Due to lack of space I am unable to repeat the arguments of Everett (2005b) for the Pirahã IEP here, so the reader is referred to that paper for full argumentation, based on the empirical points mentioned earlier, as well as (among other things) the culturally important notion of *xibipíío* 'experiential liminality', discussed in Everett (2008). This word expresses liminality as an important cultural concept and is used to describe things that go in and out of vision or hearing, from the flickering of a match to the disappearance or appearance of a canoe around a bend in the river.

In many works I have argued that Pirahã lacks recursion. I will, rightly or wrongly, assume this analysis here. The evidence for this analysis, given in Everett (2012b), is as follows:

1. The lack of recursion correctly predicts that factive and epistemic verbs will be absent (though there is a – cross-linguistically common – use of the verb 'to see' for 'to know'). This prediction is made because if Pirahã lacks recursion, then there is no way to express factive verbs as independent verbs, since these would require a complement clause, requiring embedding and thus, *ceteris paribus*, a recursive rule in Pirahã syntax. Pirahã expresses such notions via verbal suffixes, consistent with the "no recursion" hypothesis, not with complement clauses.
2. Pirahã has no marker of subordination. This is also predicted by my hypothesis, because if Pirahã lacks recursion, there is no subordination to mark.
3. Pirahã has no coordinating disjunctive particles (e.g. 'or'). The absence of explicit markers of disjunction is predicted by my hypothesis, since disjunction entails recursion.
4. Pirahã has no coordinating conjunctive particles (e.g. 'and'). There is only a more general particle, *píaii*, which may appear preverbally or sentence-finally and which means 'is thus/simultaneous' (vague meaning), which never works like a proper conjunction, but only supplies the information that these two things were simultaneous (it is related to *pixai* 'now'). Again, this is predicted by my analysis, since coordination also entails recursion.
5. Pirahã has no syntactic complement clauses. If Pirahã has recursion, where is the unambiguous data? I have claimed that it lacks embedded clauses. Others claim that it has them (Nevins, Pesetsky, and Rodrigues, 2009), but they only show that quotatives *could* be embedding. No work has ever shown that there are multiple levels of embedding, which certainly would be expected if Pirahã has recursion (modulo Chomsky's recent (2014) ancillary constraint on Merge, discussed earlier).
6. Pirahã does not allow recursive possession. The point about Pirahã possessives that I have made is not simply that Pirahã lacks prenominal possessor recursion, but that it lacks recursion of possessors *anywhere* in the noun phrase. Nevins, Pesetsky, and Rodrigues (2009) might be correct to suggest that German, like Pirahã, lacks prenominal possessor recursion. But German *does* have post-nominal possessor recursion, while Pirahã has *none*. This is predicted by my analysis.
7. Pirahã prohibits multiple modifications in the same phrase. As I have discussed above and in Everett (2008) and (2009b), there can at most be one modifier per word. You cannot say in Pirahã 'many big dirty Brazil nuts'. You'd need to say 'There are big Brazil nuts. There are many. They are dirty.' This paratactic strategy is predicted by my analysis since the use of multiple adjectives, as in English, entails recursion, but the paratactic strategy does not.

8. Pirahã semantics shows no scope from one clause into another, e.g. no "Neg-raising." Pirahã lacks examples such as 'John does not believe you left' (where 'not' can negate 'believe' or 'left', as in 'It is not the case that John believes that you left' vs. 'It is the case that John believes that you did not leave'). In this example 'not' can take scope over 'believe' or 'left'. That is not possible without recursion, so my analysis predicts the absence of such scope relations. This is also predicted, correctly, to be impossible in Pirahã under my account, since it would entail recursion.
9. Pirahã shows no long-distance dependencies except between independent sentences, i.e. discourse. The kinds of examples that are standardly adduced for long-distance dependencies include:

   'Who do you think John believes __ (that Bill saw__)?'
   'Ann, I think he told me he tried to like ___'

We have stated the IEP and rehearsed the evidence against syntactic recursion in Pirahã. It remains now to show how these fit together causally. Pirahã, like many other languages (see, inter alia, Aikhenvald, 2002; Faller, 2007), encodes evidential markers in its verbal morphology as affixes: *-híai* 'hearsay'; *-sibiga* 'deduction'; *-ha* 'complete certainty'; and *-0* (zero affix) 'assumption of direct knowledge'. The Pirahã IEP, in conjunction with its requirement that evidence be provided for all assertions, produces a narrow domain in which assertions and their constituents need to be warranted. Recalling the Potential Focus Domain developed by Van Valin (2005: 70ff.), I label this domain in Pirahã (and presumably some version of this will exist in all languages, at least those with evidentiality morphology) the Potential Evidentiality Domain (PED), i.e. the range of structures where the actual evidentiality domain could in principle fall. The actual domain of evidentiality in a given utterance will be as follows:

> *Evidentiality Domain*: The syntactic domain in a sentence that expresses the evidentiality component of the pragmatically structured proposition.

