How to Do Linguistics*

by

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* The following is a meta-theoretic exercise; the reader is advised not to expect a how-to guide. The author feels confident all will surely agree that "How to Figure Out How to Do Linguistics," would have been a damn stupid title…

I. Introduction

It is a pity that behaviorism has gotten itself such a bad reputation. Surely, it is in many respects, and on most accounts, wrong; but being wrong – as Chomsky (2000) repeatedly reminds us[1] – is the ordinary course of scientific progress. Chomsky reserves most of his venom for those whose behaviorist predilections survived past the '60s. He says wonderfully friendly things like, "Willard Quine's 'epistemology naturalized' [is] of no known scientific interest." (p.80) But Chomsky also has some choice words for those, such as Richard Popkin, whose skepticism about the whole enterprise of modern linguistics, he considers, "scarcely more than a form a harassment of emerging disciplines." (p.77) Well!

"Behaviorist!" It sounds so ugly… didn't they pass a law?

Perhaps it is time to remind ourselves that, before *Syntactic Structures* (1957) and Chomsky's famous response to B. F. Skinner (1959),[2] before we came to know better, there were good reasons to be a behaviorist. It is worth spending some time consulting Leonard Bloomfield (*Language*, 1933), one of the great linguists of the early twentieth century, on just why it was so important to him to be a behaviorist. Besides, where else will linguistics be as fun to read as is the following passage?

> The Greek generalizations about language were not improved upon until the eighteenth century, when scholars ceased to view language as a direct gift of God, and put forth various theories as to its origin. Language was as invention of ancient heroes, or else the product of a mystical Spirit of the Folk. It began in man's attempts to imitate noises (the "bow-wow" theory), or in his natural sound-producing responses (the "ding-dong" theory), or in violent outcries and exclamations (the "pooh-pooh" theory). (Bloomfield 1933, pp.5-6)

Following an examination of the motivation for, and pitfalls of, doing linguistics as a behaviorist, will be a similar treatment of generative linguistics, and a sketch of what linguistics is or should be like now or in the near future. The goal of this exercise is not to disparage any particular school of thought, or to legislate any particular way of "doing linguistics." On the contrary, for the first half of the 20th century, linguists did important and valuable work, even though, or even *because*, they believed in behaviorism. Still today, there are linguists, whose work is motivated by

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[1] E.g. pp.83-4: "The Cartesians[, observing]… the normal use of language… [and g]iven their metaphysics… postulated a second substance (*res cogitans*, mind)… [The] move was not unreasonable, [it was] normal science; wrong, but that is also the norm.

[2] Review of: B. F. Skinner. (1957) *Verbal Behavior*.

very similar concerns, and is carried out in a similar manner. Similarly, there have been excesses in generative thought; yet no one, I hope, will be tempted into the notion that generative linguistics has been anything less than spectacularly successful!

The ultimate goal of this essay is to demonstrate that the goals of the linguist are quite different from the goals of the philosopher. Of course, the previous sentence is not news – it is obvious. What is less obvious is that, because of these different goals, the philosopher and the linguist have different *epistemic commitments*. The philosopher, in asking, "What is the nature of human language?"[3] and therefore, "How should linguistics be done?" is committed to getting the right answer, especially to the *first* half of the question, from which the second half should follow more or less straightforwardly. This accomplished, the philosopher's task is complete, and (s)he may turn to other tasks... such as defending the theory from naysayers.

The linguist, in asking the same thing, is committed to getting the best available answer, especially to the second half of the question. This completed, the linguist's work has not yet begun. Since highly intelligent people have spent lifetimes pursuing such questions, it is necessarily the case that linguists cannot (and should not) commit the kind of energy to them that philosophers do. The problems arise not so much with philosophers doing philosophy, or linguists doing linguistics, as when linguists ask philosophical questions, and the answers are interpreted as philosophy. They are not: they are methodology.[4]

II. How to Do Behaviorist Linguistics

Surprising as it may seem, linguists did not decide to be behaviorists simply because philosophers told them to. Nor did they avoid mentalistic talk simply because reputable psychologists forbade it. They had their own reasons, (practical) linguistic reasons – and their reasoning had more than a little merit:

> In 1914 I based [my] exposition on the [(introspectionist)] psychologic system of Wilhelm Wundt, which was then widely accepted. ... In the present book I have tried to avoid such dependence... The mechanists demand that the

[3] Or more specifically in the present context, "What is the relationship between psychology and linguistics?"

[4] On some days I might wish to argue that the philosophically inclined linguist has a greater right to his/her conclusion, but not here, and not today. However, if any of the following should seem to betray such a bias, it probably does.

> facts be presented without any assumption of ... auxiliary [(i.e. mentalistic)] factors. I have tried to meet this demand *not merely* because I believe that mechanism is the necessary form of scientific discourse, but also because an *exposition which stands on its own feet is more solid*. (Bloomfield 1933, Preface pp. vii-viii – italics added)

In other words, Bloomfield sides with the mechanists, not simply because he thinks that they are right, but because, in working as they demand, he is required to remain faithful to the evidence. Call it a hedge against being wrong: if my conception of psychology is such that studying language will only improve my theory of the psychology of language, then, if it turns out that my conception of psychology was incorrect, I lose my entire theory of language. This fear was well founded; modern books on linguistics very rarely even mention behaviorism, and when they do, it is to refute the entire enterprise – specific behaviorist theories do not merit even this honor.

Bloomfield was well aware of this risk:

> In 1880 there appeared *Principles of Linguistic History* by Hermann Paul... Paul's *Principles* suffers from faults that seem obvious today... One of these faults is Paul's neglect of descriptive study. He admitted that descriptions of languages was necessary, but confined his actual discussion to matters of linguistic change. ... The other great weakness of Paul's *Principles* is his insistence upon "psychological" interpretation. He accompanies his statements about language with a paraphrase in terms of [supposed] mental processes... [which] add nothing to the discussion, only obscure it. ... [Lacking] knowledge of foreign types of grammatical structure... [they believed Indo-European] features to be universal, [and] resorted, whenever they dealt with fundamentals, to philosophical and psychological pseudo-explanations. (Bloomfield, pp.16-17)

Nevertheless, Bloomfield had great respect for Paul's work, writing, "students of a more recent generation are neglecting it, to their disadvantage." (p.16) But he was clearly annoyed by what he saw as a pointless waste of effort. Therefore: it is much better to have a conception of psychology, such that even if that conception turns out to be wrong, having assumed it while doing linguistic work, will not vitiate the entire enterprise. In addition, his experience as a field linguist had led him to the conclusion that, "[I]n the stress of recording utterly strange forms of speech one soon learn[s] that philosophical prepossessions [are] only a hindrance." (p.19) So aside from being wrong, unclear, and biased, mentalism is nothing but a burden to the linguist trying to do linguistics.

> The findings of the linguist, who studies the speech-signal, will be all the more valuable to the psychologist if they are not distorted by any prepossessions about psychology. We have seen that many of the older linguists ignored this;

> they vitiated or skimped their reports by trying to state everything in terms of some psychological theory. (Bloomfield, p.32)

Unfortunately, Bloomfield fails to impose a similar restriction on psychologists. That is, while he points out that, "psychologists often go astray for want of linguistic knowledge," (p.34) he misses the point that adopting the behaviorist or mechanist view that learning involves the association of specific stimuli with specific responses does not absolve the psychologist (or the psychologically inclined linguist) from proving that this type of learning will actually suffice.

As if to prove this point to future generations, he describes the child's task in learning to speak.[5] It is instructive to see just how far astray the wrong psychological theory can lead.

