Dagbani experience got me hooked on fieldwork for good; it was an easy decision to accept a call for a postdoctoral researcher to document another language. Making contact with the Urarina people in the Peruvian Amazon again involved the mediation of a third party – it can be a good idea to have someone speaking to the community on your behalf. Once more it was missionaries who introduced me to a society that lives in somewhat withdrawn circumstances and is generally suspicious of outsiders. The missionaries must have told the Urarinas to treat me well, feed me and make sure I survived, because that is what they did. Upon my arrival in Peru's jungle capital, Iquitos, there were people from a small NGO waiting for me. They showed me around and dispatched a message for the Urarinas through a local shortwave radio station that most jungle Indians listened to. The Urarinas then sent a small delegation to town to pick me up, for which I was very grateful as the trip involved three days and nights through territories that were very foreign to me. We started by embarking on an overcrowded freighter up the Amazon, then the Marañón River and then continuing on the small village boat propelled by a small *peque-peque* motor whose sound would accompany us for a further 12 hours. I spent a month in the Urarina village of Nueva Unión to collect some initial data about the language, largely based on elicitation (which is not how I would preferably collect data, but was acceptable for an initial survey). Working in such a way showed the people what to expect from me, and when I asked them at the end of my four weeks whether they would like me to return, they very happily agreed. In order to assist me with formal requirements for the university ethics committee, they even pulled out a dinosaur of a typewriter and stationary with an official letterhead to put their decision in writing. I still keep a copy of this letter as a special treasure as it explains in very simple but kind words what the Urarina people understood my linguistic research would involve. Incidentally, the ethics approval process for Dagbani, a major language recognized by the Ghanaian government, was entirely different. That process involved walking from office to office at the University of Ghana, requesting and submitting formal letters of recommendation, paying fees for enrollment as a visiting researcher, and meeting with lecturers and other officials.

I am uncertain whether my work with the Miriwoong people could be classified as fieldwork or not since I permanently live in the "field". I still study an "exotic", largely undocumented language in a remote place – but I don't have to travel. In 2005, the language center here, which is entirely governed by Miriwoong people, advertised for a linguist to assist them with language documentation and revitalization (among many other, managerial tasks). I applied and now I am employed by the Miriwoong (cf. Olawsky 2014). While the process which links the linguist to a language was in fact reversed here and entirely controlled by the language speakers themselves, part of my role still involves documenting the language.

3. Travel and arrival

"Day 1", the arrival at a field site, is always an exciting part in the process of linguistic fieldwork. The stereotype which I had beforehand involved the idea of stepping out of an old, beaten-up bus in the middle of nowhere and suddenly being surrounded by a bunch of curious kids who would wonder what this wrong-colored guy was doing at their village. In my case, this scenario was usually not too distant from reality, but thanks to some preparation as described above, a local contact who was expecting me always helped me make a start with the community. Personally I found such intermediaries extremely helpful and I would always aim at making contact with knowledgeable people beforehand. In most cases this would also lead to at least a recommendation on where to live and how to set up a small household, if not more. In Ghana I had the name of a British expatriate who knew about my arrival and was going to lend me her house while she went back home for a while. The only requirement was that I kept and paid her local staff, as they depended on the income. So, I as a university student from a rather modest home suddenly ended up employing a cook, a gardener, a night watchman, and a day watchman, which took some getting used to but made me part of the community almost instantly. In Peru, the delegation who met me in Iquitos and led me to their community assigned me a place to live as soon as we arrived in the village. My start in Kununurra (Australia) required a little more self-initiative and I actually visited the place before moving up to make sure I had a place to rent when I started eventually.

Travel to my field sites has been memorable in its own right, sometimes involving the stereotypical beaten-up bus or other unforgettable means of transportation. Getting a ticket in the first place for the STC (State Transport Corporation of Ghana) bus was not simple and involved quite a bit of research and eventually someone to buy the ticket on my behalf. The nearly 700-kilometer ride from Accra to Tamale then took close to 24 hours, including only one major breakdown, but

the passengers and their accompanying animals tried to make me feel very welcome throughout the entire trip. There I also found out that the Ghanaian Airforce sometimes flew that route and admitted paying passengers, which is what I tried on my next trip up. It meant being at the airport at 6am with hordes of prospective passengers (in fact more than a plane could carry), waiting for the Airforce official to appear (usually around 10am) and then being among the loudest and most persistent people to hand him the money. With a bit of luck, I would be chosen and able to board shortly thereafter. The planes flew in somewhat precarious conditions but the trip took only around 90 minutes. The plane was not too different from the one I boarded in Darwin, Australia, to fly to Kununurra. When I requested a window seat at check-in, the friendly person behind the counter just smiled at me and said "Sir, they are all window seats".

Catching the river boat from Iquitos up the Amazon required much patience because there was no fixed schedule. The boat owners would declare a tentative departure time such as "tomorrow" and so people would turn up the next day. Sometimes, if the boat had enough freight and passengers (if there can ever be enough), departure might actually occur during the evening hours, but at other times one would have to return to a hotel after hours of waiting. To say the least, the port of Iquitos is not a pleasant place to hang around. On one occasion, my backpack with most of my valuables, including camera and passport, was stolen there. Expecting help from the local police was an illusion and it took significant effort to organize new travel documents to even be allowed to leave the country legally. The river boats were the equivalent of old, beaten-up buses, just the floating type, and would take several days upstream before dropping me at the mouth of the Rio Chambira to wait for people from "my" village to meet me with their little boat. On one of those rather adventurous journeys we had to get out into the swamp to push the boat over fallen logs and across some patches that were too dry to navigate (while trying not to think about potential crocodiles and anacondas in the swamp around us).

4. Life in the field – basic survival

Regardless of the travel type, I always reached the "field" eventually. Field linguists work in a range of environments, from urban to rural to the most remote extreme, and I have had a taste of all three of these. Before even thinking about how to gather precious linguistic data, there is the minimal requirement of basic survival, which entails such elementary things as sleeping, food, and health.

When I traveled to Northern Ghana, it was not my first visit to West Africa and I had a rough idea of what to expect. While my field site of Tamale was a

major city of half a million people, it still felt like a massive conglomeration of little villages and in terms of infrastructure offered nothing like the big cities I knew. Communicating with people back home required buying a phone card and using a public phone booth at the post office. In the early '90s, there was no public access to e-mail or internet connection in Tamale. Most food had to be bought at the market and was restricted to what was on offer, usually yam, tomatoes, peanuts and a range of other vegetable and fruit. The market ladies loved it when a *silimi-inga* (whitefella) tried out Dagbani and the men with their talking drums would follow me telling stories about a tall white guy until I paid them off (I wish I knew what exactly they were saying about me with their drums). Once I bought some roots that I didn't know and was assured they were good for cooking. After making a meal for a visiting friend with those, mixed with tomato and yam, we both had the most terrible gastro incident over three days – perhaps I should have asked how to cook those roots? It turned out that I had used a massive dose of ginger as a vegetable! (At that time they were not easily available in European shops and I had no idea they were great for spicing a meal – but not when you use half a kilo.) The following year, my wife joined me on a trip to Ghana to do research for her own thesis – and the eventful experiences with food continued. Eating chicken for dinner turned out to be an exciting exercise; in northern Ghana, of course, you do not buy chicken breast fillets in a shop – you go to the market, choose a live one and take it home for the gruesome task of turning it into a meal. If you've never killed an animal other than flies and mosquitoes, a chicken makes a substantial task and I came close to turning to vegetarianism. Without going into detail I can only say it would have made a perfect comedy clip watching the poor fowl trying to make its escape into the garden, followed by the dog, which had also shown an interest in the meal, me chasing the dog and my wife running after me in support. We eventually had our chicken with rice but generally adjusted our meal plan to involve less excitement. There was still the one and only restaurant in town for special occasions.

Living in a remote village in the Amazon rainforest had a whole different feel and took a range of preparations – some of which I only learned of after my initial visit to the Urarinas. The fact that the village had sent a delegation to meet me was invaluable as they provided me with great instructions of what to buy in town for my trip. These essentials included a mosquito net, blanket, buckets, bowls, plates, rubber boots, and a range of canned foods recommended by locals, plus boxes of shotgun shells, batteries and fishing hooks as trading items. Locals had a very good sense of what one needed to survive and I would have lacked many items without their advice. I was initially put up in a now-empty medical post, in essence a shell of a building containing one chair, one table and one bed frame plus a little bathroom with a showering area and a toilet bowl (not connected to any drainage

system). I was probably assigned this building because its appearance resembled the *gringo* dwellings people had seen in town. The hut's most practical feature from my point of view was the roof gutter as it offered the perfect opportunity for a shower when it rained, and for gathering rainwater. After trying out the manually-operated water filter I had brought from home I realized it would take me half a liter of sweat for about one liter of filtered water and so I chose to collect rainwater, store it in a folding canister I had brought and use that for drinking. The hut had other disadvantages as it seemed to be a haven for creatures that I found difficult to handle. I was quite glad that a neighbor was just passing by when I discovered a large tarantula moving slowly through the room: I had no idea they could get so big – much bigger than the unpleasant-looking spider that found its way into my mosquito net. On another occasion it turned out that a tribe of fire ants had made its path right through where the bed was standing – I chose to give way. In terms of nutrition, I was well taken care of by the chief's family with whom I shared all of the canned foods he had suggested that I buy. Since I had no kitchen of my own, they prepared meals for me, which was very convenient and gave a me an idea of the local cuisine – plantains, bananas, cassava, and *urari* roots (a tuber that apparently gave the Urarinas their name), sometimes with fish or fresh game (usually monkey or squirrel, sometimes peccary or deer, as well as all kinds of birds). An aspect that took much getting used to was that that there were no fixed meal hours and often people had only one meal a day. There were days where someone would bring a large hot meal at 6am and then nothing for the rest of the day, so it was useful to always keep some bananas at hand for those times.

The supply situation changed when I returned with my wife and 2-year-old son the following year. We created our own household in one of the "normal" houses – constructions made of 100% natural materials (no nails – people use lianas to tie poles together), built on stilts and without walls. We brought a large tent from home and set it up in the house as it offered much better protection from the kinds of creatures I had previously encountered. Because of the various biting insects (it seemed there was a different biting bug for each time of the day, let alone the night hours) we virtually lived in a tent for half a year. But we were unprepared for another situation: while my first visit was during the dry season which is characterized by an abundance of food, the longer stay fell right into the wet season, which meant people could not move far to hunt for game, and even the fish were so spread out in the waters surrounding our village that people found it difficult to catch any. As a result, the Urarinas had hardly enough food for themselves and found it harder to share while we were hoping to trade shotgun shells for food. Only after a couple of months of a minimal diet (I had lost 15 kilos in weight), the people decided to go on a hunting expedition that would involve a week's travel. Their successful return was celebrated by the entire village. The lesson I learned

from this experience was to always travel well-informed about seasonal variations, crop cycles and other circumstances that might affect living conditions.

There were a few more skills that we had to learn: my wife became very skillful in cooking on open fire or hot coals, gutting and cleaning animals, and doing the laundry in a bucket. During the hard times we even learned to pray that our two chickens might lay an egg to provide some protein. I was once bitten by a peccary (a kind of wild pig) but otherwise found life in the jungle fascinating and gathered as much linguistic and cultural data as possible, including the art of trapping different animals with a variety of traps for each species. One of the unforgettable incidents involved a domesticated monkey who taught me to never use looseleaf collections for my field notes: this cheeky fellow ran away with three pages of my notes on Urarina adverbs which I never found again. Lesson: always use bound notebooks and make sure not to leave them lying around!

It seems difficult to describe my work with the Miriwoong as "life in the field" since the two have merged to a good extent. It feels like studying a fascinating language in a beautiful place, working hand in hand with the speakers who are hoping to preserve it for future generations.

5. Working with people

Working with people on their language is exciting and precious – but how to start? Finding the "right" people is the first aspect to deal with after one has had the chance to settle in. The choice of speakers will always be linked to circumstances to some degree, depending on the size of the speaker community, availability and other factors, but it is better not to leave everything to chance. In Ghana, I started working with my first Dagbani speaker, Moses, following a recommendation from someone who had learned the language with his support. Moses had a very good idea of the difficulties that a *silimiinga* might face in learning Dagbani and he made sure that I mastered the tones well enough – well, mostly. Following the pattern of many African languages, Dagbani turned out to have an incredibly complex tone system, which ended up taking a lot of my time. The problem was not in identifying lexical tones, but the changes which occur when words are combined in a phrase or sentence. I would proudly use the right tones on words in isolation but fail quite miserably when trying to put these together. When looking for suitable language consultants, the recommendations of others can be very helpful and I discovered that sometimes, where people offered their services unsolicited, they turned out to not be ideal consultants. Teachers can be great to work with but sometimes have a very prescriptive understanding of their language, which is not what a linguist needs in order to get an objective picture.

