Coherent Fieldwork[1]

Daniel L. Everett
University of Manchester
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1. Introduction

The history of research in general and field research in particular, is the history of fallible humans, evolved creatures, struggling to understand nearly infinite complexity in an alien environment. No one person is up to the demands of fieldwork, requiring as it does an idealized character from Arthur Conan Doyle. The outputs of our fieldwork will necessarily be incomplete records of our progress in understanding parts of wholes that exceed our abilities. Thus, our research reports, whether grammars or articles or talks or webpages are never more nor less than our efforts to communicate with interested interlocutors about the beliefs we have come to form and hold, based on our experiences and how these beliefs affect our actions in science and in life. This is our canopy of epistemic humility. Further, I am more and more convinced that the beliefs we have come to hold about a particular language or grammar are constrained and shaped by the totality of our experiences, not merely our linguistic training. If this is correct, one immediate consequence for fieldwork that emerges is that compartmentalization of knowledge and its isolation from application, the notions of 'pure' ideas, 'deep thoughts', and 'objectivity' are recast from normal expectations to just ways of talking about what works.

The pragmatist view of fieldwork which I advocate here has a philosophical pedigree to claim for its integration of life, research, and application, tracing back to the three concepts of *usefulness*, *radical empiricism*, and *coherence* urged by William James (1842-1910) (see James (1896), among many others). These are the principal calibrators of research success in pragmatism. As such let us consider each in turn. Usefulness is just the idea that a theory need not be true or even falsifiable to be a good theory. Rather it must move us towards our goals and have some utility in society. Coherence is the idea that each researcher accept and acknowledge the role of her temperament in her science and, more importantly, her status as an evolving creature with an ability to learn limited by tools provided by random mutations, natural selection, and perhaps a few other side effects of evolution. I say more about this below. Radical empiricism will be taken up directly. First, though, let us return to our fieldworker and some preliminary advice for her.

The first question the fieldworker must ask herself, i.e. before she applies for a research grant, packs her bags, or buys a plane ticket to some exotic place, is this: what do I plan to study? What is the exact object of my investigation? Am I studying something directly observable or something only inferable? Am I studying a set of rules that I can formulate about observed sentences or the characterization of a speaker's knowledge of her grammar? Am I studying Language? A language? The cognitive capacity behind Language? A corpus of texts? The behaviour of native speakers when

exposed to certain questions? The answers to a questionnaire? The emergent communicative behaviour of a particular culture? All of the above? None of the above? And once she has decided what her object of study is, she might ask what it is that justifies her choice. Could one ever prove that any of the questions above is the right question to ask, for example? Further, once one has studied an object, is there any way to quantify what it is that one purports to have come to know?

Let's consider a concrete example. In Everett (2001, 168), I defined fieldwork as in (1):

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

Why propose this definition? And could I ever prove that it is the best definition? Is there any truth to it? Well, I can say that I believe (1) to be maximally useful in grasping the coherence between a language and its culture, which I in turn find useful for understanding language itself. But that is about all I can say for it. I certainly cannot prove it. Still, it is a point of departure for coherent fieldwork, more useful than many alternatives, perhaps less useful than others, all applications of usefulness teleologically determined.

But can't we do better than to merely claim usefulness for our proposals? After all, many researchers I have spoken to over the years will say the quantifier of their findings is 'Truth', that they are after the 'Truth' about one of the questions above. This is an interesting position, though in spite of its popularity, I think it is useless. Let's consider it a bit, therefore. Imagine that we can find truth. Many scientists believe this to be a legitmate goal, after all. In recent work, Chomsky (2002, 129ff) is very explicit that he is after Truth: "So, the first question that has to be met is TRUTH [emphasis mine, DLE] for every state of the language faculty.' (133) or "Minimalist questions are substantive: they ask whether TRUE THEORIES [emphasis mine, DLE] of states of the faculty of language satisfy the interface condition in an optimal way." (132)

But what would 'truth' or a 'true theory' look like? Would we ever be able to recognize either? Can we ever find this truth? Do we asymptotically approach it by ever-closer approximations to it? Or, paraphrasing Rorty, is truth just a compliment we pay to ourselves when we have made a well-justified statement? If my statement about 'x' at time 't' in context 'c' is true, then this implies that that statement is never in need of revision. But suppose I in fact do need to revise my statement as a result of a new fact coming to light. Was my previous statement true? Maybe, you say, in the part of it that did not need revision. But how can we ever know, in principle, what part will not need revision? The world and its languages are 'out there', of course. The denial of Truth need not be a denial of Reality, though it does imply that we can never claim to have apprehended Reality. It is difficult to see how saying that we are looking for truth is any better than saying that we will do our best to be convincing. The average field worker experiences a receding truth-line. Insisting on truth is just insisting that one drink the water from a mirage.