III. How Not to Study Language Acquisition. (Behaviorism)

Bloomfield introduces the problem of language acquisition as follows:

> Every child that is born into a [speech community] acquires... habits of speech and response in the first years of life. This is doubtless the greatest intellectual feat any one of us is ever required to perform. Exactly how children learn to speak is not known; the process seems to be something like this: (p.29)

These three sentences do not sound all that controversial, but they contain the roots of all the mistakes that follow. Indeed any modern text on language acquisition might start with three very similar sentences. Replace "habits of speech and response" with "a native language," and the first sentence might have been written by Chomsky. But note that this would not be an inconsequential change; the phrase "habits of speech and response" betrays an *a priori* commitment to behaviorist psychology. The conclusion that succeeding in acquiring a language is, "the greatest intellectual feat any one... perform[s]," is a direct consequence of this commitment. It is of course a strange concept that something borne entirely of stimulus and response should be "intellectual," but, since for Bloomfield all human action is properly analyzed in these terms, "intellectual" must be seen as shorthand for any (organized) product of

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[5] One could just as well concentrate on Bloomfield's quaint tale of Jack, Jill, and the apple, presented in terms of stimulus and response (pp.22-26). But while the account is charmingly anachronistic (not to mention sexist), and has the advantage of being thoroughly ludicrous, the developmental story has an advantage in that there is a great deal of experimental evidence refuting Bloomfield's story.

the nervous system – including tying one's shoes, riding a bicycle, or presumably breathing.

Any other interpretation of "intellectual" leads to conclusions that are still more ludicrous. If the "intellectual" activity of the child is literally like that of Bloomfield himself in learning a language, then we are led to the conclusion that any child of two is a genius on the scale of the greatest minds of her generation. While it is certainly not *a priori* impossible that we are all born geniuses and that some of us get dumber quicker, it is quite implausible. To begin with, two to four-year-olds really don't seem all that smart at anything *other than* learning language; worse, there are highly intelligent adults, geniuses in their chosen professions, who just can't seem to learn a second language, even though they learned the first one just fine.

The most misleading of the three sentences quoted above is the third. It is not that this sentence is false – very little *was* known about language acquisition at the time, and really, the main difference today is that we know a whole lot more about just what it is that we don't know. The seminal work in language acquisition, *Le langage et la pensée chez l'enfant* (1926) by Jean Piaget had only just been published. So it is not surprising that the course of language acquisition would turn out to be quite different than it "seemed" to Bloomfield at the time. However the key word in this sentence is "seem." It implies that children have been observed learning language, and that the process seemed to the observer to go a certain way. This is not the case – Bloomfield's account is in actuality a logical deduction: given specific prior psychological convictions, this is how acquisition *must* progress.[6] The difference is crucial, and, as it turns out, disastrous:

> (1) Under various stimuli the child utters and repeats vocal sounds. This seems to be an inherited trait. Suppose he makes a noise… *da*… The sound vibrations strike the child's ear drums while he keeps repeating the movements. This results in a habit: whenever a similar sound strikes his ear, he is likely to make the same movements, repeating the sound *da*. This babbling trains him to reproduce vocal sounds which strike his ear. (Bloomfield, pp.29-30)

Not too bad yet. While it is unclear that children need any outside stimuli to begin babbling – deaf children babble, although their babbling is "later and simpler" than that of hearing children (Pinker 1994, p.266) – it

[6] This statement may be a little strong, clearly babies *do* babble, and Bloomfield knew about it, however he proposes other stages for which there is no evidence, as well as denying some for which the evidence is overwhelming – i.e. that learning one's native language is at some point "complete" and that all subsequent learning is lexical.

is true that they need to babble in order to train their vocal apparatus.[7] Since Bloomfield believed that "*thinking* [is] *talking to oneself*," it is not surprising that hearing/ listening/ understanding should also be "talking to oneself" – "As children, we talk to ourselves aloud, but, under the correction of our elders, we soon learn to suppress the sound-producing movements and replace them with very slight inaudible ones." (Bloomfield 1933, p.28 italics in the original) Leaving aside the implausibility of this account, it leaves unexplained why there is no stage of development during which infants go about repeating random syllables that they hear – apparently babies learn to suppress them *in advance* and in the absence of "the correction of [their] elders."

> (2) Some person... utters in the child's presence a sound which resembles one of the child's babbling syllables... *doll*. When these sounds strike the child's ear, his habit (1) comes into play and he utters... *da*. We say that he... "imitate[s]." Grown-ups seem to have observed this everywhere, for every language seems to contain... words like *mama*, *dada*: doubtless these got their vogue because children easily learn to repeat them. (Bloomfield, p.30)

Doubtless. Or maybe parents like to repeat the funny little words their children produce; to this day, my parents like to recall that my first word was *buppon* ("button"). It is interesting that Bloomfield does not even consider that the child might *actually be* imitating. Of course, according to his *psychological* theory there was no coherent interpretation of the word "imitate."

> (3) The mother, of course, uses the words when the appropriate stimulus is present. She says *doll* when she is actually showing ... [a] doll. The sight... of the doll, and the hearing and saying of the word *doll* (that is *da*) occur repeatedly together, until the child forms a new habit: the sight... of the doll suffice[s] to make him say *da*. He has now the use of a word. (Bloomfield, p.30)

Of course. Eminently reasonable, but false. While it is difficult to imagine how children can learn without this kind of association, and while it does seem likely that children do learn *some* words through this type of association,[8] most language learning can not proceed in this manner.

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[7] "[In] babies with breathing abnormalities... pediatricians... surgically [open] a hole in [the] trachea below the larynx. The infants are then unable [to babble]. When the normal airway is restored... those infants are seriously retarded in speech development, though [not permanently]" (Pinker 1994, pp. 265-266)

[8] To abstracting away from the behaviorist terminology of Bloomfield's description: The child learns to associate a piece of language with some set of objects in the world; whether this is done with the aid of mental representations is irrelevant for present purposes. Note, however, that this type of associationistic learning only works well with words for things: I am told that, after having spent a day with my grandfather's housekeeper, I came back under the impression that "bello" means "truck." Being

Studies on child-directed speech have shown that the input children receive is not nearly unambiguous enough to allow this type of learning. **find references**. Not only do caregivers use a lot of connected speech,[9] they frequently talk about things which are not present. "Daddy's in the kitchen heating your bottle! Yes! Daddy! Yes he is!"

> (4) The habit of saying *da* at the sight of the doll gives rise to further habits. Suppose, for instance, that day after day the child is given his doll (and says *da, da, da*) immediately after his bath. He has now a habit of saying *da, da* after his bath; that is, if one day his mother forgets... the doll, he may nevertheless cry *da, da*... the mother [interprets this as,] "asking for" or "wanting" ... The child has now embarked upon *abstract* or *displaced* speech: he names a thing even when that thing is not present. (Bloomfield, p.30)

Bloomfield considers this a favorable result, since "an adult's 'asking for' or 'wanting' ... is only a more complicated type of the same situation." (p.30) More complicated, indeed! It is hard to argue against something that is so patently absurd. Imagine that after every one of our weekly business meetings, I say, "Whew, I need a drink!" Then we all go out for a beer. After several months of this, we come out of the meeting, and before I can take a good deep breath, you say to me, "You need a drink, Claude?" It may be (alcohol being the addictive substance that it is) that you have become as enamored of our weekly binges as I am, and are anticipating my invitation. More likely, however, is that you have embarked upon making fun of me, and that the issue of our going for a drink is entirely tangential.