In the Amazon, the choice of consultants was restricted to the population of one small village and it was determined that the obvious person to work with me had to be the village chief. It also helped that he was rather fluent in Spanish, which made things much easier for me at first. Over the years, the chief turned out to be a wonderful consultant with special skills but initially, things were difficult, because the one thing he could not do was tell stories. He would wait for me to ask questions and then answer them briefly. Fortunately, as I expressed my desire to include narratives in my data corpus, he took me to a man who was the perfect storyteller. Medardo did not speak a word of Spanish, so it was good to have the chief with me as a mediator and interpreter. He knew all the traditional tales including creation stories, the encounter with the first white people, and grueling accounts of fights with surrounding cannibals, all told with passion and expression – a true treasure. And the chief demonstrated his skill by proficiently helping me transcribe and interlinearize those recordings, spending hours, days and weeks patiently going through the same passages again and again with me, and answering questions about paradigms, meanings, and grammatical structures. Urarina proved to be very different from Dagbani: it does not use tone to make lexical distinctions (about which I was somewhat glad) but the challenges appeared in a different area – morphology. I discovered twenty-four morphological slots on verbs and despite producing a 900-page grammar still have many questions – let alone getting used to OVS word order (cf. Olawsky 2005, 2007), which early linguists assumed did not even exist.

It is important to always work with a variety of different speakers, ideally involving different age groups, genders and social classes, where possible. Out of cultural reasons it was inappropriate for me to work with Urarina women and so the chief and I came up with a plan to include these anyway: my wife (who was in the field with me during two out of five lengthy field trips) would operate the sound recorder while the chief's wife narrated a procedural text planned in advance. The chief would explain to his wife what to do. We agreed that she could talk about how to make *masato*, a traditional alcoholic drink made from cassava and plantains. Thus, the two women, not understanding a single word of each other's languages, sat there, providing a fine recording of an interesting text genre. On another occasion in a different village during a dialect survey I talked to a man and his wife together and thus obtained a recording from another female speaker.

It has always been my policy to compensate people for their time. One will have to negotiate the level of compensation and how often payments will be made. In Ghana, anything involving money required some bartering, whether I had to buy food on the market, take a taxi, or engage a language consultant. A good approach was to consider the equivalent of what a schoolteacher would be paid, do

the math and pay a bit more than that. After all, the people who worked with me very much fulfilled a true teaching function. Some of my Urarina consultants had never been to a town and had no use for money and so we thought about appropriate ways of compensation together. Their needs would always somehow match what their time could be deemed worth and so I ended up buying fishing nets, machetes and other tools that functioned as trade items for their efforts.

I find talking with people about their language extremely rewarding; it feels like opening a treasure box and taking out one interesting piece after another when one discovers yet another detail about a language. Of course not all sessions are like that – some require more patience than others. One of the obstacles I have come across in people I have worked with is related to the fact that some of my consultants found it difficult to relate concrete objects to 2D representations of them. For me it might be a perfectly logical process to look at a photo and talk about what I see on it. For some of the older Miriwoong people I have worked with it was not as straightforward: the items shown in the photo were much smaller than the actual objects or otherwise hard to recognize – and they simply were not 3D, touchable objects. Drawings were worse because they seemed even less real, so the best way of obtaining the relevant information would be going on a trip and looking at "the real thing".

Elicitation is not the best method to learn about a language but it definitely is part of a fieldworker's range of tools in exploring grammatical structures. Among many elicitation techniques are direct questions for translation – and often these can be a real challenge. While some speakers have a natural talent for seeing the linguist's need and will guide you through entire paradigms almost unsolicited, others can find it difficult to distinguish between a real conversation and a hypothetical question. An example: (Linguist) "How would you say 'What is this?' in your language?" – "It's a dog". – "No, I want to know how to ask the question". – "Dog?" – "Uhm, I know the word for dog but if I didn't know it and wanted to ask, what would I say?" – "But I told you it's a dog!" … – This could go on for a while longer and it will take ages to fill the gaps, say, in a verbal paradigm: "How do you say 'I am tired, you are tired, he is tired'?" – Answer: "Yeah, I'm tired, too".

After working with one elderly Miriwoong consultant in a lengthy elicitation session (during which he got upset more than once because of the stupid questions I would ask), the consultant finally sighed and said, "You made me think really hard today and I know it's important. But these are very difficult stories you want from me and they make absolutely no sense!" In a way, hypothetical scenarios can be very confusing to people whose senses tend connect to the "real" world around us much more efficiently than ours. To me, this is a good reason to base linguistic work largely on texts that speakers produce naturally, rather than relying on elicitation. Of course elicitation has the advantage that the linguist is in

control of the variables and can steer the conversation in a direction that is likely to lead to the "right" answers. But there is the risk that a response we get from elicitation is somewhat distorted, unnatural or not cohesive. In each of my field sites, I built up a text corpus of different genres, typically including narratives (traditional and temporary, first and third person, fiction; e.g. Olawsky 2002), and where possible, I added songs, poems, jokes, instructions, and dialogues. I also found it useful to add tasks that lean more towards semi-structured elicitation such as prompted descriptions of static or dynamic stimuli (pictures, videos). Among a number of descriptive tasks, I asked Urarina speakers to describe geometric shapes such as triangles and odd-shaped figures in their own words. This led to interesting insights on what these were perceived as, often involving comparative statements.

While emphasizing the significance of natural texts, I must admit that elicitation has been indispensable in my work with all three languages. Take Miriwoong, for example, which has a verb system more complex than anything I have seen as a linguist: the twenty inflecting verbs (on top of thousands of uninflecting coverbs) have an array of over 2000 largely unpredictable forms, involving different stems for different tenses and somewhat irregular cross-reference morphemes for subject and object. No corpus in the world would happen to include contexts for all of these forms and therefore going through paradigms becomes unavoidable.

In transcribing my data I found that working with a speaker (not necessarily the same speaker who was recorded) made this task infinitely easier. Chances are that the helper might help me obtain a broader, tidied-up transcription rather than a narrow phonetic one but knowing what the speaker *meant* to say is an invaluable starting point.

In working with people I have always made an effort to learn to use their language to the best of my ability. While some might consider this to be a waste of time, I find it rewarding in terms of connecting with the people I live and work with, showing my appreciation for what defines them and immersing myself in their lives at least on a linguistic level. There also are practical benefits of becoming a partial speaker of a language. In Urarina territory, it helped me greatly in conducting a dialect survey during which I was able to communicate with new speakers without relying on the continuous support of my familiar travel companions. With speakers of Dagbani I found it inspiring being able to do my shopping at the market, engage in small talk, and take a taxi using the local language. In working with the Miriwoong people I value their way of presenting me as a wonderful speaker despite the fact that this is a gross overstatement of my proficiency. One of the Elders loves prompting me to answer questions in Miriwoong to show off, since he is one of those who have taught me. The smiles I keep getting for even trying have become a treasured memory.

6. Ethical considerations

Initially for me as a researcher, conducting fieldwork and obtaining a tangible output from it was linked to an academic career that would determine my further professional path. For the community, a linguist's visit tends to be just one additional aspect of their lives with all its complexities and chances are that such a visit will be incredibly less important to the community than it is to the researcher. Yet, the presence of an outsider will always impact a community in one way or another and it is important that we keep this in mind as we live in and with a speaker group for a limited time. For me, having good, positive relationships with the people who share their knowledge with me has been the key to wellbeing and success during fieldwork. While these relationships will always benefit from a solid collaborative approach, they are also affected by the community's cultural settings and attitude towards the fieldworker. I found it important to always explain from the beginning why I was interested in the language and what my research would entail. Probably more than once people were somewhat puzzled that someone from so far away had come to learn about their particular language – why would anyone do that? Would they ever travel thousands of kilometers to a tribe they didn't know just in order to learn their language and then go back to their own group and tell them about it? Some level of bemusement can probably never be entirely eliminated, for good reasons.

So how can a community get something out of the fieldwork exercise? One may wish that community desires match what a linguist thinks will benefit the community, at least to some extent, but this is not always the case. For instance, the grammars I was compiling as the main output of my linguistic fieldwork (e.g., Olawsky 1999, 2006) may not have been the most precious benefit in the eyes of the community; a dictionary and literacy materials, as well as non-linguistic support definitely were instead immensely welcomed by the community. In working with the Miriwoong people, the match between the community's and my expectations is as good as it can get since our goals have been defined and discussed as a team. In earlier work, it turned out that the Dagbani orthography was not standardized, resulting in different ways of spelling by the various NGOs who assisted with literacy and economic development. I applied for a grant to organize an orthography workshop and the key people from different parts of *Dagbong* had a chance to come together and discuss orthography reform. For the Urarinas, different matters were important and while they valued the collection of traditional stories in Urarina and Spanish that I had printed for them, they also expressed needs that were of a much more physical rather than linguistic nature. I was the one who in their eyes had access to material resources. Thanks to the generosity of friends and relatives we were able to raise donations that funded a solar panel and

a shortwave radio for the village, helped repair the community boat and addressed a few further needs. Not a big deal for us, but the world for our less-privileged friends in the community, given that other villages in Urarina territory already had access to such resources.

The community should also own copies of the recordings and outcomes of a linguist's research and so it was with great delight when I heard, years after my fieldwork in the Amazon, that the CDs with recordings of traditional stories were still playing. I now have to get USB sticks ready in order to keep up with new technology.

Anecdotes like these also remind me that linguistic fieldwork has been more than a job – it has become part of a lifestyle. Thinking back to embarking on the first adventure as a fieldworker and twenty-plus years later, I am grateful for having had the opportunity to move through distant landscapes and among peoples and their cultures and languages.

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CHAPTER 9

My fieldwork, from Georgia to Guinea

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Nina Sumbatova working with Negidal speaker Galina Ivanovna Kondakova in Vladimirovka village, Khabarovskij kraj

1. A few words on "Kibrik's field trips"

My first experience of fieldwork was in 1988, when I took part in a collective field trip headed by Aleksandr Evgenyevich Kibrik (1939–2012). Professor Kibrik, one of the most outstanding Russian linguists, worked all his life at Moscow State University; from 1992 he was the head of the Department of Structural and Applied Linguistics.

Collective field trips ("linguistic expeditions") are a special phenomenon in Soviet and Russian linguistics that have now been discussed in a number of publications (Plungian & Fedorova 2016; Borshchev 2001; Kibrik 2007, 2008, 2010a, b;

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Belikov et al. 2012; Dobrushina & Daniel, this volume; see also <http://otipl.philol.msu.ru/~kibrik/site/expeditions>). This research form was first tried by Kibrik in 1967, when he was still a very young man – not much older than his first students. In a collective field trip, a group of friends and colleagues goes to study a language that was not studied before. Each of the participants has an individual research domain, but the work is supposed to be really collective: all participants let each other know immediately of any results obtained in the field, and all new facts and working hypotheses are discussed by all team members immediately after they are attested or suggested. This creates the possibility of intellectual breakthroughs and extraordinary effectiveness of research. A relatively short collective project (say, 3–6 weeks) often gives results that could only be obtained after a year of extensive fieldwork by an individual.

Kibrik's first field trip was to Daghestan to study the Lak language. For many years after that, Daghestan remained the main and most beloved area for Kibrik's field trips – of course, the main reason for that was the high number of unexplored languages spoken in Daghestan, but it is clear that this was compounded by Kibrik's special affection to that remote land in the mountains.

In Soviet and Russian high schools, students are usually obliged to have several weeks of *praktika* 'practice' – practical work in their subject field. Whereas future teachers did their practice at schools and future engineers at industrial objects, Kibrik managed to organize fieldwork *praktika* for the students of linguistics. This made collective field trips an officially sanctioned and financially supported form of study and research. Although officially sanctioned, the fieldwork *praktika* was not bureaucratically regulated: the organizers of field trips were free to choose the area, the research tasks, the methods and the participants.

For a student, the advantages of a collective field trip are quite obvious. In such a field trip, you learn fieldwork methods and techniques from more experienced colleagues who are working with you and ready to help you. You also learn a lot about linguistic theory – while preparing your own elicitations, participating in seminars, discussing other people's linguistic problems and just listening to the talk at the dining table. You train your social skills through establishing friendly contacts with language consultants. At that time, relating to the language consultants was relatively simple, because all collective field trips were inside the Soviet Union or, later, Russian Federation: Russian was the meta-language and fieldworkers and consultants shared aspects of cultural backgrounds: school programs, social problems, books, movies, TV, etc. The data obtained during field trips are a good basis for one's future semester projects, diploma papers, and even PhD dissertations.

Collective field trips helped a large number of Russian linguists to find their way in linguistics – a person who, say, studied class agreement in a Daghestanian language did not necessarily continue in Caucasian studies, but it is quite probable

that he or she went on researching the typology of agreement or studying similar phenomena in other languages, or doing formal syntax: the chains of research subjects could be very different, but for many of us, they started with our first research topic in our first field trip.

Another important thing about the field trips is the introduction they offer to life outside big cities. Russia is a highly centralized country with serious differences between Moscow and a few other big cities, on the one hand, and many small towns and villages all around the country, on the other hand. Many inhabitants of Moscow and Petersburg do not have a slightest idea of life outside the capitals. This is especially true with respect to the areas inhabited by national and religious minorities. Even now, when many Russian linguists have the possibility to study and work abroad, traveling to remote corners of Russia – like the villages in Daghestan, Siberia or the Far East – is still an exotic experience.

So, for the students and other team members, a collective field trip is a fantastic thing, which combines many things that are important for a young linguist and a young person in general: freedom, order, intellectual challenge, creative work, friendship, and the spirit of a big adventure.

Kibrik headed his field trips from 1967 to 2008. There were also other collective trips in the '70s and '80s, and there are even more now – now not only at Moscow State University, but also at some other universities in Moscow and Petersburg. In most cases, the organization of work and everyday life in the collective field trips generally follows – with certain modifications – the traditions established by Kibrik.