There are two applications of the explicit acceptance that Truth is ever-receding, or, in my view, non-existent. First, lack of guilt and arrogance. Second, the healthy

refusal to be locked in to a given solution. All statements about a language are subject to revision in principle. But this just means that in principle no statement about a language is true. Let's consider these in turn. To begin, how does an eschewal of the concept of Truth enable us to work guilt-free? It does so by freeing me to follow my own interests with no sense of servitude to intellectual fashion. I can accept, say, a definition like (1) of my enterprise, or some other, or even none. If Truth existed, I might feel that I should not work on false ideas when other ideas had been shown to be true. But since nothing can be shown to be true, by my pragmatist reasoning, no such burden can be placed on me. And yet at the same time, just as I no longer feel inferior for my specific choice of goals, such as fieldwork over theory, I have no basis for feeling superior either. My choice is no truer than any other's choice. Pragmatism would simply urge us to do what is most useful from our perspective and, as I discuss directly, what 'coheres' most usefully with our life experiences and goals. But this is not to say that theory no role to play in fieldwork, that I should work alone, or that I should ignore other linguists and their results. All field researchers should reflect on the role of theory in the enterprise of fieldwork. And all should endeavour to learn from the past. As I have stated and as reflection should inform any experienced linguist, our field research is inescapably and rightly constrained and motivated by our life experiences, including reading, thinking, and other engagements with linguistic theory. The field researcher without knowledge of linguistic theory, without an on-going reading program in modern linguistic theory, whether eclectic (which I recommend) or focused on a single theory, is severely handicapped. But I suspect that most linguists know this. The real question is not whether the field researcher (should) know(s) linguistic theory but to what degree linguistic theory should constrain her fieldwork. I would say 'up to coherence', in the sense given.

Another question relevant to the fieldworker is what role she plays in the development of linguistic theory. Should fieldworkers and theoreticians be different people or, ideally, should each fieldworker be a theoretician as well? The answer is surely that this ultimately depends on the tastes and preferences of the individual fieldworker. At the same time, I believe that linguistics benefits when fieldworkers are doing more than merely gathering data for a theoretician to interpret. This would place fieldworkers in the same position as Gideon Mantell in relation to Richard Owen in the study of the fossil record. Owen considered Mantell at best a collector of data for him, Owen, to interpret (see Deborah Cadbury's (2000) fascinating account of their relationship). Yet science suffered greatly because of Owen's ascendance over Mantell and his failure to accept Mantell as a theoretician of the first order. As for Mantell and his fossils, so with the fieldlinguists and their data - there is really no one better placed to interpret field data from a theoretical perspective than the fieldworker who collected the data, given the theses of 'radical empiricism' and coherence discussed in this paper, so long as the fieldworker is hard on herself and develops the requisite stringency of thought. To get at the import of this, we return to the third component of James's pragmatism, viz., radical empiricism. According to radical empiricism (James (1987 [1909])), 'reality is just the flux of pure experience' or 'reality consists in nothing but experience'. It is our experience with an object that gives that object reality. But since no two experiences will be exactly the same, no object can be the same to two people (or for one person at two times). This includes grammars and the other outputs of fieldwork. James' thesis seems particularly

useful to answering the questions posed above about the object of fieldwork. But to fully appreciate it, we need to make the connection between radical empiricism and coherence.

If experience is all there is, as it is according to radical empiricism, then there is no 'Truth', only usefulness. Moreover, the experiences which have the greatest coherence with the rest of our lives will be those which are most useful to us. Radical empiricism almost by definition favors the specific experience over the less directly experienced reasoning behind 'big picture' theorization. Although James was by no means averse to generalizations, yet he advocated the view that most useful 'big picture' was the generalization that best coheres with one's life experiences. This will require some exemplification. Let me first exemplify what coherence means for me in fieldwork and then exemplify what I think it means for my object of study.

Coherence for me personally means that I cannot objectify myself, i.e. prevent my own history and person from interacting causally with my observations and conclusions. It might be useful to try, but at the same time I realize that my efforts in this regard, should I expend them, will always fall short. (Kenneth Pike recognized something like this in his many references to the role of the 'observer' in the scientific process.) So, for example, I oppose much of the conforming power of Western culture. Therefore, I have noticed that what I often observe and most enjoying finding out about in my fieldwork are those things which make a language different from other languages, the 'relativity' among people and their languages found in fieldwork. I like to find and look for counterexamples to theory, rather than what is to me mere confirmation of some other linguist's beliefs. It took me some time to admit this personal aspect or that it affected my fieldwork. But I am happy now to acknowledge it and am unconcerned about it. My perspectives add to the tapestry of experiences emerging from fieldwork around the world and will be useful to others (or not). As they are useful to other linguists, then I am doing useful linguistics. That is about all I can hope for, frankly. If you take nothing else from this paper, remember this: *If your fieldwork is coherent for you and useful to others, then you must be on the right track.* Ascribing Truth to any part of the enterprise is little more than a religious incantation.