More generally, Bloomfield has entirely missed the difference between association, and representation. The fact that daffodils appear every April, does not cause them to *represent* the month of April. No more in German, where daffodils are called "Osterglocken" – "Easter bells" – do they come to represent Easter, or bells for that matter. In much the same way, the fact that every Friday all across the English speaking world, people say to each other, "T.G.I.F." does not cause this expression to come to represent the end of the workweek. It is and remains a speech act the *expresses* relief, and is simply not the type of thing which can *represent* anything.

Italian and having a certain preconception about what little boys like, she pointed to each passing truck, and exclaimed, "Bello!" ("Beautiful!")

[9] As anyone who has listened to a foreign language being spoken knows, word boundaries are not audible in the speech stream. Bloomfield, obviously, knew this better than most – this is probably the reason he believed that child-directed speech (at least initially) must involve words in isolation. Believing as he did, *his* child directed speech probably did sound like a foreigner trying to get directions: "Mona Lisa... where... is?"

Of course, Bloomfield would dismiss talk of "representation" as mentalistic drivel. But we need not commit ourselves to a mentalist psychology of language for the point to go through. All that is needed, is to demonstrate that some linguistic tokens behave, or are used in behavior, *as if* they represented some object or action, while others do not. Therefore, one may point to a child and say, "(That is) John." Thereafter one may substitute the expression "John" for pointing to John, and say, for instance, "John just moved here from Kansas." Likewise, one may point to John skipping, and say, "That (what John is doing) is skipping." Thereafter, we may say, "Skipping (you know, that thing John was doing) is good exercise." even if John has skipped right out of sight. When we say of a word that it "represents" something, this is the kind of distribution that it exhibits.

Note that, having learned "T.G.I.F." as described above, we may *not* then say anything like, "T.G.I.F. is my favorite time to be at work!" Although we may say, "'T.G.I.F.' is my favorite thing to say at work!" This is the crucial difference between "John" and "T.G.I.F." that the word "representation," well, represents. How to construct a behaviorist theory of learning that correctly distinguishes the difference between expressions that represent things, and expressions that do not, all while learning that expressions that represent nothing may be used to represent themselves, is an exercise that I will leave to the behaviorists.

> (5) The child's speech is perfected by it's results. If he says *da, da* ... his elders... give him his doll. [T]he sight and feel of the doll act as an additional stimulus, and the child repeats and practices... [I]f he says his *da, da* imperfectly... his elders are not stimulated to give him his doll. ... In short, his more perfect attempts as speech are likely to be fortified by repetition, and his failures to be wiped out in confusion [and by tantrums]. This process never stops. At a much later stage, if he says *Daddy bringed it*, he merely gets a disappointing answer such as *No! You must say "Daddy brought it"*; but if he says *Daddy brought it*, he is likely to hear the form over again: *Yes, Daddy brought it* and to get a favorable practical response. (Bloomfield, pp.30-31)

This basically concludes Bloomfield's account of language learning, and while the account started in a way that did not seems all too far off base, it ends in a hopeless disarray of self-contradiction, and a flurry of fitting the evidence to the theory – exactly the type of thing that Bloomfield so despised in Paul's *Principles*. To begin with, if *da* is good enough to produce the doll, it is unclear what stimulation the child has to improve his enunciation. Furthermore, if *da* is not sufficient, what is to lead the child to the conclusion that it is his *pronunciation*, which is at fault? It could just as well be that he used the wrong word – *da* could mean, "Take this and be quite!" Certainly, the child has no reason to believe

(behave) otherwise. In any case, at some point the child's speech will be clear enough to bring about the conventional response – perfection is overkill.

Unfortunately, this is not the worst of it. As anyone who has spent any time with children has experienced, correcting an incorrect form such as "Daddy bringed it" is useless. The likely response to, "No! You must say 'Daddy brought it,'" is "OK, daddy bringed it!" Meanwhile, Bloomfield has contradicted his statement that, "It is not likely that children ever invent a word," (p.30) with his own example! The probability that the child ever heard the word "bringed" is infinitesimal. Of course, Bloomfield probably felt that the bound morpheme /-ed/ would be learned independently of the word "bring" or any other word. This is undoubtedly true, but in no way explains why many children will begin to produce "bringed" at a stage *after* they had already been using "brought" correctly for some time. (See Elman et al. 1996 and references cited therein on the "U-shaped" learning curve)

Bloomfield's most passionate criticism of Paul and his contemporaries was that they took the features of their own languages to be universal, without taking other languages, that might disconfirm their theories, into account. How ironic, then, that Bloomfield should take it as given that 1) people in his own social group correct children's speech consistently and accurately. 2) People in other cultures or social groups commit a similar amount of energy to this task. 3) Children respond favorably to this kind of training. And 4) the evidence provided by the feedback that children get is even in principal, good enough for children to rely on. It turns out that all four assumptions are false.

It turns out that, while we cannot know for sure what kind of feedback people of Bloomfield's social stratum gave children, we do know that the feedback that white middle class parents give their children is neither particularly consistent, nor particularly reliable (Brown & Hanlon 1970, Hirsch-Pasek et al. 1984). Second, the amount of linguistic attention that children receive varies widely from culture to culture – in some cultures (e.g. Inuit), children apparently are not considered worth talking to until they have learned to speak (Crago et al. 1997).[10] Third, as already mentioned above, children are notoriously bad at recognizing what it is that is being corrected (NcNeill 1966). Finally, while a statistical difference has been found between the rate of correction for

[10] I had a friend once, whose first (rather belated) word(s) was something like, "Please mommy, may I go out to play?" Apparently, he had been practicing on his own, unwilling to make a fool of himself by using his language before it was ready!

grammatical, as opposed to ungrammatical child utterances, that difference has only been observed in interactions with 2-year-olds. Even with this group, the rate of correction of ill-formed sentences was only 20.8% as opposed to 12% for well-formed sentences (Hirsh-Pasek et al. 1984).

IIII. How to Do Generative Linguistics.

Bloomfield believed that, "in all sciences like linguistics, which observe a specific type of human activity, the worker must proceed exactly as if he held the materialistic view." That is, "above all, [a linguist] must not select or distort the facts according to his views of what the speakers ought to be saying." (p.38) These words are as true today as they were almost seventy years ago – and they are likely to remain true as long as people study language scientifically. Too many scientists (not just linguists!) have embarrassed themselves by observing what they wanted to see, rather than what they actually did see.[11] But despite this belief, and his commitment to Behaviorism, Bloomfield knew that there would be a next phase in the study of language, "when we have adequate data about many languages, we shall have to return to the problem of general grammar… but this study, when it comes, will not be speculative but inductive." (p.20)

His message in writing those words was clear: the time will come, but 1933 is not that time; when that time comes the method will be scientific, not philosophical. Of course, he may well have thought that it would take more than twenty-four years, and he probably anticipated something rather in the form of *Verbal Behavior* (Skinner 1957) than *Syntactic Structures* (Chomsky 1957). In the meantime, he contended that the following should be regarded as the truth of the matter – shall we say, the *psychological reality*:

> The arrows in our diagram [S⇒R] represent the sequence of events within one person's body – a sequence of events which we think is due to some property of the nervous system. (p.26)… The… *mechanistic* … theory supposes that the variability of human… speech… is due only to the fact that the human body is a very complex system. … [T]he human body is so complex a structure that even a relatively simple change… can set off some very complicated chain of consequences… The part of the human body responsible for this delicate and variable adjustment, is the nervous system. (p.33)

[11] One may recall that in 1989, Fleischmann and Pons called a sensational news conference to announce their success in producing cold fusion. In their haste, they unfortunately failed to notice that their results were not as convincing as originally thought. I hasten to add that there are still some true believers, but then again, (while I truly hope that they are correct) there are also those who truly believe Elvis is alive.

