

The Chomskyan Revolution

In the late forties . . . it seemed to many that the conquest of syntax finally lay open before the profession.

At the beginning of the fifties confidence was running high. Many linguists felt that a new synthesis of the discipline was needed and that a suitable time was rapidly approaching. This would continue the Bloomfield tradition taking into account the results achieved in two decades. Indeed, some spoke of the need for a “revision of Bloomfield,” not a replacement but an updating. No one, however, felt able to undertake the task.

H. Allan Gleason

We shall have to carry the theory of syntactic structure a good deal beyond its familiar limits.

Noam Chomsky

Looking for Mr. Goodstructure

In one reading of linguistic history, the Bloomfieldians of the 1950s were biding their time for some convincingly complete model to displace their picture of language. The “fullest flowering” of Bloomfieldian grammar-construction was Trager and Smith’s *Outline of English Structure* (Stark, 1972:414). It was at once a reworking and a practical application of Trager’s earlier classic (with Bloch), *Outline of Linguistic Analysis*. Since the application was to English, it had enormous educational advantages, and it was a self-conscious exemplar of the program, illustrating by example “a methodology of analysis and presentation that we believe to be representative of the scientific method as applied to a social science—linguistics” (Trager and Smith, 1957 [1951]:7). It was brief. It promised significant inroads into syntax; even—though this was relegated to an area of concern labeled *metalinguistics*—into meaning. Reflecting the growing confidence of the field, it was also a deliberate attempt to put the best Bloomfieldian foot forward into the scholarly world at large: “Educators, diplomats, anthropologists, and others were presented with a promise of a linguistics that was rigorous, central, expanding, and useful” (Hymes and Fought, 1981 [1974]:138). But, as everyone could see, it leaked.

Some of its weaknesses came up for discussion at the first Texas Conference on Problems of Linguistic Analysis in English, when the participants fell into a discussion about scientific progress. Paradigm shifts cropped up, and Archibald Hill told his colleagues how science works in such circumstances:

When things don't fit, scientists labor to patch up the system by adding things, taking them away, or rearranging. It is only when a complete new system is presented, a system more complete, more consistent, and simpler in its totality than the old system, that any real change is made. (1962a [1956]:17)

The obvious analogy surfaced—the Ptolemaic-to-Copernican cosmological shift—with Trager and Smith in Ptolemy's hot seat. James Sledd, one of the discussants and an outspoken critic of Trager and Smith's *Outline*, could only manage a backhanded compliment for their model:

The great strength of the Trager-Smith system is that it has pretensions to completeness, and Mr. Hill is right that if we want to overthrow the Trager-Smith system we can do it only with a system which has more justifiable pretensions to completeness. (Hill 1962a [1956]:17)

This exchange looks for all the world like a symptom of the historical stage in the growth of a science that Thomas Kuhn calls *a crisis*, when the science goes through “a period of pronounced professional insecurity,” the prelude to a revolution (1970:67f). A science in crisis, says Kuhn, is a science looking to shuck whatever program gave rise to its insecurity, looking for a new, more complete, more consistent, more simple system than the old one, to give it back some confidence, looking, in many ways, for a messiah.

And perhaps linguistics, as an abstract and collective entity, *was* looking for a savior. Subsequent events suggest as much—in particular, they suggest there was some generational discontent, with younger members impatient to get at the good stuff that had been kept at bay for twenty-five years, meaning and mind. But there is little indication in the literature of the period that there was a crisis on any front, and this exchange in Texas is certainly not a symptom of messianic longings. Aside from Sledd (who was putting words in Hill's mouth about wanting to overthrow Trager and Smith), there was no serious talk of doing away with Trager and Smith's model at the conference—the comments are more on the order of patching it up—and not the slightest hint of frustration at the Bloomfieldian program underwriting their model. Indeed, Robert Stockwell, who had just been talking with Trager, passed on the good news that the *Outline* was, even as the Texas discussants spoke, being overhauled in a direction which promised to satisfy some of the system's pretensions. Trager and Smith were aiming for a good deal more completeness. Word-formation processes (morphology) were to get increased attention, and “the syntax, further, will be completely redone and much expanded.” This syntax, phonological syntax, played very well at conferences in the early and mid-fifties, attracting a good many adherents, especially among younger linguists eager to get at new material. As the name implies, it built systematically on the very attractive base of Bloomfieldian phonology, representing the natural and desired expansion of the field: incremental science at its best.

Linguistics was changing and expanding in the fifties, showing sporadic dissent over the central tenets, increased tolerance for other approaches, and some dalliance in the banned domain of psychology. But measured dissent, pluralism, and exploration, at least in this case, represent the exact opposite of Kuhn's definition of crisis. They were symptoms of a pronounced sense of professional security. The earlier hostility toward Europe, and meaning, and mind, and the undue reverence for method, and the chest-thumping war cries of "I'm a scientist and you're not": *these* were the signs of insecurity. By the fifties, paranoid aggressiveness had given way to a quiet satisfaction and optimism (in some quarters, as we have seen, to an almost gloomy optimism that all the real problems had been solved). The other major Bloomfieldian codification published in the fifties, off the presses almost in a dead heat with Trager and Smith, was Zellig Harris's (1951 [1947]) *Methods in Structural Linguistics* and it was hailed as "epoch-marking in a double sense: first in that it marks the culmination of a development of linguistic methodology away from a stage of intuitionism, frequently culture-bound; and second in that it marks the beginning of a new period, in which the new methods will be applied ever more rigorously to ever widening areas" (McQuown, 1952:495). A glorious period of advancement may have been over, but a new and more glorious one, building on those advances, was just beginning.

Into this atmosphere came *Syntactic Structures*, published the year after the First Texas Conference. It couldn't have fit the mood better. It appeals calmly and insistently to a new conception of science. It promises the transformational taming of syntax. And it elegantly walks the tightrope of the signified—supporting Bloomfield's argument that they couldn't be allowed to taint the analysis of signifiers, but offering persuasive suggestions that linguists could get at meaning anyway.

Chomsky's book was welcomed. But it was not—and this point is often missed in histories of the period—taken to herald the arrival of a complete new system, more consistent and simpler, that would revolutionize linguistics. Chomsky was not hailed as the messiah, not immediately. For one thing, *Syntactic Structures* had virtually nothing to say about the old system's strongholds, sounds and words. But more importantly, its implications for the Bloomfieldian superstructure were almost entirely submerged. Chomsky's program looked much more like the projected steady expansion of Bloomfieldianism, ever more rigorous, to ever-widening areas; all the more so as Chomsky was the favored son of Harris, author of the double-epoch-marking *Methods*.¹

Soon there was talk from Chomsky and his associates about plumbing mental structure; then there was a new phonology; then there was an explicitly new set of goals for the field, cut off now completely from its anthropological roots and hitched to a new brand of psychology. By this point, in the early sixties, it was clear that the old would have to be scrapped for the new. These last developments—accompanied for the most part with concerted beatings of one or more of the Bloomfieldians' sacred cows—caught most of the old-line linguists somewhat unawares. They reacted with confusion, bitterness, and ineffective rage. Rapidly, the whole kit and kaboodle of Chomsky's ideas swept the field. The entrenched Bloomfieldians were not looking for a messiah, but, apparently, many of their students were. There was a revolution.

Syntactic Structures

Chomsky's *Syntactic Structures* is a striking and original book, which forced its readers to look at familiar things from a fresh angle. But in taking this view, he did not destroy his predecessors' basic concept of the structure of language. Rather he gave new life to it.

P. H. Matthews

For the 1950s, Chomsky was, in the terms of one lapsed Bloomfieldian, "a very aberrant young linguist" (Gleason, 1988:59). He was something of an outsider, always an advantage for seeing the limitations and weaknesses of an established program. His exposure to the field came almost entirely through Harris, and Harris was a card-carrying Bloomfieldian, but *in extremis*, representing, in many ways, the best and the worst of the program. He had a fixation on esoteric, if not peripheral, issues, and a preoccupation with methodology which far outstripped even that of his contemporaries. He, too, had a somewhat unusual background for a Bloomfieldian—coming not from the rolled-up-sleeves-and-loosened-collar world of anthropology, but the bookish, intensely logical world of Semitic philology—and, except for Hockett, he was the only linguist of the period pursuing the natural, but largely ignored, ramifications of the Saussurean conception of *langue* as a "rigid system," the only linguist of the period seriously exploring the mathematics of language. Chomsky's education reflected Harris's interests closely. It involved work in philosophy, logic, and mathematics well beyond the normal training for a linguist. He read more deeply in epistemology, an area where speculation about the great Bloomfieldian taboo, mental structure, is not only legitimate, but inescapable. His honors and master's theses were clever, idiosyncratic grammars of Hebrew, and—at a time when a Ph.D. thesis in linguistics was almost by definition a grammar of some indigenous language, fieldwork virtually an initiation rite into the community of linguists—his doctorate was granted on the basis of a highly abstract discussion of transformational grammar, with data drawn exclusively from English. When his thesis made the rounds at the Linguistic Institute in the summer of 1955, it looked completely alien, "far more mathematical in its reasoning than anyone there had ever seen labeled as 'linguistics'," and, predictably, it fell utterly flat:

A few linguists found it very difficult; most found it quite impossible. A few thought some of the points were possibly interesting; most simply had no idea as to how it might relate to what they knew as linguistics. (Gleason, 1988:59, 60)

That was, of course, the rub, the dragging friction on any acceptance of his ideas: how to make his work palatable to linguists. His thesis—"Transformational Analysis"—was not only forbiddingly technical, but completely unrelated to the daily activities of Bloomfieldian linguists. And it was only one chapter of a massive manuscript—*The Logical Structure of Linguistic Theory*—he had feverishly worked up while on a fellowship to Harvard in the early fifties. A few copies of *Logical Structure* were available here and there in mimeograph, but it was known mostly by rumor, and had the whiff of Spinoza or Pierce or Wittgenstein about it,

or some other fecund, mathematical, relentlessly rational, but cloistered mind. Chomsky must have been considered, when considered at all, somewhat the way Crick recalls the feeling about his collaborator on the structure of DNA: “Watson was regarded, in most circles, as too bright to be really sound” (1988:76).

With this particular background, Chomsky was not, despite acknowledged brilliance, the ideal candidate for a job in an American linguistics department, and found himself in the Research Laboratory of Electronics of the Massachusetts Institute of Technology. His research was open-ended, allowing him to continue his abstract modeling of language, but the appointment was only partial and he had to teach to round out his income: German, French, philosophy, logic. And linguistics. Since there was no one there to tell him otherwise (MIT had no linguistics department), he taught *his* linguistics, and the lecture notes for this course became the answer to the rhetorical gulf between the audience for *Logical Structure* (written for Chomskyan linguists when there was only one, Chomsky) and everyone else in the field.

These notes, revised and published as *Syntactic Structures*, constitute one of the masterpieces of linguistics. Lucid, convincing, syntactically daring, the calm voice of reason calling from the edge of a semantic jungle Bloomfield had shooed his followers from, it spoke directly to the imagination and ambition of the entire field. The most ambitious, if not always the most imaginative—the young—responded most fully, but the invitation was open to all and the Bloomfieldians found many aspects of it very appealing.

Science and Generative Grammar

By a generative grammar I mean simply a system of rules that in some explicit and well-defined way assigns structural descriptions to sentences. . . . Perhaps the issue can be clarified by an analogy to a part of chemical theory concerned with the structurally possible compounds. This theory might be said to generate all physically possible compounds just as a grammar generates all gramatically ‘possible’ utterances.

Noam Chomsky

Especially attractive to the Bloomfieldians was the conception of science Chomsky offered in *Syntactic Structures*. The first few sentences of the book advance and defend the conception of linguistics as an activity which builds “precisely constructed models” (1957a:5), and building precisely constructed models was the mainstay of Bloomfieldian linguistics (though they were happier with the word *description* than with *model*). But Chomsky also made the motives behind such construction much more explicit than they previously had been. There are two, he says. One motive is negative: giving “obscure and intuition-bound notions” a strict formulation can quickly ferret out latent difficulties. The other is positive: “a formalized theory may automatically provide solutions for many problems other than those for which it was explicitly designed” (1957a:5). In short, the clear and precise formulation of a grammar has the two most important attributes that recommend

one scientific theory over another, greater fragility and increased scope. If you can break a scientific theory, it's a good one, since that means it has clear and testable connections to some body of data; if you can break it in principle but not in practice, so much the better, since not only can it be tested against data, the testing proves it compatible with that data. The law of gravity you can test by dropping a pen and measuring its descent; if it floats upwards, or zips sideways, or falls slowly to the ground, then the law is in trouble. But the pen never does (unless you're someplace weird, like a space capsule or a centrifugal chamber, when the bets have to change), so gravity is fragile in principle, resilient in practice. And the more coverage a theory has, the more efficient it is. The law of gravity is (more or less) equally applicable to falling pens and orbiting planets. Two laws for those phenomena, rather than one, mess things up, and scientists like to be tidy whenever they can.