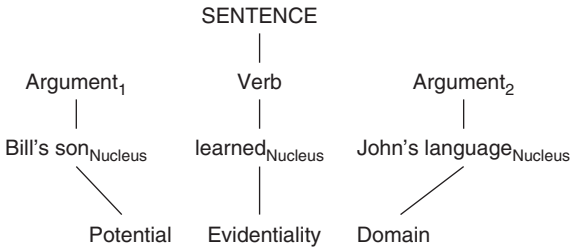
The PED in Pirahã is limited to the lexical frame of the verb, i.e. the verb and its arguments (more technically, the phrasal nuclei of the predicate and its arguments in Van Valin's Role and Reference Grammar (RRG) terminology[4]). Let's assume that the IEP is one of the reasons that Pirahã has evidentiality markers and that it dramatically strengthens their effect by narrowing their scope to the PED just mentioned.

The PED then rules out syntactic recursion in Pirahã. As stated, the PED clearly depends on the main verb as the core of the speech act. The PED will include only nuclei (semantic-syntactic heads, not heads in the X-bar sense) directly licensed by the predicate (its semantic frame). No nuclei are allowed outside the PED of a containing sentence.

By virtue of the PED there are no embedded predicates and no embedded possessives. There are only arguments licensed by the main predicate. For example, in a noun phrase like *John's house*, *house* is the nucleus – the semantic core, what this phrase is about. John is the possessor, a type of modifier of the nucleus house: the possessor tells us which house we are talking about. On the other hand, in a larger noun phrase such as *John's brother's house*, *house* and *brother* are each a nucleus of a separate containing phrase. *House* is the nucleus of the phrase *brother's house* and *brother* is the nucleus of the phrase *John's brother*. *John* is not a nucleus of any phrase. This means that *John*, not being the possessor

of an argument of the main verb (it is a nucleus of *John's brother* but *brother* is not a nucleus of the verb), is unwarranted in the PED and the sentence is disallowed. An embedded predicate would contain arguments not licensed by the main predicate. Therefore, there can be no phrases within phrases and no sentences within sentences in Pirahã. There can also be no productive compounding in the morphology. Such apparent compounds as are found are in fact synchronic or diachronic phrases.

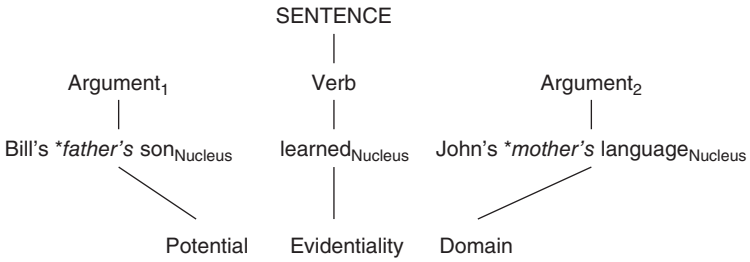
This is exemplified below, in a theory-neutral representation:



This example is allowed because each nucleus is found in the semantic frame of the verb, represented along the lines of the following lexical representation: [BECOME ***know*** (son, language)].

This is a very strict evidentiality requirement. It predicts that the number of arguments in a sentence cannot exceed the number allowed by a standard (e.g. RRG) verbal frame. It rules out all embedding and all syntactic recursion.

The lexical representation of the accomplishment verb *learn* ([BECOME ***know***] indicates the change of state of knowledge) projects three nuclei to the syntax – the verb *learn*, and the nominal nuclei/arguments *son* and *language*. Each of the nominal nuclei is possessed by a non-nuclear nominal. So the requirements of the PED are met. However, in the example below, there are two non-warranted nuclei, i.e. appearing in the PED without being found in the lexical representation:



This sentence would therefore be ungrammatical in Pirahã , though it is fine in English. This analysis then claims that the PED, evidentials, and the lack of recursion are all reflexes of the cultural value IEP in Pirahã grammar.