This opinion, to the extent that it says much of anything that could be controversial, was merely wrong, not unscientific. The implication is that there is no more to say; more explicitly:

> [It is an] error [to seek] correlations between anatomically defined parts of the nervous system and socially defined activities... [such as] a "visual word center" which is to control reading and writing: one might as well look for a specific brain-center for telegraphy or automobile-driving or the use of any modern invention. Physiologically, language is not a unit of function. (p.37)

It turns out that there are brain centers for the most surprising things; for example, an fMRI study has demonstrated that bilinguals who learned their second language as adults have two distinct areas in the brain that specialize in the two languages, respectively (Kim, Relkin, Lee, and Hirsch 1997). It may be that, for all his denials, Bloomfield did actually have a brain region specialized for automobile-driving. Or not; in one thing he was surely correct, "language in not a unit of function," it is many. Just as there appear to be separate areas of the brain that specialize in the perception of color, form and motion (Zeki 1992, 1993), there may be separate areas of the brain that specialize in phonological form, the lexicon, syntax, or some other combination of functions.

It may not seem fair to hold linguists of Bloomfield's generation answerable to the discoveries of the late twentieth centuries; and indeed it isn't. And lest we get ahead of ourselves, it is not fair to hold the early theories of *generative* linguistics accountable to these late discoveries either. But by the 1950's there was something new to hold linguistics accountable: the digital computer – or more generally, a non-biological model for computation.[12] John von Neumann compared the newly invented electronic computer to the brain in his opening address to the Hixon Symposium in 1948 (Gardner 1985, p.10). In 1956, at the Symposium on Information Theory at MIT, Allen Newell and Herbert Simon showed the first complete proof of a theorem carried out by a computer (p.28). And at this same symposium, Chomsky presented a paper called, "Three models for the description of language," which he also summarized in Chapters 3, 4 and 5 of *Syntactic Structures*.

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[12] I rely here, for this unfortunately abbreviated and distorted mini-history, on Howard Gardner's *The Mind's New Science* (1985 – Chapters 1 and 2 provide the early history of cognitive science). "The computer serves, in the first place, as an "existence proof": ... There is little doubt that the invention of computers in the 1930s and 1940s...were powerfully liberating to scholars concerned with explaining the human mind." (p.40)

The concerns that Chomsky expresses in his preface and introduction to *Syntactic Structures* are notable both in their similarity and difference from those expressed by Bloomfield a quarter century earlier. Where Bloomfield rejected Wundt's theory in favor of an approach that remained as true as possible to the observed facts, Chomsky railed against, "obscure and intuition-bound notions" since they "can neither lead to absurd conclusions nor provide new and correct ones." (1957, p.5) But note that Chomsky regards the possibility of leading to "absurd conclusions" as favorable – i.e. they make explicit the faults of the theory – whereas Bloomfield felt that to avoid absurdity it was worth it to avoid drawing any conclusions at all that went much beyond the surface generalizations.

However, unlike Bloomfield before in 1933, Chomsky had some new (intellectual) tools at his disposal. These tools – really, a set of ideas and proofs – find their origin in the work of Alan Turing (1936, 1963), and his proof that a machine could be constructed to carry out any explicitly stated computational task, and that in theory all computers are equivalent (though they may differ in execution) to his simple model (see Gardner 1985, p.144).[13] The possibility of constructing precise models of language elevated the inexact "process talk" that Bloomfield (rightly) despised to a science that could be evaluated objectively. One might still disagree about the evidence (e.g. is "What did who see?" a grammatical sentence of English or isn't it?), but a theory would either succeed or fail at predicting that evidence:

> The search for rigorous formulation in linguistics has a more serious motivation than mere concern for logical niceties or the desire to purify well-established methods of linguistic analysis. Precisely constructed models for linguistic structure can play an important role, both negative and positive, in the process of discovery itself. By pushing a precise but inadequate formulation to an unacceptable conclusion, we can often expose the exact source of this inadequacy and, consequently, gain a deeper understanding of the linguistic data. (Chomsky 1957, p.5)

It is clear then, that the reason that generative grammar can be productive despite being, "propped at various points by another and changeable doctrine," (Bloomfield, p.viii) is that it is the "doctrine" itself that is at issue. The advantage of formalization is that competing theories may be evaluated and compared. Of course, in order to realize the benefits of formalization, a certain intellectual rigor is required – as was

[13] One could look back still earlier, to the work of the eccentric Charles Babbage and Lady Lovelace (Gardner 1985, p.142), or forward a few years to the seminal work of von Neumann, but it seems to be generally accepted that Turing deserves most of the credit for getting the ball rolling in the 1930s.

foreseen by Bloomfield when he commented that, "philosophical prepossessions [are] only a hindrance." (p.19) That is, observing data through the lens of a particular theory is only permissible, if one is prepared to allow that data to disprove, or at least point out the faults of, the theory. So while Bloomfield recommended simply eliminating all such prepossessions, Chomsky makes them explicit:

> I think that some of the linguists who have questioned the value of precise and technical development of linguistic theory may have failed to recognize the productive potential in the method of rigorously stating a proposed theory and applying it strictly to linguistic material with no attempt to avoid unacceptable conclusions by *ad hoc* adjustments or loose formulation. (Chomsky 1957, p.5)

Chomsky immediately goes on to demonstrate this approach by rejecting two possible theories of English grammar (a finite state grammar, and a phrase structure grammar), and suggesting a third – transformational grammar, which was soon replaced by the Standard Theory (also called the "Aspects" model – Chomsky 1965). The Standard Theory, of course, did not remain standard for very long; it was soon replaced by the Extended Standard Theory... which also did not remain standard for very long. What *has* remained standard is the method of "rigorously stating a... theory and applying it strictly," with the aim of "pushing [it] to an unacceptable conclusion." None of this is unique to linguistics, nor was it new to the scientific method in the mid-twentieth century, but it was new to linguistics in the mid-twentieth century.

It is perhaps misleading to refer to generative linguistics as "computational," since attempts to date to actually program computers with generative grammar have been less than spectacularly successful. In fact, most speech recognition systems today use a Hidden Markov Model in parsing (Allen 1995, p.612); this model is an extension of the "finite state Markov process" which is the first model rejected by Chomsky in *Syntactic Structures* (p.20). It is ironic that the system declared least adequate is the one that has (so far) proven to work best.

If we take the grammar that the linguist proposes as a model of the speaker/hearer, we are led to the inevitable criticism that the system or model that has been shown to work best should also be the one that is taken as *the* model of the speaker/hearer. However, this criticism is a product of several logical fallacies. While it is clear that human language users are both "speaker" and "hearer," it is unclear that the two functions make use of the same capacities – any more than, for instance, *seeing* and *smelling* do. Note also that generative linguistics' emphasis on formalization requires a formal descriptive model: e.g. mathematics,

formal logic, etc. It does not require that the object of that description be a formal system; that is, we may construct a formal model of a rock rolling down a hill. However, we might also construct a formal model *of another* formal system. Chomsky, for one, has always been extremely vague on which version he takes to be the correct view of linguistics.