Two definitions are crucial for Chomsky to achieve these scientific virtues: a *language* is "a set (finite or infinite) of sentences" and a *grammar* is "a device that generates all of the grammatical sequences of [that language] and none of the ungrammatical ones" (1957a:13): a grammar is a formal model that predicts which strings of words belong in the set of sentences constituting a language and which strings do not belong.² An adequate grammar of English, then, would generate sequence 1, but not sequence 2 (which is therefore stigmatized with a preceding asterisk, following the now-standard linguistic practice).

- 1 Kenny is one cool guy.
- 2 *guy cool one is Kenny

Now, a grammar which aspires to generate all and only the set of sentences possible in a language—a generative grammar—by Chomsky's definition, is a scientific grammar:

A [generative] grammar of the Language L is essentially a theory of L. Any scientific theory is based on a finite number of observations, and it seeks to relate the observed phenomena by constructing general laws in terms of hypothetical constructs such as (in physics, for example) "mass" and "electron." Similarly, a grammar of English is based on a finite corpus of utterances (observations), and it will contain certain grammatical rules (laws) stated in terms of the particular phonemes, phrases, etc., of English (hypothetical constructs). These rules express structural relations among the sentences of our corpus and the indefinite number of sentences generated by the grammar beyond the corpus (predictions). (1957a:49)³

Beyond this very attractive identification of grammar and theory, Chomsky also offered a new philosophy of science. By 1957 philosophy of science had shifted considerably, and Bloomfield-endorsed positivism had sunk from almost complete dominance to an approach that dared not speak its name—the 1957 presidential address to the American Philosophical Association was "Vindication of L*G*C*L P*S*T*V*SM" (Rynin, 1957).

Methodological fretting had fallen into disrepute and all that now counted was the results, however obtained. Linguistics should proceed, went Chomsky's articulation of this new methodological indifference, by way of "intuition, guess-work, all sorts of partial methodological hints, reliance on past experience, etc."

(1957a:56). The crucial interests of linguists *qua* science should be those revolving around whether the grammar stands up once it has been formulated.

Does it generate the sentences of L? Does it preclude non-sentences of L? Does it fit established scientific constraints like fragility, elegance, and generality?

There is a measure of antagonism in this move for those Bloomfieldians who cared about such things (Trager and Hall, for instance), and some no doubt found Chomsky's methodological nonchalance distasteful—even, in the familiar curseword, *unscientific*. But most linguists weren't very troubled by foundational issues of this sort. More importantly, *Syntactic Structures* doesn't frame its philosophy of science in antagonistic terms. It comes, in fact, in a frame that couldn't help but appeal to the Bloomfieldians' scientific fondness—defining their principal concern, grammars, as on a par with physical or chemical theories. Chomsky was, from a Bloomfieldian perspective, confirming and elaborating their notions of what makes for good science.

What most of them didn't notice (though their students did) is that Chomsky changed the focus of linguistics radically—from discovering good grammars to justifying and evaluating them. Linguistics was slipping from a primarily descriptive enterprise into a theoretical enterprise directed toward exploring the general principals underlying descriptions.⁴

Syntax and Transformational Grammar

I find myself differing with Harris and Chomsky chiefly on points that I regard as minor: I am glad to see syntax done well in a new format.

Ralph B. Long

By far the most attractive aspect of *Syntactic Structures* for the Bloomfieldians was its titular promise to advance the structuralist program into syntax. Chomsky's first step was to translate Immediate Constituent analysis into a more testable format. Immediate Constituent analysis was a body of "heterogeneous and incomplete methods" (Wells, 1947b:81), which had begun hardening into a more systematic theory of syntactic structure—most attractively in the phonological syntax of Trager and Smith—but was still a long way from the rigid formalism called for by Chomsky's notion of generative grammar. Out of the relatively loose group of Immediate Constituent procedures, Chomsky extracted a notation based on variables and arrows such that a simple rule like $X \rightarrow Y + Z$ defined the relations among the variables in an easily diagrammable way; that is, in the way illustrated by figure 3.1.

From this notation, Chomsky built a rule system for English of the following sort.⁵

- 3 a $S \rightarrow NP + VP$
- b $NP \rightarrow Det + N$
- c $VP \rightarrow V + NP$
- d $Det \rightarrow the$
- e $N \rightarrow \{dog, duckling, sandwich, farmer, affix . . . \}$
- f $V \rightarrow \{bite, chase, hop, kill, passivize . . . \}$

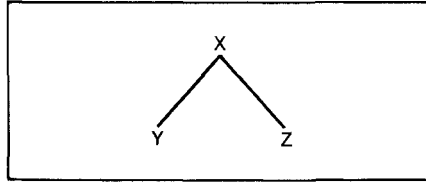
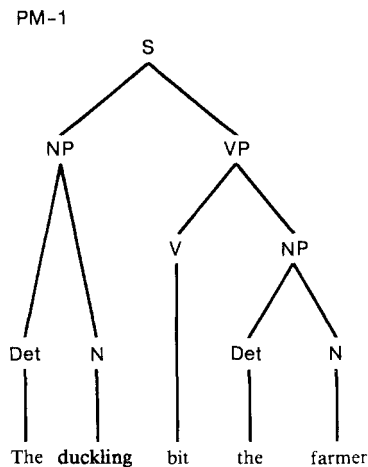


Figure 3.1. A diagram of the abstract, hierarchical relationship generated by the formal rule, $x \rightarrow y + z$.

The symbols in 3 are all mnemonic: *S* stands for *sentence*, *NP* for *noun phrase*, *VP* for *verb phrase*, and so on. (The only one that may not be immediately apparent from a grade-school knowledge of language, *Det*, stands for *determiner*, and isn't especially important for our purposes; its main function here is to help identify one of the members of noun phrases—namely, *the*, as specified by 3d.) The rules, then, are descriptions of how sentences, noun phrases, and verb phrases hang together. They express such notions about the syntax of English as “sentences have noun phrases and verb phrases, in that order” (in more traditional terms, sentences have subjects and predicates), and “verbs are such things as *bite* and *chase*.”

The rules of 3—*phrase structure rules*—cover only the tiniest portion of English syntax, of course, but they illustrate conveniently the type of expressions that Immediate Constituent Analysis (or, in Chomsky's rechristening, phrase structure grammar) handles most efficiently. Consider how they work. Each rule is an instruction to rewrite the symbol to the left of the arrow as the symbol(s) to the right of the arrow, yielding a derivation of a sentence in exactly the sense that word has in calculus. Representing this derivation graphically, we get a tree diagram (or *phrase marker*) of the sort that has become ubiquitous in modern linguistics, illustrated by PM-1 (where *S* dominates *NP* and *VP*, as called for by rule 3a; *NP* dominates *Det* and *N*, as called for by 3b; *N* dominates *duckling* in one instance, *farmer* in another, as allowed by 3e; and so on).



Once Chomsky has this machinery in place, he briefly demonstrates that phrase structure grammars are superior to the only legitimate candidates proposed as formal models of syntax—Shannon and Weaver’s (1949) nascent finite state grammars—which, not coincidentally, had been endorsed by the Bloomfieldian boy-wonder, Charles Hockett (1955:2). Any satisfaction Immediate Constituent fans might have felt from this triumph, however, was very short lived. Applying to grammar the same principles necessary for a good scientific theory, Chomsky demonstrates that whatever phrase structure grammar’s representational virtues, its treatment of some fundamental phenomena is surpassingly ugly: “extremely complex, *ad hoc*, and ‘unrevealing’” (1957a:34). That is, they may be adequate as flat descriptions of the data, in the way that randomly ordered lists adequately describe all the elements of a compound, but they lack the simplicity and concision found in a chemical formula.

Lo, in the east, a transformation.

Several transformations, in fact; a small flock; and Chomsky shows how they can, rather effortlessly, clean up after phrase structure analyses. Two of these transformational analyses, centering on rules which became known as *Affix-hopping* and *Passive* (or *Passivization*), rapidly achieved the status of exemplars in the next few years, as transformational grammar solidified into a paradigm.

Affix-hopping depends on too much detail about the English auxiliary system to treat very adequately here, but it was extremely persuasive. Bloomfieldian linguistics was fundamentally a distributional pursuit, fundamentally about accounting for the distribution of sounds and words—what comes before what—and getting the distributions right for English auxiliary verbs is a very complicated matter when it is left in the hands of phrase structure rules alone. As one sliver of the problem consider the progressive aspect (4b, in contrast to the simple present, 4a):

- 4 a Andrew skateboards.
- b Andrew is skateboarding.

The tricky part about 4b is that progressive aspect is clearly coded by two chunks separated by another one: *is* and *-ing* are both necessary, but that darn *skateboard* gets between them. Chomsky made a number of innovations to the phrase structure rules in order to describe the discontinuous distribution, *is . . . -ing*, but the really ground-shifting move was his proposal of this elegant little transformation:

- 5 Af V \Rightarrow V Af

(The structure to the left of the double arrow “becomes” the structure to the right.)

Rule 5 simply attaches the affix preceding a verb to its backend, making sure that the suffix (*-ing*) in fact shows up where it’s supposed to show up, abutted to the verb’s hindquarters. The modifications Chomsky made to the phrase structure rules ensured that they produced something like 6:

- 6 Andrew is *-ing* skateboard

Then Affix-hopping would kick in, leapingfrogging, the *-ing* over the *skateboard*, and the (distributionally correct) 4b was the result. What's the big deal? Well, the phrase structure rules generate *is* and *-ing* side-by-side, capturing the fact that they serve as a unit to signal progressive aspect, and Affix-hopping redistributes that unit, capturing the fact that they don't in fact occur side-by-side in people's speech.

If Affix-hopping isn't very convincing about the merits of Chomsky's system, consider how badly a phrase structure account of sentences like those in 7 does. It leaves completely unexpressed the important fact that actives and passives have very clear syntactic and semantic parallels.

- 7 a The duckling bit the farmer.
b The farmer was bitten by the duckling.

A grammar that handles syntax exclusively with phrase structure rules would generate the sentences in 7 independently, with two sentence rules like these ones:

- 8 a $S \rightarrow NP + V + NP$
b $S \rightarrow NP + be + V + by + NP$

Two rules for two phenomena (8a for 7a, 8b for 7b), necessarily implies that any connection between active sentences and passive sentences is wholly accidental; an active is an active, a passive is a passive, and the only loosely connecting point about them is that they are both sentences. However, as every native speaker of English knows, there is an obvious pattern to these correspondences. For instance, 9a and 9b are clearly legitimate, sensible, English sentences; 10a and 10b are clearly illegitimate and nonsensical (or, legitimate only under a bizarre construal of *sandwich*):

- 9 a The farmer bit the sandwich.
b The sandwich was bitten by the farmer.
10 a *The sandwich bit the farmer.
b *The farmer was bitten by the sandwich.

An adequate grammar of English—that is, one which meets Chomsky's criterion of enumerating all and only the legitimate sentences of English—must therefore generate the first pair of sentences and preclude the second; the phrase structure account can only do so at the expense of unintuitive redundancy. For instance, it must stipulate independently what subjects and objects the verb *bite* can take in an active sentence and what it can take in a passive sentence, although the two sets are strictly inverse (the subject must be able to bite and the object must be biteable in actives; the subject must be biteable and the indirect object must be able to bite in passives). But supplementing the phrase structure rules of 3 with the following transformation (rather than with the rules of 8) gives a much more satisfactory account of the obvious correspondences between 7a and 7b, between 9a and 9b, and even between the anomalous 10a and 10b.

- 11 $NP_1 V NP_2 \Rightarrow NP_2 be -en V by NP_1$

(The subscripts simply mark NPs which are identical on either side of the double arrow.)

In a grammar organized along these lines—a transformational grammar—the phrase structure rules generate 7a (or 9a), which can then become the input for the transformation, Passive (rule 11), with 7b (or 9b) as the output, or it can “surface” without engaging 11 at all.