The other side of coherence, at least in my interpretation of James's notion, concerns my full experience with my object of study. That is, that whatever I say or might say about one aspect of my object should cohere with other statements I have made about the object, and the sum of my experience with the object. For example, as I reside in an Amazonian community, my understanding and reports of the language or grammar or phonology, etc. of the language of that community ought to cohere with what I know about the speakers of that language and, where possible, tell me something about the cultural matrix in which the language is embedded. In section 2, I give an example from the communities I know best, those of the Pirahã people scattered up and down the Maici River.

Before I turn to specific empirical cases, however, I would like to round out this introduction to fieldwork with a brief, impressionistic view of its history, focusing on the Americas. Arguably linguistics in the Americas and elsewhere began as an extension of colonial activity, specifically missionary work. In Brazil, the French Calvinist chronicler Jean de Lery (1534-1613?) compiled fascinating and extremely useful records of Tupinambá conversations. Today this once widely-spoken language lives as a communication system only in the conversations recorded by Lery. Contemporary with

de Lery was the amazing Padre Jose de Anchieta (1533-1597), founder of the city of São Paulo, co-founder of the city of Rio de Janeiro, and the author of a brilliant grammar of Tupinambá and translator of many catechisms into this language. Anchieta could plausibly be called the founder of linguistics in the Americas. Following Anchieta was Padre Antonio Ruiz de Montoya (1585-1652), who produced brilliant studies of the Guarani language. (Montoya has what I believe to be the first insightful discussion of Noun Incorporation and Possessor Raising anywhere, offering a brilliant account of how the verb's case is freed up after Noun Incorporation to be re-assigned to the possessed NP – over 300 years before, say, Mark Baker's (1988) theory of incorporation.) Of course, the study of language motivated mainly by science did not begin until much later. Still, these Jesuit priests in Brazil and elsewhere set standards of scholarship that endure through the centuries.

In the Americas, the scientific concern for fieldwork began with Franz Boas (1858-1942), who trained a core of linguistic anthropologists (Ruth Benedict (1887-1948), Edward Sapir (1884-1939), and in some classes and via Sapir, Mary Haas (1910-1996), among others) responsible for the birth and growth of North American linguistics. During the years of Boas's influence, roughly during his life and following his death until the 50s, North American linguistics was concerned about describing specific languages in detail, producing integrated studies of texts keyed to cultural studies, grammars, and dictionaries, providing exactly the kind of pragmatist study that has proven to be so important to knowledge of little-studied peoples and their languages throughout the intervening years. In fact, though this is not the place to attempt a more detailed intellectual history, a case can be made that this earlier descriptive linguists were heavily influenced by the pragmatist philosophy underlying much American intellectual endeavor until at the least the death of John Dewey (1859-1952). Consider some remarks of Boas in his 1917 introduction to the first volume of the new **International Journal of American Linguistics** (IJAL). According to Boas one of the principal goals of the new journal was to provide what I would call a 'coherent' report of languages. For example, he (1917, 201) laments the fact that "… the available material gives a one-sided presentation of linguistic data, because we have hardly any records of daily occurrences, everyday conversation, descriptions of industries, customs, and the like. For these reasons the vocabularies yielded by texts are one-sided and incomplete." That is, Boas felt that a full 'picture' of a given language was only possible by looking at the language in the cultural context. Or consider Sapir's (1915, 186) assertion that more studies are needed of cultural 'modalities of attitude' and consonantal alternations (I discuss this further in 2 below), thus explicitly connecting grammar with culture.

Thus, for the first half of the Twentieth Century, the normal conception of the linguist's 'job' was to study little- or un-studied languages in the field and to produce coherent bodies of data on the interaction of culture, lexicon, texts, and grammar.[2] But by the 60s this had changed radically, with fieldresearch given more or less the intellectual status of butterfly collecting. The 'withering of fieldwork' began innocuously enough, in the restlessness of a graduate student at the University of Pennsylvania with his MA research:

> *"Harris suggested that I undertake a systematic structural grammar of some language. I chose Hebrew, which I knew fairly well. For a time, I worked with an*

> *informant and applied methods of structural linguistics as I was then coming to understand them. The results, however, seemed to me rather dull and unsatisfying. Having no very clear idea as to how to proceed further, I abandoned these efforts and did what seemed natural; namely, I tried to construct a system of rules for generating the phonetic forms of sentences, that is, what is now called a generative grammar." (Chomsky 1975,25).*