> "When we say that Jones has the language L, we now mean that Jones's language faculty is in the state L, which we identify with a generative procedure." (1995 p.15) "The point of occasional forays into formalism below is to clarify just how closely C_{HL} [the computational system] keeps to minimalist conditions." (p.225)

Let us be clear: parsing is not production. Even if human parsers do make use of a Markov process in speech recognition, this does not mean that production does. While it may be very useful in parsing to consider the probability that any particular word will occur next in a particular string (as a Markov process does), it is a strange conception of production that it should be *probabilistic*. In addition, parsing is not comprehension; simply recognizing each of the words in the speech stream is fine for a speech recognition system, but a human hearer/understander must do more. In order to understand an utterance, one must uncover the structure of that utterance. Broca's aphasics, among their many deficits, are thought to have trouble with those aspects of comprehension that require them to recover the syntactic structure of an utterance (reference?).[14]

There is also another line of reasoning: the challenge faced by the human parser is (usually) quite different from that of a machine parser. A machine that parses with 95% accuracy misses one word for every two lines of typewritten text – a major annoyance; even this accuracy is only possible with words that are already known. In most situations, a machine would be useful if it could correctly identify 98-99% of the words it hears.[15] Such a feat only seems within reach because of the enormous and ever

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[14] Therefore, Broca's aphasics generally understand sentences like, "The boy kicked the ball," (after all, if you recognize all the words, what else would it mean?) but they have trouble differentiating between "John kicked Mary," and "Mary kicked John." For this reason, they also do not find fault with sentences like "The ball kicked the boy."

[15] If such a system were incorporated into a dictation machine, the resultant text would still require some editing, but dictated text is likely to require a bit of editing anyway. But how many errors are acceptable in more critical situations, say simultaneous translation of a business transaction (as is predicted by a current television advertisement)? With 98% recognition accuracy, 98% translation accuracy, and 98% production accuracy, the total accuracy is only 94%. Note however that 94% accuracy is on *words*, not *sentences* – this level of accuracy works out to at least one mistake in every two to four sentences. While some mistakes will not make any great difference, some will. None of this takes into account that some translations, while accurate, may nonetheless be inappropriate.

increasing computational capacity of today's computers. It seems extremely unlikely that humans come anywhere close to that kind of accuracy – or need to.[16] It's a good thing too, since we spend so much time communicating in noisy, distracting situations: where several people are speaking simultaneously, where the music is too loud, with howling wind, airplane noise… the list goes on. In fact, as anyone who has spent time doing detailed transcriptions of recorded speech quickly becomes aware, in day-to-day interactions it is important to parse what the speaker *meant* to say, not what they actually *did* say. We barely notice the difference, but imagine the chaos that would ensue if we attempted to give an interpretation to every speech error that we faced; or worse, if we failed to understand any utterance that had a speech error. The fact is that even if we did correctly parse every nuance, this still would not suffice to understand exactly what a speaker meant. Human language is simply not that specific.[17]

But the question remains: are we justified in believing that human language is a computational system? I will return to this question below, but in the present context, since we are considering the genesis of generative linguistics, the question can be posed somewhat more specifically: Does (and did) Chomsky believe that human language is a computational system? Here the answer is an unequivocal "yes." Not only does his most recent work refer to syntax as the "Computational Component" of the language faculty (1995, p.225), but even the earliest descriptions have us think of, "grammar… as a device of some sort for producing the sentences of [a] language." (1957, p.12)

The word "device," above, is ambiguous. It can refer either to a physical object – a machine – or to a process. However, even under the second interpretation, the process must be executed by some physical object (e.g. a person). The ambiguity seems intentional, since what Chomsky describes is a process, but he insists that his theory is to be taken as true, not just of the language, but also of the speaker (Chomsky 2000). Furthermore, the process, as he describes it, is not meant to be taken as *literally* true (see discussion in Devitt, section 4.2), just true of the speaker's competence (more recently, I-language).

What it means to be "true of a speaker's competence" is not well defined, but this is due – at least in part – to the fact that, "we do not

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[16] This is not the received view, however the received view has been wrong before.
[17] See, for instance, the discussion in Fauconnier (1997, pp.59-60) of five different interpretations of "a rabbit" in "Achilles believes he will catch a rabbit." In fact *any* sentence, even with its structure laid bare, will define a *range* of possible interpretations.

know enough about the 'external' systems at the interface [with the computational component] to draw any firm conclusions about the conditions they impose." (Chomsky 1995, p.222) It is interesting to note that all of the interface conditions that Chomsky proposes are *output* conditions – there are no *input* conditions. This perspective makes sense only on the local view; that is, the explanation comes with the caveat, "insofar as the cognitive system of the language faculty is responsible." (p.225) Insofar as the external systems are concerned, "we proceed in the only possible way: by making tentative assumptions... and proceeding from there." (p.223)

The absurdity of taking the theory as a model of performance presumably comes about for roughly the same reason that it seems absurd to talk about the human competence of "walking" in the absence of a destination, a surface to walk on, gravity, and legs. Humans have such a competence – one might even claim that it is innate – but any description of it, even if true, would appear patently absurd. Destinations, like any pragmatic construct, are crucial to any theory of locomotion, yet, clearly, invoking them in a theory of walking would leave pacing unexplained. Surfaces and gravity are also crucial to walking; yet, they are not *part of* our competence to walk, even if they are always involved in the performance of walking. Legs, of course, are in crucial respects, part of our competence for walking, but nonetheless, in the case of a double amputee, we (or at least those who are not philosophers) should wish to say that it is the *legs* that have been removed, not the competence to walk – likewise for someone who has been tied to a chair.

In summary, the progression from structuralist theories of language, which were predicated on behaviorist theories of psychology, to generative theories of language, which are predicated on cognitive theories of psychology, was made possible by the insight that it is possible to make such theories specific, and *explicit* by formalizing them. The progression was *motivated* by the fact that previous theories were inadequate to account for the full range of linguistic data, and their interrelations. The computational component of the language faculty (if it exists) is embedded in other complex systems, which are not as well-defined as the "computational component." Therefore, an account of the language faculty is not – and is not meant to be – a complete account of *language*, and a (computationally instantiated) model of the language faculty cannot, and should not be expected to be a complete or adequate model of (a) language – defined as a social construct. It is important to realize that formalization, for linguistics, is a *tool* for describing linguistic phenomena. In linguistics, as in the physical sciences, formalization is unavoidable, since it provides the only way to make

theories explicit, and to evaluate them systematically. We do not therefore begin by assuming that the formalization is a part of the linguistic phenomenon, although, unlike the physical sciences, it is possible that linguistic phenomena are themselves formal objects. This additional question is, from the perspective of the linguist, largely irrelevant, and usually given scant consideration. It is enough that formalization is the optimal method for linguistic study, and that the form (although not necessarily the formalization) of the theoretical construct is isomorphic to form of the object of study.[18]

V. How Not to Study Language Acquisition. (Generativist)

As earlier with behaviorism, there have been excesses in Generative Linguistics. However, whereas behaviorists were guilty of excesses because they were unwilling – understandably, if wrongly, unwilling – to assign any causal role to the intellectual contents of the mind, generative linguists have sometimes been guilty of assigning *too much* content to the mind. Again, understandably. Understandably, that is, because assigning content to the mind has worked so well. However, one must be careful when proposing mental content – at least, *unconscious* mental content – that that content is well motivated. We cannot require that the theory be correct; that would be asking too much. At the same time, we cannot allow our theorizing to go beyond the minimum required to account for the data. This is basic to all science.

Generative linguists seem to have gotten themselves into the hottest water where concerns of learnability, innateness, and language evolution are involved.[19] One of the earliest tenets of Generative linguistics is that the problem of language learning is just too difficult, and that it would be impossible without the aid of some sort of innate guidance. This, of course, is the well-known "poverty of the stimulus" argument; the solution that Chomsky proposes is that humans come pre-

[18] Actually, even this is too strong. There is generally no claim that the *present* version of any theory is the *final* (therefore true) version of the theory. There is a hope that such a theory will eventually be attained. Claims are occasionally made which go beyond these constraints, however, in most cases I find these to be an unfortunate misapprehension of the place of linguistics, and the explanatory power of linguistic theory. The fact is that no one has a very clear idea of how theoretical (i.e. cognitive) constructs are specifically instantiated in the brain.