Each participant in a field trip had an individual research topic. Sometimes, this topic was shared by two participants, a novice and a more experienced person. Such a group, called *kust* 'shrub', had to work and discuss all the problems together. At the end of the field trip, each participant or each *kust* had to write a field report – a description of the research topic in the language in question, for example "Case forms in the language L", "Tense and aspect in L", "Verbal derivations in L", etc.

Everyday life was organized along some very simple rules and followed a standard daily routine. Since the field trips were always in the summer, we used to live in local schools: it was vacation time. The work with the language consultants usually lasted from 9am to 1pm – four "classes" of 45–50 minutes each with short breaks between them. After lunch, we had time to analyze the obtained data and to prepare questions for the next working day. In the afternoon, we very often had working seminars where the participants exchanged their preliminary results and discussed them. The time after dinner was usually left for rest: we gathered in the living room and organized something fun or even romantic: dancing, reciting poetry, playing a game or, most frequently, singing along with the guitar.

All participants took part in turns in preparing food and other housekeeping activities. It was a good opportunity to train one's culinary skills – as a rule, in relatively difficult conditions. In Soviet times, the problem of food was not very simple to solve. It was impossible to buy food at the villages where we were working. Several months before the start of a field trip we made a list of necessary foodstuffs and distributed shopping responsibilities: each of the future field linguists had to buy certain foods (cereals, noodles, tinned meat, cheese, jam, condensed milk, etc.). Fortunately, we did not have to carry all that stuff ourselves – there was a special truck that belonged to Moscow State University and was designed to carry all our things and ourselves. The truck met us at the railway station or the airport and took to the place of the field trip. Roads are always a serious problem in Russia – the state of roads in some places is so poor that the destination point can hardly be reached without a truck or tractor.

I took part in four field trips organized by Kibrik. In 1988 and 1989, we were working on Svan, a minor language of the Kartvelian language family. The field trips of 1990 and 1991 were to Daghestan where we studied dialects of Dargwa (East Caucasian). Later, I organized field trips and did fieldwork alone or with small groups of colleagues. Below, I will tell a little about the main fieldwork projects that I have worked on.

2. My field trips

2.1 Svaneti: Imprinting

My first field trip was in 1988. We went to Abkhazia – now a region with complicated status, at that time, an autonomous republic within the Soviet Republic of Georgia. A part of Abkhazia, called Abkhazian Svaneti, was inhabited by speakers of Svan. As far as I remember, Kibrik planned to study the Upper Bal dialect of Svan. But when we arrived at our intended village, we found that it was divided into two parts with two different dialects – and neither of them was Upper Bal. We decided that each team member would adhere to one of them, but the two dialects were interacting and mutually influencing so that the real number of language variants spoken by our consultants was approximately equal to the number of the consultants themselves. This meant that the results of our fieldwork were inconsistent: asking the same question of any two speakers, you inevitably received two different answers. Since it was my first experience, I obtained a partially wrong idea of fieldwork: since then, whatever language I work with I invariably get surprised when two speakers give similar answers to any question of mine. At the same time, it was very useful to know from the very beginning that the results of fieldwork can be inconsistent, vague, and difficult to interpret.

The problems with the dialects convinced Kibrik that if we wanted to hear Upper Bal speech we had to go to Svaneti proper – a highland area in the northwest of Georgia – at that time, part of the Soviet Union. This plan was executed the next year (1989) when we traveled to Upper Svaneti, one of the highest and most beautiful regions of the Caucasus. And, very naturally, the Svan language seemed to me as beautiful as the Svan mountains. In a certain sense, all languages are equally interesting for a linguist, but it happens that you fall in love with the first language you study in the field – I think it is a kind of imprinting. This is what happened to me in the first two field trips. Like Georgian and other Kartvelian languages, Svan is a language with complicated morphology, especially in the verbal domain, with many tense-aspect-modality forms and rich patterns of verbal derivation. In the two field trips, I studied the causative derivation and morphophonemic rules, which are rather intricate in Svan. After 1989, I did not return to Svaneti, but I still remember every verb from our small working dictionary and hope that one day I will be able to return there.

When we were staying in Svaneti, a tragic conflict arose between the Abkhazians and Georgians in Abkhazia. Though this did not touch Svaneti directly, there was serious tension in Mulakhi and other villages. The television did not work so that people did not know what exactly was happening and to what extent it was dangerous for them. The inhabitants of Mulakhi, the village where we were working, were afraid of mysterious Muslims who they believed were going to attack their village. The men went to the passage in the mountains between Svaneti and the Balkar Republic: they thought that they had to protect their village from the potential Muslim invasion, which, fortunately, was a pure fantasy (this group returned in a couple of days). The events in Abkhazia left 18 dead and more than 400 injured. We did not know it at the time and learned much later, when we returned to Moscow. The conflict was finally stopped by the army, which occupied the key points of Abkhazia. In several days, when we were going home through Sukhumi (the capital of the republic), the military phase of the conflict was over, but there were still military checkpoints and patrols in Sukhumi and along the whole road from Svaneti. Of course we felt compassion for the victims of the conflict, but I must admit that I did not understand how really tragic the situation in the Caucasus was at that time. I did not know that we were in the beginning of a long chain of social, religious, and ethnic conflicts in the area that were to follow.

2.2 Daghestan: "Perestroika"

The two next field trips with Kibrik were to Daghestan: in 1990, we went to the village of Mehweb, and in 1991, the village of Itsari. The inhabitants of both villages speak "dialects" of Dargwa (East Caucasian). Dargwa, which is officially treated

as one language, is rather a cluster of about 20 related languages: some of its variants, which we traditionally call "dialects", differ more than, for example, Slavic languages.

Mehweb is an isolated dialect of Dargwa that is under strong influence of Avar and Lak. As a result it has obtained some features that are not typical of Dargwa and can be attributed to Lak or Avar influence: the system of genders, for example, is very much like that in Lak.

The trip to study Itsari, another dialect of Dargwa, was a very important event in my life: it determined the main direction of my work for several years in the future. First, I got involved in Dargic studies and, second, our group became friends with Rasul Mutalov, a Daghestanian linguist and native speaker of Itsari. Long after this field trip, Rasul and I prepared a grammar sketch of Itsari (Sumbatova & Mutalov 2003). I returned to the studies of Dargwa in the 2000s, and this language (or language group) remains the most important domain of my research work.

Again, our quiet life in the mountains was interrupted by politics, but this time, the main events were happening in Moscow. In the morning of the 19th of August, we heard on the radio that President Gorbachev was not able to perform his presidential duties and that power went to the hands of the State Committee of the State of Emergency ("GKChP"). This was the well-known August Coup when some Soviet leaders who were against Gorbachev's politics of *perestroika* made an attempt to take control over the country from the president. Exactly as in 1989, we could not obtain information on what was happening. The radio was hardly heard, the television was not working, and there were no accessible telephones in the village. We knew that there were tanks in Moscow, but could not know if there was any danger for our families and friends at home – and, of course, we imagined the worst. Besides, the victory of the coup meant the end of *perestroika*, which for most of us was the start of a new and better life. The next evening, some villagers invited us to their house to watch TV on the only set that was working in the village. The speaker of the news program reported happily that the inhabitants of Moscow were now used to seeing the tanks at their houses. His forced cheer and intonation – typical of Soviet television – made us even more discouraged: they showed that the old life was returning together with the old discourse. To our joy, the August Coup did not last long. On the 21st of August 1991, everything was finished, and the "GKChP" disappeared.

I visited Itsari again in 2007: the village was then almost empty, because most of its inhabitants had moved to another place on the plain. Itsari looked very sad with empty houses, an empty school and packs of stray dogs in the empty streets. However, for me Itsari remains a positive place, that reminds me of the victory of *perestroika*.

2.3 Adyghea: Complexity and responsibility

Between 1992 and 1999, personal reasons meant I did not participate in any field trips. By 2000, I was teaching at the Russian State University for the Humanities in Moscow and dreaming of organizing collective field trips for the students of this university. In 2000, my friends and colleagues Elena Kalinina and Svetlana Toldova, who were working at Moscow State University, invited me to participate in a field trip to the Republic of Mari El (Russian Federation). After that, together with Yakov Testelets and Svetlana Toldova, I managed to start field trips at my home institution. The two first field trips were to investigate the Khakas language (Turkic), which is spoken in South Siberia. Between 2003 and 2008, I was engaged in organizing field trips to a new area in the Caucasus – the West Caucasus, the Adyghe Republic, to study the language of Adyghe.

Adyghe and other languages of the West Caucasian family are polysynthetic; this language type is extremely rare in Russia and all over Eurasia. The West Caucasian languages show extraordinarily complex predicate structure and many other unusual features, like difficulties in differentiating parts of speech. For a speaker of Adyghe, there is no problem in producing a word like this:

(1) sə-qə-t-de-p-fə-r-a-ʁa-dže-šʼtəʁ
1SG-DIR-1PL-ASS-2SG-BEN-3SG-3PL-CAUS-read-IPF
'they forced me along with (the rest of) us to read it to you'

All non-auxiliary words – both those which we translate using verbs and those that are usually used as nouns – can express number, person, tense, modality, negation. For example, in sentence (2), the word 'wife' bears the past tense suffix *-ʁ* and is used as a predicate without any copula.

(2) mə bzəlfəʁe-r se s-jə-$ŝ_{w}$əzə-ʁ
this woman-ABS I 1SG-POSS-wife-PST
'This woman was my wife.'

In Example (3), the predicate of the sentence is the question word 'what', which bears the question marker *-r*. The word *wə-z-ç̣'ə-k_{w}əwe-re-r*, which means 'shout' (or, rather, 'the reason for shouting'), can be viewed as the subject of the sentence. In the second question of the same example, the word *načʼalʼnjək* 'boss' bears the question marker, which more typically occurs with verbs.

(3) səd-a wə-z-ç̣'ə-k_{w}əwe-re-r? wə-načʼalʼnjək-a?
what-Q 2SG.ABS-REL-RSN-shout-DYN-ABS 2SG.ABS-boss-Q
'Why are you shouting (at me)? Are you a boss?'

In (4), the personal pronoun 'you' is used as the predicate. This predicate agrees in person with the subject *se* 'I' and expresses negation through the negative suffix *-ep*.

(4) se sə-we-r-ep
I 1SG.ABS-you-PRED-NEG
'I am not you.'

Studying this type of grammar was very interesting, and it yielded good results: we discovered a number of facts that were interesting and important both for Caucasian studies and for linguistic theory and typology, especially in the domain of syntax. For me it was also a period of testing and improving my managing abilities. It was at this time that I realized that the collective field trips are very different for the head of the team and for team members. While students are enjoying intellectual pleasures, their leaders have to think about their safety, health, alimentation, semester work, and mental states. Any field trip includes different types of work, which tend to multiply when people are going to a field trip in a big group.

First of all, somebody chooses a language or dialect to work on and establishes contacts with the community before you go to the area. When you are working alone or in a group of two-three people, it is sometimes possible to go to a place without any preliminary agreements: if something goes wrong, you can immediately try another place. But this option does not exist if you come in a group of twenty. The head of the collective field trip should be sure that the local people agree to host you at their village. He also has to find a place to accommodate the whole team, which is not always simple.

Second (or first?) you have to find money for the trip. Theoretically, you can go with the money that is assigned for the students' *praktika*. However, the money we used to receive from the university only covered transport and accommodation. The worst thing is that we did not have any source of funding to pay our consultants. In some cases, we managed to get a grant from the Russian Foundation for the Humanities, but when we were not so lucky, we had to cover the surplus expenses with our own money. As far as I know, my colleagues are still doing the same every year.

Organizing students' *praktika* or getting a grant includes a lot of bureaucratic activities: writing applications and reports, signing many official papers, wandering across the university's corridors of power and trying to convince those in power that your field trip will be immeasurably useful for the students and will not devastate the university's bank account.

You further have to think about how to get to your destination in the safest and cheapest way. On arrival, you have to organize work and everyday life: to transform an empty village school into a place where twenty people can live and

work, to buy food and arrange the place to prepare it, to think about the personal safety of the team.

You have to establish contacts with the local people and with the village administration, to find language consultants, to organize the work. If you must provide consultants for a group of twenty, you need a whole group of people who agree to come and work every day for several hours. By the way, this is the reason why there were very few Soviet collective field trips to strongly endangered languages. When you deal with dying languages (Tungusic, Eskimo, Chukotko-Kamchatkan, etc.), you just cannot find language consultants for a group of more than three or four people.

As a result of all these duties, the head of an expedition usually has much less time to do his own research than other team members. Organizing collective field trips requires either outstanding leadership skills (which was a very noted feature of A. E. Kibrik) and interest in organizational work, or incredible altruism. That is why after several years of organizing collective field trips, I decided to concentrate on Daghestanian studies and to pass the Adyghe field trips to colleagues who were ready to continue them.

In general, the West Caucasian field trips had been a rather successful project. They resulted in a number of publications, diploma theses, and even PhD dissertations. In 2009, we published a collection of papers on the grammar of Adyghe (Testelets et al. 2009). Since 2009, the West Caucasian field trips have been headed by my colleagues Yury Lander and Peter Arkadiev. They organized two research teams for two closely related languages, Adyghe and Kabardian. Now they are planning to start studying Abaza, the most endangered language of the family.