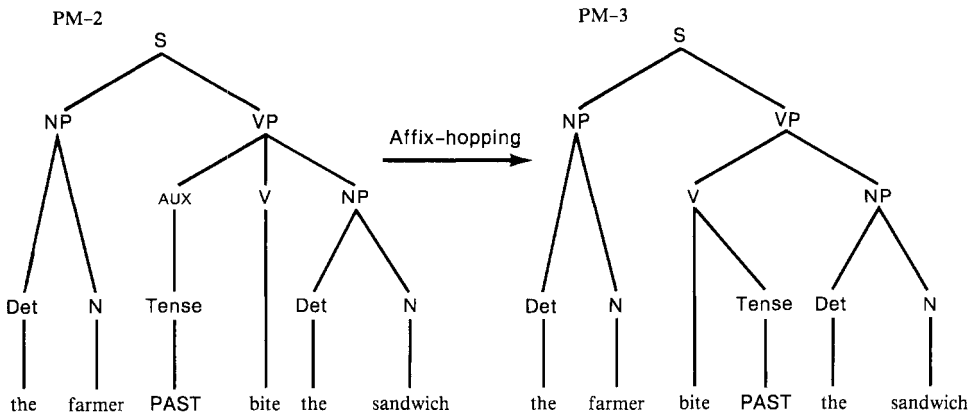
In short, a transformational grammar explains the systematic correspondences between actives and passives by deriving them from the same source.⁶

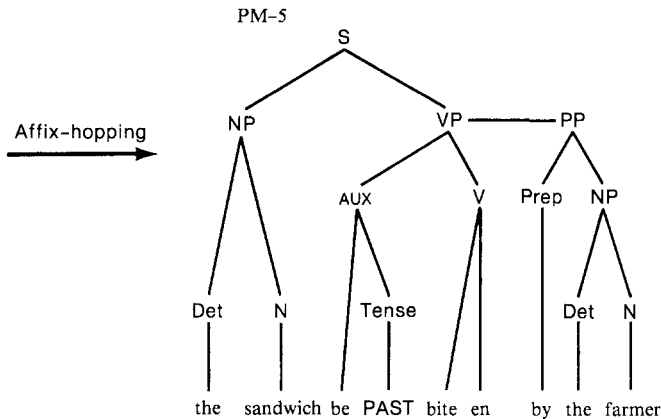
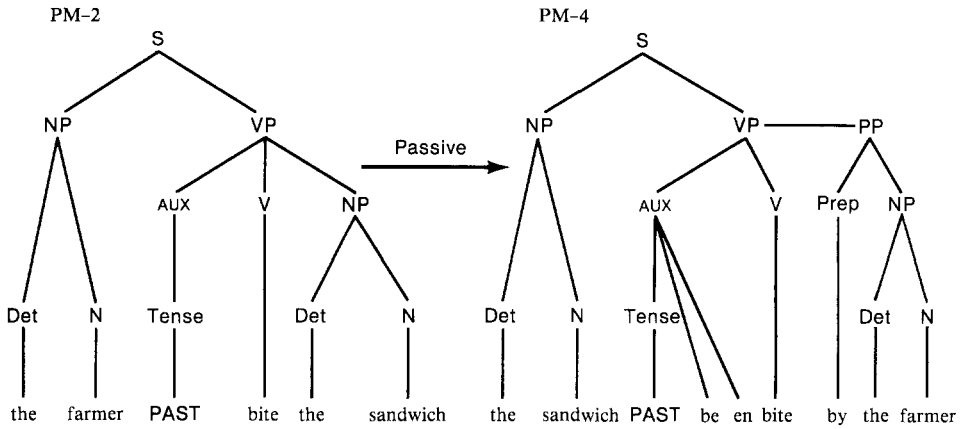
But 11 can’t do the job on its own: since the rule introduces *be* and *-en* side-by-side and since passive sentences have a verb in between them, Affix-hopping also needs to apply in the derivation, gluing the *-en* onto the butt of the main verb. Chomsky’s transformations occur in tandem. They are ordered; in this case, Passive applying before Affix-hopping. Notice, then, that we have another—and in terms of the subsequent history of the field, a much more important—application of the notion, “derive,” to consider. We spoke earlier of a tree (or phrase marker) as derived from phrase structure rules. Now we are talking about the derivation of a sentence, the transformational derivation of a sentence. In fact, with 9a and 9b we are talking about two transformational derivations, one in which Passive applies, one in which it doesn’t. For 9a, only one rule applies, Affix-hopping, so its derivation is relatively simple—though quite abstract, since Affix-hopping moves the tense marker, PAST, over *bite* and the final result doesn’t really have an affix at all. For 9b, two rules apply, Passive and Affix-hopping, in that order, making for a slightly more complicated derivation.

Moving up a level of abstraction to phrase markers, consider this process graphically, as shown in PM-2 through PM-5.

In the first derivation (PM-2 ⇒ PM-3), only Affix-hopping applies; in the second derivation (PM-2 ⇒ PM-4 ⇒ PM-5), two rules apply, Passive and Affix-hopping. The job isn’t complete here—later sound-based rules have to apply in order to get *bit* out of *bite* + PAST, to get *was* out of *be* + PAST, and to get *bitten* out of *bite* + *-en*—but these were all quite straightforward in the Bloomfieldian sound-and-word scheme of things.

In both cases, the rules ensure the quintessential Bloomfieldian goal of getting the distributions right, but the most important feature of these two derivations for





many people is that they start from the same place (PM-2): two derivations from a common source, yielding two distinct but clearly related sentences corrects “a serious inadequacy of modern linguistic theory, namely, its inability to account for such systematic relations between sentences as the active-passive relation” (Chomsky, 1962a [1958]:124).

In sum, phrase structure rules establish basic patterns and introduce words; they say such things as “a determiner followed by a noun is a legitimate noun phrase” (rule 3b, $NP \rightarrow Det + N$),” and “*the duckling* is a legitimate example of that pattern” (rules 3d, $Det \rightarrow the$, and 3e, $N \rightarrow \{ \dots duckling, \dots \}$). Transformations alter those basic patterns to account for a wider range of sentences and phrase types; they say such things as “if *the farmer killed the duckling* is a legitimate English sentence, then so is *the duckling was killed by the farmer*” (rule 11, Passivization, $NP_1 V NP_2 \Rightarrow NP_2 be-en V by NP_1$, along with rule 5, Affix-hopping, $Af V \Rightarrow V Af$, which helps get the affix-and-verb order right).

The grammar that emerges from Chomsky’s discussion is extremely rudimentary, accounting for only the tiniest fragment of English. Chomsky sketches a num-

ber of other transformational solutions to syntactic problems, and outlines a division of labor into singular and generalized transformations; the former for such phenomena as affix-placement and active-passive relations, the latter for such phenomena as relative clauses and conjoined clauses, capturing the intuition that sentences 12a (with a relative clause) and 12b (two conjoined clauses) are “made up of” 13a and 13b.

- 12 a Logendra abused the duck which had buzzed him.
 b The duck buzzed Logendra and he abused it.
- 13 a Logendra abused the duck.
 b The duck buzzed Logendra.

But even after Chomsky has laid out a nice sample of equally appealing solutions, the case for transformational grammar in *Syntactic Structures* is grossly underdetermined; the book is in many ways, remember, a summary of his massive *Logical Structure*. Still, by the time Chomsky is through: (1) the only other explicitly proposed generative grammar (the Hockett-endorsed finite state grammar) is disconfirmed; (2) the case for phrase structure rules working on their own (therefore, Immediate Constituent analysis) is eviscerated; and (3) the outline of a very powerful, novel approach to syntax is served up in a few, short, compelling strokes. This approach (schematized in Figure 3.2⁷) does the main Bloomfieldian work better than any previous syntactic model and does a few additional jobs to boot.

A set of phrase structure rules generates a core of underlying phrase markers, which feed into a set of transformations that massage them into their final, observable shapes, the ones we talk and write with (with all the affixes in place, for instance): the system purring harmoniously to generate all and only the grammatical sentences of a specific language.

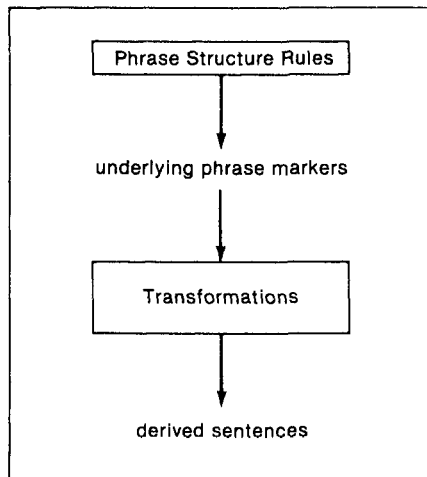


Figure 3.2. The transformational grammar sketched in *Syntactic Structures*.

There is still something missing from this picture, however: a privileged notion that Chomsky inherited from Harris and subtly altered, the kernel sentence. For Chomsky, the kernel sentence hinges on the fact that transformations come in two flavors, obligatory (like Affix-hopping) and optional (like Passive). Obligatory transformations go to work whenever their structural requirements are met (that is, whenever the conditions on the left of the arrow occur; for Affix hopping, whenever the sequence “Af + V” shows up in a derivation). Optional transformations only go to work sometimes, without any real guiding mechanism (so, Passive would apply some of the times that its structural requirements are met, some of the times that the phrase structure rules generated the sequence “NP V NP”).

All generalized transformations are optional.

The optional/obligatory distinction may look peculiarly unnecessary, but consider the alternative. If Passive and Affix-hopping, for instance, weren't different in this regard, the model would be in all kinds of trouble—generating some sequences that aren't English, and failing to generate some that are. If Affix-hopping were optional, then the grammar would produce gibberish like “Andrew is -ing skateboard,” since the affix would fail to be moved. If Passive were obligatory, then the grammar would fail to produce sequences like “The dog bit the mailman,” since every time the phrase structure rules generated such a sequence, Passive would turn it into a passive.

If the generalized transformations (the ones which made complex sentences out of simple ones) were obligatory, then the grammar would again fail to produce some sentences (namely, all simple ones, since the relevant transformations would necessarily combine them all).⁸

The distinction was crucial, which is where the kernel comes in.

Kernel sentences in the *Syntactic Structures* model are those derived sentences which had only undergone obligatory transformations. More than just kernels, they were also said to be the kernels *of* other sentences—parallel ones which had undergone optional transformations. A derived active sentence, then, was the kernel sentence of a derived passive (7a for 7b, for instance, and 9a for 9b). Two or more derived simple sentences were the kernels of a derived complex sentence (13a and 13b for 12a, for instance, and also for 12b). All of this probably sounds unduly complicated; the important point is simply that the grammar generated two classes of sentences, kernels and everything else, and that kernels had more cachet.

The kernel was the seed of meaning in transformational grammar.

The Appeal of Meaning

We should like the syntactic framework of the language that is isolated and exhibited by the grammar to be able to support semantic description, and we shall rate more highly a theory of formal structure that leads to grammars that meet this requirement.

Noam Chomsky

Chomsky's distributional interests—virtually inevitable under the tutelage of Harris—were not the only elements of his Bloomfieldian heritage. He also had a deep

methodological aversion to meaning, and his work reinforced one of the key elements of the Bloomfieldian policy toward meaning: it had to be avoided in formal analysis.

But *Syntactic Structures* was instrumental in reversing a far more problematic trend in Bloomfieldian linguistics: that meaning was unavailable for study. To some extent, Chomsky was catching a wave. Just as syntax saw increased action in the fifties, meaning was making a tentative comeback from the periphery. The anthropological linguist, Floyd Lounsbury, was beginning his soon-to-be influential work on componential analysis (1956, 1964 [1962]). The missionary linguist, Eugene Nida, had published his “System for the Description of Semantic Elements” (1951), in the European emigré journal, *Word*. Dwight Bolinger had even argued (also in *Word*) that, as defensible or desirable as meaning-aversion might be in phonology, it was a handicap for higher levels of analysis. “Meaning is the criterion of the morpheme,” he said, and, therefore linguists have a duty to “develop a theory of meaning and apply it consistently” (1950:120). Martin Joos had even hailed Harris’s transformational analysis as “a beginning . . . on a structural semantics,” calling it “the most exciting thing that has happened in linguistics for quite a few years” (1957:356).

Joos’s characterization is off the mark for Harris, but Chomsky’s extension of Harris *can* be viewed as such a beginning. Chomsky says his work was, from the outset, “an effort to construct a semantically responsible theory of language,” and the way to tackle meaning for him is through structure:

The focus in both *LSLT* [*Logical Structure of Linguistic Theory*] and *Syntactic Structures* is on trying to figure out what formal structures languages must have in order to account for the way we understand sentences. What’s the point of trying to figure out what the structures must be, except to see how they mean? The evidence is all semantic evidence. The facts are: Look, we understand the sentences this way, that way, the other way. Now how must language be working so as to yield those results?⁹

The structure of utterances—syntax—has long looked like the way to study meaning. That was the route taken by the Modistae, for instance, and by most philosophers of language in this century. For good reason: whatever sounds and words do, however they function in language, it takes syntax to make assertions and claims about the world, to really mean something. *Apple* is an orthographic symbol which stands in for a certain class of fruit, but it doesn’t get seriously involved in meaning until it participates in a structure like “John ate an apple” or “Did John eat an apple?” or “Who ate an apple?”—to borrow some of Chomsky’s examples in *Syntactic Structures* (1957a:71). In other terms, turning the chair briefly over to one of the most accomplished syntacticians ever, Otto Jespersen, the Bloomfieldian strongholds of phonology and morphology look at language from the outside; not syntax. Syntax “looks at grammatical facts from within, that is to say from the side of their meaning or signification” (Jespersen, 1954.2:1).

Back to Chomsky: “This purely formal investigation [in *Syntactic Structures*] of the structure of language has certain interesting implications for semantic studies” (1957a:12).¹⁰ And, after he has established their syntactic worth, Chomsky proceeds to argue for transformations in explicitly semantic terms. For instance, he asked his readers to consider the phrases in 14.

- 14 a the shooting of the hunters
 b the flying of the planes

Both of these phrases are ambiguous between readings where the nouns are objects of the verbs (the hunters are being shot, the planes are being flown) and where they are subjects (the hunters are shooting, the planes are flying). Again, we are faced with a problem about which the Bloomfieldian program has little to say, but which reflects clear facts of English, and again, transformational grammar has an answer. The best the Immediate Constituent approach can do with phrases like this is—using the phrase structure apparatus Chomsky supplies—to treat them as members of the same class, with the structure given in 15.