Chomsky's intellectual frustration with (an extremely easy version of) standard fieldwork led indirectly to some of the most important developments in the 2000 + year history of the study of language, so I am hardly complaining about the direction Chomsky decided to take. Nevertheless, the very intellectual vigor and power of Chomsky's subsequent work sufficed, in my view, to pull most linguistics students and departments away from the traditional emphasis on fieldresearch to theoretical work on, for the most part, the linguist's native language. Though there is nothing inherently anti-fieldwork in Chomsky's research programme, his attitude, as expressed in the passage just cited, and his rejection of the intellectual priorities of Boasian linguistics led to an abandonment of fieldwork in the US and a nearly five-decade neglect of the study of indigenous languages and fieldwork throughout the linguistics world, as his influence soon became massive and international. Over the past decade as the spotlight has begun to shift to fieldwork once again, it has been primarily concerned with the study of endangered languages (see 5 below) and has not yet recovered the 'Boasian imperative' of coherent, integrated fieldwork. This is unfortunate and one hopes that we will continue to make our way 'back to Boas'. Perhaps my reasons for this statement can be better understood by means of an example. I will now turn, therefore, to what I hope will one day be a new trend of 'Ethnogrammatical studies'.

2. Coherent Fieldwork: Ethnogrammatical Studies

In his 1921 book on language, Sapir (p172) talks of the need to understand the 'genius' of each language. By this Sapir refers to that which makes each language unique, the essential core of a language, that part less subject to historical change (a sort of Heraclitus-inspired question of what changes and what remains). Judging by his intellectual output, Sapir was always concerned with coherent fieldwork. His concern was with difference, the *relative* value of a given language, as opposed to it as merely an exemplar of one setting for an absolute UG. One good example of what I mean is found in a study he undertook of Nootka (Wakashan, Canada) consonant alternations. In this language, as Sapir (1915, 181) observes, there are extremely interesting consonantal alternations that cannot be explained grammar-internally.:

> *"It is possible and often customary in Nootka to imply in speech some physical characteristic of the person addressed or spoken of, partly by means of suffixed elements, partly by means of 'consonantal play'. Consonantal play consists either in altering certain consonants of a word, in this case sibilants, to other consonants that are phonetically related to them, or in inserting meaningless consonants or consonant clusters in the body of the word. The physical classes indicated by these methods are children, unusually fat or heavy people, unusually short adults, those*

suffering from some defect of the eye, hunchbacks, those that are lame, left-handed persons, and circumcised males."

Sapir exemplifies this 'consonantal play', concluding that to understand the grammar of a language, we must therefore understand the culture in which that grammar is found. Sapir's study of Nootka is well-known, of course. But perhaps it has failed to exert modern influence because it is considered to be a marginal example. In fact, I believe such situations to be fairly common. So let me give some other examples, this time from my own research, which buttress the case for coherent fieldwork.

As first pointed out in Everett (1982), Pirahã (Amazonian language isolate) has two rare sounds, one of which is found in no other language. These sounds are [B] and [L,]. The former is a voiced bilabial trill and the latter is a lateral-apical double-flap. These sounds are allophones of /b/ and /g/, respectively and, according to Everett (1979), derive historically from *b and *d. The special interest of these sounds for our present discussion is that they are not used in the presence of non- Pirahã -speaking outsiders. This means that (i) Pirahã speakers are able to control sub-phonemic elements (a bit problematic for traditional views of the phoneme) and that (ii) Pirahã phonology cannot be fully described or understood without a knowledge of how it interacts with culture. There are other examples from Pirahã phonology. Let me present two of the strongest, in ascending order of importance for coherence.

Pirahã women have a different phonetics and phonemics from Pirahã men. Pirahã women manifest a smaller articulatory space than Pirahã men. In general , females' points of articulation are retroflexed compared to men's and the 'guttural sound' one associates with their speech is the result of contracting the walls of their pharynx. Further, women's speech has one phoneme less than men's: where men's speech has both /s/ and /h/, women's speech has only /h/ where men would have /s/ and where men's would have /h/. A full statement of the phonetics and phonology of Pirahã must, therefore, include gender-based differences and would be seriously incomplete without this additional data. A formal phonology would not care about this, but coherent fieldwork does.

But, one could ask, aren't these first two Pirahã examples just run-of-the-mill sociolinguistics? I would say 'not quite', though in fact, I did have something more ambitious in mind for this section. If extragrammatical considerations could in principle play a causal role in phonological structures (not merely selecting them but forming them, to use a distinction made in Everett 1994), how would our conception of phonology change? What would constitute a 'causal role' for these factors in the phonology?

Here is a possible scenario. Imagine that a language could have various systems/modalities of sound structure, beyond its phonetics and phonology. And then consider the possibility that one modality can affect another, but not necessarily via constraint-rankings or rules, the standard devices of phonological theory proper. If so, then to understand the sound system of language, **L**, at any level (e.g. 'what happens' or 'what native speakers know when they know the sound system of their language') we must look carefully at the modalities of expression made available via an ethnography of communication and not merely at a supposed universal formal apparatus. Corollaries of this scenario might include, e.g. the appearance of new roles for old constraints (e.g. mode-faithfulness of segments being highly ranked to mark syllable types; syllables are maintained, a form of prosodic faithfulness, in order to parse the larger speech stream,

not merely to enhance the perception of segments; and thus arguments for syllables may go beyond phonotactics and segmental enhancement and the syllable may have roles not envisioned by the so-called 'phonological hierarchy'). If this were true, the coherent fieldwork would evolve from a curiousity or desideratum to an imperative. Is there such a case? Indeed. Consider the following facts about Pirahã phonology, beginning with its phonemes.