[19] I concentrate here on learnability and innateness in order to highlight the parallels with, and divergences from Bloomfield's approach outlined above. If this were not a concern, I might concentrate on Bresnan and Kaplan's "Strong Competence Hypothesis" (1982), which runs afoul by failing to realize that the computational component of the language faculty might be embedded in other systems in such a way as to obscure its operation from outside observation. (see footnote 18)

equipped with a "Language Acquisition Device" (LAD), preprogrammed with "Universal Grammar" (UG), which provides all those elements of human language that are not predictable to a child learning language by observing its environment. Determining the exact nature and scope of UG has been one of the major goals of Generative linguistics.

That we come pre-equipped to learn language is not controversial, otherwise one would predict that cats and dogs, being exposed to so much human language, would learn to speak (or at least understand!); one could say the same for tables and chairs (although, how would one test that knowledge?). Thus, it is a legitimate topic of research to determine just how much innate baggage is necessary. It is here that controversy arises: how much is enough? It would seem that one should answer the question empirically; and indeed, the promise of Generative linguistics is that this will eventually be the case.[20]

Patricia Kuhl and Andrew Meltzoff (1997) present an overview language acquisition research culminating in a description of their *Native Language Magnet* theory (NLM). NLM is both a theory of the child's initial state – before language learning commences – and the changes that learning engenders.[21] One caveat: it is really only the last step of Kuhl and Melztoff's theorizing (that of innate knowledge) which I find objectionable – indeed if the work on which it is based were not so impressive, it would not be of any interest to refute NLM.

> Infants have innate perceptual abilities that support the acquisition of language at the level of speech. Two pieces of evidence stand out: (a) categorical perception, a phenomenon showing that infants' perceptual systems partition sound to roughly define the phonetic categories of language; and (b) talker normalization, a phenomenon demonstrating that infants perceive their own vocalizations as "matching" adult's vocalizations... Such biological endowment is necessary for infants to acquire the ability to speak. (Kuhl and Meltzoff 1997, p.9)

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[20] Many philosophical and metaphysical problems arise around these questions, and I shall not try to address them here; there is little I could add, and nothing that would fit within the scope of this essay. That scope, it is easy to forget, is that the linguistic philosophy of linguists is really methodology. Where the two get confused, disaster ensues.

[21] I choose to address a phonological theory for two reasons. First, phonological learning can be analyzed rather straightforwardly as a structuring of a perceptual space; addressing a syntactic theory would be considerably more abstract (although see below). Second, syntax is embedded in systems which are themselves poorly understood; as such, the theory must be seen as an abstraction based on only an vague notion of the systems in which it is embedded. To argue about the extent to which such a metaphor is a "correct" theory is a mind-bending task, which I prefer to sidestep in this context.

There is nothing much in the above that is objectionable. However, three elements are noteworthy:

1) "infants... partition sound" – this is clearly true, but why not "*can* partition"? After all, if they *do* partition, it is clearly true that they *can*. This may seem like nitpicking, after all shouldn't one be maximally specific about the evidence? There is an issue of cause and effect here: do infants partition sound the way they do because those partitions are innate, or because these are correct generalizations about ambient language? The fact that infants can learn correct generalizations in as little as three sessions of five minutes each (p.26), while impressive, does not indicate that those generalizations were already innately present. Indeed, it would be surprising if infants could *not* learn from five minutes of undiluted evidence, since the *actual* evidence that most children receive is considerably more diluted.

2) "infants perceive their own vocalizations as "matching" adult's vocalizations." Again, this would seem to be true; but the challenge of recognizing the match between the vocalizations of various adults is already very demanding – as shown by the fact that, "even the most sophisticated computers have not succeeded" in besting this challenge (p.9). Why then, if children have this ability, shouldn't they have the ability to recognize their *own* vocalizations as well? To even begin in the challenge of talker normalization, the infant must already have the ability for (call it) "pitch normalization" – speech, especially child directed speech (motherese – mentioned by Kuhl and Meltzoff, p. 27), varies continuously – and widely – in pitch. The ability – pitch normalization – would seem to indicate that infants have identified the relevant variables for (phonetic) speech recognition. It would seem that the infant gets recognition of it's own vocalizations *for free*.

3) "Biological endowment is necessary." This is an interesting assertion given that (a) it is so obvious, and (b) which endowment? Perhaps this is just one of those platitudes scholars use to guide their readers through their chain of thought, but what exactly is it meant to convey? Surely not that language requires hearing (it doesn't), or that the human nervous system is capable of categorical behavior (so are individual nerve cells). That a *biological* endowment is necessary is obvious, that *innate knowledge* is necessary is considerably more questionable. It is, or course, possible that innate knowledge is present, even if it is not necessary – in the way that the vocalizations of some birds are innate, even though other species manage to (indeed, must) learn their songs.

> Tests of [categorical perception] ask listeners to identify each one of the sounds in [a continuum]. Early researchers expected that the sounds in the series would be perceived as changing gradually from /ba/ to /pa/... That did not occur. Adults reported hearing a series of /ba/'s that abruptly changed to a series of /pa/'s... The fact that listeners' responses were "categorial" gave the phenomenon its name... For adults, CP occurs only for sounds of their native language. When Japanese listeners were tested on a series... from /ra/ to /la/... they did not hear a ... change. The Americans were unlikely to have one set of innate endowments (a /ra/ - /la/ detector) and Japanese another; that CP was language-specific suggested that it might be learned. ... If so, then very young infants not be expected to show CP. ... [however] The results showed that young infants demonstrated CP. ... Although adults were "culture bound," infants were not; they were... "citizens of the world." (Kuhl and Meltzoff 1997, pp.9-11)

The relevant chain of reasoning goes as follows: "CP [is] language specific," therefore, "CP [is] learned," thus, "infants [should not] show CP." Stated baldly, this does seem something of a stretch! First, categorical response to *all* and *only* the sounds of one's native language is clearly an acquired characteristic. I have no wish to quibble over semantics, so readers may decide for themselves whether this constitutes "learning" or some other form of acquisition. However, it seems almost absurd to say that categorical perception *itself* is learned; after all, categorical response is a basic property of neurons! Likewise, while there is no clear definition of salience to lean on, surely some minimal, perceptual (i.e. salient) difference is necessary for two distinct stimuli to *seem* different. Simply put, the (human) brain has the capacity for categorization, not just of phonological representations, but for *all* mental representations.

If categorization is one of the most basic functions of the mind/brain, it should not be too surprising if infants react categorically to continuously varying stimuli. In fact, animals with auditory systems similar to our own also demonstrate CP that is similar to our own (see Kuhl and Meltzoff, p.28). Curiously, the evidence from animals is not taken to show that the auditory system is not innately structured, but rather:

> [T]he innate CP effects in human infants are not... evidence compelling a speech module. ... Kuhl theorized that CP in infants – the perception of "basic cuts" – was attributable to a *general auditory processing mechanism* rather that a special language module, and that it was very deeply embedded in our phylogenetic history... On this view, the perception of basic cuts in auditory signals, which would have been available in early hominids, was exploited in ... evolution. (Kuhl and Meltoff 1997, pp.28-29)

It is not that any of the above is wrong *per se*.[22] Note however, that Kuhl and Meltzoff assume a (strongly) modular view of the mind, otherwise a term like "innate speech module" or "(innately specified) basic cuts" would not have any reasonable interpretation. In fact, they reject the idea that there is a speech module, but what should one make of the notion that there is a "general auditory processing mechanism" that is "deeply embedded in our phylogenetic history"? After all, this is clear from the fact that most mammals have ears! It is Kuhl and Meltzoff's own contention that the innate "basic cuts" predate human language, thus also the vocal tract; hence, natural selection could not specifically have selected for recognition of human speech sounds. Indeed, Kuhl and Meltzoff claim the opposite: the basic cuts influenced the selection of speech sounds. How then did the basic cuts themselves arise? It could be that the cuts are themselves simply the random result of a general evolutionary need to partition acoustic space. However then it would seem that one set of cuts would be as good as any other, and thus there would be no reason to expect the cuts to remain stationary over evolutionary time spans – except possibly in humans, since the acoustic cuts must correspond roughly to the range of possible articulations. That this is the case would apparently be contradicted by the fact that animals *do* display the same cuts.