Although the PED (forced by the IEP) rules out recursion in Pirahã, this analysis does not predict that another language, e.g. Riau, derives the absence of recursion in the same way. Recursion serves several purposes (Everett, 2012a) and thus there are many different reasons why a language might lack recursion. For example, Riau might simply

rank the value of slower information rate above the value of recursive sentences in its language. Many oral traditions use repetition and slower information rate as aids to communication in the noisy environments of human speech.

Let's turn now to the cultural effects on the emergence of Pirahã phonology.

6. Culture and Segmental Phonology

Though I have discussed these data elsewhere (Everett, 1979, 1985, 2008), it is worth reviewing them here to round out our picture of the effects of culture on grammar more generally. As pointed out in Everett (1979, 1982, 1985), Pirahã phonology cannot be fully described or understood without knowledge of how it interacts with culture. There are other examples from Pirahã phonology. Let me present two of the strongest.

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, for example, the appearance of new roles for old constraints (e.g. the 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 is true, then coherent fieldwork (Everett, 2004)

Table 16.1. Pirahã phonemes

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

will evolve from a curiosity or desideratum to an imperative. Is there such a case? Indeed. Consider the following facts about Pirahã phonology, beginning with its phonemes (see Table 16.1).

Pirahã's segmental inventory is one of the smallest in the world (the only two other languages with inventories of this size are Rotokas and Hawaiian, though they lack tones). It is also worth noting that the /s/ is in ( )s because it is not found in women's speech, but only in men's (women use /h/ where men use /s/ and /h/).

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 with 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 and 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):

> *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 this rule is just this: Voiceless consonants are always longer than voiced consonants and there are five syllable weights based partially on this contrast:

> 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 show this, I will just review the simple set of examples below. In these examples tone is independent of stress: ′ = high tone; no mark over vowel = low tone. The stressed syllable is marked by **!**. There are no secondary stresses.

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

Thus, alongside Pirahã's extremely simple segmental phonology, it manifests a 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* shown below, where Hymes (1974) defines a channel as a "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:

Channel	*Functions*
a. Hum speech	Disguise Privacy Intimacy Talk when mouth is full Caregiver–child communication
b. Yell speech	Long distance Rainy days Most frequent use: between huts and across river
c. Musical speech ("big jaw")	New information Spiritual communication Dancing, flirtation Women produce this in language teacher sessions more naturally than men. Women's musical speech shows much greater separation of high and low tones, greater volume.
d. Whistle speech (sour or "pucker" mouth'; same root as 'to kiss' or shape of mouth after eating lemon)	Hunting Men-only One unusual melody used for aggressive play

Example (4) illustrates how prosodic information in Pirahã is exploited to create these channels. The inventory above also partially shows how little the segments contribute to the total set of phonological information in a given Pirahã word. We see that the phrase 'There is a paca there' has a quasi-musical tonal representation (where an acute accent over a vowel represents high tone and no mark over the vowel means that the vowel has low tone), the basis for the channels just summarized.

(4) *káiʔihíʔao* *-ʔaagá* *gáihí*
paca -poss/exist-be there
'There is a paca there.'

All channels must include full prosodic information (stress, tone, length, intonation), though only the consonant and vowel channel ("normal speech") needs to include the vowels and consonants. In the musical form there is a falling tone, followed by a short low, with a preceding break in the whistle (where the glottal stop, ʔ, would have been in *kai*ʔ*ihi*), 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 and offers an abstract context for tone placement, and the overall word is stressed according to syllable weight (Everett, 1988). 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 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:

*tí píai ~ kí píai ~ kí kíai ~ pí píai ~ ʔí píai ~ ʔí /íai ~ tí píai, etc. (*tí tíai, * gí gíai, *bí bíai)* 'me too'
*ʔapapaí ~ kapapaí ~ papapaí ~ ʔaʔaʔaí ~kakakaí (*tapapaí, *tatataí, *bababaí, *gagagaí)* 'head'
ʔísiihoái ~ kísiihoái ~ písiihoái ~ píhiihoái ~kíhiihoái 'liquid fuel'[5]

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 above 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 below is violated:

Constraint on functional load and necessary contrast (Everett, 1985):

a. Greater dependence on the channel ⟶ greater contrast required
b. Less dependence on the channel ⟶ less constrast required

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] and [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, even an understanding of Pirahã's segmental phonology is impossible.