Table One
Pirahã Phonemes

Consonants () = missing from women's speech[3]			
p	t	*k	?
b		g	
	(**s**)		h
Vowels			
i			
			o
	a		

The first thing to notice about Table One is that the segmental inventory is one of the smallest in the world. The next is to recall that it includes allophonic sounds found in no other language, subject to cultural constraints. The third is that the /s/ is in ()s because it is not found in women's speech, but only in men's.

Though this is one of the simplest segmental phonemic inventories in the world (the women's inventory does seem to be the simplest known), we should juxtapose alongside this simplicity, the complexity of Pirahã's prosodies. Pirahã's stress rule is a good place to begin, since it is well-known.

This rule, from Everett & Everett (1984), is considered one of the more complex and unusual stress rules in the literature, mainly for its phonological consequences (rather than, say, any difficulty in stating or recognizing it):

(1) Pirahã stress rule: stress the rightmost token of the heaviest syllable type in the last three syllables of the word.

The phonetic basis of 'heaviness' in (1) is just this: Voiceless consonants are always longer than voiced consonants and there are five syllable weights based partially on this contrast:

(2) Pirahã 's five syllable weights: CVV>GVV>VV>CV>GV

Pirahã is a tonal language, as well. But stress, tone, and syllable weight vary independently in the language. To see this, I will just review one simple set of examples, in (3), from Keren Everett (1998). In the examples in (3), tone is independent of stress. ´ = high tone; no mark over vowel = low tone. The stressed syllable is marked by **!.** There are no secondary stresses (7=glottal stop).

(3) a. **!**tígí 'small parrot'
b. **!**pìgì 'swift'
c. **!**sàbí 'mean, wild'
d. **!7**ábì 'to stay'
e. tíí**!**híí 'bamboo'
f. 7ì**!**tì 'forehead'
g. tì**!**7í 'honey bee'
h. tí**!**hì 'tobacco'

Thus alongside Pirahã's extremely simple segmental phonology, it manifests an extremely rich set of prosodies. This leads us to ask a reasonable question, namely, does the language exploit this differential complexity in any way? Indeed, as Everett (1985) describes it, Pirahã communication makes crucial use of the CHANNELS in (4), below, where Hymes (1974) defines a channel as 'sociolinguistically constrained physical medium used to carry the message from the source to the receiver'. The four principal modalities or channels in Pirahã after 'normal' speech are:

(4) *CHANNEL*	*FUNCTIONS*
a. **HUM SPEECH**	Disguise Privacy Intimacy Talk when mouth is full Child language acquisition relation
b. **YELL SPEECH**	Long distance Rainy days Most frequent use – between huts & across river
c. **MUSICAL SPEECH** ('big jaw')	New information Spiritual communication Dancing, flirtation Women produce this in informant sessions more naturally than men. Women's musical speech shows much greater separation of high and low tones, greater volume.
d. **WHISTLE SPEECH** (sour or 'pucker' mouth' – same root as 'to kiss' or shape of mouth after eating lemon)	Hunting Men-only (as in ALL whistle speeches!) One unusual melody used for aggressive play

Example (5) is illustrates how prosodic information in Pirahã is exploited to create these channels. The inventory in Table One above, also partially shows how little the segments contribute to the total set of phonological information in a given Pirahã word. In (5) we see that the phrase in (5a) has the quasi-musical representation in (5b), the basis for the channels just summarized.

(5) a. **kái?ihí?ao -?aagá gáihí**
paca poss/exist-be there
'There is a paca there.'

b. 

All channels must include the information in (5b), though only the consonant and vowel channel needs to include the information in (5a). The notes represent syllables, with 'ties' indicating unbroken falls/rises in whistle speech.

In the musical form in (5b) there is a falling tone, followed by a short low, with a preceding break in the whistle (where the glottal stop would have been in **kai*?i*hi**), followed by another short break (where the **h** would be) and a short high tone, and so on. Thus, the syllable boundaries are clearly present in whistle (humming, and yelling) channels, even though the segments themselves are missing. The syllable in this case indicates length, offers an abstract context for tone placement, and the overall word is stressed according to syllable weight (see Everett (1988) for details). The syllable in these cases is vital to communication in differing channels, primarily in parsing the input.