Talk of innateness begs an explanation in terms of evolution. The problem of language evolution always seems to bring out the most foolish in those who theorize about it, and lest I too fall into the trap, I will restrict my comments to just a few words. The following is from Pinker (1995):

> First, if language involves... [two] individual[s], who did the first language mutant talk to?... [A] general answer is that the neighbors could have partly understood what the mutant was saying... just using general intelligence. [Just as] English speakers can often do a reasonable job understanding Italian newspaper stories based on similar words and background knowledge. If... important distinctions may be decoded by others only with uncertainty and great mental effort, it could set up a pressure... to evolve... an automatic, unconscious parsing process. Selection could... [have favored] those speakers... that the hearers could best decode, and the hearers who could best decode the speakers. (Pinker 1995, p.365)

To be fair, Pinker is not proposing a theory here, he is simply running a plausibility argument: that is, it is plausible that evolutionary pressures influenced the development of language. Unfortunately, it falls flat. It

[22] The usual caveat applies: based on the evidence, there is no inherent contradiction.

seems unclear that the evolutionary pressure on most individuals to understand the few "language mutants" would not be vastly outweighed by the evolutionary – not to mention social! – pressure on the mutants to talk in a way that everybody understands. Furthermore, a hallmark of human language is that underlying relationships are obscured in surface forms; therefore, in the absence of a preexisting language faculty (for use in parsing), the utterances produced by individuals with more advanced language faculties will be vastly more difficult for others to understand than whatever plodding mechanisms are in common usage. Pinker relies on the notion of "proto-language," (Bickerton 1990), which is defined as a "primitive type of language… [which is] more robust than language (having formed part of the hominid endowment for much longer)." (Bickerton p.118) Bickerton contends that proto-language is *not* just based on general reasoning abilities; that is, it is also innate and it is specifically linguistic. Bickerton's claim is that adults who never learned language as children, apes trained in sign language, and ordinary adults attempting to communicate without a common language could not participate in even these impoverished communications without the aid of proto-language. Maybe this is right, but by the same reasoning, why shouldn't I conclude that my cat has *proto*-proto-language, since she recognizes her name? Talk of proto-language always reminds me of the following joke:

This is probably not a fair portrayal of proto-language, but in the domain of language evolution, it is probably wise to remain particularly skeptical. Related to claims about language evolution are claims about the innate aspects of human language. Clearly, the fact that humans have language is due to our innate endowment; we know this because *only* human among all species have language, and because we believe that what distinguishes species from one another is their genetic endowment and nothing else. Therefore, elephants have trunks, fireflies glow, and humans speak. Note however, that this does not mean that there is some stretch of elephant DNA that we will one day be able to

label "trunk," and some stretch of human DNA that we will one day be able to label "UG" – although there may well be an identifiable stretch of firefly DNA for the substance in fireflies that glows.

VI. How to Do Linguistics Now.

Recently, linguistics has again taken some new turns, and acquired some new priorities. Today, some linguists seek theories that are "Minimalist" (Chomsky 1995), or "Optimal" (Prince & Smolensky 1993), while others ask, *Is the Best Good Enough?* (Barbosa et al. 1998), or point out that the linguistic facts are not nearly as orderly as we might wish them to be (Culicover 1999).

Chomsky (2000) puts considerable effort into denying that he is responsible for any "innateness hypothesis" (e.g. pp.187-189). That is:

> Presumably, cognitive capacities, like all others, are rooted in biological endowment, and FL (if it exists) is some kind of expression of the genes. Beyond that, I know of no innateness hypothesis, though there are specific hypotheses about just what is innate. (Chomsky 2000, p.187)

These "specific hypotheses about just what is innate" are, of course, subject to reanalysis, disconfirmation or revision, such as is necessary in order to account for the evidence. Therefore, in early generative work it was assumed that language learners had the innate ability to write linguistic rules – e.g. S→NP VP. Under the Principles and Parameters approach it was assumed that humans come endowed with an innate set of (binary) parameters, which the language learner sets based on linguistic input – e.g. Is the language verb second (±V2)? Yes, or no.

More recently, parameters are conceived as properties of functional categories; unlike rules or parameters, these properties are quite abstract and are not easily illustrated by a simple example. The change allows for several advances:

1) It allows children to learn more than one language – the parameter theory requires that a child acquiring both German and English, either simultaneously set the verb second parameter to +V2 (for German), and -V2 (for English), or construct a second bank of parameters, one for each language. The former solution is, of course, logically untenable, while the second question begs the question of why children cannot arbitrarily construct extra banks of parameters! Under the newer proposal, learning about the properties of functional categories proceeds just as lexical learning does, bilingual children learn about the functional

elements of the languages that they learn, in the same way that they must sometimes say, "cat" and at other times "Katze."

2) In the years since Chomsky (1981) proposed the theory of parameters as a solution to the problem of how children might learn syntax, progress has been remarkably slow on determining just what those parameters might be. Presumably, the number must not only be finite, but also quite small, otherwise what advance is there over the original rule-writing theory? If parameters are simply properties of functional heads, the problem of determining what they are becomes considerably more tractable: take the number of relevant functional elements, multiply by the properties they might possess, and *voilá!* – not only a number, but also the identity of all the parameters (in theory).

3) Of those parameters that have been determined, one of the most successful is the so-called "PRO-drop" parameter, which says, "May the sentences of this language occur without a grammatical subject? Yes, or no." Italian is a prototypical PRO-drop language, therefore in Italian, one may say, "ran-pl to the store." instead of, "They ran to the store." English is a prototypically –PRO-drop language, therefore, "It rains." contains the meaningless subject *it* (there is, after all, no such entity, such that **it** is the thing that is raining), while in Italian, one would simply say, "rains." Unfortunately, even this parameter does not really work out so well, "Diary English" turns out to be +PRO-drop (e.g. "Went to the store today. Bought some milk. Saw Sue. Showed me her kids' pictures."). Furthermore, there are cases like the following: Swiss German is a –PRO-drop language (like Standard German), except in the second person singular informal (unlike Standard German). Therefore, one may leave out the subject only when speaking about the person that one is speaking to – but not with strangers. Parameters, since they refer to global properties of a language, may only be set in one direction (or possibly left completely unset, there is some disagreement on this point), and thus cannot easily account for data such as this Swiss German example. Since functional elements are simply one more piece of a combinatorial structure, they function locally, like any other syntactic element. Thus they can be *conditional*. See Culicover (1999) for many other examples that are problematic on the parameter setting approach.

VII. Knowing the Difference.

A few things seem reasonably clear about natural language. It is a product of the human mind. The mind (or better, the brain) is an information-processing device. Therefore, at the root of all language is some sort of computation. It is also reasonably clear that there is more

than one way to be interested in language. Actors, singers, orators, politicians, logicians, yes even linguists and philosophers are all interested in language – but the nature of that interest is different. While most people are primarily interested in *using* language, linguists are primarily interested in language for its own sake. For the linguist, language is whatever scientific study tells us it is. For now, the most promising approach – to my mind, the only coherent approach – is to assume that language is the product of a computational process, and to ask, "What is the nature of that process?"