2.4 Far East: Language shift

When I was a student at Moscow State University, I hardly realized that linguistic fieldwork was possible outside collective field trips. This is why my first attempt to do fieldwork outside a big team came rather late. Between 2005 and 2010, I took part in several field trips to the Russian Far East. This was a project aimed at documentation of several highly endangered languages of the Tungusic language group (all of them spoken in Khabarovski kray). The project was headed by Elena Kalinina and funded by the Endangered Languages Documentation Project (ELDP). There were only four of us: Elena Kalinina, Svetlana Toldova, Valentin Gusev and I, and in most places we were working in twos. One more new thing was that now we did not aim to fully investigate the grammar of these languages: our principal task was to document them, which mainly meant recording as many oral texts as possible and carrying out their morphological analysis (glossing). In

the first two field trips, we visited several places: our task was to decide what languages or language variants we would be documenting. After that we worked with three languages: Ulcha, Negidal and the Kur-Urmi dialect of Nanai.

Traveling to the Far East and wandering across the Far East was an unforgettable experience, and I liked working in a small team, but in a certain sense, the Tungusic affair was the most depressing project I have taken part in. We had to work with dying languages and to observe how they were dying. For example, in 2005, we got acquainted with four speakers of Oroch, a fourth language of the Tungusic language group. You still can find this language in any language classification, but it is no longer spoken at all. The speakers we found in 2005 were two men and two women, all about 75 or even older. In fact, they did not properly speak their mother language: they knew a certain number of words and could recite some texts that they had already recited several times to previous visitors, but they could not speak Oroch fluently or translate from Russian into Oroch. Since some publications of Oroch texts existed by that time and we obviously were not able to add much to those texts, we decided that it was too late to document Oroch. In five years, some colleagues from St. Petersburg came to the same places and did not find any speakers of Oroch: now, the language was extinct.

Other languages, which we still tried to document, were very close to that state – Negidal with five speakers, Kur-Urmi with approximately seven in 2005 and three in 2010. Ulcha was in a better state, but still seriously endangered, with no speakers younger than 45–50. We managed to record several hours of oral texts, but it was difficult to find fluent speakers. Even the people who still remembered their languages did not usually use them in their everyday life. This resulted in serious difficulties when they did attempt to speak: they tended to forget common words and use a reduced set of grammatical forms and constructions; their speech abounds in hesitation, pauses, repetitions, and code-switching.

2.5 Daghestan again: Ideal place for fieldwork

About ten years ago I decided to resume my Daghestanian studies and started a research project on the dialects of Dargwa. Again, it was a small team of four people: three of them, Dmitry Ganenkov, Natalia Serdobol'skaya and myself, are now continuing our typological and Caucasian studies; the third participant, Viktoria Khurshudian, teaches Armenian in Paris. In 2008, within the framework of this project, I went to the village of Tanti with my friend and colleague Yury ("Yura") Lander. Now I feel that it is in Tanti where I finally found my ideal style of fieldwork, and fieldwork location.

Tanti is a relatively small village in Central Daghestan (about 600 people). Like many other languages of Daghestan, Tanti is a small, but still-vibrant language (or, rather, a dialect of another Dargic language, Tsudakhar). Although many people in Daghestan now migrate downwards, from the mountains to the villages and cities in the plain, the language shift has not gone as far as in other regions of Russia. In particular, Tanti is still acquired by children as their first language.

Yura and I lived with a wonderful family who are now our best friends. The head of the family, Magomed Mamaev, is a schoolteacher, and we could work with him, other members of his family and his colleagues at school. The family of Magomed consisted of his wife Zhuma and three children, two boys and a girl – Magomed and Zhuma's children are now married young adults, with six children among them.

It is worth mentioning that during our first three visits to Tanti, Magomed's family refused to take any money from us. We lived at their house, worked with Magomed, ate with their family – this all took them a lot of effort and time, but they refused to receive any reward for their excellent work. This is the way they understand hospitality.

Magomed is an ideal language consultant. He is smart, creative, patient, and interested in his language. Now he understands most linguistic problems we are trying to solve for his language.

The language that we have been studying in Tanti is to a certain degree similar to other languages of the Dargic group that I had studied before (first of all, Itsari), but it had never been investigated before us. Thus, all our findings were absolutely new, but at the same time the work was efficient, because I knew what phenomena to expect and therefore what questions to ask. Every day we made discoveries of various sizes, which is the most pleasant part of all fieldwork. Yura was mainly engaged in nouns and noun phrases; I studied verbs and clause structure. Yura impressed the native speakers by showing them the enormous table of nominal forms that he managed to fill in: the table contained about 90 forms in the singular. I tried to construct a model of clause structure and agreement that covered most clause types and cases of agreement that we came across.

Since 2008, I have visited Tanti every year, with Yura or alone. I used the Tanti data to prepare my doctoral thesis; in 2014, Yura and I published a book that contains a grammar sketch of Tanti and several chapters on the problems of its syntax (Sumbatova & Lander 2014).

2.6 West Africa: Creating a pidgin

In recent years, in January 2014, 2016 and 2017, I tried an absolutely new fieldwork type – I mean, new for me. These were field trips to West Africa (Guinea and Côte

d'Ivoire) headed by Valentin Vydrin. Valentin Vydrin is now Professor at INALCO (Institut national des langues et civilisations orientales) in Paris. He specializes in the Mande languages (Niger-Congo) of West Africa. Valentin pursues a different type of collective field trip: in his teams, each participant studies a language of his/her own, but they all live and work together with their language consultants in the same place in the relevant country. I joined Valentin's field trips, but studied a language of a different language family from him and the other members of the trips. This is the language Landuma (Mel family); genetically and typologically, it is very different from the languages of other team members. However, I get much from collaboration with the people studying Mande languages: in spite of my field experience, I still feel a novice in Guinea and do not properly understand the peculiarities of local mentality, social life, communication patterns, etc. In the Caucasus, I am at home – in Guinea, I am still a very timid visitor.

In my first field trip to Guinea in 2014, I was the only person who worked with a female language consultant. Sale Kumbassa is a woman from the village of Kimiya in Guinea. In 2014, she was about 38 years old and had six children. She was incredibly smart and surprised me with her excellent memory: after a month of work she remembered every word that we had discussed in the first days of our acquaintance and could easily find it in my notebook. However, our collaboration had a serious complication: this was the meta-language. It was assumed that we would work in French, but neither of us knew it well. I am not very fluent in oral speech, but I used to prepare my grammatical questions in advance and could of course consult dictionaries. As to Sale, she was very fluent in using the French words and expressions that she knew – but, unfortunately, she did not know many of them and could not read in French. The first two weeks of our work were torture for both of us: Sale did not understand most of my questions. The only thing that helped us was a working dictionary of Landuma that I already had. But we were eager to find a way out and did our best to improve our communication. To understand each other, we used the dictionary of Susu (the lingua franca of her area), asked other language consultants, drew pictures and tried to speak with gestures. I remember Sale jumping down a ladder in order to illustrate the meaning of the Landuma word 'jump'. These efforts had an unexpected happy end: after approximately two weeks of work, I realized that the work was now moving quite well and that our comprehension problems were no longer so serious. Of course, Sale and I did not improve our French. I did learn some Landuma, but the main improvement was that Sale and I both learned to communicate in a new "language", which consisted of simplified French with some lexical additions of Landuma. When I remember this case, I always say that now I know how pidgins are created. It is a pity that Sale and I were engrossed in studying Landuma, and there were no linguists to document our own discourse.

3. Why field linguistics?

Why do I prefer fieldwork to other ways of getting the data? It is difficult to give a rational answer, because it is rather a matter of feelings and taste than of reasoning. For some, it might relate to the emotions that arise from traveling, visiting new places and meeting many new people. Personally, I do like traveling, but this is not the main reason for choosing fieldwork. Tourism and hiking are better ways to travel, especially in Russia, where you yourself must often pay both for tourism and fieldwork. As to making new acquaintances, this is generally difficult for me, so it is rather a challenge than a pleasure.

The real reason for my preferring fieldwork is that I like getting the raw data – free of other people's analysis and interpretation. I like to know that if I lack some information, I can ask the speaker. Of course, fieldwork implies a lot of routine, but at a certain stage, it also includes a sort of heuristic component that is different from those in other linguistic disciplines.

When you work with a texts corpus, be it a corpus of old texts in a dead language or the National Corpus of Russian, you first get the mass of data and then start analyzing it. In many cases, you cannot choose the data: you just take the data that exist. When you are doing fieldwork, you can obtain the data that you want and need, and you receive them in relatively small portions. Fieldwork is recursive: you get some data, construct models of the relevant phenomena, and then check them: new data show where you are right and wrong, so that you can improve your models and start the next turn of the helix.

The best moment of fieldwork comes when you have already built a provisional model of a linguistic phenomenon, but you lack a couple of sentences, or even one sentence, or just a verb form: a small piece of information you need to be sure that your model is adequate. At that moment, you run to your language consultant holding your breath and timidly ask your questions. If you get the desired answer, you jump for joy. If you don't, you are confused, but not discouraged: this means a new puzzle to solve, and solving linguistic puzzles is always fun.

Abbreviations

1	1st person
2	2nd person
3	3rd person
ABS	absolutive
ASS	associative

BEN	benefactive
CAUS	causative
DIR	directive
DYN	dynamic
IPF	imperfect
NEG	negative
PL	plural
POSS	possessive
PRED	predicative
PST	past
Q	interrogative
REL	relativizer
RSN	reason
SG	singular

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CHAPTER 10

The linguist as a demon and as a human

Fieldwork in Greater Awyu communities of West Papua

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Lourens de Vries with Kombai speakers in Wanggemalo

1. Introduction

Between 1982 and 2016 I spent around seventeen years in Indonesian West Papua, learning and studying Papuan languages. From 1982 to 1992 I lived in the area between the upper parts of the Digul and the Eilanden rivers where Greater Awyu

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languages are spoken. I focused on the northernmost members of the language family, in the foothills and swamps south of the central mountain ranges: Wambon (de Vries 1989; de Vries & de Vries-Wiersma 1992), Kombai (de Vries 1993) and Korowai (van Enk & de Vries 1997).

After the Greater Awyu period, in which description and documentation of individual languages were the primary aims, I crisscrossed West Papua and the emphasis shifted to areal, comparative and typological work (from 1992–2016). The incredible cultural and linguistic diversity of New Guinea hit me in full force for the first time when I went to the south coast of the Bird's Head peninsula to survey the South Bird's Head family and to write a sketch grammar of the Inanwatan language, in 1994 and 1995 (de Vries 2004).

The Greater Awyu fieldwork is the main topic of this paper. The northern parts of the Greater Awyu area were an exceptional area, both from a global perspective and within the context of West Papua of the early 1980s. The nation-state and its institutions (infrastructure, schools, clinics, police stations) had not yet reached northern parts of the area where Greater Awyu languages were spoken. Many speakers of northern Korowai and Kombai had heard about those institutions but not seen them with their own eyes. They lived in tree houses, 12 to 20 meters above the ground, on their clan territories in the jungle, did not know Indonesian and many did not know that they were citizens of Indonesia.

The reason that they lived (and partly still live) in relative isolation from the Indonesian nation-state and its institutions was a combination of factors: vast and dense, swampy rainforests with less than one person per square kilometer, dangerous diseases (cerebral malaria, elephantiasis, tuberculosis, skin diseases, parasitic diseases), absence of economic opportunities like oil, gold, minerals or other things worth mining or harvesting. There were no roads, most upstream parts of the rivers were unfit for boats, and only a few grass airstrips for small missionary planes connected the area to the wider world.

Having been victims of annual Asmat headhunting raids for many generations (with the last raids in the 1960s), they had withdrawn deep into the jungle, often somewhat away from main rivers that carried intruders into their area. These intruders could be Asmat headhunters, Asian collectors of bird-of-paradise feathers (in the 19th century), Dutch exploration vessels in the 1920s and 1930s (like the vessel of Captain Becking, who charted the Becking river) or missionaries (late 1970s). The upper reaches of the rivers in the Greater Awyu area are often too shallow for vessels; even dugout canoes have to be carried across stone and sand banks. When temporarily navigable because of heavy rains, the waterways are often blocked by huge tree trunks and massive amounts of driftwood. The early explorers of this area were few and far between; they stuck to their boats on the lower reaches of the rivers and generally did not venture deep into the jungle far

from their boats. Greater Awyu communities in the upriver parts were difficult to find, and my first survey of the speech communities was by helicopter, counting tiny clusters of two or three tree houses dispersed over a wide area.

The anthropologist Rupert Stasch did 18 months of fieldwork in the Korowai area between 1995 and 2007, resulting in an excellent dissertation published in 2009. His work helped me greatly to understand the way in which Korowai language practices are embedded in and constitutive of their cultural practices. On a more personal level it helped me to understand and articulate my experiences while I lived there, from the celebration of life, growth and fertility in the sago grub feast cycles to the rituals and practices by which Korowai come to terms with death: witchcraft trials, anthropophagy and avoidance of 'after-death demons'. In this chapter I draw heavily on his analysis of Korowai cultural practices, including Korowai linguistic anthropology (Stasch 2001, 2007, 2008a, 2008b, 2009).

I look back at my interaction with Greater Awyu language consultants from two points of view. First, from the perspective of Korowai and Kombai speakers, based on my interpretation of their talk about me and other foreigners as reflected in oral texts, recorded in the early 1980s in the period of first contact (§ 2). In § 3 I look back at my interactions with speakers and consultants from the external perspective of notions such as indirection, vagueness, responsibility, dyadic relations, reciprocity, otherness, opacity of others and predictability.