- 15 *the V -ing of NP*

But transformational grammar can easily formulate rules of the following sort:

- 16 a $NP\ V \Rightarrow \textit{the V -ing of NP}$
 b $NP_1\ V\ NP_2 \Rightarrow \textit{the V -ing of the NP}_2$

Transformation 16a changes an NP like “the hunters” followed by a V like “shoot” into structures like 14a; transformation 16b changes structures like “someone shoots the hunters” into the same structure. That is, the two senses of 14a each have a distinct transformational history—the same post-transformational structures, but two different pre-transformational structures—offering an explanation for the ambiguity.

Chomsky’s goal is to chart a small part of the huge and daunting semantic rain-forest, to construct a “theory of linguistic structure [which] may be able to incorporate a part of the vast and tangled jungle that is the problem of meaning” (1957b:290). The ambition is a guarded one to be sure, but far more enterprising than Bloomfield’s attempt to turn his back on meaning altogether, shucking it off on other disciplines. *Syntactic Structures* offers an impressive general outline of how linguists could begin to talk meaningfully about meaning, and it is clear in retrospect that many linguists found this outline to be the single most compelling feature of Chomsky’s program. Three of his most prominent recruits, in particular—Paul Postal, Jerrold Katz, and Jerry Fodor—soon set to work on an explicit incorporation of semantics into the *Syntactic Structures* model, and it was this work which inspired the more thorough incorporation of meaning that defined the appearance of generative semantics.

The Bloomfieldians were ready for Chomsky. They were ready for his notions of science—explicitly defining a grammar of a language as a theory of that language, subject to the criteria for any theory: simplicity, generality, testability. In fact, Hockett had said pretty much the same thing a few years earlier (1954:232–3). They were ready for his advances in syntax. No area of linguistics was more ripe—indeed, overripe—for investigation, and everyone knew it. They were even ready, despite the injunctions of their great, defining, scientific benefactor, Leonard Bloomfield, to follow Chomsky’s (or, in their minds, Harris’s) transformations into the uncharted jungle of meaning—well, into the edges of that jungle. Hill says that most of the leading linguists of the period, while all followers of Bloomfield, were nev-

ertheless all “eager to break into semantics when they felt it possible” (1991:79),¹¹ and one of Bloch’s students in the fifties recalls that even Bloch, an old wouldn’t-touch-meaning-with-a-ten-foot-pole hardliner if there ever was one, “was poised to accept semantics,” at least in the tightly manageable, formal methods of symbolic logic. It’s just that he, along with perhaps most of the defining Bloomfieldian theorists, “didn’t feel up to doing it himself. He said he would wave encouragement as the logicians took off.” Certainly, he waved encouragement as Chomsky took off.

The Bloomfieldians were ready for some elaboration of their program, some revisions and extensions. They were ready for *Syntactic Structures*. They weren’t ready for a replacement. They weren’t ready for what followed *Syntactic Structures*.

Chomsky *Agonistes*

I was told that my work would arouse much less antagonism if I didn’t always couple my presentation of transformational grammar with a sweeping attack on empiricists and behaviorists and on other linguists. A lot of kind older people who were well disposed toward me told me I should stick to my own work and leave other people alone. But that struck me as an anti-intellectual counsel.

Noam Chomsky

There are myths aplenty in linguistics these days surrounding Chomsky’s spectacular rise, celebrating his brilliance and prescience, his predecessors’ obtuseness and dogmatism. We have already seen the finished-field myth, which, if we take Harris to fill Planck’s shoes, puts Chomsky in Einstein’s. There is also that recurrent feature of scientific breakthrough stories, the Eureka Moment, Chomsky’s moment putting a nice twist on the archetypical Archimedes in his more literal tub:

I remember exactly the moment when I felt convinced. On board ship in mid-Atlantic, aided by a bout of seasickness, on a rickety tub that was listing noticeably—it had been sunk by the Germans and was now making its first voyage after having been salvaged. (Chomsky, 1979 [1976]:131)

Less dramatically—with neither nausea nor Nazis—but still good copy, Chomsky’s introduction to linguistics is said to have come by way of reading the proofs to Harris’s dense, highly technical *Methods in Structural Linguistics* (1951 [1947]), which is roughly akin to an introduction to mathematics by way of Russell and Whitehead’s *Principia*. After this abrupt immersion, the stories go, he toiled in virtual obscurity, turning out masterpieces, first for the uncaring eye of Zellig Harris, then for the indifferent publishing world, convinced all the while that his work would never amount to more than a private hobby. Fortunately for science, however, clear-eyed and forceful supporters persuaded him that he owed his work to the world, as Copernicus’s supporters had persuaded him, and Darwin’s supporters, and, in the most extreme case of reluctance overcome, Saussure’s exhuming supporters. When he followed this advice, he was confronted by phalanxes of blindly opposed Bloomfieldians, whom he demolished effortlessly, enfeebling the arguments and dumbfounding the arguers. He is said, in short, to have rescued linguistics from a long dark night of confusion, to have pulled back the curtain Bloomfield

mistakenly drew over the mind; to have finally—and we could see this one coming for some time—made linguistics a science.

Like all good myths, these ones are true, and, of course, false.

To fan away the unpleasant smell that usually attends such comments: the falseness of these origin myths doesn't involve specifics. There is no implication here, in the word *myth* or even the word *false*, that anyone is a liar. Harris may or may not have had the feeling that linguistics was so successful it was about to go out of business as a science (Harris was an inscrutable character), but Chomsky certainly developed that impression himself, working under Harris. Nor was the impression exclusive to him and (possibly) Harris; the finality of John Carroll's early fifties overview of linguistics, for instance, recalls Lord Kelvin's remarks that physics had little more to look forward to than increasingly precise measurements—"Since the publication of Bloomfield's work in 1933, theoretical discussions among linguistics have been largely on matters of refinement" (1953:30). And if the proofs of Harris's *Methods* did not constitute Chomsky's first exposure to linguistics (his father, William, was a respected Semitic philologist; little Noam was reading historical linguistics by the age of ten and studying Arabic in his teens), they were certainly his first serious exposure to the themes, techniques, and motive forces of Bloomfieldianism; he had not, for instance, taken so much as a first-year college course in structural linguistics.

And his transformational-generative research was carried out in relative obscurity: Holding a prestigious fellowship at Harvard, he was a lively, precocious, influential member of an early fifties intellectual scene in Cambridge that included philosophers like W.V.O. Quine, Nelson Goodman, and J. L. Austin, psychologists like George Miller, Jerome Bruner, and John Carroll, and itinerant intellectuals like Benoit Mandelbrot and Marvin Minsky; but his ties with linguists were limited and unorthodox. He was at least as isolated from the Bloomfieldian community as, say, Saussure in Geneva was from the neogrammarians, or Sapir in Ottawa was from the Boasian community. And he certainly produced masterworks in this obscurity—most notably, the massive *Logical Structure of Linguistic Theory*. (It isn't clear how indifferent either Harris or the publishing world was to his efforts, but *Logical Structure* wasn't published for another twenty years; see Chomsky, 1975a[1973]:1ff, 1988b:78n2; Newmeyer, 1980a:33–35, 1986a:28–31; Murray, 1980.)

And a small group of supporters (most notably, Morris Halle and philosopher Yehoshua Bar-Hillel) undoubtedly convinced him that his ideas were valuable, not just as his own cerebral toy, but for the entire field. And the generative light bulb surely clicked on for him exactly where he remembers it clicking on, above a sea green face, reeling and listing in mid-Atlantic. And, along with the accommodation of Bloch and others, Chomsky also encountered resistance, increasingly vociferous resistance as he developed and spelled out the implications of his thought for the Bloomfieldian infrastructure. And Chomsky dealt with the resisters very effectively, if not to the satisfaction of all his opponents, certainly to the satisfaction of a far more crucial element in the debate (in any debate), its audience; Chomsky is one of the hardest arguers in modern thought. The supporters and resisters and supporters-cum-resisters among the old guard were swept aside indiscriminately, if not

by Chomsky, certainly by the quickly growing cadre of transformationalists in the audience. And, while linguistics was a science before he came along—as it was before Jones, and Saussure, and Bloomfield came along—it was, also as with those men, a much different science once his ideas took root.

No, the falseness is not in details. It is in the routinely extreme interpretations put on these details by the great majority of post-revolutionary linguists: that the study of language begins in real terms with Chomsky; that all linguists before him “were hopelessly misguided bumbling, from whose clutches Chomsky has heroically rescued the field of linguistics” (Lamb, 1967:414). Listen to Hockett’s bitter lament:

I . . . view as genuinely tragic the success of the “eclipsing stance” of [Chomskyan linguistics.] We have currently in our ranks a large number of young people, many of them very bright, from beginning students up to and including a few full professors, who know nothing of what happened in linguistics before 1957, and who actually believe (some of them) that nothing *did* happen. (Hockett, 1980:105)¹²

Hockett has reason to complain—not least because he was the Bloomfieldian-most-likely, the late master’s favored son, and he was, along with Nida, Householder, Hill (even, aside from a sort of John-the-Baptist role in linguistic folklore, Harris)—pretty much swept aside in the prime of his career. None of this is new, of course, nor peculiar to science. “The first eruption,” Priscilla Robertson says, in her nice refraction of Tocqueville’s volcano image for the 1848 French revolution, blew off “not only the King but also, indifferently, the top layer of men who had hoped to reform the monarchy and who had by their criticism helped prepare for the revolution” (1952:14), an observation which generalizes to almost every abrupt social or scientific shift. Among the more spectacular political examples this century has provided, two from Russia spring most readily to mind, Kerensky and Gorbachev.

If many linguists’ view of history is not exactly tragic, then, a word more appropriate for the daily curses of lives much harsher than the ones lived out in academic hallways—in revolutionary France, for instance, and in the turbulence and oppression surrounding the various revolutions in Eastern Europe, and in South-Central Los Angeles—it is certainly wrong. The falseness of the Chomskyan myths, again, resides in the general mood enveloping their ritual retellings that all was for naught between the 1933 publication of Bloomfield’s *Language* and the 1957 publication of Chomsky’s *Syntactic Structures*.

But part of their truth resides here as well. Bloomfield and his progeny had not ushered in a linguistic night of the living dead, grammar-zombies lurching from longhouse to longhouse, stiffly cataloging the phomemes, morphemes, and rudimentary syntactic patterns of language after language after language after language. But things *were* getting a little mechanical. And, more crucially, the most compelling aspects of language had not only been relegated to the bottom of a very long-term agenda, they had been given over to other disciplines altogether. Meaning and mind could be treated only in the distant future, and only by sociologists, psychologists, ethnologists; seemingly, everyone but linguists were given Bloomfield’s license to hunt down meaning in the deer park of the mind. Linguists had to stick to their sounds and words.

Chomsky—and here another aspect of the myths' truth resides—almost single-handedly shook linguistics free of its description-sodden stupor, and gave linguists leave to talk about meaning, and to talk about mind; indeed, the force of Chomsky's argument on the latter point was such that linguists were virtually the only ones with leave to talk about mind. *Almost* single-handedly. He was not without coworkers and proselytes—most notably, Morris Halle and Robert Lees—who fed his theories, and milked them, and brought his wares to market. Nor would it do to forget that there *was* a market; that Saussure and Bloomfield and Harris had made the mathematicization of linguistics possible; that Harris and Wells and Trager and Smith were making some headway with syntax; that Nida and Lounsbury and Bolinger were clamoring about meaning. It certainly makes some sense to talk of language studies BC, Before Chomsky, but the linguistic calendar, even for generative and transformational and semantic notions, does not begin in 1957.

Calendars aside, Chomsky is the hero of the story. He is a hero of Homeric proportions, belonging solidly in the pantheon of our century's finest minds, with all the powers and qualities thereof. First, foremost, and initially, he is staggeringly smart. The speed, scope, and synthetic abilities of his intellect are legendary. "Most of us guys who in any other environment would be absolutely brilliant," one colleague says, "are put to shame by Noam." He is dedicated to his cause, working long, full hours; in fact, he is dedicated to a constellation of causes, linguistic, psychological, and philosophical (and social; like Russell and Einstein, Chomsky has deep political convictions, for which he also labors tirelessly). He is, too, a born leader, able to marshal support, fierce, uncompromising support, for positions he develops or adopts. (Inversely, he is many linguists' Great Satan, certain to marshal fierce, uncompromising opposition to almost anything he says or does.) Often, it seems, he shapes linguistics by sheer force of will. And—the quintessential heroic trait—he is fearless in battle.

Peeling Off the Mentalist Blinders

HILL: If I took some of your statements literally, I would say that you are not studying language at all, but some form of psychology, the intuitions of native speakers.

CHOMSKY: That is studying language.

Exchange at the Third Texas Conference

The first unmistakable battleground of the Bloomfield-to-Chomsky changing of the guard was mentalism, though it is unmistakable only in retrospect. The generative challenge to mentalism looms so large in the rearview mirror that it is difficult to see how the old guard missed it. But they did.