But does the discovery of channels like this imply any causal interaction between culture and grammar? Or are these channels outside the grammar proper? Notice that these channels rely crucially on the syllable weights and stress rule in (1) and (2) above. So, if nothing else, they help account for what is otherwise an anomalous level of complexity in the stress rule. Yet the facts cut deeper than this. Consider the following example of what Everett (1985) calls the 'sloppy phoneme effect' :

(6) **tí píai ~ kí píai ~ kí kíai ~ pí píai ~** ?**í píai ~** ?**í** ?**íai ~ tí píai, etc. (*tí tíai, * gí gíai**, *__bí bíai__) 'me too'

(7)?**apapaí ~kapapaí ~papapaí ~**?a?a?aí **~kakakaí ~(*tapapaí, * tatataí, * bababaí, * gagagaí**) 'head'

(8) ?**ísiihoái ~kísiihoái ~písiihoái ~píhiihoái ~kíhiihoái ~** (alternations with /t/s or involving different values for [continuant] or [voicing] are unattested) 'liquid fuel'

Pirahã allows a tremendous amount of variation among consonants, though not for the features [continuant] or [voice]. This can be accounted for, but only if we refer to Pirahã's channels. The ungrammatical examples in (6)-(8) show that the features [continuant] and [voice] are linked in the sense that they may never vary in the effect. Only place features may vary. With no reference to channels this is without explanation. But in light of the channels this follows because [continuant] and [voice] are necessary

for stress placement (Everett (1988)) which in turn must be preserved in every discourse channel, or the constraint in (9) is violated:

(9) *Constraint on functional load and necessary contrast* (Everett (1985)):
a. Greater Dependence on the Channel → Greater Contrast Required
b. Lesser Dependence on the Channel → Less Constrast Required

Notice that I am not claiming that the absence of variation for different values of [continuant] is predicted by 'channels' alone. This case in fact demands that we further investigate the connection between [continuant] [voice]. There is no claim that ethnography replaces phonology! But I am claiming that without the study of channels and their role in Pirahã culture, not even an understanding of Pirahã's segmental phonology is possible.

Moreover, this type of example is important for the theory of phonology, i.e. as part of UG, if indeed it is (which I doubt). The lesson is just this: as a modality-dependent channel, phonology may be subject to constraints that are (i) language specific and (ii) grounded not only in the physical properties of the instantiating modality (the phonetics) but also or alternatively on the culture-specific channels of discourse employed. This is a very important result because it shows that the 'interface conditions' of the HUMAN COMPUTATIONAL SYSTEM, in Chomsky's (1995) terms, may range beyond PF and LF, if we define an interface system as a system setting bounds on interpretability for HC_L. Such examples also show how coherent fieldwork can be useful for theory. Thus not only the fieldworker, but also the phonologist must engage the language as forming a coherent whole with culture. And this in turn means more fieldwork, the reconsideration of old phonological themes, new training for graduate students, new data-bases, and on and on.

It seems to me that such findings also provide support for the proposal in (10), harmonious with Jamesian philosophy:

(10) The study of 'Universals' is no more vital than the study of 'Particulars':.

This is so because, among other things, if UG is the CORE of linguistic knowledge, the study of particulars leads to knowledge of the perimeter, setting the outer boundaries. This is also consistent with research by Ladefoged and Everett (1996), in which it is found that there are phonetic rarities, particulars, which are simultaneously violations of distinctive feature theory yet not ignorable nor solvable by that theory. That is, that these rarities are particulars with general theoretical import qua particulars. Let us move now to some practical considerations in fieldwork.

3. Fieldwork and technology

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

The first is that the most important equipment for the fieldworker are talent and training, the former greatly outweighing the latter. And this talent and training will most clearly manifest themselves in the linguist's five senses (mental/physical data input

devices) and her ability to interpret the results she gathers (mental data-processing). Does she think and read regularly about other languages? Does she have a well-developed ability to distinguish segments and prosodies? A talent for language-learning? An enjoyment of the exotic? A strong constitution? Ability to learn and teach with patience and clarity? These are by far the most important toolkit the fieldlinguist will possess.

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

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

(i) Who will be able to use the output of your equipment now and in the future?
(ii) Is the equipment portable?
(iii) Does the equipment provide state-of-the-art accuracy, or as close to it as the fieldworker can afford?
(iv) Will the equipment help record both the grammar and its cultural matrix?
(v) Does the equipment use a practical power source for the location in which it will be used (such as solar power)?
(vi) Does the fieldworker's equipment include satellite-based communication equipment, for email and phone contact from any part of the world to any part of the world?

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

Now let us turn to consider data-preservation and digitization when processing one's data back from the field, at one's home institution. This has become a vital issue in Twenty-first century fieldwork.