2. The linguist as an 'after-death demon'

Field linguists have all sorts of ideas about their role and place in the speech communities where they are temporary residents. But communities also need to give a place to linguists, physically and socially. New Guinea communities have used various strategies of placing foreigners, from incorporating a person as an adoptive child of a host family, sometimes in a *rite de passage*, to understanding them as offspring of a mythical ancestor who traveled to a faraway place of wealth and abundance, only to return in the form of the linguist.

Such practices of accommodation and making room acknowledge both the unsettling experience of alterity and the effort to neutralize and incorporate that alterity, to domesticate it and make room for the other. Sustained co-residentiality turns the other, the foreigner, slowly but steadily into something else: he or she increasingly becomes 'our' other, with a more and more predictable familiar otherness. Co-residentiality translates eventually into an increasing range of rights to the 'place,' including territorial rights, because the outsider is perceived more and more as belonging to the place (Stasch 2001: 308). The nouns that mean 'place'

in Greater Awyu languages, such as Korowai *bolüp* and Kombai *mbürü*, are also used for clan territories when they form compounds with clan names. These clan 'places' are the very foundation of the dispersed and very small clan communities: the 'place' feeds you, the 'place' connects you to the ancestors who gave you the 'place' and who watch over you and over the 'place'. The clan place, rather than your language, is central to identity construction (de Vries 2012: 10).

The Kombai noun *xwai* and Korowai noun *laléo* 'after-death demon', the unmarked terms for foreigners, squarely express the unpredictable and intrusive otherness of foreigners. The term completely lacks an aspect of incorporating and giving a place to the foreigner, found elsewhere in New Guinea in the terms used for foreigners, for example as an adopted child of a local family or as a returned ancestor of the clan. By the Korowai and Kombai's calling me *laléo* (Korowai) or *xwai* (Kombai) 'after-death demon,' they placed me in the periphery of their social world, emphasizing my otherness, unpredictability and the potential negative consequences of close interaction with me.

This peripheral position was also expressed spatially in southern parts of the Greater Awyu area, for example, along the middle stretches of the Digul river, where processes of integration into the nation-state had started around twenty years earlier than in the Korowai area. The foreigners lived together in and around places with a mission or government station, usually around the grass airstrips made by the missions, and the clan members lived (and still live) on their 'places', the clan territories, often between one or two days' travel away from the mission or government stations. The missions and local government officials stimulated (and quite often forced) Greater Awyu speakers to live in or around the mission stations. But in practice the houses that Greater Awyu constructed on the mission stations were mostly empty or served as temporary hostels for people who needed something they could only get at the mission stations: medicines, access to shops, sometimes attendance at mission schools or churches.

Of course, the land of the mission stations also belonged to clans, but this problem was solved by establishing such posts on one of the many places where nobody lived anymore, preferably in border areas and transition zones, and/or by paying clans for the right to use their land (but land cannot be bought: clan territories are the inalienable possession of the clan, including dead ancestors and children yet to be born).

What is a *laléo* 'after-death demon', the noun they used to refer to me and other foreigners? When someone has just died, before he finally arrives in the land of the ancestors, there is a period of transition. The dead person is still around as an 'after-death being', a *laléo*, with a corporeal existence in the process of putrefaction, decaying, deforming, disintegrating (Stasch 2009). The problem with *laléo* is that they need time to accept that they are now no longer normal human beings

(*yanop* 'human being'): they still want to be with their relatives and friends, share food with them, and be accepted by them. Accordingly, they become intrusive and dangerous, desperately seeking contact with their beloved ones. These beloved ones, however, do not appreciate such efforts of the recently dead, because contact with them may be very harmful. Moreover, putrefaction has turned them into something unspeakably horrid, zombie-like monsters.

I had the impression that when I traveled deep into Korowai territory in the early 1980s and sometimes met unsuspecting Korowai who were scared to death of seeing me, trembling and shaking, that they called me *laléo* because they thought I was a *laléo* in the literal sense: an after-death zombie or demon. This is confirmed by the observation of Stasch (2007: 105), quoting Korowai speakers who told him: "At first their thoughts were that they were grave-pit demons" and Stasch continues with the observation that "this full identification of newcomers with dead monsters was a matter of wide, commonsense agreement among Korowai at the time". The Korowai accounts of encounters with the first whites entering their clan territories in the early 1980s are replete with the term *laléo*, often in explicit opposition to *yanop* 'person'. The following examples are taken from the *xenil-xenil* 'the beginnings' narrative published in van Enk and de Vries (1997: 186–203).

(1) noxu yanop xedi-fafon=daxu laléo-ma
we person finish-1PL.ADH=SS demon-also
ül-me-fon de-té
kill-SUPP-1PL.ADH say-NON1.PL.REAL
'"Let us kill the person and the demon," they said.'

The Korowai men, having seen the canoe with the Korowai guide and the missionary Veldhuizen, propose in (1) to kill both the Korowai *yanop* and the white *laléo* in the canoe. The epithet 'demon' stuck to me all the years that followed, and it stuck to all things associated with me and other 'demons':

(2) Kombai

doü	'sago'	xwai-doü	'rice' (demon-sago)
riya	'torch'	xwai-riya	'flashlight' (demon-torch)
lu	'voice; language'	xwai-lu	'Indonesian' (demon-language)

Many Korowai and Kombai speakers, especially the younger ones, were very curious about the *laléo* and their habits, food and tools. When I used the two-way radio or a flashlight or a mosquito net, there were always people joining me to watch, explore and feel those objects. The fact that these things were *laléo*-related made them no less desirable. In fact, people were very open-minded and fearless with respect to new technologies and tools. For example, Korowai speakers who

were invited to board our small helicopter used to survey the tree house clusters immediately did so, enjoyed the ride, and recognized and named many streams, rivers and clan territories from above.

The term *laléo* gradually developed a more neutral sense of 'foreigner' in some contexts such as the compounds in (2), but always retaining some ambivalence, with its roots in the fear that 'foreigners' eventually will claim one's 'place', one's clan territory (Stasch 2007: 105). And this claim has merit because 'foreigners' indeed did want land for airstrips, government buildings, churches, schools and clinics. After two decades, the term *yanop* started to be used for foreigners by new generations of Korowai who grew up in a world very different from the world I was a part of in the 1980s and 1990s. For example, foreign tourists started to be called *turis-anop* (Stasch 2007: 105). The tourists were drawn to the area by several high-profile television documentaries following a *National Geographic* article on the Korowai and Kombai.

Interestingly, I repeatedly heard resident 'foreigners' (Javanese and Biak persons working in the area) tell Greater Awyu speakers in Papuan Malay *saya bikin kamu manusia* 'I turned you into (normal) human beings', meaning that they had taught them to behave in such a way that they could function as normal persons as defined by the Indonesian nation-state. The addressees not only accepted this but also expressed gratitude to the people (for example Biak teachers in elementary schools) who had made them 'person' in the wider national community.

The noun *laléo* 'after-death monster/zombie' is metaphorically extended not only to foreigners but also to newborn babies. Only when a newborn baby is accepted by the mother will the child start its transition from non-person (*laléo*) to social personhood (*yanop* 'person'), through a number of rituals (Stasch 2001, 2009). Infanticide was a common and completely acceptable cultural practice while I was doing fieldwork. Infanticide practices gradually stopped in the course of the 1990s.

The common element in all the senses of *laléo* seems to be related to social personhood: first, *laléo* are persons who have lost their personhood because of death, before they regain it as an ancestor, second, *laléo* refers to newborns not (yet) accepted in the social order of (normal) persons, and finally *laléo* refers to intruding and uninvited foreigners with weird skin, hair, habits, language, and behaviors, clearly not normal persons (*yanop)*. This also explains why *laléo* is a favorite insult of many speakers.

Dyadic relations

Greater Awyu speech communities are highly dispersed and fragmented. Fragmented into numerous clans numbering around 15–20 persons, with their own

'place' (clan territory), but also fragmented within clans. The stereotype of small-scale non-industrialized societies as tight-knit, close, collectivistic *Gemeinschaft* does not fit Greater Awyu clan communities where individual autonomy is valued, and with high residential mobility and an emphasis on dyadic relations (Stasch 2001: 7). The combined network of dyadic social relations of an individual determines someone's social universe. Women bring their own language or dialect to the patriclan 'place' of their husbands, and their children grow up with the languages of their father and mother, to which they add a varied linguistic repertoire based on the languages spoken by their key dyadic partners of other clans. This leads to a kind of individual multilingualism with different speakers of the same clan having partial or complete competences in different sets of languages. Languages were not named, clans speaking the same language did not feel that they belonged together, culturally, politically or otherwise (de Vries 2012: 10). Long dialect continuums meant that mutual intelligibility with neighbors shifted from place to place on the continuum. One of the mistakes of my early fieldwork was to take the exonyms such as Kombai and Korowai for granted, assuming they stood for coherent, discrete, bounded ethnolinguistic groupings. Naming practices and other meta-linguistic practices of speech communities should be studied thoroughly, in the initial stages of linguistic fieldwork. Because the exonyms denote arbitrary sections of dialect continuums, and do not reflect a meaningful grouping in Greater Awyu terms, exonyms and the 'languages' they refer to, come and go in the history of Greater Awyu research. Language names used in early studies of southern Greater Awyu languages are not recognized or known by newer generations of speakers.

Korowai speakers may build close dyadic relations with individual *laléo*-foreigners; when they do so, they mix their speech with words and constructions of the *laléo-aup* 'the demon-language' (i.e., Indonesian) to express these bonds. But a close relative of the speaker, living in the same clan place, may not be interested in dyadic relations with 'demons' and accordingly may use no or only a few Indonesian loans.

Fieldwork and witchcraft

Laléo is the not the only noun that speakers often contrast with *yanop* 'human being, (normal) person'. The other noun in the network of nouns related to social personhood and belonging is *xaxua* 'male cannibalistic witch' (Stasch 2001: 444). Both *laléo* and *xaxua* are outside the boundaries of what counts as *yanop*, 'person', but *laléo* either were *yanop* before their death (demons or zombies), may become *yanop* after their birth (babies) or may acquire *yanop*-like status in the periphery of normal personhood ('foreigners'). Crucially, the outward appearance and

behavior of a *laléo* leave no doubt who is a *laléo* (whether a newborn baby covered with blood and birth fluids, a putrefying after-death demon or a pale foreigner with strange hair). All of these are considered weird-looking, ugly and repulsive. The skin plays a key role in this and people often stroked my skin to feel its texture and weirdness, to tell me that my skin indeed looked like the skin of a *laléo*, because of its paleness. The same is said about the skin of newborn babies, weirdly pale, sign of their *laléo* status (Stasch 2001: 169).

But *xaxua* 'male cannibalistic witches' have normal skin and look like perfectly normal *yanop* 'persons', they live in close association with you, for example as a brother or father or son-in-law. But on the inside they are the very opposite of a normal social person, completely anti-social and with just one horrible urge that they cannot control: to eat the vital organs of the people they live with or are closely associated with. For Korowai and Kombai witchcraft is an instantiation of their idea of the opacity of the mind of the other, including the other who is close to you (Stasch 2008b). The victim does not know that the heart, liver and guts have been eaten out of his or her body. A few days later they get sick and die. The *xaxua* performs his cannibalistic acts by means not available to normal *yanop*: he can fly, transform himself into animals, and open up somebody's chest to consume the heart, filling up the empty spots with grass and leaves, then close the wound without any trace and leave the victim undetected and unseen.

The *xaxua* is the epitome of otherness: you trust your uncle, nephew or friend but he may be planning his witch attack on you while you enjoy his food, fellowship and company. When somebody dies, there is always the suspicion that some *xaxua* is responsible, unless the one who died is very young or very old. Various means are applied to find suspected witches; they are caught and tied up very tightly and painfully with rattan strings, then interrogated with physical and psychological pressure until they confess. An avowed *xaxua* is then taken away from his clan territory to another clan territory where he is killed by shooting an arrow into his heart, butchered, cooked, and eaten. The procedure of killing, dismemberment and cutting the meat follows that of slaughtering a pig. *Xaxua* arrests often took place during my fieldwork. Occasionally I tried to intervene, especially when the 'witch' had been in a dyadic relation with me, for example as a jungle guide, language consultant or domestic helper: then it was culturally accepted to defend the 'witch' by denying that he was a witch and rejecting the 'proofs' of his *xaxua* activities. Sometimes close relatives of the 'witch' try to defend the accused and this may lead to fights where arrows fly through the air. Occasionally, an accused 'witch' is set free when he sticks to his denial and the proofs seem weak. But it takes only another death in his vicinity for him to be re-arrested. In the end, when a consensus is reached that someone is a *xaxua*, he will always be killed and eaten, sometimes after years. Before his final execution,

the suspected *xaxua* is socially dead and feared by everyone. Both infanticide and cannibalism of executed witches decreased gradually (Stasch 200: 168) until they probably ceased to exist, sometime around 2000. Occasional later cases may have occurred, however.