Despite a general expansion of Bloomfieldian interests, mentalism was still taboo. Morris Swadesh, for instance, published a stinging attack on "the fetish that anything related to the mind must be ruled out of science" (1948; cited in Hymes and Fought, 1981:159). Swadesh was one of Sapir's most respected students. He had a formidable reputation in fieldwork and several influential papers, including one of the earliest distributional discussions of the phoneme (1934). Yet his critique

couldn't even make it into a linguistics journal (it was published way out of the mainstream, in *Science and Society*), and had absolutely no impact on the field. Even the increased linguistic interest in psychology that marked the early-to-mid-fifties, spawning the term *psycholinguistics*, was distinctly behaviorist, psychology without the mind.

Chomsky came to see any study of language that didn't attend to its mental tentacles as completely sterile, and began promoting linguistics as a fundamentally psychological enterprise, coupling this promotion with a crushing attack on behaviorism.¹³ The triumph on both fronts was staggering. Within a few years, behaviorism, Bloomfield's inspiration for a new and improved science of language, was virtually extinguished as a force in linguistics, invoked only in derision. It was also in rapid retreat at home, where psychologists hailed Chomsky as a champion in the promising emergent program, cognitive psychology (the term is too complex for proper treatment here, but, very roughly, cognitive psychology is oriented around the systems of knowledge behind human behavior; in principle, it is completely the inverse of behaviorism).

There were some murmurs of dissent toward behaviorism in mid-fifties psychology, especially in Cambridge, out of which the new approach was emerging, an approach whose birthday, according to George Miller, is 11 September 1956, the second day of a symposium at Harvard which ended with Chomsky outlining the arguments behind *Syntactic Structures*. We can't be sure what Chomsky said in that lecture, but his attitude to behaviorism at the time is apparent in *Syntactic Structures*' unambiguous rejection of "the identification of 'meaning' [that Bloomfield effects in his foundational tome—1933:22–32] with 'response to language'" (1957a:100). Chomsky was in fact extremely important to the emergence of cognitive psychology. In particular, his arguments against behaviorism (published a few years later in a review of Skinner's *Verbal Behavior*) were considered absolutely devastating.¹⁴ Like most of Chomsky's finest arguments, his case against Skinner is as effective emotionally as it is intellectually. The reaction of Jerome Bruner, one of the founding voices of the cognitive psychology movement, is representative. He recalls the review in very charged terms: "Electric: Noam at his best, mercilessly out for the kill, daring, brilliant, on the side of the angels . . . in the same category as St. George slaying the dragon" (1983:159–60).

Dragon does not overstate the case. Behaviorism was tied up with some ethical perspectives that many intellectuals in the fifties were beginning to see as irredeemably vicious. There was, in the wake of the bloodshed and madness early in this century, a great deal of interest in the human sciences about the control of individuals and groups. Some of this interest was manifestly evil, where *control* meant *building better soldiers* or *making citizens more docile*, but much of it was very well intentioned, with the goal of happier, less aggressive, more fulfilled people, individually and collectively: in both cases, evil and good, behaviorist psychology, stimulus-response psychology, was the shining light of these interests. It held out the mechanical promise that getting people to behave would just be a matter of finding out which buttons to push, and pushing them. If you wanted a certain response, behaviorists would find the right stimulus for you. And linguists, since language is the cheapest, most omnipresent stimulus, were very concerned observers of this

project. Bloomfield, for instance, heartened the troops in his 1929 LSA address with this prediction:

I believe that in the near future—in the next few generations, let us say—linguistics will be one of the main sectors of scientific advance, and that in this sector science will win through to the understanding and control of human conduct. (1970 [1929]:227)

With the stunningly bad behavior of the Second World War—millions dead in Europe, apocalyptic explosions over Asia—segueing into the worldwide existential trembling of the cold war, and with the ever-growing reverence for science that accompanied these events, some linguists' faith in the powerful future of their field increased until they found themselves "at a time when our national existence—and possibly the existence of the human race—may depend on the development of linguistics and its application to human problems" (McDavid, 1954:32).¹⁵

Nowadays, there is a disturbingly Orwellian ring to such talk, even in its best-intentioned varieties. Understanding human conduct is fine, desirable in fact, but *control* had begun to stir a chilling breeze in the fifties (cued, in part, by the publication of *Nineteen Eighty-Four*). *Control* and its various synonyms (*manipulate*, *cause*) therefore play a large role in Chomsky's review, as does Skinner's principal source of authority, his bar-pressing rodent experiments. The first mention of Skinner, stuck awkwardly (therefore, prominently) into a more general discussion, is this sentence: "Skinner is noted for his contributions to the study of animal behavior" (1959 [1957]:26). Animals, especially rats, recur incessantly thereafter, Chomsky repeatedly stressing the vastness of the gulf between a rat navigating a maze for a food pellet and even the most elementary verbal acts.

Even Bloomfield, in the heady early days of behaviorism, realized the distance between a stimulus and a response in linguistic terms was formidably wide; that was the chief reason he outlawed meaning (considered, essentially, as the response to some stimulus) and mind (the mediative organ between stimulus and response). But Chomsky tattoos home the point that this gulf renders a stimulus-response, billiard-ball model of language completely vacuous:

A typical example of a *stimulus control* for Skinner would be the response to a piece of music with the utterance *Mozart* or to a painting with the response *Dutch*. These responses are asserted to be "under the control of extremely subtle properties" of the physical object or event [Skinner, 1957:108]. Suppose instead of saying *Dutch* we had said *Clashes with the wallpaper, I thought you liked abstract work, Never saw it before, Tilted, Hanging too low, Beautiful, Hideous, Remember our camping trip last summer?*, or whatever else might come into our mind when looking at a picture. . . . Skinner could only say that each of these responses is under the control of some other stimulus property of the physical object. (1959 [1957]:31; Chomsky's italics; the interpolation replaces his footnote)

Chomsky is on the side of the angels here, all right, St. George to Skinner's dragon, but he is also on the side of free, dignified, creative individuals, people who belong to a tradition that includes Mozart and Rembrandt, people who cannot be controlled: his audience.¹⁶

The intellectual aspects of Chomsky's case, complementing the emotional aspects, are wide-ranging and damning. The long review has a steady commentary

bulging from its footnotes, some of it bitingly glib (“Similarly, ‘the universality of a literary work refers to the number of potential readers inclined to say the same thing’ . . . i.e., the most ‘universal’ work is a dictionary of clichés and greetings.”—52n42), but most of it detailing the counter-evidence, qualifications, and questionable claims Chomsky has gleaned from the vast literature of learning theory; Chomsky, the reader can never forget, has done his homework. The most effective part of Chomsky’s attack for almost every reader, however, is not the extent of the counter-evidence he marshals, but the two brief and devastating arguments he levels at behaviorism. One argument is based on the notion of creativity. The other goes by the name (presumably inspired by the dragon’s own terminology), the *poverty of stimulus argument*.

Chomsky is a steadfast champion of creativity in the review, a notion broad enough to evoke Mozart and the Dutch Masters in its own right, but which has a very specific, narrow, and technical meaning in his work, coupled intimately with generative grammar. With a moment’s reflection (as the conventional argument in an introductory course in Chomskyan linguistics now runs), it is clear that there are innumerable grammatical pieces of potential verbal behavior which have never been performed before, innumerable grammatical pieces of language which have never been uttered, never been a stimulus, never been a response; for instance,

17 Nanook put a pinch of yellow snow between his cheek and gums.

A simple behaviorist model has huge difficulties accounting for such facts. The sentence is not just unpredictable, in the sense of “Remember our camping trip last summer?” It is *unique*. Yet speakers of English have no trouble recognizing 17 as a legitimate, if unsavory, sentence of their language. They understand it immediately, and they would have no trouble, in the unlikely event that the circumstances become appropriate, producing it themselves. In a word, sentence 17 illustrates that human grammars are *creative*: they produce output which is not part of their input.

Output and input are important for Chomsky because he came to see the single most important factor about human language to be the ability children have to move rapidly from the input data of language they hear to a full command of that language, to a controlled and grammatical output. From this review on (anticipated by Lees, 1957, and to some degree by Hockett, 1948), language acquisition becomes an essential component of Chomsky’s argumentation: the central problem for linguistics to solve, Chomsky insists, is how this creative ability establishes itself so quickly in the brain of a child. This problem is the one with which he most successfully flays not only Skinner but all things Skinnerian.

Behaviorist learning theory, Chomsky says, is based on a “careful arrangement of contingencies of reinforcement” and on the “meticulous training” behaviorists regard as “necessary for a child to learn the meanings of words and syntactic patterns” (1959 [1957]:39). This position, he hastens to add, “is based not on actual observation, but on analogies to lower organisms,” so we are behooved to see if in fact these ingredients are necessary. They aren’t. As the poverty of stimulus argument goes, one of the most remarkable facts about human languages—which are highly abstract, very complex, infinite phenomena—is that children acquire them in an astonishingly short period of time, despite haphazard and degenerate data (the

“stimulus”). Children hear relatively few examples of most sentence types, they get little or no correction beyond pronunciation (often not even that), and they are exposed to a bewildering array of false starts, unlabeled mistakes, half sentences, and the like. Sounds and words, the principal Bloomfieldian units of focus, are amenable to stimulus-response acquisition; the child says “ice cream” and gets some ice cream. Syntax, Chomsky’s natural medium, is not. Sentences are too variable, too dynamic, too creative, to have any significant correspondences to a rat and its bar.

Neither psychologists nor philosophers (to whom the review is also pointedly addressed—1959 [1957]:28) would have had any difficulty seeing the significance of Chomsky’s critique for contemporary views of the mind. With linguists, the matter isn’t so clear. For one thing, Bloomfieldians had a poacher-shooting tradition, and many were probably happy to cheer an up-and-comer’s participation in the sport, thrashing a big-shot psychologist with the audacity to hunt in the preserve of linguists; Bloch, who published the review, “delighted [in] this superb job of constructive destruction” (Murray, 1980:80). More importantly, psychology was largely peripheral for most Bloomfieldians. It is noteworthy, for instance, that no other linguist reviewed *Verbal Behavior*, which was published two years before Chomsky’s bludgeoning. And the constructive part of Chomsky’s assault, the part that really threatened Bloomfieldian assumptions, was still somewhat amorphous in 1959. Poverty of stimulus has long been a well-known fact of language (Whitney had observed that children generally “get but the coarsest and most meagre of instruction”—1910 [1867]:12), but building a positive program around that observation was something new.

Chomsky started slowly. He ended his tanning of Skinner with the poverty of stimulus argument, but his clues for a replacement to behaviorist learning theory are suggestive at best:

The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity suggests that human beings are somehow specially designed to do this, with data-handling or “hypothesis-formulating” ability of unknown character and complexity. (1959 [1957]:57)

Chomsky adds the invitation that

The study of linguistic structure may ultimately lead to some significant insights into this matter.

And he thereby—with his *somehow* and his *unknown character*—makes it clear that the door is now open, for anyone bold enough to follow him through, on the exploration of mental structure. The door is open for a younger generation, but it is not yet closed on the older one. Chomsky’s review rehabilitates mentalism in the clearest terms since Bloomfield eclipsed Sapir on language and the mind, but it does not spell out in any detail the essential differences between Chomsky’s view of mental structure and Skinner’s view. These differences, when he does spell them out over the next few years, cut to the very bone of the Bloomfieldians’ picture of science; therefore, of themselves as scientists.

Meanwhile, the Bloomfieldians had more to worry about than Chomsky’s skin-

ning of the behaviorist dragon in 1959, the year Morris Halle published his *Sound Pattern of Russian*.

Morris Halle and the Phoneme

I could stay with the transformationalists pretty well until they attacked my darling, the phoneme.

Archibald Hill

Chomsky met Morris Halle in 1951. They “became close friends, and had endless conversations” over the next several—extremely formative—years (Chomsky, 1979 [1976]:131). Like Chomsky, Halle was something of an outsider. Although he came to the U.S. as a teenager and later earned his doctorate from Harvard, his intellectual heritage—especially what it meant to be a “structuralist”—was much more European than American. Certainly he never swam, or even waded, in the Bloomfieldian mainstream. His doctorate was under the great Prague School structuralist, Roman Jakobson, from whom he inherited both mentalism and a certain friendliness to meaning (Halle’s influence on Chomsky in both these areas was very likely much more substantial than has generally been appreciated, though Chomsky also had a great deal of direct contact with Jakobson). His thesis was on the sound system of a venerable European language, Russian; there was no Amerindian imperative, no description-for-the-sake-of-description compulsion, and it was published (1959a [1958]) under a title that paid deliberate homage to Bloomfield’s partial rival, Sapir. Halle had also studied engineering for a while before entering linguistics, so there were mathematico-logical interests in his background, as in Chomsky’s, beyond those of most American linguists.

He helped Chomsky get his position at MIT. He was also instrumental in establishing first a transformational research group there, then a doctoral program in linguistics (under the auspices of Electrical Engineering), and finally an independent linguistics department, of which he became the first chair. And he joined Chomsky in his first clear challenge to the orthodoxy—a paper on English stress phenomena which challenged a critical Bloomfieldian assumption about the independence of phonology from other grammatical processes (Chomsky, Halle, and Lukoff, 1956).¹⁷

Most importantly, at least in the short run of the late 1950s, when Chomskyan linguistics was gaining its polemical stride, Halle had an argument.