4. Web-design, digitization, and data-preservation

Before travelling to the field, the fieldresearcher and all members of her team (which may include no one else but the lone fieldworker) should be trained in the field-collection of audio, video and pictorial primary data and determine the form of the notes and metadata to be associated with the collected data in the data base, e.g. Name, Tribe, Dialect; Gender; Age; etc. Any less-experienced members of the field team should also practice data-collection and field-analysis prior to the fieldwork seasons. Once in the field, members of the team should follow their pre-agreed upon plan to collect, transcribe,

and conduct preliminary analysis of captured data. Unprocessed data is almost always useless away from the field situation. It is also vital that the team ensure that all collected data are secured and backed up to alternate media, e.g. DVDs and copied onto each team member's computer in the field (i.e. each team member's laptop should contain a full copy of the entire team's data). Returning to the home institution the data should be further backed-up, preferably using institution-wide resources that enjoy a long-term commitment of the institution's resources and administration.

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

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

These planning stages will then be followed by actual site construction. During this phase the research team, in conjunction with the web-programmer, will: (a) construct the underlying database; (b) populate the database from analysed field data; (c) construct the website front-end working from the agreed story-board; (d) program the agreed methodologies for the extraction and viewing of the underlying data; (e) conduct usability trials on the prototype system with interested parties (e.g. linguists experienced in web design and/or field linguistics); (f) consult with a wide-range of experienced experts to ensure best-practice and comply with emerging international standards in web-archiving of field data, e.g. the Open Languages Archive Community (OLAC); Electronic Metastructure for Electronic Languages Data (EMELD); Oxford Text Archive (OTA) and others.

It is also important to consider something often referred to as 'data-development'. What kind of data will the fieldresearch collect? How will this data be processed in the field? How will the data be processed after the field experience? For example, some types of data from my own research include the following: (i) Speech: Digital tape in AU/WAV formats; b) Video: Digital tape in Apple ‘QuickTime’ format; c) Pictures: Digital images in PNG format; d) Metadata related to the above data.

In my research, I attempt to pre-process field data along the following lines: (i) prior to a first team trip, team members agree on an initial system for annotating and tagging collected data, following standard linguistics field guides and my own previous experience; (ii) in the field, data collected will be checked with multiple native speakers. All video and audio recordings are downloaded onto each team member's computer and

annotated, summarizing analysis and discussions among team members. All these annotations and data are also copied to DVDs, two copies made of everything. Audio files are analyzed using PRAAT phonetics software or its equivalent. This analysis will be especially important for developing and testing hypotheses in the field, as well for training team members. For example, by viewing acoustic measurements of the data in the field, the research team can more effectively train members' hearing to distinguish between unusual (i.e. non-Indo-European) combinations of tone, intonation, and stress placement; (iii) transcriptions of data are first done longhand in indelible ink in hard-backed field notebooks. These are then keyboarded and copied to each member's computer and then to DVDs (it is useful to do this for various reasons. An important one is that it is always useful for field researchers to handle and process the data multiple times to better control it. But fieldnotes in longhand also provide for creating symbols not available on computers initially, and can be done when not near one's computer, as often arises in field situations). These transcriptions will all include metadata, tagging them to the appropriate audio and video files (e.g. which computer file they are in). Digital photos are made of each informant/language consultant and imported into the Word .doc files of the appropriate text transcription (or their equivalent); (iv) at my home institution (the University of Manchester), all data are copied to the Linguistics Department server. DVD copies are also made and then ideally would be made available to the appropriate institutions of the country of research, e.g. for Brazil, the Museu Nacional in Rio de Janeiro. At the home institution, initial field transcriptions are expanded to include as much of the phonetic nuances as possible, up to the point that members of the team to have reached the 'point of diminishing returns' relevant to the project. Sound files are also analyzed in more detail, again using an advanced acoustic measurement program, focusing on prosodic features in the case of my own research. Video files are studied and discussed, e.g. as to how hand gestures, facial expressions, and other features (might) correlate with grammatical and cultural features of the texts being told. A secretary is appointed at all group meetings in which we discuss the data and metadata. This secretary distributes minutes of these meetings to team members, maintaining copies on, say, a computer server at the level of the department or the university. Tagging of texts, audio files, and video files will include any special/additional information agreed upon by the team.

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

5. Field Research and Language Endangerment

There are nearly 7,000 languages spoken in the world today. Many of them will disappear beneath the waves of time without being documented or described in any depth. This is a tremendous loss. But there simply are not resources or personnel at present to cover all of these. Field research is still a minority activity in international linguistics. Even in anthropology it has lost its luster.

This seems a tremendous step backwards, given the history of our discipline, at least in North America. One might plausibly claim that North American linguistics began from the interest in documenting and describing endangered languages that was temporarily lost with the 'withering of fieldwork' mentioned in section one. In any case, the use of the phrase 'endangered language' is a fairly recent development. Its history can be traced from the early 1990s.