The whole *xaxua* complex created a range of difficulties for my fieldwork. The oral texts on which my grammatical description was based were saturated with references to the stages of the *xaxua* complex: binding them with rattan, leading them away after confession, killing, cooking and eating them. To give an example (taken from a narrative published in van Enk & de Vries 1997):

(3) Mafém y-até xomilo=do a-è mo wof-e=xa
Mafém her-father die.NON1.SG.REAL=DS EXCL-EXCL just that-TR-CONN
Goloxofalé=lo nolél-è de-té
Goloxofalé=FOC eat.NON1.REAL-EXCLM say-NON1.PL.REAL
'Mafém's father died and they said, "It is Golokhofalé, who has eaten him."'

(4) Goloxofalé=lo nolél-è de-té=dakhu
Goloxofalé=FOC eat.NON1.SG.REAL-EXCLM say-NON1.PL.REAL=SS
Goloxofalé lamélo Didonalé lamélo Didonalé
Goloxofalé tie.NON1.SG.REAL Didonalé tie.NON1.SG.REAL Didonalé
y-afé lamélo
his-older.brother tie.NON1.SG.REAL
'Saying, "It is Golokhofalé who has eaten him," they tied Goloxofalé, and they tied Didonalé, they tied his older brother Didonalé.'

(5) Goloxofalé lambil=efè Didonalé y-afé
Goloxofalé family=TOP Didonalé his-older.brother
fe-nè fe-té
take-SS take-NON1.PL.REAL
'Goloxofalé, his kinsman Didonalé his older brother, they took.'

(6) Didonalé y-afé lambil=efè Goloxofalé fe-nè
Didonalé his-older.brother family=TOP Goloxofalé take-SS
fe-té Lemaxa-bolüp fe-nè fe-té=do ül-nè
take-NON1.PL.REAL Lemaxa-place take-SS take-NON1.PL.REAL=DS kill-SS
alü-no-nté=do xenè
cook-eat-NON1.PL.REAL=DS next
'Goloxofalé, and his kinsman Didonalé his older brother, they took, to the place of the Lemaxa clan they (=accusers) took him and they (=Lemaxa people, the executioners) killed, cooked and ate him and next….'

The story is not a witchcraft story and the narrator was not asked to tell about witchcraft and cannibalism. The real-life narrative is about a medical evacuation

of a man, Didonalé, who had multiple arrow tips in his back, about his operation in the mission station of Boma where a nurse cut the arrow tips out of his body while he was under narcosis. The narrator had gone with him, and was fascinated by what he saw: how the nurse 'killed' Didonalé, removed the arrows, closed the wound and 'resurrected' him. The witchcraft section is just a background section to explain how Didonalé had received his wounds: he had escaped after being arrested as a 'witch'. All the stages of the witchcraft trial (except the interrogation) are present in this story: the accusation (3), binding of the accused (4), the accusers taking the witch to an allied clan (4), followed by the execution, cooking and eating of the witch (5).

When I tried to intervene in witchcraft trials after a consensus had been reached that the accused was in fact a witch, pleading with the accusers that he was a human being, a *yanop,* and one of their own, the standard answer was *yanop da* 'he is not a human being'. Such interventions confirmed my *laléo* status: I did not understand as a *laléo* foreigner that the accusers tried to keep the social order intact and tried to prevent the witch from killing and eating other people (which he could not stop doing) and that I sided with the ultimate anti-social forces of witchcraft with my ignorant intrusive behavior.

Cannibalism and unwelcome publicity

A description of a language has three parts, a grammar, a lexicon and a corpus of recorded, transcribed and annotated texts. The text corpus is an essential component because it makes the description falsifiable, because it illustrates the described patterns and because it contains oral traditions that reflect the worlds of Greater Awyu clan communities. But publication of the texts would also reveal socially-sanctioned practices of cannibalism. That type of cannibalism was a highly sensitive topic in the academy of the 1980s. It was widely believed by anthropologists that socially-sanctioned cannibalism did not exist and was in almost all cases a product of colonial fantasies or sensationalist journalism (e.g. Arens 1979). Making cannibalism public was also not received well by groups supporting the Papuan independence movement OPM because such reports would be politically exploited to support the idea that Papuans were too primitive to govern themselves. Some activists defending the rights of indigenous peoples in Europe resisted publicity that would strengthen negative stereotypes and sensationalist representation of Papuan peoples. The *xaxua* complex was therefore academically, emotionally and ethically difficult terrain for me to navigate. Was I disloyal to the people who shared their 'places' with me, who helped me to learn their wonderful languages, when I referred to their witchcraft-related practices in scholarly publications in the context of annotating text editions? Would the media somehow

pick it up and portray Korowai and Kombai people, or more generally Papuans, as primitive and cruel barbarians?

One of the main reasons why Korowai in contact with foreigners dropped cannibalism and infanticide at an early stage and seemingly without force of foreign agents is that they quickly internalized the Indonesian social personhood perspective of becoming a *manusia* 'human being' in which 'the cannibal' was the very symbol and icon of the non-*manusia*, 'the primitive barbarian'. Younger people started to go to elementary schools with an Indonesian curriculum and became increasingly aware of the disgust and rejection that practices of cannibalism and infanticide evoked in the wider world around them.

3. Dyads with a Dutchman

This section looks at my relationships and interaction with language consultants from the perspective of notions such as indirection, vagueness, responsibility, dyadic relations, reciprocity, otherness, opacity of others and predictability. It may seem unpleasant and difficult for a field linguist to be called *laléo* 'after-death zombie' and to be perceived as a potential threat to both clan and individual autonomy, to be 'contained' physically and socially in a peripheral 'place', but things were not nearly as bad as they might appear. The dispersed, politically fragmented Greater Awyu way of life, emphasizing both the autonomy of the clan and the autonomy of the individual, offered ample room for a field linguist to relate to speakers, build up dyadic relationships, and become part of the lives of some people.

Two things were important in finding a place among Greater Awyu speakers. First, they do not form a tight-knit integrated *Gemeinschaft* where individuals would need some sort of collective permission to interact with foreigners (Stasch 2001: 7). As noted above, the autonomous clans are very small (around 15–20 people on average) and within these small groups, individuals are allowed to build up their own networks, including networks through marriage. And the building block of the network is the dyad. Two persons may form a dyadic relationship, in which the whole is more than, and inherently different from, the two partners that form it. Greater Awyu languages have many exocentric compounds to refer to the dyads, e.g. Kombai *momo-langge* 'mother's brother-sister's son', the avuncular dyad. The dyad is also a story, a history of two people (Stasch 2001: 90–91). There is a range of types of dyads based on types of historical foundation. For example, kinship-based dyads such as mother's brother-sister's son deepen and change when the dyadic partners share key experiences. Two unrelated people of different clans may form a dyadic relationship on the basis of an unexpected, strange or funny event that brought them together. The historical

foundation of this particular type of dyad could be, for example, sneezing very loudly at exactly the same moment or intervening when a mother rejects her newborn baby and is at the point of infanticide (Stasch 2001). This may establish a dyad between the intervening partner and the partner for whom the intervention took place. This type of 'joking avoidance' dyad (Stasch 2001: 90–93) is recognizable for others because the two partners must avoid each other's names and address each other only with the noun denoting the historical foundational event, always a self-reciprocal term of address. Since the vast majority of the foundational events that brought two people together are perceived to be funny (e.g. two people stepping into the same cassowary turd), the term joking avoidance refers to this humorous aspect of forms of address such as 'my sandcrab', 'my cassowary turd', and so on.

The second important aspect to finding a place among Greater Awyu speakers was that otherness for them is not a quality associated exclusively with foreigners. These communities are 'societies of others', in the words of Stasch (2009). In other words, alterity is part of the human condition from the perspective of these communities: unpredictable and dangerous otherness and the opacity of the mind of others is not restricted to foreigners but part and parcel of all relationships, including people closest to you. In fact, since foreigners are never accused of being *xaxua* witches, but can be victims of witch attacks, the foreigner may be weird, repulsive, intrusive or a downright nuisance, but their *laléo* otherness lacks the lethalness and evil of *xaxua* witches.

Greater Awyu speakers did not need permission from anyone to enter into a linguist-consultant dyad with me, to teach me their language, to tell me stories of the oral tradition and so on. But all adverse consequences of the dyadic relationship with me would be the responsibility of the speakers deciding to work with the linguist. To form a dyadic relationship always involves both risks and rewards from a Greater Awyu perspective, whatever the basis of the dyad and whoever the partners in the dyad are. All actions of an individual may have (unintended or intended) undesirable consequences for others. Actions create responsibility and the risk of compensation claims. These claims were daily routine when I lived in the Greater Awyu area. People were constantly and loudly claiming compensation from each other for all sorts of damages. Such cultural practices encourage people to be cautious, slow to act, slow to speak, and to be careful with relationships. When warning, accusing or helping people, speakers routinely package their intentions in extremely vague talk, full of hidden hints and indirection. If something harmful came out of relationships with a *laléo*, the full blame would rest on the shoulders of the Greater Awyu person in a dyadic relationship with that *laléo*. For example, when my consultant Sapuru took me to his mother's brothers' clan place, he took a risk. There were people there who had not seen a white *laléo* before. If (the shock

of) my visit had resulted in damage, anger or accidents, he would have been the one to pay for it, in shell money, pigs or pork.

Payment of language consultants took place in this framework of mutual dyadic responsibility between me as a field linguist and Greater Awyu language consultants. The modest payment in money to compensate consultants for taking time away from hunting, catching fish or cutting down sago trees was perceived as part of a dyadic relationship that entailed much more. For example, when the language consultant or his children had serious health issues, I was expected to help them with the means available to me, means that I would also rely on when my own health was at stake: using the two-way radio to talk to the mission's jungle doctor, drawing on my supply of medicines, or in some cases chartering a plane to get him out to the hospital in the city. The dyadic relationship between a *laléo* linguist and a *yanop* consultant therefore entails risks and rewards for both, and the longer the linguist lives in the area, the more history attaches itself to the dyad. The way dyadic partners respond to compensation claims becomes part of that history.

One of my best 'dyadic' friends and an excellent language consultant in the Kombai area fell very ill. When I arranged a flight and an operation for him in the Wamena hospital, far away in the Baliem valley of the central mountain ranges, he died on the operation table. His relatives blamed me for these tragic and unintended consequences of my actions to help their relative and demanded compensation payments. They demanded a lot of money because not only did he die but his body was cremated in Wamena, and they never saw him back, a horrible tragedy. Cremation is normal among the Lani people of Wamena, but unknown and unacceptable in the Greater Awyu area.

Fieldwork extending over many years

Although it is highly recommended to study the different aspects of a language at the same time and in relation to each other, rather than to compartmentalize a language into phonetics, phonology, morphology, lexicon, syntax and discourse, in practice most field linguists studying undocumented languages from scratch will start with a focus on phonetics and phonology, then pay more attention to morphology. Syntax and discourse usually are the last topics to receive full attention. This does not mean that field linguists ignore interfaces and interdependencies between subsystems of a language, but they tend to look at them from a bottom-up perspective, from the smallest to the largest units of language. In fact, many field linguists have just enough time in the field to publish a phonology and a morphology.

One of the advantages of fieldwork over many years is that the field linguist has the opportunity to travel the opposite journey and look systematically from

a top-down perspective and from a larger and better understood corpus of transcribed texts. For example, I could only fully understand the syntax of experiential clauses, double-headed relative clauses or the conflation of adverbial and relative clauses in Greater Awyu (de Vries 2005, 2006) when I started to look at my syntactic data from the perspective of specific patterns of language use that Greater Awyu speakers employ with very high frequencies in my recorded and transcribed texts (e.g. thematization, argument distribution, recapitulative patterns, quotative framing).

The morphology of topicality became fully clear to me when I looked at it from the perspective of the syntax of stative clauses with resumptive demonstratives (of the type: that man, that is my-father, de Vries 1995). It is often in the interface zones of languages that very interesting and complex phenomena can be observed. For example, Greater Awyu languages have relatively simple segmental sound systems, but when morphemes are connected into words, complex rules determine how the sounds that become adjacent through morpheme-sequencing are adjusted. Connective discourse devices in the initial part of sentences form the interface between syntax and coherent discourse, and again, complexities arise in that zone, often followed by diachronic changes to smoothen the interface between these aspects of language (e.g. bridging clauses petrifying into conjunctions).

Numerous aspects of Greater Awyu languages became only (fully) clear to me when long-term fieldwork allowed me to look at linguistic phenomena from the perspective of cultural practices of Greater Awyu communities, discovering the intricacies of the interaction and co-evolution of linguistic and cultural practices. Field linguistics, if not linguistics in general, is anthropological linguistics. This is also true at very practical levels. For example, in early fieldwork I had serious trouble in establishing the referents of A, O and S arguments from clause to clause in Greater Awyu clause chains. Speakers strongly prefer simple clauses, avoiding nominal expression of arguments as much as possible. When I asked consultants to tell me who did what in certain clauses, the answer was so obvious to them that they often did not understand my question. Only after becoming familiar with cultural practices, I realized that I missed knowledge of key parts of the linguistic valency of many verbs, namely qualitative valency arising from cultural practices. For example, gendered selection restrictions of verbs that demand either a male or a female subject. When a pig is butchered, it is the women who take the intestines and other innards to a stream to clean them. When sago is harvested, the men cut down the palm and the women pound the fibers and use water from a nearby stream to separate the flour from the fibers. The verbs used to describe the intricate processes of sago production have gendered selection restrictions and also assume the locations where certain actions are carried out. Selection restrictions on verbs therefore are an important interface between cultural practices and

linguistic practices. They are an integral part of the clues that speakers give to enable listeners to track participants. Another example is the numeral systems of Greater Awyu languages, which I could only understand once I had witnessed the cultural practices of counting (de Vries 2014).