The argument is highly corrosive to a cornerstone of the Bloomfieldian program, the phoneme, and many linguists, then as now, regarded it as absolutely devastating. For the emerging Chomskyan of the early sixties, the argument—or, as Sadock later called it (1976), the *Hallean syllogism*—was totemic, a clear and present sign that even the most respected and impressive, the most beloved, of Bloomfieldian results, was made of unfired clay. For the fading Bloomfieldians of the early sixties, the argument was exactly the inverse, a sign of absolute and unwarranted hostility to an object of scientific beauty, and it earned the new movement their undying enmity. The Bloomfieldian resistance movement begins here.

The Bloomfieldians could not claim sole proprietorship over the phoneme. It

crystallized in Europe, in Kazan and Prague, about the same time it was crystallizing in America, and the lines of influence are quite complicated. But it was their darling. Chomsky and Halle went after it like pit bulls (as did Lees, their student, who gave the first presentation of Halle's anti-phoneme argument at the 1957 LSA Annual Meeting).

Halle's argument is an impressive, persuasive, dismissive assault on a cornerstone of Bloomfieldian phonology, but it was neither powerful enough on its own to cheese off the guardians of linguistic orthodoxy nor compelling enough on its own to win over a band of revolutionaries. It was not, however, on its own. It came with an elegant new phonology, whose virtues Halle demonstrated in a winning treatment of the "highly complex patterns of phonological relationships in Russian" (Anderson, 1985:321). Negative arguments have a very short shelf-life, and, regardless of conviction and oratorical prowess, if they don't come with a positive program, there is little hope for widespread assent. Indeed, only a very weak form of assent is called for by an exclusively negative rhetoric—a consensus of dissent, a communal agreement that something is wrong, without a clear idea of how to put it right. Einstein and Schrödinger, as passionate, eloquent, and sharply reasoned as their attacks on probabilistic models of subatomic behavior were, had no remotely comparable program to offer if Bohr's work had been overturned. Their arguments failed. Scientists need something to do. Halle gave the new generation something to do.

Moreover, this new phonology, Chomsky and Halle both insisted, was part of a package. If you liked the syntax, and many people loved it, you had to take the phonology.

At this point, it was teeth-rattlingly clear to the old guard that they were, in fact, the old guard, that Chomsky, Halle, Lees, and the other MITniks (as the generationally charged term of derision tagged them) meant to shove them aside. Trager and Smith's codification of Bloomfieldian phonology (actually, Bloomfieldian *phonemics*; even the label has changed since the fifties) had a few loose belts, perhaps some squeaky pulleys, but it was the foundation upon which they thought syntax would have to be built. Even Sledd, who was fairly harsh about that phonology, spliced it to Fries's syntax for his textbook (1959), and Stockwell had proposed hitching it to Chomsky's syntax in 1958 (Hill, 1962c:122). Halle's *Sound Pattern of Russian*, and Chomsky's presentation of Halle's work in 1959—again at Texas—ruled this splicing out completely. It was all or nothing at all.

The Bloomfieldians, of course, were unmoved. The whole anti-phoneme argument rests on only a very few scraps of data—four words, both in Halle's original presentation (1959a [1958]:22-3), and in Chomsky's more famous representations (1964d [1963]:88-90; 1966b [1964]:78-82)—which hardly seems warrant enough to throw out twenty years of effort, and the data was known to be problematic before Halle worked it into his assault. Hallean phonology, cried Hockett, was "completely bankrupt" (1968 [1966]:3; 1965 [1964]:201-2). "Worse than 'bankrupt!'" Trager chimed in: "a product of a fantastic never-never land" (1968:80). They felt that the phoneme bought them more expressiveness than it cost, and were unprepared to discard it on the basis of a minor anomaly. Less rationally, Hallean phonology also borrowed rather heavily from Jakobson's work, and the Bloom-

fieldians had a history of antagonism toward the Prague Circle. But the argument was considered absolutely crushing by the Chomskys—primarily because it was embedded in a carefully developed and comprehensive phonological theory which fit more closely with their syntactic work (it was called generative phonology and had very close parallels to syntactic transformations).

The first concerted counterattack came from Fred Householder, one of the earliest supporters of transformational syntax, teaching it at Indiana and implementing a number of early innovations. But he drew the line at this new phonology, launching an urbane and nasty assault in the inaugural number of the new generative-flavored *Journal of Linguistics* (1965).

The response from Cambridge was immediate (the lead article in the very next number of the very same journal), extensive, and brutal (Chomsky and Halle, 1965).¹⁸ It is almost twice as long as Householder's original critique, and brimming with thinly veiled *ad hominem*s. Actually, it would be more accurate to call them *ad homineses*—attacks to the *men*—since Householder is recurrently taken to represent overall Bloomfieldian blockheadedness (pp. 103, 105, 106, 107n4, 109n6, . . .). Chomsky and Halle suggest that Householder and his ilk don't understand the nature of problems confronting the linguist, "or, for that matter, the physical scientist" (104). They turn his mock-humility (Householder regularly expresses puzzlement over Chomsky and Halle's arguments) back against him, implying incompetence (119, 127, 129n26). They hector him like a schoolboy ("To repeat the obvious once again . . ."—127n24; also 103, 133n27, 136). He is inattentive (126, 127, 128n25). He is confused (*passim*). He doesn't even understand Sapir's classic paper on the "Psychological Reality of Phonemes" (136; Sapir, 1949b [1933]:46–60). He trucks with inconsistencies, and "a linguist, who, like Householder, is willing to accept inconsistent accounts—in fact, claims that such inconsistency is ineliminable—has . . . simply given up the attempt to find out the facts about particular languages or language in general" (106): he isn't even doing linguistics. It is numerology (108).

Householder answered right away, but briefly and anemically, giving only a two-page policy statement reiterating some earlier points and wholly ignoring Chomsky and Halle's arguments. Hockett (1968 [1966]:4n3), for one, thought the reply sufficient, and Trager quotes Hockett approvingly, with a slight reproof to Householder for taking Halle's work seriously enough to dignify it with comment in the first place (1968:79, 80). But Chomskys, and most non-Bloomfieldian observers, considered the matter closed: Chomsky and Halle had been challenged, they answered the challenge, and completely dumbfounded the opposition.¹⁹ The new phonology was here to stay and one of the Bloomfieldians' most sacred possessions, the phoneme, was tagged as a worthless trinket.

There was more.

Enlisting the Grandfathers

It seems to me that the traditional analysis is clearly correct, and that the serious problem for linguistics is not to invent some novel and unmotivated alternative, but to provide a principled basis to account for the correct traditional analysis.

Noam Chomsky

Syntactic Structures was no threat to the Bloomfieldian program, so it must have been something of a surprise at the 1958 Texas conference—a deliberately staged contest of several emerging syntactic programs—when Chomsky came out battling. He was very active in all of the post-paper discussion periods, particularly so (and at his sharpest) following Henry Lee Smith’s presentation of the only real competitor to transformational syntax in terms of rigor or prestige, phonological syntax. His own presentation essentially condensed *Syntactic Structures*, but put more of an edge on its notions. The paper argues that transformations are an important advance over Immediate Constituent analysis, and that generative grammar is an important advance for the field as a science, and that transformational-generative grammar can make important semantic inroads—all the carrots come out.

But Chomsky also wove in his mentalist concerns (his review of Skinner was written in this period, but still to be published), introduced some noxious data for certain Bloomfieldian principles, and sketched Halle’s argument against the darling phoneme. He also said that Harris’s work on transformations brought to light “a serious inadequacy of modern linguistic theory”—the inability to explain structural relatives, like active and passive versions of the same proposition—and that this inadequacy was the result of ignoring a major “chapter of traditional grammar” (1962a [1958]:124). These two elements, explanation and traditional grammar, became the primary themes of his anti-Bloomfieldian rhetoric over the next few years.

The following year he came back to Texas with an exclusively phonological paper (“The discussions were animated and sharp.”—Chomsky, 1979 [1976]:133), establishing unequivocally that his program was a replacement of Bloomfieldian linguistics, not an extension.

The most pivotal event in the campaign against Bloomfieldian linguistics, however, was another conference, the 1962 International Congress of Linguists, where Chomsky was the invited speaker at the final plenary session. The four other plenary speakers that year were august Europeans (Nikolaj Andreyev, Emile Beneviste, Jerzy Kuryłowicz, and André Martinet), which gave young Chomsky “the appearance of being THE spokesperson for linguistics in the United States” (Newmeyer, 1980a:51; Newmeyer’s emphasis). He used the moment brilliantly, putting his work, on the one hand, into very sharp relief against the Bloomfieldian program, and, on the other, aligning it closely with traditional grammar, the amorphous pre-structuralist program which Bloomfieldians delighted in “grandly berating” (Sledd, 1955:399), but which was still favored in many parts of Europe. Better yet, the whole Bloomfieldian program, which left many Europeans sour, was subjected to a withering attack.

Chomsky’s paper, in these and many other ways, also makes inescapably clear that his work isn’t just a new way to do syntax. The bulk of the paper, in fact, is devoted to phonological issues, to showing how thoroughly the Bloomfieldians had mismanaged an area everyone regarded as their strongest, and how, therefore, “the fundamental insights of the pioneers of modern phonology have largely been lost” (1964b [1962]:973).²⁰ His arguments are wide-ranging, compelling, and extremely well focused. The number of themes Chomsky smoothly sustains, and the wealth of detail he invokes, are remarkable, but the paper effectively comes down to:

- traditional grammar was on the right track, especially with regard to uncovering the universal features shared by all languages;
- Bloomfieldian work, despite some gains, is on completely the wrong track—in fact, has perverted the course of science—especially in its disregard of psychology and its emphasis on the diversity among languages;
- the only real trouble with traditional grammar is its lack of precision;
- fortunately, in the last few decades, the technical tools have become available, through work in logic and the foundations of mathematics;
- transformational-generative grammar, which incorporates these tools, is therefore exactly what the field has been waiting for, the ideal marriage of modern mathematics and the old mentalist and universal goals that American structuralists had discarded.

The emblem of traditional grammar in Chomsky's 1962 address was one of the pre-structuralist Wills, Wilhelm von Humboldt, whom he quotes early and at length on the enterprise of linguistics generally. "We must look upon *language*, not as a dead *product*," he quotes Humboldt, "but far more as a *producing*." And "the *speech-learning* of children is not an assignment of words, to be deposited in memory and rebabbled by rote through the lips, but a growth in linguistic capacity with age and practice." And "the constant and uniform element in this mental labour . . . constitutes the *form* of language."²¹ Coseriu (1970:215) says that the person speaking in these quotations is not Wilhelm, but Noam, von Humboldt, and Chomsky later admits to a certain "interpretive license" (1991a [1989]:7). The quotations are unquestionably selective; as the title of Humboldt's essay suggests, *On the Diversity of Human Language-structure (Über die Verschiedenheit des Menschlichen Sprachbaues)*, he was at least as caught in the tension between uniformity and uniqueness, between inner form and outer realization, as Sapir. But these are still the words of Humboldt and they reflect important concerns—creativity, language learning, and linguistic universals—that the Bloomfieldians had largely disregarded, and that Chomsky was resurrecting. The linchpin in Chomsky's case is in the first quotation from Humboldt, through a slight but natural refraction of *producing (Erzeugung)* to *creating*—that is, exactly the feature of language Chomsky used so effectively in hiding Skinner.

In other published versions of his International Congress paper (there were at least four—1962c, 1964b [1962], 1964c [1963], 1964d [1963]), Chomsky heralded two seventeenth-century texts as even better representatives of the traditional grammar Bloomfield had banished from linguistics, both from the Port-Royal-des-Champs abbey outside of Paris, the *Art of Thinking* and the *General and Rational Grammar*. These books (now more commonly known as the Port-Royal *Logic* (Arnauld and Nicole, 1662 [1662]) and the Port-Royal *Grammar* (Arnauld and Lancelot, 1675 [1660]) epitomize the "general grammar idea" that Bloomfield (1933:6) saw as wielding a long and pernicious influence over linguistics. Bloomfield had reason to complain. The Port-Royal linguistic work implied that the common mental structure underlying all language was that bane of American descriptivism, Latin. But Chomsky saw something very attractive in the general grammar

idea which Bloomfield had ignored and disparaged: that there *is* a common mental structure underlying all languages.

Moreover, beyond the clear mentalism that Port-Royal linguistics shared with Humboldt, it exhibits a far more transformational style of reasoning, particularly as a manifestation of creativity. One example that Chomsky got a good deal of mileage from illustrates the point very well. Consider sentences 18a–18d.

- 18 a Invisible God created the visible world.
- b God is invisible.
- c God created the world.
- d The world is visible.

The Port-Royal *Grammar* says that 18a is a proposition which includes the other three propositions, 18b–18d, and that 18b is the main proposition, in which 18c and 18d are embedded (Arnauld and Lancelot, 1975 [1660]:99). That is, the *Grammar* here is talking, in a very natural interpretation, about kernel sentences, and its rather vague idea of “inclusion” looks like the Harris-cum-Chomsky notion of generalized transformation (which splices one kernel sentence into another). In short, Chomsky has little trouble supporting his position that the *Syntactic Structures* model “expresses a view of the structure of language which is not at all new” (1964b [1962]:15); in fact, that it is “a formalization of features implicit in traditional grammars,” or, conversely, that traditional grammars are “inexplicit transformational grammars” (1964b [1962]:16).