The lessons to be drawn from these examples by the field researcher and theoretical linguist are just these: first, language and culture should be studied together; second, as a modality-dependent channel, phonology may be subject to constraints that are (1) language-specific and (2) grounded not only in the physical properties of the instantiating modality (the phonetics) but also or alternatively in 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 Phonological Form and Logical Form, if we define an interface system as a system setting bounds on interpretability for Computational System of Human Language. Such examples also show how coherent fieldwork can be useful for theory. Thus not only the fieldworker but also the phonologist must engage the language as forming a coherent whole with culture. And this in turn entails more culturally informed fieldwork.

7. Towards Ethnogrammar

This section attempts to develop a methodology for ethnogrammatical studies, building on suggestions by Saville-Troike (1982: 108ff.). The beginning steps for the ethnography

of communication are to (1) identify recurrent events, (2) analyze these events, examining their function, form, and relationships between different constituents, and (3) examine the relationship between these events and other speech events and to the society and culture in which they occur.

For example, one might study the use of whistle speech on the Canary Islands. One variety, Silbo Gomero, is used in and around La Gomera. In relation to (1), each use of whistle speech is thus an event. Some questions that might be asked about these events are: When is it used? Who uses it? What are the constraints on its intelligibility? (For example, can two people understand Silbo under any circumstances or does a topic of conversation need to be established first to provide context?) How many other channels of discourse are there among speakers who use Silbo? Are there contents or types of discourse in which the people prefer to use Silbo? Are there types of contents or discourse genres in which the people prefer not to use Silbo? What are the phonetic details of Silbo and how is it possible (since the language it is based on is not tonal, does it use inherent segmental frequencies – intonation, etc. – as a basis)? How does this whistled language relate to the consonant and vowel channel (i.e. normal speech)?

Beyond these suggestions there are further methodological preliminaries for investigating the culture–language connection. These preliminaries include at least the following:

1. Are there irregularities that have no obvious structural explanation?
2. Are there examples of "free variation," i.e. where there are choices between two structures which are not constrained by the structures or the grammar, so far as can be determined?
3. Are there unusual facts about the cultural events, values, or explanations that involve principles or phenomena that at any level look similar to principles operative in the grammar?

As to the methodology that follows from such questions, Enfield (2002: 14ff.) offers some cogent and very important considerations and suggestions for the study of ethnogrammar. First, he recommends that the fieldworker "examine specific morphosyntactic structures and/or resources and make explicit hypotheses as to their meaning." Second, following development of this and related methodological considerations, he raises the crucial issue of "linkage," namely, how can we establish a causal connection between facts of culture and facts of grammar? I turn to this below. Before doing this, however, I want to point out what seems to be the biggest lacuna in the study of ethnogrammar, whether in the studies in Enfield (2002) or elsewhere. This is the effect of values, especially cultural taboos like the IEP above, in restricting both culture and grammar. That is, previous studies, like those in Enfield (2002), while reasonably focusing on meaning, which is after all a principal contribution of culture (guiding its members in finding meaning in the world), fail to consider cultural prohibitions or injunctions, however deeply or shallowly embedded in the community system of values. The Pirahã example of this section is evidence that such values should also be considered in ethnophonological, as well as ethnosyntactic studies – hence "ethnogrammar." However, before we can draw any conclusions at all about ethnogrammar in a given language, we need to consider the vital issue of Enfield's "linkage," i.e. the establishment of a causal connection between culture and language. That is, how can we convince someone, or at least

effectively argue, that property *p* of culture C causally determines feature *f* of grammar G? According to Clark and Malt (1984), cited by Enfield (2002: 18ff.), there are four prerequisites to establishing linkage between culture and language:

1. Empirical grounding: Are the phenomena clear and well established?
2. Structure independence: Are the cultural and grammatical structures or principles independently needed in the grammar?
3. Theoretical coherence: Does the analysis follow from a clear theory?
4. Avoid circularity.

A circular argument in ethnogrammatical studies would be to claim that a particular linguistic feature is simultaneously determined by an aspect of culture and evidence for that aspect of culture. So, it is circular to claim that a language has evidentials because the culture values empirically based reasoning, and then to further claim that we know that the culture values empirically based reasoning because it has evidentials. The way to avoid this is first to establish, using *non-linguistic evidence*, particular values or meanings in a certain culture such as the IEP. Next, using *noncultural evidence*, establish the meaning and structure of the relevant linguistic examples (examples would include standard arguments for constituency, displaced constituents, and so on). Finally, show how linking the two provides a conceptually and empirically (in terms of predictions where possible, or explaining independent domains such as historical change) superior account of the facts than leaving them unconnected.