Perhaps the most important advantage of life-long fieldwork is the possibility to return with either modified or totally new research questions. Our research agenda and research methods are inevitably influenced by the linguistic theories and training we received at home. Linguistic models come and go, and what counts as proper methodology or as a 'hot topic' also changes. Technological tools change quickly too. My first language description was produced on a typewriter, and all data were on paper. Glossing was an extremely time-consuming activity until computers and Shoebox (later Toolbox and Flex) arrived.

I was trained initially in what then was called Transformational Generative Grammar (N. Chomsky). When I became familiar with Functional Grammar (S. C. Dik), and still later with Basic Linguistic Theory (R. M. W. Dixon) my methods and research questions changed, allowing me to look at Greater Awyu languages from different perspectives. My early fieldwork was still too much based on decontextualized elicited examples that answered the questions relevant to the linguistics of the 1980s or to what at that time was considered essential for a descriptive reference grammar. For example, I was impressed by the elaborate formulas for noun phrases in other reference grammars that informed the reader about the order and types of modifiers in noun phrases (demonstratives, adjectives, genitival noun, numerals, and so on). But my natural speech data never contained phrases of the type 'these three very fat pigs with long tails'. Fortunately, my long-term fieldwork gave me the opportunity to redeem the sins of my fieldwork youth by systematically looking at syntax of real phrases and clauses that occurred in spontaneous speech of different genres, and, crucially, from the perspective of patterns of language use. The same is true for clausal syntax. Only when looking at clausal syntax from the perspective of preferred patterns of speech in discourse, such as argument distribution (Heeschen 1998), did I begin to understand clausal syntax of Greater Awyu languages more fully (de Vries 2006).

Long-term fieldwork also helped me to really appreciate the earlier work by Father Drabbe on the Greater Awyu languages spoken in downriver areas, published in the 1940s and 1950s (and not well-received by academic linguists in the 1960s). Drabbe was not an academic linguist but a Catholic priest who used grammatical terms from the Latin-based Dutch grammar school tradition that I knew from my own grammar school days. It became clear to me that for Drabbe the Latin model was just a metalanguage to describe what he observed. Whenever his linguistic metalanguage was unfit to describe the nature of Greater Awyu languages, he did not force the data to fit his metalanguage but described precisely

how and why his terminology and model was inadequate. For example, he notes that the *aanwijzend voornaamwoord* (demonstrative pronoun) *mene* 'this' in Yonggom Wambon is also used in ways that are hard to reconcile with the notion of demonstrative pronoun in Latin or Dutch, and then he gives examples where *mene* is used as a topic marker (Drabbe 1959: 20).

4. Final remarks

In other parts of New Guinea (white) foreigners may have been assimilated to positively valued categories of persons, e.g. dead kin, ancestors or divinities (Stasch 2007: 105) but Stasch correctly observed that the 'demon' metaphor of Greater Awyu speech communities is quite negative. The contexts in which the 'demon' terms are used – insults, infanticide, and putrid corpses – are mostly contexts of negative abnormality and alterity, beyond the boundaries of normal personhood and appropriate behavior.

But the emphasis on individual autonomy to engage in dyadic relationships, on the opacity of mind of both strangers and kin, combined with the fact that the ultimate threat to life does not come from the outside but from within, created room for me as a linguist to engage in meaningful long-term dyadic relationships with speakers and consultants.

The rewards of long-term fieldwork are many. It enables the linguist to study linguistic practices and structures both bottom-up and top-down, from the features of sound segments all the way up to the linguistic pragmatics of a community as shaped by their cultural practices.

Abbreviation

ADH	adhortative
CONN	connective
DS	different subject in next clause (switch-reference)
FOC	focus
EXCL	exclamative
EXCLM	exclamative masculine
NON1	second and third person
PL	plural
REAL	realis
SS	same subject in next clause (switch-reference)
SUPP	supportive verb
TOP	topic
TR	transitional sound

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CHAPTER 11

From here to there and back again

Fieldwork in the Andean foothills

Mary Ruth Wise
SIL International

Mary Ruth Wise working with Yánesha' speaker Kosepa, 1954

1. An overview

My linguistic fieldwork began in Peru among the Yánesha' in the foothills of the Andes (May 1953-May 1964) and continued during my dissertation research among the Nomatsigenga (September 1966–May 1967). Linguistic fieldwork has also taken me to eleven countries as a consultant and editor for other fieldworkers.

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My co-worker, Martha Duff, and I published a Yánesha' (called Amuesha at that time) vocabulary in 1958 (Wise & Duff 1958). That same year I had a phonology paper published (Wise 1958). Then in 1963 my analysis of Yánesha' verb morphology was published (Wise 1963).

My 1968 doctoral dissertation on identification of participants in Nomatsigenga discourse was completed while many scholars still considered studies "beyond the sentence" to be "literary criticism" (Wise 1968, 1971). Later that year, I was asked to assume the roles of Linguistic Consultant and Editor of SIL[1] Peru's publications. In 1972, Coordinator of all of SIL Peru's academic work was added to my responsibilities.

Consulting with co-workers who were studying Quechua languages helped me see many loan words from Quechua and start thinking about Yánesha' history. In 1976 my first paper on this aspect of the language and culture was published (Wise 1976). In 2011 I published my hypothesis that the Yánesha' migration route from around Manaus was up the Marañon and Huallaga Rivers to their present location (Wise 2011); this explains many puzzling traces of language contacts in the phonology and lexicon of the language.

Comparing word order and various discourse structures in languages from different families led to typological studies (first presented in 1979 and published in Wise 1980), and that quite naturally led to comparative Arawakan grammatical studies and organizing the first symposia (1988 and 1991) on Arawakan languages. At the 1988 symposium I first presented the hypothesis that there was a proto-Arawakan third-person prefix *pa- that is the origin of Yánesha' *p(a)-* 'third-person possessor' (Wise 1991, see also Wise 2005).

In 1999, I assumed the role of Senior Editor for SIL International at their headquarters in Dallas, Texas. I stepped down in 2008 to take on the role of Editor, and then officially retired from SIL in July 2013. Since then I have served as a consultant for fieldworkers writing grammars and dictionaries of languages of Peru.

In January 2010, I received an SOS e-mail from Yánesha' bilingual teachers to help them revise their alphabet. All of their books were out-of-print and the alphabet required special fonts. In May 2011 they voted for the changes that make their alphabet easy to write on any Spanish keyboard (Wise 2014). Since that time I have been working with them on the preparation of new materials.

1. SIL International was first incorporated as the Summer Institute of Linguistics of the University of Oklahoma.

2. How did I get into this?

Maybe it all started back in the sixth grade when my friend Lois Jean and I were the only ones who really caught on to diagramming sentences – in fact, we thought it was fun. And then there was Latin in high school – so much more interesting than Biology.

Or maybe it started my first day at Columbia Bible College (now Columbia International University). I was already committed to being a missionary but had very hazy notions about how and where. That first day my roommate, who with her fiancé had just studied at the Summer Institute of Linguistics of the University of Oklahoma, told me about linguistics and Bible translation. That's it! And everyone who knew me agreed. Not many days after college graduation in May of 1951, a carload of classmates stopped overnight at my parents' home in DeWitt, Arkansas, and the next day we were on our way to Norman, Oklahoma to study linguistics.

3. Yánesha' fieldwork (1953–1964)

Martha Duff and I landed in a small float plane at the village of Shethomaso on the Palcazu River in Peru on May 6, 1953 and learned our first word, *tse'm* 'buzzard' (one was flying overhead) as the friendly Yánesha' women walked with us up the cliff to the village. After we settled into the thatched roof house that was empty at the time, we arranged for me to go to Kosepa's house to learn the language while Martha would study with Shañe at our house. For three years we continued studying and analyzing the language, and beginning translation of the New Testament at Shethomaso. Then we moved to Yurinaki, a village further up in the foothills to continue our work. After I left the work in 1964 for doctoral studies, Martha continued the New Testament translation which was completed in 1976 and published in 1978. Let me tell you about some of the lessons learned during those years with the Yánesha'.

Drowning and going downriver

Ten-year old "Rag Doll" was hanging over my shoulder as I studied. (We called her that because she wore a cloth over her head after it was shaved to get rid of the lice.) So I thought I would practice conversation with her about her aged father whom everyone called *tho'*[2] 'grandfather':

2. Unless indicated otherwise, examples are given in the practical orthography currently in use.

– *Omarra tho*', I said.
– He did? wild-eyed, terrified Rag Doll responded.

You see, I thought I had said "Grandpa went downriver". Literally *omarra* means 'to go downriver', but I should have added the suffix *-amps* that means (among other things) the subject is in some vehicle. The correct word is then *omarrampsa*. I had just told Rag Doll that her father had drowned although I intended to comment that he went downriver by canoe.

A year or so later I learned the difference by personal experience. Martha and I and several bilingual teacher candidates were on a balsa raft (*yomarrampena*) to make the first leg of the journey from Omaiz to the SIL center at Yarinacocha. It was a beautiful afternoon when we started down the crystal-clear river, but suddenly at a bend in the river we had to pass a very big rock in the middle. I was so sure nothing could happen that it didn't occur to me to jump clear (as Martha did). A couple of the men had to pull me out from under the overturned raft or they would have had to report that I had *omarra*.

You and I

We had a head start on analyzing the basic phonology from our predecessor Peter Fast who studied Yánesha' from 1947–1953, however there were still significant problems. By the end of two years we had pretty well figured out that the open transition between many consonants should not be written, aspiration after a vowel was not an allophone of the velar fricative, labialization of a consonant was not w, and that there are basically three vowels although we write four in the practical orthography. But what of the four syllable nucleus patterns? We heard contrasts between short vowels, vowel plus aspiration, long vowel, and vowel plus glottal stop. We intuitively pronounced most of them correctly, but couldn't figure out the rules that governed their occurrence. So we painstakingly wrote thousands of words with different numbers of syllables and syllable types (open or closed, or closed by two consonants, etc.) on 3″ x 5″ slips of paper and sat by the hour with Kosepa while she carefully repeated them for us. I listened especially for glottal stop, aspiration and length, while Martha listened for accent and intonation.

Aspiration is easier to hear in the second person prefix (usually *pe-* or *peh-*) than the first person prefix (usually *ne-* or *neh-*); so we asked Kosepa to change the words from first to second person. Poor Kosepa: we didn't know enough of the language to really explain what we were looking for; so she must have thought "I haven't taught them a thing in all this time". After a while she went to great pains to explain the words, pointing to herself and then to one of us hoping we would understand that *ne-* means 'I' and *pe-* means 'you'.

And why had we divided the task as we did? Well you see, I could imitate tone and intonation fairly well, but I hadn't a clue whether the tone was up or

down, much less how far. That part was easy for Martha to transcribe, and I found aspiration and glottal stops easy to hear – a good reminder that it takes a team for long-term fieldwork.

Should it be *-a*, *-e*, or *-o*?

Every afternoon Monday through Friday, I went to Kosepa's home to learn more of the language. Learning how Yánesha' women keep house, discipline their children and care for their babies was a side benefit. One afternoon I also learned how they teach their little ones to speak correctly. One of the rules for combining roots, prefixes and suffixes in words is that the vowel with which many noun suffixes begin depends on the preceding consonant. One day Sholle, who was around four years old at the time, told his mother he had a splinter in his hand; he called it *naj*ar, but she corrected him: "We say *naj*or (my splinter)".

Eventually I figured out that if he wanted to say "my fish", the form would be *nekak*ar and "my peanut" would be *nechech*er. I haven't seen such a rule in any other language, and there are exceptions; but the general rule for noun suffixes in which the initial vowel varies is:

- e following palatal and palatalized consonants and r: ch, th, sh, ph, kh, bh, mh, ñ, ll, r
- a following stops and nasals (not palatalized): p, t, k, m, n
- o following affricates and fricatives (not palatalized), retroflexed consonants and liquids: xh, rr, g, w, y, j

"If we forget to say one, we say the other"

Kosepa had an excellent memory. I know that because I observed her weaving wrist bands with intricate designs on a tiny loom. She was illiterate, but she knew exactly how many warp threads to lift and how many to depress with the very thin sticks she carefully placed before each pass of the shuttle (stick on which the thread is wound) through the warp threads. The resulting tiny designs in her quarter-inch-wide wrist bands were exquisite.

She evidently had no problem with memory, but analyzing subtle differences in meaning was not her forte. I learned that when I was trying to get an explanation from her of the difference between two forms of the verb 'to fly': *ahnom*ame't*ena* and *ahnom*amp*ena*. Her explanation was: "If we forget to say one, we say the other".

Shañe, who worked with Martha, was gifted differently. She explained: "*Ahnomame'tena* is like a bird that flies and alights, flies and alights. *Ahnomampena* is like people flying in a plane". From her example, we could see that the suffix

-am (really *-amh* but we don't write the palatalization before *e*) means 'intermittently', and the suffix *-amp* (really *-amph*) means 'in a container or vehicle'.

Were you ever afraid?

Of the Yánesha'? Never: they were our friends and guardians from the very first day. But one evening after we had been in the village about a month, a group of surveyors (looking for oil, I think) came into the village. We didn't have a door, much less a lock for it, but we remembered Psalm 4:8: "I will lie down in peace and sleep, for you alone, O Lord, will keep me safe". And we did just that even though we heard the people chasing the men out of their houses during the night.