Bloomfieldian linguistics (or, as Chomsky took to calling it in the 1962 ICL address, the *taxonomic model*), it seems, had sinned in two interrelated and horrid ways when it left the garden of general grammar. It neglected universals, and it avoided explanations. The master, of course, has the definitive words here:

Features which we think ought to be universal may be absent from the very next language that becomes accessible. Some features, such as, for instance, the distinction of verb-like and noun-like words as separate parts of speech, are common to many languages but lacking in others. The fact that some features are, at any rate, widespread, is worthy of notice and calls for an explanation; when we have adequate data about many languages, we shall have to return to the problem of general grammar and to explain these similarities and divergences, but this study, when it comes, will not be speculative [as with the Modistae and the Port-Royalists] but inductive. (Bloomfield, 1933:20)

Now, the Bloomfieldians were certainly interested in general, even universal features of language. It is telling that not only Sapir, but Bloomfield and the LSA embraced the title *Language*. They didn’t choose *Languages* or *Tongues*, or *A Bunch of Unrelated Facts about the Noises We Make When We Want Someone to Pass Us the Salt*. But the master’s pervading cautiousness, always looking over his shoulder for another language that could sink his inductive generalizations, had led the Bloomfieldians to avoid all talk of universals. Taking the descriptive mandate to its logical extreme, in fact, means that there are *no* universals: “languages could differ from each other without limit and in unpredictable ways” (Joos, 1957:96).²² So much for the first sin, ignoring universals.

Chomsky cites Joos's without-limit expression of sin in his 1962 ICL paper to illustrate Bloomfieldian misguidedness on universals; a few years earlier, he had paraphrased another Joos extremity, expressing the other primary Bloomfieldian sin, "that the search for explanations is a kind of infantile aberration that may affect philosophers and mystics, but not sober scientists whose only interest is in 'pure description' . . . [a position] which can find little support in well-developed sciences" (Chomsky, 1962a [1958]:153n25).²³ Returning to this theme with a vengeance in 1962, Chomsky says that there is only one real virtue to a theory of language, it explains the structure of specific languages, and the Bloomfieldian aversion to universals made explanation completely unattainable.

Jakobson's work, as the best illustration of this goal, involved a theory of phonetic universals: a finite inventory of features that characterizes all the possible phonemic differences in human languages, just as a finite inventory of atoms characterizes all possible chemicals. The existence of a chemical is explained by combinatory possibilities of atoms. Now, Jakobson's inventory (adopted in principle by Halle's *Sound Pattern*) included articulatory and acoustic features that, for the most part, the Bloomfieldians subscribed to as well. But the extreme descriptivism of the languages-can-differ-from-each-other-without-limit-and-in-unpredictable-ways position is completely antithetical to an inventory that could be considered universal in any meaningful way. If the differences between any two languages are unpredictable, they are likewise unexplainable.

Or, so went Chomsky's argument at the International Congress, and, with that argument, almost all the essential pieces were in place for unseating Bloomfieldian linguistics: it ignored the mind; it failed to recognize language acquisition and creativity as the fundamental problems of linguistics; its phonology was off base; it perverted linguistics from the search for universals; it was concerned with taxonomy when it should be concerned with explanation. But there was one more problem with Bloomfieldian linguistics. It was irredeemably empiricist.

The Rational Chomsky

Empiricism insists that the mind is a *tabula rasa*, empty, unstructured, uniform at least as far as cognitive structure is concerned. I don't see any reason to believe that; I don't see any reason to believe that the little finger is a more complex organ than those parts of the human brain involved in the higher mental faculties; on the contrary, it is not unlikely that these are among the most complex structures in the universe.

Noam Chomsky

Chomsky took something else from his Port-Royal grandfathers, their epistemology, and among his main projects in the few years after his International Congress presentation was championing their views of knowledge and the mind. Those views, usually bundled up in the word *rationalism*, had long been in a serious state of disrepair. Their patron saint is Descartes, and Whitehead had defined the general

disregard for rationalism by saying “We no more retain the physics of the seventeenth century than we do the Cartesian philosophy of [that] century” (1929:14). It was *passé* philosophy. Its perennial opponent in the epistemic sweepstakes was, largely due to the work stemming out of the Vienna Circle, on top. Empiricism was *au courant*.

To rehearse these terms:

Empiricism: all knowledge is acquired through the senses.

Rationalism: no knowledge is acquired through the senses.

Nobody in the history of epistemology, naturally, has bought (or tried to sell) either position; the only function they have served is as straw men in various polemics. The members of the loose philosophical school known as British Empiricism—a school with a varying roll, but which usually includes Locke, Hume, Berkeley, and Mill—held positions that fall more fully within the first definition than within the second, along with several other eminent minds, such as Epicurus, Aquinas, and Ayer. The opposing tradition is ably represented by Plato, Descartes, Spinoza, and Leibniz. But even the most casual reading of any these thinkers makes it clear that the only useful definitions here are fuzzy rather than discrete, and that the quantifiers should be tempered to reflect genuinely held beliefs:

Empiricism: most knowledge is acquired through the senses.

Rationalism: most knowledge is not acquired through the senses.

Even with this tempering, however, we have to keep in mind that *knowledge* refers to domains like mathematics, language, and hitting an inside fastball, not to the name of your sixth-grade teacher or where you left the car keys. But the definitions are workable.

Getting back to Chomsky, his attraction to rationalism goes hand-in-glove with his involvement in the late fifties emergence of cognitive psychology. Behaviorism was undergoing reconsideration in the early sixties, in part because of Chomsky’s recent excoriation of Skinner, and behaviorism rests heavily on empiricism. The big problem with empiricism for cognitive psychology is that the more sophisticated mental functions don’t look like they could arise from a blank slate. The nascent cognitivists believed it to be “a hopelessly wrong epistemological base from which to view the higher functions of the mind” (Bruner, 1988:91). Besides, Bruner says, pointing out that cognitivists could take courage from the growing rationalism in related fields, “There were, so to speak, such nearby figures as Von Neumann, Shannon, Nelson Goodman, Norbert Wiener, and the vigorous young Noam Chomsky who were making such claims loudly and convincingly.” The vigorous young Chomsky, in fact, not only made his rationalism explicit and backed it up with bold arguments in mid-sixties books like *Aspects of the Theory of Syntax* and *Language and Mind*, he entitled another book adjectivally after Saint René, *Cartesian Linguistics*, to make sure the point couldn’t be missed.

And Chomsky’s rationalism is radical. Rationalism, stripped of its straw-man status, makes the unobjectionable claim that some mental capacities come as part of the start-up kit of the mind. One of the best formulations of rationalism is by Leibniz, who compares the mind to “a block of marble which has veins,” and who

says that learning is essentially a “labor to discover these veins, to clear them by polishing, and by cutting away what prevents them from appearing” (1949 [1705]:45–46). For Chomsky, in his starkest formulation of rationalism, one of these genetic veins in the marble of our minds enables us to grow a language. That’s right: *grow* a language, just as we grow an arm or a leg or a kidney.

A prominent subcomponent of this claim is that such growth could take place only in human brains; it is not that we have a quantitatively more sophisticated command of symbols than other species, the way we have, say, a more sophisticated thumb than apes, or better vocal control, or more acute phonological discrimination, but that we have a qualitatively different “mental organ.” To many Bloomfieldians, rationalism was bad enough, but topping it off with species specificity made it look as if Chomsky was placing man outside the natural world. It was claims of this order that finally convinced them that his grammatical elevator didn’t go all the way to the top floor.

The grow-a-language position is actually quite compelling, absurd as it looks at first pass, and follows rather naturally from the poverty of stimulus argument. It might be, as Chomsky suggests in his review of *Verbal Behavior*, that the relevant innate endowment of humans is no more (but certainly no less) specific than general-purpose data-handling or hypothesis-formulating abilities, that the same cognitive properties which guide the growth of vision also guide the growth of language: for the visual cortex, they handle data like “horizontal” and “vertical” and “in-front-of”; for the language faculty, they handle the data like “noun” and “verb” and “sentence.” Or it might be, as Chomsky began forcefully articulating in the sixties, that the language faculty is itself a highly specific mental organ with its own special and independent character, that such things as noun and verb and sentence are not just in the data, but genetically prewired into the brain. But, in either case, rationalism is a necessary part of the explanation and a strictly interpreted (straw-man) empiricist philosophy of mind must be discarded.

Rationalism and empiricism are very important for a later part of our story, when epistemological foundations came back under scrutiny in the generative-interpretive brouhaha, but, for the moment, the central point is that they illustrate just how deep the Bloomfieldian-Chomskyan division rapidly became. What looked to most of the old guard like a new way to do syntax mushroomed in less than a decade into a new way to do linguistics, a new way to look at human beings, and a new way of doing science; new, and completely inverse. They were baffled and enraged.

Many Bloomfieldian camels had collapsed by the time Chomsky’s rationalism became explicit, but that was the last straw for Hockett. In 1964, giving his presidential address to the LSA, Hockett was hailing *Syntactic Structures* as one of “only four major breakthroughs” in the field, placing it in the company of Jones’s Asiatic Society address and Saussure’s *Course*, and as late as 1966 he was working in generative grammar (1965 [1964]:185). But after Chomsky’s rationalism had become inescapably clear, Hockett began fulminating about “the speculations of the neo-medieval philosopher Noam Chomsky” (1967:142–44). Hall, playing on Hockett’s theme (but with fancier spelling), joined in to rail about Chomsky “threatening to negate all the progress achieved over four centuries . . . [and] dragging our understanding of language back down to a state of mediaeval ignorance and obscuran-

tism” (1968:128–29). Trager, keying on the mysticism most Bloomfieldians equated with rationalism, condemned Chomsky as “the leader of the cult [that has] interfered with and interrupted the growth of linguistics as one of the anthropological sciences for over a decade, with evil side-effects on several other fields of anthropology” (1968:78). The sky was falling. The sky was falling.

Burying the Bloomfieldians

Is it really true that young linguists use my name to frighten their children?

Fred Householder

In and among these early polemics about behaviorism, the phoneme, and rationalism, Chomsky and Halle attracted some of the best young minds in the field to the Research Laboratory of Electronics, the eclectic and very well funded branch of MIT which was the incubator of Chomskyan linguistics. The group—including Lees, Postal, Katz, Fodor, Edward Klima, and Jay Keyser—quickly formed very close intellectual ties and began hammering out the details of transformational grammar. As Fodor recalls,

It’s not much of a hyperbole to say that *all* of the people who were interested in this kind of linguistics were at MIT. That’s not quite true. There were others scattered around. But for a while, we were pretty nearly all there was. So communication was very lively, and I guess we shared a general picture of the methodology for doing, not just linguistics, but behavioral science research. We were all more or less nativist, and all more or less mentalist. There was a lot of methodological conversation that one didn’t need to have. One could get right to the substantive issues. So, from that point of view, it was extremely exciting.

It was also very successful. The group made rapid headway on a number of very thorny issues, particularly in the Bloomfieldians’ weakest areas, syntax and semantics. Success, we all know, is heady, and the group’s most definitive character trait was cockiness: they were young, they were bright, and they were working on a novel and immensely promising theory in collaboration with one of the finest intellects of the century. “In a situation like that,” Katz notes, “it’s quite natural for everyone to think they have God’s Truth, and to be sure that what they’re doing will revolutionize the world, and we all thought that.”

Developments spread rapidly. Everyone spoke in the hallways, attended the same colloquia, and saw each other’s papers long before they reached publication. They also saw many papers that never reached publication at all, the notorious *samizdat* literature that still characterizes work at MIT: arguments and analyses circulated in a mimeograph (now electronic) underground, never making their way to the formal light of day but showing up in the notes of important works that did. This situation, quite naturally, infuriated (and infuriates) anybody trying to follow the theory but failing to hook into the right distributional network.²⁴

The most famous of these quasi-publications was naturally Chomsky’s massive *Logical Structure of Linguistic Theory* (1975a [1955]), which is cited a dozen times in *Syntactic Structures* despite extremely modest and dog-eared circulation.

Though still programmatic, it is far more detailed, far more closely argued, far more mathematically dense than Chomsky's published arguments, and it gave the impression that the foundations of his model were firmly in place. It looked to be the iceberg of which *Syntactic Structures* formed the tip (see, in fact, Halliday's remarks in Lunt, 1964 [1962]:988). Chomsky's *A Fragment of English Grammar*, the mimeographed notes for his Third Texas Conference paper, was also cited widely, and Halle's suitably evangelical *Seven Sermons on Sounds in Speech*, was available through IBM. Mostly, though, the citations were to little more than memoranda floating around Cambridge.