Ethnogrammatical studies thus range from showing that a language has, say, honorifics because of a severe social structure, or a particular set of kinship terms because of its restrictions on marriage, to (what most researchers have overlooked), the kinds of global, architectonic constraints on grammar from, for example, *taboos* like the IEP.

Another issue is whether the researcher can get the semantics right. The so-called "translation fallacy" is well known, but field linguists in particular must be ever vigilant not to be confused by it, to be on guard against the mistake of concluding that language X shares a category with language Y if the categories overlap in reference. As Gordon (2004) argues, much of Pirahã is largely incommensurate with English and so translation is simply a poor approximation of Pirahã intentions and meaning.

8. Conclusion: Culture, Community, and Communication

Cultural learning is discussed in a multitude of studies (e.g. the entire field of cultural psychology, neuroanthropology). But perhaps the two most important mechanisms are (1) what Everett (2012a), going back to Aristotle, refers to as "the social instinct," and (2) general cognition. Another way of referring to the social instinct is as the "interactional instinct" (Lee, Mikeseli, Joaquin, Mates, and Schumann, 2009; Levinson, 2006). By general cognition I refer in particular to the general ability of the human brain to generalize and recognize patterns.

What might be the evolutionary utility of a social or interactional instinct? The social instinct (however it is ultimately characterized) is the presumably unlearned need for

humans to communicate, to interact with one another. Levinson (2006) makes a convincing case for the independence of interaction from language. The need to interact and the ability to interact are prior to language. The appeal of such an instinct is that it is a simple reflex that requires no learning curve (such as is required for the so-called "language instinct," for example). The instinct is not the final product, of course, but it triggers movement in that direction and is arguably what distinguishes humans from other species that lack this social or interactional instinct. The social instinct is the "initiator" in that it provides the problem while language and society provide the solutions. In this sense, language is the principal tool for building social cohesion through interaction.

Many researchers (e.g. Tomasello, 2001) have made a case for qualitative differences between the interactions and social organizations of humans vs. other species. Clearly, though, humans have bigger brains, an interactive instinct, and a transmitted linguistic history (passing along to subsequent generations both the content and the form, i.e. grammar). The idea of general learning (including such things as memory, motivation, emotion, heuristics, categorization, perception, and reasoning), heavily dependent as this is on the keen human ability to make tacit statistical generalizations, as a key to language differences between humans and other creatures, has been defended many times in the literature. Kurzweil (2012) makes this case to a popular audience. But many researchers in Bayesian approaches to learning (e.g. Goldsmith, 2007; Pearl, 2013; Perfors, Tenenbaum, Gibson, and Regier, 2012; MacWhinney, 2005; and many others) present much more technical and nuanced evidence, backed by extensive experimentation. Such claims in fact go far back, with a form of the argument to be found in Benedict (1934), at least implicitly.

The effects of culture on the lexicon take on a greater significance these days when many linguists deny a strong bifurcation between syntax or grammar and the lexicon. In fact, if constructions (see Goldberg, 1995) are lexical items that produce families of syntactic constructions, then the culture can affect the syntax of constructions just as all linguists now agree it can affect the lexical items of any language. This chapter is not intended as a list of noncontroversial results. It does, however, provide evidence that culture profoundly affects grammar and that understanding and studying this relationship between culture and grammar is not beyond our grasp.

Finally, the considerations above lead to the proposal of a simple formula for the development of grammar in our species:

Cognition, Culture, and Communication → Grammar

In other words, given human cognitive abilities, cultural/community shared experiences and the interactional instinct, grammar emerges as a solution to the communication, as an outgrowth of interaction and the building of culture and community.

Acknowledgments

I would like to thank Nick Enfield, Caleb Everett, Emily McEwan-Fujita, Larry Hyman, Christina Behme, Sascha Griffiths, Gareth O'Neill, and Robert Van Valin for comments on this chapter. I would especially like to thank Brian MacWhinney and

William O'Grady for numerous insightful criticisms and suggestions over the entire chapter. Perversely, I have not followed all the advice I was given.