Then there was the time we were sleeping in the back of the schoolhouse at Sherenkmaso while visiting the village, observing the bilingual school and making suggestions to the teacher. About 2:00 a.m. we were awakened by a gunshot very near. The next morning the chief explained that there had been an owl (an evil omen to them) perched on the flagpole and he didn't want us to be frightened by it.

Secrets of language learning

In our third year of learning and analyzing Yánesha' at Sherenkmaso, Martha and I learned of villages further up in the Andean foothills where the people wanted a bilingual school and would welcome us to live among them. So in May of the fourth year, after nearly a week's travel by plane, broken-down truck, log truck and finally nine hours on a mule trail with mud up to our knees, we arrived at Yurinaki and were welcomed with a sample of the "*café más fino del mundo*" and manioc, the staple food of the Yánesha'.

The people were mystified that we were speaking their language – after all no other outsiders had ever learned it; so they went to Oxapampa to get an explanation from the diviner who explained that we were the nieces of the sun-god; of course, we could speak it. Later back at Sherenkmaso, my patient teacher Kosepa laughed and laughed as she told me about it.

But now we are in the electronic age. Recently at a workshop sponsored by the Ministry of Education for the preparation of new materials for the schools by some of the bilingual teachers, I happened to be leading a session when a lady from the outside entered and asked teacher Adriano:

> "What language is that?"
> "That's Yánesha'; that's our language."
> "How on earth did she learn it?"
> "Oh, she has a chip that she puts in when she comes here. Then when she goes back to her country, she puts her English chip back in."

When cousins turn their backs to each other

One of Pedro's sisters had come from Cacazu across the mountain to visit him. (Pedro was the bilingual school teacher at Yurinaki). While she was at our house, one of her male cousins also dropped by. I was puzzled to see her standing by a corner post with her back turned away from the cousin standing by another corner post with his back turned while they carried on their ceremonial salutations. Later I realized he was her mother's brother's son – a potential spouse, so they needed to avoid direct conversation.

Then there was the chief's wife who treated us in a joking relationship as though we were her sisters-in-law. That implied we were her husband's sisters and thus not potential rivals for his affection.

Another time I heard a young woman call her mother-in-law *ach* 'mother' instead of *so'moe* 'mother-in-law'. When I questioned her about it, she explained that, as they reckon kinship, she and her husband were "brother and sister" (although several generations removed), but since she had been orphaned when young, they didn't know it until they were already married. Thus I learned that congenital relations take precedence over those of affinity.

One afternoon I was trying to make conversation with *posonko'mer* 'the smallest child (of the schoolchildren)' and asked him about his *pemo'nerr*. He laughed and laughed because I had used the word for a woman's sister instead of *phoxh* 'man's sister'.

Eventually I learned how their system of kinship terms works. But now I am sad to see that they realize that their Yánesha' system is quite different from the Spanish system and think they have to accommodate to Spanish. They now use some terms such as *tío* for 'uncle' since they know that the term *apa* refers to both their father and father's brother, while the term *go'* refers both to their mother's brother and to their father-in-law.

How do you write *-rra*?

In 1953 a bilingual school program was initiated by the Peruvian Ministry of Education with the cooperation of ILV (SIL in Peru). None of the teachers had completed primary school at that time, but after three months' study at the training course established by the Ministry of Education eleven young men, including one Yánesha', were approved to start bilingual schools. By 1956 forty-five teachers were approved; five of them were Yánesha'. Pedro, one of the five, left in early April with his wife and little daughter to make the long trip to Yurinaki and start a school with children and young teenagers who had never seen writing. The older boys were so eager. One afternoon in early July they came to our house asking how to write a syllable that sounds like *rra*. "We know how to write *ko*; if we knew *rra*, we

could write *korra*". It hadn't taken them long to learn some of the basic principles of reading.

A few weeks after that while we were visiting a downriver school, teacher Valerio said to Martha: "The students have read all of their books, what shall I have them do now?" Martha suggested that he have the students write stories in their notebooks and then read each other's stories. At the end of the school year, she collected the notebooks and used the stories as the basis of more advanced reading materials. When the students saw their names in print as authors, there was no stopping them; they wrote and wrote. Writing became a new means of expressing themselves "ever since (as one boy wrote) we learned that ink is to write with".

The end result is that today at least 90% of the Yánesha' adults are literate – a very high percentage for an indigenous group of the Peruvian Amazon.

4. Nomatsigenga fieldwork

It turned out that, for various reasons, I should tackle a second language for my dissertation: Nomatsigenga, another Arawakan language. Harold and Betty Shaver, SIL colleagues who had been studying the language for about five years, very generously hosted me in their village home and helped me find good language teachers and storytellers.

Aha!

As I started studying the language it was easy to recognize some of the roots and affixes that had cognates in Yánesha', but some weren't so easy to recognize until I looked closely at the word for 'wrist band (possessor unspecified)': *ormets* in Yánesha' and *maretsi* in Nomatsigenga and the other Campan languages. The final vowel is dropped in Yánesha', then *a* becomes *o* following the labial *m*, and finally the order of sounds in the root *mar-* turns around becoming *orm-*. Aha! I began to understand one of the reasons why Yánesha' can be Arawakan but so different from its neighbors.

Literary criticism or discourse grammar?

Nomatsigenga storytellers use few nouns to refer to participants in a story; so my dissertation research centered on discovering how participants are identified in folktales, personal narratives, and other types of discourse. That necessitated recording and studying hundreds of pages of transcribed narratives. Samuel (a pseudonym) was a great storyteller, but after he told several stories he realized that the tapes were pretty permanent and refused to tell more because "I don't

want to be talking when I am dead". The Shavers and I respected his wishes, transcribed the stories, and then erased the tapes – one of the unanticipated lessons on working with native speakers of a language and respecting their wishes and rights.

Other Nomatsigenga storytellers didn't have the same qualms about having their voices recorded; so I eventually had plenty of material for my 1968 dissertation which I titled "Identification of participants in discourse: A study of aspects of form and meaning in Nomatsiguenga".

When I presented a synthesis at an SIL forum, a graduate student from another university remarked that it was an interesting bit of literary criticism. Discourse studies were in their infancy at that time since most linguists considered the task of linguistics to be to specify the well-formed sentences of a language. Today, the study of texts as a whole, that is discourse grammar, is a generally accepted task of linguistics.

5. Comparing notes

Millie Larson and Jeanne Grover, who were studying Aguaruna (now officially called Awajún), lived next door to us when we were at our center in Yarinacocha, near Pucallpa, Peru. We each had study halls for the bilingual teachers to study at night during the teacher training courses. Right away we noticed one of the differences between the two groups: There could be a dozen soft-spoken Yánesha' in the next room and one would scarcely hear them, whereas one couldn't miss two Aguarunas several hundred feet away. As they explain it: "The man who is quiet is probably doing something bad".

We compared lots of other notes while we ate popcorn on Sunday evenings. I learned that Awajún grammar is very different from Yánesha'. For example, the basic order of subject, verb and object in Awajún is subject-object-verb, whereas it is verb-subject-object in Yánesha'. I had learned that from sentences such as *rrenan atollop mam* '(he/she) is-eating chicken manioc', that is, 'the chicken is eating manioc'.

As we compared notes about what the teachers were writing, Millie showed me a one-sentence story consisting of 137 clauses; each verb in the 136 subordinate clauses ended with a suffix that indicated if the subject was the same or different as that of the following clause until the final independent clause, which had a pronoun 'thus he did'. That was my introduction to clause chaining and switch-reference systems.

Later as I consulted with field linguists on other languages with switch-reference systems, I noted that they all had subject-object-verb order in the

sentence. I presented the hypothesis that a switch-reference system implies subject-object-verb order at the Congress of Americanists in 1979; it was published in Spanish in *Lexis* in 1980. Since that time, I have looked at languages from several different families comparing basic word order with other features such as theme and focus of attention in discourse and come up with other hypotheses relating to this general theme.

6. Language contact

One of the first folktales Martha and I recorded and analyzed was about *Ink* who was a very angry person and married 'our mother', named *Palla,* who was the daughter of *Yato*' *Yos* 'our God'. We gathered from that story that there must have been a good bit of interaction between the Inca empire and the Yánesha'. Many Amazonian languages have stories about Inca: some as though there were only one, as in Yánesha', and some as though there were several Incas. They also have obvious loan words from Quechua but in Yánesha' the number of loan words is overwhelming.

In 1969, I consulted with Helen Larsen on her analysis of Huaylas Quechua narratives, and then in 1973 I began consulting with co-workers studying other varieties of Quechua. I soon found I could almost follow the thread of a narrative through the words that I recognized from Yánesha' – scores and scores of roots from almost every semantic field. In 1976 I published my findings in a paper titled "*Apuntes sobre la influencia Inca entre los amuesha: factor que oscurece la clasificación de su idioma*".

Consulting with other field linguists who were studying Candoshi, Muniche, the Jivaroan languages, and languages from other families brought to light other puzzling similarities with Yánesha' in phonological features and loan words (or cognates?). Then Steve Parker studied Chamicuro (on the Huallaga River) for his Master's thesis and later showed that it and Yánesha' do indeed form a branch of the Arawakan family tree. One morning at the SIL center in Dallas, while out for my daily walk, I thought: "What if the Yánesha' didn't migrate up the Ucayali and the Pachitea Rivers to their present location, as some have hypothesized? What if they migrated up the Marañon, then the Huallaga, passing through the area where the Panao Quechua live now to the Pozuzo River and their present location? That would explain how Yánesha' and Chamicuro form a branch; how there are words in Cholón that look like Yánesha' although the two are not related; and why Yánesha' shares phonological features with Candoshi, Shawi and the Jivaroan languages although they are unrelated". I first published on that hypothesis in 2011.

There are enough related unanswered questions to keep me busy with linguistic fieldwork for a long time to come.

7. Recycled

Many years had passed since I had spoken Yánesha' or thought much about the language except for comparative work and editing Martha Duff-Tripp's[3] dictionary and grammar. Then in January 2010, an e-mail SOS arrived from four of the bilingual teachers who were working with a literacy consultant on a new reading book for the schools. They asked me to please come and help them modify their alphabet. All of their books were out-of-print, and they desperately needed new ones. However, once they learned how to use the correct fonts to produce tildes over p, b, m, t, k to represent palatalization, and dieresis over ch for the retroflex affricate, the printers replaced those letters with rectangles and it would be impossible to reprogram every commercially owned printer that might be used by someone in the vast Yánesha' area.

So, I returned to Yarinacocha and worked with four teachers and three other Yánesha' adults. I suggested a couple of alternatives and they chose what they thought would work best. (They can't use ty, py, etc. for palatalization since there are sequences of py, ty, my, and ky.) They eventually decided to use h following the consonants (ph, th…) and to substitute x for the c with dieresis. They also decided to write k rather than c/qu and w rather than hu.

The next step was to present a request for change to the Ministry of Education in Lima, which one of the Yánesha' men and I did the next week. Four meetings of representatives from different villages were held in different areas over the course of the next year, and in May 2011, the Ministry of Education sponsored a congress with representatives of most villages. After three days of discussion, they voted in approval of the new alphabet, one that can be written with no special fonts.

Where does recycling enter the picture? When the teachers first invited me to help them, it had been over 45 years since I had had continued interaction with speakers of the language. I did a lot of code-switching in the first few days, but now that I have worked with them on four new books in the language I am speaking fairly well and have so much fun working with them on the books, especially those that help them appreciate some of the riches of their language.

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3. Martha completed the Yánesha' New Testament translation, married Robert (Bob) Tripp in 1976, and worked on literacy among the Amarakaeri while Bob completed the New Testament translation. She also completed and published a Yánesha' reference grammar and dictionary. Martha died in February 2014.

Sadly, Yánesha' is among the seriously endangered languages of Peru. I hope the publication of many new and revised books in the language will be a factor in their language never dying.

Would I do it again?

Oh yes! Yánesha', Nomatsigenga and the other indigenous languages I have studied – however briefly – present endless fascinating questions about phonology and grammar, language universals, and possible pre-historic migration paths. Linguistic theories have come and gone since 1953; the endangerment of Amazonian languages increases daily as trucks and cars replace travel by foot on trails through the jungle or by canoe on the rivers, and colonists from the coast and Andes seize possession of more and more of the Yánesha' homeland.

During my first years with the Yánesha', visitors from downriver took pride in telling me that their children spoke only Spanish, although all I ever heard them say was Yánesha'. Now, however, when I visit some of the villages I realize that the children really do not speak Yánesha'. The language is, indeed, highly endangered, but thanks to a high percentage of literacy resulting from bilingual education and literature in their language, the Yánesha' have some important tools to help them maintain their language and culture.

And what about the Bible translation? As we finished translation of a book such as John, we had it printed. When we visited the schools, the first question of the students who met us was, "what new Scripture books did you bring us?" Many became Christians and when the New Testament was presented in 1979, representatives from about twenty churches that had sprung up took part in the program. One anthropologist who did extensive fieldwork among the Yánesha' mentions the "massive conversions". Now the Christians are all asking for a translation of the Old Testament in their language. And they are not waiting for me: when about forty-five of the pastors met in April of 2016 to make initial plans for the project, I was presented with a hand-written draft of the book of Genesis. Yes, I would do it again!

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