Appropriately, the term seems to have been introduced into modern linguistics by Kenneth Hale. At the 1991 annual meeting of the Linguistic Society of America, Hale organized and led a session on 'endangered languages', which led to the founding of an LSA Committee for Endangered Languages and their Preservation, in 1992, which was initially chaired by Michael Krauss. From 1993 on, the LSA meetings have included special sessions on field reports, focused on endangered languages. The Linguistics Association of Great Britain (LAGB) held a workshop on endangered languages at its 1993 Fall Meeting, with participation from Nigel Vincent, Greville Corbett, Alastair Walker, and Colette Grinevald (Craig) and has since emphasized the study of endangered languages. Since the early nineties, other meetings and organizations have come into being, with support from private foundations, specific governments, and UNESCO. It is probably fair to say that the most significant development has been the founding of the Endangered Languages Documentation Project (and other initiatives) at the School of Oriental and African Languages in London, under the direction of Peter Austin, with funds from the Lisbet Rausing Charitable Trust.

But in this paper I am less concerned with history than with the role of endangered languages research in the ethos of fieldwork. On the one hand, I am an active proponent of the need to document and describe endangered languages. But on the other hand, I do not believe that one should take on field research for this reason alone. Coherent fieldwork calls, again, for fieldwork that resonates with one's total life experiences, not merely with a particular, externally-pressured objective. What is it that the fieldworker herself is most passionate about in fieldwork? That is what she should undertake to study. 'Salvage operations' per se are not the only nor even the most significant reasons for taking on the responsibility of fieldwork.

6. Field Research Desiderata

So what are the desiderata of fieldwork? Let me suggest just a few, with others to be filled in or substituted, according to one's own life experiences. First, and foremost, is that the fieldworker see her commitment to fieldwork as a human connection between herself and the people with whom she will work. This connection entails friendship, mutual support, and assistance in many ways. Naturally, because there is usually a power-differential in the relationship, favouring the fieldworker, the fieldworker may find herself offering more help than she might have ever anticipated. Let me give some

examples. The fieldworker may be called on to provide medical assistance (and should, therefore, include first-aid and basic medical knowledge in her basic training - as can be found, for example, in the excellent book, **Where There is No Doctor** by David Werner). Or she may be asked to help arrange legal aid (e.g. for landrights), to represent or accompany representatives of the people to local or national government offices/officials, etc. or to bring manufactured articles otherwise unavailable to the community (e.g. fishhooks, ammunition for hunting, bicycles, etc.). The fieldworker unwilling to make this kind of commitment has not recognized the significance of pragmatist, coherent fieldwork. People are not library books to be consulted and then returned to their shelves when the fieldworker is done with her research.

But I also believe that it is desirable to undertake fieldwork for the purpose of advancing linguistic theory. This is certainly the principal scientific motive for fieldwork in my opinion. In my own case, I undertook fieldwork as a linguist to, I thought at the time, understand the nature of the human mind. On the other hand, each person can legitimately be motivated by her own theoretical concerns, or not. Field research could also model what pragmatist science is about, i.e. illustrating the principles of coherence, usefulness, and radical empiricism. This would be a marvellous object lesson, it seems to me, for scientists generally, and linguistic theory in particular.

Finally, let me inject a 'corny' desideratum here: field research should promote love and world peace. There is no better way I am aware of for developing meaningful relationships across cultures than by fieldwork, for showing, promoting, and developing love and respect and passing this on to one's home institution through training and example, as well as to the many people contacted in the course of fieldwork. That is a worthy goal, perhaps the most worthy. It is certainly in the spirit of William James. And directly connected to this and all the other worthy goals and desiderata of field research is the fundamental task of training new generations of fieldworkers to continue on. This really is an area of science in which it makes sense to establish apprenticeships, where future researchers learn by example and doing from their mentors. A fieldworker will, I hope, also learn to love the specific and distrust (though not reject) the general, to think relative and to doubt absolutes.

I ascribe truth to none of the above. But I hope it is all useful. Peace.

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Notes

[1] I would like to thank Caleb Everett, Joan Baart, Sally Thomason, Bob VanValin, and David Weber for comments on earlier versions of this paper.

[2] One should mention the very important role of the Summer Institute of Linguistics, through its leading linguists, e.g. Kenneth Pike and Eugene Nida. SIL has probably documented and described more endangered languages and fieldwork situations than any other organization in history. I do not mention it in the text, however, because its role and activities have been parallel to and outside the mainstream of scientific concerns, due to its connection with the missionary organization, Wycliffe Bible Translators. Nevertheless, its role should not be overlooked.

[3] ***k*** is somewhat problematic. It seems to be a phoneme, but in most of its appearances it can be analyzed as a portmanteau realization of /h/ + /i/ or /h/ +/u/. See Everett (1979) for details.

[4] The length of the notes is determined by the relative lengths of the syllables, as is the height of the notes. The wedges under the lines indicate stress. The values are CVV = whole note; GVV = dotted half; VV = half; CV = dotted quarter; GV = quarter.