NOTES

1 And also, "Language is primarily a cultural or social product and must be understood as such. Its regularity and formal development rest on considerations of a biological and psychological nature, to be sure. But this regularity and our underlying unconsciousness of its typical forms do not make of linguistics a mere adjunct to either biology or psychology" (Sapir, 1929: 214).
2 One vital area of language, cognition, and culture that I will omit from my discussion here is the work on gesture, as represented in McNeill (2012) and many other works. See Everett (in preparation b) for more detailed discussion of gesture.
3 Both of these examples were collected by Steven Sheldon, a missionary among the Pirahãs, in the mid-1970s. Sheldon, who speaks Pirahã fluently, did the initial transcriptions and most of the translations.
4 I use Role and Reference Grammar here because to my mind it most effectively blends structural and functional-semantic principles into a theory of grammar. Nothing crucial hangs on this, however, and other theories might be compatible with the analysis offered here.
5 Alternations with /t/s or involving different values for [continuant] or [voicing] are unattested.

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Part IV Language Learning

17 Learnability

ALEXANDER CLARK

1. Introduction

One of the paradoxes of modern linguistics is that though one of Chomsky's lasting contributions to linguistics is to locate the central empirical problem of linguistics as an acquisition problem, the attention of the field as a whole has been focused on the goals of finding descriptively adequate grammars for natural languages. To a certain extent this has reflected the relative levels of development of the underlying mathematical theory. While the theory of linguistic representation or description has been very well developed, starting with Chomsky's own seminal contributions in the 1950s, the theory of grammatical inference, the corresponding formal discipline that concerns itself with the problems of learning such representations, is much less mature. Indeed the history of grammatical inference has largely been a history of negative results, to such an extent that the only thing most linguists know about grammatical inference is the negative result of Gold (1967), which seems to indicate that the task is impossible. Thus reviews of learnability in linguistics (for example, Niyogi, 2006; Yang, 2008; Clark and Lappin, 2012; Heinz, in press) tend to focus on negative results, with, caricaturing somewhat, the nativists stressing the negative results and the researchers of a more empiricist persuasion downplaying them. Such negative results can in principle serve to rule out purported solutions to the language acquisition problem, and have in the past been taken to rule out a naive "blank slate" empiricism.

More interesting of course are positive results, since we are interested in explaining a phenomenon, language acquisition, which certainly does occur; and these positive results have been, to say the least, in very short supply. Again, Gold's early paper has proved enormously influential: the very limited range of positive results he considered in that paper have been widely, though incorrectly, taken to exhaust the possibilities for language acquisition. In particular his elementary proof that any finite class of languages can be learned from positive data alone has been taken as an important point of reference, and a justification for theories of grammar that take the class of possible human grammars to be finite.

The absence of other positive results has been in its own way more important than the negative results, motivating the strong linguistic nativism advocated by Chomsky (1981) and Pinker (1994), and causing problems for those who find these claims implausible.

The Handbook of Language Emergence, First Edition. Edited by Brian MacWhinney and William O'Grady.

While one can convincingly argue that the negative results have been overstated, that is unsatisfying: merely saying that something is not impossible is not an adequate scientific hypothesis. What is needed is a range of alternative positive answers: here is how language acquisition might take place. Once there are a variety of options on the table, one can explore them using the standard methodologies of science.

The seminal work of Dana Angluin on learning regular languages changed the situation enormously (Angluin, 1982, 1987). Her work showed for the first time that it is possible to learn languages just from a finite amount of information about the strings that are or are not in the language. The algorithms she defined are "inferential" – they deduce the structure of the language from examples, given certain assumptions. Unfortunately, her results were limited to the acquisition of regular grammars (or finite state automata), which, though infinite, lack many of the structural properties which are characteristic of natural language syntax. As a result, with a few exceptions (Pilato and Berwick, 1985), they have not been widely influential in linguistics, though they have been extensively studied in the technical literature on grammatical inference.

In recent years, this work has been greatly extended to encompass the acquisition of the sorts of grammars that are needed to account for natural language syntax: to context-free and mildly context-sensitive grammars. This newly developed theory, which we call "distributional learning," takes the old ideas of distributional learning of the American structuralists (Harris, 1951), and builds on them a rigorous mathematical theory of learnability. While this is still quite new, and thus incomplete, already we have efficient learnability results for classes of languages that plausibly include all natural languages. Of course, since the theory is incomplete, the set of positive results we currently have is still only a partial picture. More such results are on the way. The negative results therefore do come into play; they have an important role: to curb excessive optimism as these results tell us that certain types of positive result are impossible.

Here, we do not make the empirical claim that any of the algorithms is how language acquisition proceeds, but we do claim that from combinations of the results we already have, which we sketch below, there are learning algorithms that fit the gross facts of language acquisition.