It is far from uncontroversial, even given that humans must (somehow) come pre-equipped to learn language, that there is a poverty of the stimulus problem, and therefore that UG is required. I will not address here the argument that there is simply nothing for UG to describe – in other words, that there are no universal regularities. This argument is obviously specious – defending generative linguistics from this claim would require an extensive survey of the linguistic data, and would be about as productive as trying to reeducate creationists, or flat-earthers. There is, however, a more subtle argument that merits serious attention. That is, perhaps brain is a *really good* pattern recognizer, and its overall architecture is enough to account for UG.

This argument does not really eliminate, or replace UG; rather, it *dis*places it. Thus, on this account, UG is not a property of the language faculty; it is a property of the *environment* of the language faculty. This proposal seems entirely in the spirit of Chomsky's idea that the, "output conditions [are] determined 'from the outside,' at the interface." (1995, p.224) It is my belief (subject to revision based on evidence) that it will eventually be possible to reduce UG in this way; but let us be clear, a *belief* is not a *theory*, and as such has no empirical content. Without a proposal as to the processing architecture at the interface(s), and some theory of what is on the other side of the interface(s), the idea is not testable and therefore useless. This said, is there really such a deep divide between theories of UG and the sketch above?

There is often an ambiguity in the language acquisition literature, between the *language learner* learning a grammar and the *language faculty* learning the same grammar. First, what the difference is *not*: by definition, the language faculty is that, in the competent speaker of a language, which allows her to correctly produce syntactically well-formed sentences of her language. While the language faculty is believed to be functionally equivalent for everyone (i.e. like eyes), each person's language faculty (unlike eyes) is set somewhat differently based on experience (i.e. we each speak our own *idiolect*); the idiolects of people

who speak different dialects or languages may differ significantly. In linguistics, we call these different settings, "grammars." Unfortunately, "grammar" is a term that carries a great deal of baggage; Chomsky addressed the problem as follows:

> The term "I-language" was introduced to overcome misunderstanding engendered by the systematic ambiguity of the term "grammar," used both to refer to an I-language and to the linguist's theory of it. Thus Jones's knowledge of his I-language (grammar, in one sense) is nothing like some linguist's (partial) knowledge. (Chomsky 2000, p.201n)

On this view, the ambiguity between talk of humans vs. language faculties learning a grammar is not due to any difference in what is learned, and we must be careful to keep our "grammars" straight – learning a grammar (I-language) is setting whatever variables are required by the language faculty. So if the content to be learned is the same, what is the difference? To put it simply: the environment. The environment of a language learner is occupied by people, and things, while the environment of the language faculty is occupied by nerve impulses, which reflect the structure of the brain, and the state (learning) of adjacent brain structures. The distinction is not nominal.

It is important to remember that the external world has *no linguistic structure*.[23] Even if we restrict ourselves to the most superficial linguistic phenomena – for example, the "phoneme" – there is no clear sense in which even these exist in the real world; phonemes are defined by our ability to produce and distinguish them.[24] At the phonetic level, one may describe phonemes either by reference to acoustics or articulation. However, even the acoustic definition of a phoneme[25] is predicated on the anatomy of the inner ear, which essentially converts a complex

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[23] This is by no means a new idea; it has been stated in many different ways by many different people (going back as far as Plato?). In any case, my interest here is not metaphysical, but pragmatic. Given what we know about the structure of the world, and given what we know about the structure of our (linguistic) representations of the world, how can the two be related?

[24] In fact, the *phoneme* may not even be a particularly useful theoretical construct, and does not play much of a role in recent work in phonology (e.g. Goldsmith 1989, Prince & Smolensky 1993). I use the term "phoneme" here to refer to both phonemes and *phones*.

[25] More accurately, "phone." Technically, a phoneme is an abstraction – a linguistic representation of a sound used in language – while a phone is the sound itself. Thus, in English, the "t" in "at" is phonetically distinct from the "t" in "to" (which is aspirated, [t^h]), however, the native English speaker hears them as indistinguishable; one may thus posit a unified underlying representation: /t/. In other languages – Korean, for example – these two sounds can be used to create a semantic distinction; such evidence is traditionally used to argue for the existence of two distinct phonemes is such languages.

waveform into its component frequencies (Fournier transform). Because of this (mechanical) transformation, the neural correlates of sound are thoroughly unlike the original sound – and the untransformed sound is not identifiable as a phoneme (or even as a phone).

The articulatory definition of a phoneme is still further removed from the external reality of linguistic sounds as they propagate through air. Edward Sapir (1933, *The Psychological Reality of Phonemes*) wrote about the "phonetic illusion"[26] – that is, the independently verifiable acoustics of a sound underdetermines its phonemic representation. Some might dismiss Sapir's argument as relying too much on intuition, however it is possible to run a similar argument at the level of articulation. There are vocalizations, which, while real, are either virtually silent, or virtually indistinguishable from other sounds. In the former category are sounds like [p], [t] and [k]. Because these sounds are produced by completely closing off the air stream, they are mostly composed of silence – in fact, the only silent moments in connected speech. In the latter category are sounds such as English [r], or German [ü]; in some dialects of English [r] is produced in the back of the throat, what in others it is produced by curling back the tongue (retroflexion). In Standard German, [ü] is pronounced like English [ee], but with lip rounding, in Swiss German, [ü] is pronounced like English [u], but *without* lip rounding; as in the case of the two English [r]s, the two sounds are almost identical.

Thus, even at the most superficial level, there is no clear description of human language as separate from the human speaker/hearer. It is only possible to simulate parsing and production to the extent that it is possible to simulate the ear and the vocal tract – or by relying on written language and thus circumventing the problem.

Returning to the difference between a theory of UG, and a theory that the brain is just a pattern recognizer, we find that the latter theory now has some content. There is a set of systematic regularities in the speech stream as transformed by the inner ear; these may be described by a theory of acoustic phonetics. Very roughly, two pieces of

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[26] Discussed in Kenstowicz (1994, p.3). English speakers, for example, are likely to believe that "hatter" and "had'er" ("had her") do not sound the same – in fact, they are acoustically identical. Whether or not a language has a written form is irrelevant to this effect. Sapir believed that this was a good reason to believe in the psychological reality of phonemes; and indeed, the argument is still quite convincing. However, given that phonemes, as such, do not play an explanatory role in current phonology, the "psychological reality" of phonemes is of a different order than that of phonological features (which *do* play an explanatory role). "Adverb," is another example of the, "but it's real *to me*!" type of psychological reality.

information are produced by this transformation: the fundamental frequency contour (intonation) of the speaker's vocalization and the frequency spectrum of the speech sounds relative to that fundamental frequency (phonetics). There is a mechanism of producing speech sounds; this mechanism may be described by a theory of articulatory phonetics. There is a set of systematic regularities at the interface between phonetic representation and articulation; these may be described by a theory of phonology.

This description underestimates the complexity of the relationships and may well be wrong in certain respects, however it points the way to a method of describing linguistic phenomena without the aid of innate symbols, or symbol processing. Nonetheless, even if this method of reduction were one day to succeed, it is only symbol manipulation, *per se*, that can be eliminated from the theory; such a reductive theory would still need to encompass various levels of computation, and the information at each of these levels must properly be defined as a representation. Consequently, the theory of any particular level of representation must include a (symbolic) formalization of the structure of adjacent levels, and must itself be formalized as symbol manipulation.

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