In this chapter, we discuss the theory of learnability or grammatical inference, from a positive perspective. We start (in Section 2) by looking at the methodological issues involved in applying the tools of mathematical analysis to the empirical problem of language acquisition, and the various assumptions that we make, and by discussing the problems of grammatical inference. Then in Section 3 we review, non-technically, some recent developments in the field based on the classic ideas of distributional learning.

2. Methodology

We start by articulating some foundational assumptions that make it possible to discuss this from a theoretical perspective. First, we assume that the brain/mind can be fruitfully considered to be a computational system; without this assumption no mathematical analysis can start, and we will approach this formally using the machinery of mathematics and theoretical computer science. So rather than considering computer programs that we can run experimentally on natural language corpora, we study algorithms that are guaranteed to learn in some precise sense. The mathematical proofs guarantee the

correctness of the algorithms in a way that even the most thorough empirical experiments cannot. Moreover, they can give us direct insight into the classes of languages that are learnable under various paradigms. Running a computer program on a CHILDES corpus (MacWhinney, 1995), even if it works to some extent, tells us nothing about the properties of the language that make it work, nor about whether it will work on other corpora of child-directed speech from other languages not in the CHILDES collection. As Keller and Asudeh (2002) say: "A generative grammar is empirically inadequate (and some would say theoretically uninteresting) unless it is provably learnable." The real story of language acquisition – a decade-long interaction between a rapidly developing child and a community of adults – is far beyond what we can capture in a tractable mathematical model. Nevertheless, we can define simple models of learning that give insight into the basic possibilities of learning grammars from examples.

2.1 Outputs

We are interested in modeling at some level of abstraction the language acquisition process; a first step is to consider the inputs and outputs of the process, to define what the data is that the learner receives, and what sort of output the learner should produce. We start by considering the outputs of the process: what kind of object is the thing that is learned by the language acquisition device (LAD)? What type of object is the grammar or I-language that is produced?

In Chomsky's phrase, language is a system of "discrete infinity": though acoustically it varies continuously, linguistically it consists of discrete words arranged sequentially, and the sentences can be of unbounded length. We take a fairly standard view: we assume we have some internal grammar that generates some unobserved (or "latent") hierarchical structures that are then mapped to phonological and semantic representations. We wish to draw our net as widely as possible. If we commit to one particular representation, such as the version of the Standard Theory studied in Wexler and Culicover (1980), and that representation is abandoned for empirical or other reasons (as the Standard Theory was), then the analysis becomes outmoded. Recently a broad consensus has developed in the mathematical linguistics community (Stabler, 2011) on the appropriate class of grammars. It transpires that formalisms that appear very different superficially are in fact mathematically equivalent in a strong sense. This equivalence even spans one of the most fundamental divides in syntactic theories: between theories that use movement and those that do not. Most current proposals are equivalent to some subclass of the class of multiple context-free grammars (Seki, Matsumura, Fujii, and Kasami, 1991): this includes tree adjoining grammars of various types, minimalist grammars, and so on (Joshi, Vijay-Shanker, and Weir, 1991; Borsley, 1996; Stabler, 1997). Thus we can consider learning approaches that output grammars of this type, and be confident that they are adequate.[1] As Stabler (2013) puts it, "This consensus is stable and rather well understood."

2.2 Inputs

We now consider the inputs to the learning algorithm. Classically, the input to the learner has been considered to be only strings: sequences of sounds that the child passively

observes (Gold, 1967; Chomsky, 1962). We take these to be sequences of categorized speech sounds, as phonemes or phones, glossing over the problems of low-level acoustic processing, phonology and such like, all of which raise interesting issues.

The normal situation of the child is much richer in a number of respects; the child can interact with the parent/caregiver in a number of ways, has information about the situational context of the utterance, and so on. In particular the child can observe what objects are present, and what events are happening as utterances are being made, together with other indicators of salience such as which objects are held in the parent's hand, the gaze of the parent, and other factors. Moreover, the child is not entirely passive: the child can act by moving, pointing or looking in particular directions, picking up objects in response to requests and questions, and so on; additionally, the child can generate utterances of its own, well-formed or ill-formed, and these actions will have direct or indirect effects on what happens (Tomasello, 2003; E. Clark, 2009).

Of course, language acquisition is not just a case of learning which sentences are grammatical and which not: The child also learns the meanings and communicative functions of sentences. This can clearly not be learned just from the strings, but requires information about the situational context.

In terms of the architecture of the grammar, then, we have three types of object: we have the surface strings of acoustic symbols, the meanings of the utterances, and the putative hierarchical structures that underlie these pairings. We assume that in all plausible models the child has access to the sequence of speech sounds. The next more controversial question is about the degree to which learners have access to the meanings of the utterances they hear. Here opinions are divided.

The most optimistic proposal is that the learner has access to the complete semantics of the utterance: the learner is thus presented with sound/meaning pairs, where the meaning is taken to be some hierarchically structured semantic representation, a well-formed formula in some innate language of thought (Wexler and Culicover, 1980; Pinker, 1995; Siskind, 2000; Kwiatkowski, Zettlemoyer, Goldwater, and Steedman, 2010). The assumption is then that the child is able to infer the meaning of the utterance by combining some ability to reason about the intentions of the speaker with information about the various salient events happening, and with some partial information about the syntactic structure of the utterance. Certainly, in the later stages of language acquisition this may be possible; but in the very early phases of language acquisition, when the child does not know what the words are or what the syntactic structure is, it seems implausible.[2]

Finally, we consider the question of whether children have direct information about the hierarchical structures. Clearly they do not: some models assume the trees as input, but this is only in the context of the existence of some other learning component that can infer the trees (Wexler and Culicover, 1980); from a learnability point of view this begs the question. We therefore must assume that the child does not have access to these structures. Any structures that the learner uses must be constructed by the learner during the course of acquisition. Thus our learning model must account for these inside the theory, and not posit them as inputs.

Clearly, having one model that is intended to cover the entire process of language acquisition from the earliest stages to adult grammar is a gross simplification; so perhaps the models considered here are best thought of as models of the earliest period of language acquisition, when the child has learned the acoustic categories of the language

and nothing else. Once the learner has some partial knowledge of the syntactic structure of the language, of the lexical categories and of the meanings of frequent words, the learner can leverage its existing knowledge in a number of ways. The problem is at its most acute when the child is youngest and so that is where we should focus our analysis.

2.3 The problems

Grammatical inference is hard but not impossible; it is important to understand the various difficulties in order to see how they can be overcome. Classically, the difficulties arise from two distinct yet interacting factors. First there are problems to do with whether there is enough information in the input data for the learner to succeed; we will call these, rather loosely, information-theoretic problems. The second class of problems concern the computational issue of using this information to construct a hypothesis; it may be that, though there is enough information available in some mathematical sense, there are computational problems that cannot be solved efficiently. We can call these computational complexity problems. The information-theoretic problem has been studied extensively for 50 years, and the computational complexity problems for nearly as long (Gold, 1978), and they are now well understood; recently a third problem has come into sharper focus, which we call the strong learning problem, which interacts with our assumptions about the semantic information available in the input, which we discuss later.

Attention in linguistics has focused largely on the first of these, the information-theoretic problems, which has been considered to be a "logical problem" (Baker and McCarthy, 1981). There are a number of reasons for this focus: the study of grammatical inference predates the development of the theory of computational complexity, and, furthermore, these problems are to some extent more fundamental than the complexity problems, since if the information is inadequate then the question of the computational complexity of inference cannot even be formulated coherently. The negative results here, such as those of Gold (1967), show that, for any learner, the class of languages learned must be limited in some way. These results are often taken to show that the learner must have constraints on its hypothesis space, on the set of grammars that it might output. However, the arguments here are unsound: they conflate the hypothesis space of the learner with the learnable class of grammars, which can and sometimes must be very different. The hypothesis space of the learner must contain, but in general is not equal to, the class of grammars that can be learned. Though the arguments from Gold's theorem show that the latter must be limited, they say nothing about the former, and it is the former – the hypothesis space of the learner – that is the object we are primarily interested in, in the case of language acquisition. See Clark and Lappin (2013) for detailed discussion of this point.

Nonetheless, the Gold analysis put the focus sharply on one particular aspect of the problem: the absence of negative evidence. Gold's results suggested that without explicit negative evidence only very small classes of languages could be learned. In particular the problem of recovering from overly general hypotheses without correction seemed to be impossibly hard. If the learner's hypothesis failed to generate a particular grammatical sentence, then the learner could notice this since he/she would observe one of these sentences and realize the error, but the converse problem of overgeneralization seemed much harder. If the learner has a hypothesis that generates some ungrammatical sentences, then in the Gold model there seemed no way that the child could recover. It