In the publications that did issue formally, the program took clearer and clearer shape. The most important early publication, next to *Syntactic Structures*, was Lees's review of it. Chomsky overstates the case wildly when he says that Lees "was basically their [the Bloomfieldians'] hit man. He was the guy they sent around to denounce this, that, and the other thing. They heard about this heresy brewing at MIT, and he came down to take care of it for them." But Lees came to Cambridge (to work on a machine language project) with firm structuralist convictions, with a good standing in the Bloomfieldian community, and with a confrontational personal style. He found Chomsky's work arresting and effectively became his first doctoral student.²⁵ Lees was in part an expositor, and his review provided a rather careful account of Chomsky's key principles and solutions, but it was also the first resounding shot in the campaign against the Bloomfieldians. Using the familiar we're-doing-science-and-you're-not war cries, the review put Chomsky's work in very sharp relief against the rest of the field: transformational-generative grammar was chemistry, everything else in linguistics was alchemy. Lees's dissertation was also a major contribution to the emerging Chomskyan paradigm. It came out in 1960 as *The Grammar of English Nominalizations*, and was, as Benfey said of Bopp's *Conjugationssystem*, "the first work to be totally imbued with the spirit of the new linguistics" (Hoenigswald, 1986:177). Almost instantly, it became an exemplar for the program—a template for how to do transformational syntactic analysis, the perfect complement to Halle's *Sound Pattern*, a template for the new phonology.

Katz was also very influential. He teamed up with Fodor to contribute an extremely important paper to the Chomskyan enterprise, "The Structure of a Semantic Theory" (Katz and Fodor, 1964b [1963]), an article which made the first explicit proposals on how transformational grammar could accommodate semantics, and then he teamed up with Postal (Katz and Postal, 1964) on a book which brought those proposals closer to the heart of transformational grammar and precipitated the next major technical advance in the theory, the notion of deep structure.

But the publications streaming from Cambridge were not restricted to positive proposals. Many were attacks, following the lead of Chomsky's keel-hauling of Skinner, and his obstreperous performance at the 1958 Texas conference, and his International Congress attack on the theoretical underpinnings of Bloomfieldian descriptivism, and Halle's attack on the phoneme, and Chomsky and Halle's joint pummeling of Householder. But the disciples outdid their masters. The most famous polemic is Postal's *Constituent Structure* (1964), something of a negative

exemplar, or an exemplar of negativity—a template not for working in the new program, but for eviscerating the opposition. It is a methodical, closely-reasoned, and withering argument to the effect that all varieties of structuralist syntax collapse into Chomsky's phrase structure notation, and consequently are decidedly inferior to transformational analyses. The book's reputation for brutality is so firm that one of Postal's colleagues describes it as

a character assassination of all the major players in syntax: Bloch [under whom Postal had studied], and Hockett, and Sid Lamb, and Ken Pike. Immediate Constituent analysis, he said, was all hopelessly inferior and inadequate. So, his personality in the early days was . . . well, he was just a mad dog.

The mad-dog assessment is a little harsh, perhaps reflecting Postal's conference performances, or his later *Aspects of Phonological Theory* (1968 [1965]), but it does capture the unstoppable, unalterable tone of absolute certainty that pervades the book, and virtually everything else Postal wrote on transformational-generative grammar; one gets the sense that there is just no point trying to reason with Postal. He'll just come up with another argument. If that doesn't work, he'll find another, and another. This attitude suffused MIT, and gave rise in many Chomskyans to the "pretentious and cavalier" style that Bar-Hillel (1967:542) deplored in Katz—they had all the answers and most everyone else was hopelessly misguided. The attitude bewildered and aggravated even the most sympathetic, smooth-tempered linguists. Einar Haugen, for instance, as catholic and openminded a linguist as there was in the Bloomfieldian period, called Chomsky's program "a great advance," but lamented that

once one begins to have discussions with the people who advocate this new approach, one discovers a certain dogmatism . . . and I wish that somehow the people who are so enthusiastically pursuing this new form, would understand some of the problems in presenting their ideas to other people, so that those others could accept them willingly. (Dallaire and others, 1962:41)

The result, for many, was the one reached in "On Arguing with Mr. Katz" by Uriel Weinreich (another broad and generous *indépendiste* from the Bloomfieldian period), that, since his opponent has completely abandoned "the ordinary conditions of scholarly fair play," the argument simply has to be abandoned (1968:287).

But the antagonism that surfaced in print was only a dull echo of the clamoring at conferences, the tone being set by Chomsky's featured appearance, the year after the publication of *Syntactic Structures*, at the Third Texas Conference on Problems of Linguistic Analysis in English—an event, in retrospect, almost significant enough to warrant a title so cumbersome. Both the motive behind this invitation and its results in the Bloomfieldian community are subject to some dispute. Some analysts suggest that the conference organizers invited Chomsky to give him a deserved comeuppance (Newmeyer, 1980a:46; Anderson, 1985:314); others find the organizers more benign (Murray, 1983:184).²⁶ Some Bloomfieldians apparently came away persuaded that the brash young Chomsky had been put in his place; others left the conference openly sympathetic to the new program, or at least its syntax. But the importance of the conference was not in its impact on the members of the entrenched paradigm (though it clearly helped to enlist at least one Bloom-

fieldian, Robert Stockwell, an erstwhile fan of phonological syntax). Rather, it played very well to the youth of the field, Chomsky's performance at the conference occupying a substantial role in the mythology formed among the growing cadre of young transformationalists, particularly once the proceedings reached publication (Hill, 1962c [1958]):

Here we see linguistic history documented as nowhere else: Chomsky, the *enfant terrible*, taking on some of the giants of the field and making them look like confused students in a beginning linguistics course. (Newmeyer, 1980a:35, 1986a:31)²⁷

The Bloomfieldians were not entirely outraged by the terrible infant, though, and invited Chomsky back the following year, when he gave a paper on the application of generative principles to phonological analysis. This second appearance was a more decisive, and divisive, sociological event than the 1958 conference, since Chomsky attacked the Bloomfieldians on their theoretical home court, phonology, armed with Halle's work on Russian.²⁸ Chomsky's performance at the 1962 International Congress served a similar role; again the proceedings document contention, and again Chomsky appears to take most of the points soundly. The conference galvanized the transformationalists (who were, of course, present *en bloc*), and the various published versions sparked a good deal of interest outside Cambridge.

But Chomsky has always been very careful about how and where his public disputations occur, and he has never been a very avid conference-goer. Most of the frontline proselytizing fell to other partisans, particularly students, who took up the cause with "missionary zeal" (Newmeyer, 1980a:50, 1986a:42), a phenomenon for which Holton offers a very useful illustration:

It was not Cortéz but the men he had left in charge of Mexico who, as soon as his back was turned, tried to press the victory too fast to a conclusion and began to slaughter the Aztecs. (1988:35)

While it is not exactly Holton's point, his analogy suggests that there is frequently an aspect of intellectual genocide to the onset of a new scientific program, and the emergence of Chomskyan linguistics is a textbook example, though it would be a considerable stretch to talk about Chomsky's back being turned while the slaughter went on. The level of the attacks was often so excessive that it is difficult to believe they were uniformly condoned, but he and Halle strongly encouraged their students to enter the fray. Too, they had coupled their work inseparably with a rejection of all things Bloomfieldian. A big part of guiding their students toward the light was steering them away from the darkness. One of the most efficient ways to define an approach is in opposition to something, or someone, else—what those guys are/were doing is hopelessly misguided, and we're not going to commit the same errors. Ostoff and Bruggmann beat up on the comparativists. Boas and Sapir beat up on the Latinizing missionaries. The Bloomfieldians took their habit of grandly berating traditional grammar so far as to personify it into a crusty old cipher, one Miss Fiddich, a symbolic schoolmarm whom they regularly cited with contemptuous bemusement as the source of some grammatical observation that they wanted to dismiss as trivial or of an attitude that they wanted to ridicule.

Both Chomsky and Halle deny any excesses in their presentation of previous

work, but their students of the period recall classes on the Bloomfieldians that hallway banter labeled “Bad Guys Courses,” and it is noteworthy that contributors to transformational grammar from outside MIT—Charles Fillmore, for instance, and Emmon Bach, and Carlota Smith—were far less polemical than Lees, or Postal, or Katz, or Bever, or Chomsky and Halle. Inside the citadel, the mood was us-against-them. Infidels were rushed to the stake. This recollection is from Robin Lakoff, a Harvard linguistics student in the early-to-mid-sixties (and later an important generative semanticist) who was a frequent and enthused spectator to the carnage:

I remember well the times that non-transformationalists would speak at MIT, in those early years when the field still saw itself as fighting for survival in a hostile world. Rather than attempting to charm, conciliate, find points of connection, the circle at MIT regularly went for blood. Points were made by obvious public demolition; the question or counterexample that brought the offender to his knees [was] repeated for weeks or months afterwards with relish. (R. Lakoff, 1989:967–68)

On the other coast, where an early convert, Robert Stockwell, had set up shop, Victoria Fromkin remembers that “the weekly seminars at the Rand Corporation in Santa Monica more resembled the storming of the Winter Palace than scholarly discussions” (1991 [1989]:79).

The two most fervent revolutionaries were Lees and Postal. Lees was the earliest, and the most flamboyant. A very direct man, he employed a style calculated to shock and enrage which he now describes (with characteristic bluntness) as “getting up at meetings and calling people *stupid*.” These tactics made him a legend among the transformationalists, but they did not endear him to the other side; Householder cautiously begins a review of Lees’s *Grammar of English Nominalizations* with the remark that Lees “is noted as a redoubtable scholarly feuder and cutter-down-to-size” (1962:326), probably the mildest terms used by his opponents.

Postal was even less loved by the Bloomfieldians. Like Lees, he is warm and genial in personal settings, and quite tolerant of opposing viewpoints. But his reputation for intellectual savagery is well-deserved, rooted firmly in his public demeanor at conferences, especially in the early years. The stories are legion, most of which follow the same scenario. Postal sits through some anonymous, relatively innocuous, descriptive paper cataloguing the phonemic system of a little-known language. He stands up, begins with a blast like “this paper has absolutely nothing to do with the study of human languages,” and proceeds to offer a barrage of arguments detailing its worthlessness—often making upwards of a dozen distinct counter-arguments against both the specific data used and the framework it is couched in. The performances were renowned for both intellectual precision and rhetorical viciousness. One tirade against Joos was so ruthless that it was stricken from the record of a Linguistic Society meeting (Hill, 1991:74), and some sense of his style is apparent in the casualness with which he categorizes his opponents’ positions as “empirically and logically contentless remarks” (of Hockett) and “substantively empty assertions” (of Gleason) and “tortured with a kind of intellectual schizophrenia” (of the whole Bloomfieldian program) in his published counterattacks (respectively, 1968 [1965]:4, 5, 6). And this (of the descriptive mandate):

One cannot argue with someone who wishes only to classify utterances. People have a right to do what they want. We can ask, however, whether this has the right to be called

'linguistics'; whether it has the right to claim to be a significant field of inquiry. (Dallaire and others, 1962:10)

Complete and utter dismissiveness is not unusual in these circumstances. Of a similar contemptuousness and smugness among Oxford philosophers in the thirties, Isaiah Berlin says, "This was vain and foolish and, I have no doubt, irritating to others." But, he adds, "I suspect that those who have never been under the spell of this kind of illusion, even for a short while, have not known true intellectual happiness" (1980:115). Arnold Zwicky, an MIT graduate student at the time, recalls the mood in exactly these terms. The viciousness, he says, was propelled by an intense conviction that Chomsky's program was closing rapidly in on the Truth:

there was a kind of holy war aspect to some of this, a feeling that some people had that they had to turn people's minds around, and that it was *important*, and that any device that did this, including ridicule, was legitimate.

Frederick Newmeyer, who entered the field just at the tail end of these events, finds the overall effect of the Chomskyan's confrontational tactics to be salutary, because the encounters showed an entire generation of linguists that language and science are important enough to arouse the passions, and because they showed clearly that the Bloomfieldian program was on the defensive; indeed, on the retreat (1980a:50f; 1986a:42). Still, there is a somewhat apologetic tone in his observation that "even undergraduate advocates of the theory embarrassed their teachers by ruthlessly lighting into linguists old enough to be their grandparents" (1986a:40). Postal, too, shows some empathy for their position:

It was really a psychologically painful situation, because [Bloomfieldian linguistics] was itself a revolutionary linguistics that had gained its ascendancy by proclaiming that it was the scientific way to study language, and that traditional linguistics was unscientific. They had, themselves, trampled on people rather forcefully, made a lot of enemies, did a lot of unpleasant things. Now, bang, not very long after they were really in place, *they* were suddenly being attacked, and in a way that was incomprehensible to them. They were being told that *they* weren't being scientific. That just had to be a nightmare for them.

It was. They reacted with horror and lasting bitterness. But the sky had fallen. As early as 1963, the more dispassionate Bloomfieldians were beginning to admit defeat (Wells, 1963:48). By the middle of the decade it was clear to everyone, friend and foe alike, that "neither linguists nor psychologists [were] doing to language what they did as recently as five years ago" (Saporta, 1965:100); just ten years after the publication of *Syntactic Structures*, "the great majority of the papers" at the 1967 LSA summer meeting "were now firmly in the Transformational-Generative area" (Hill, 1991:89). And the Bloomfieldians had become, quite literally, jokes to the new generation. A parody of a table of contents page from the journal *Language* was compiled at the 1964 Linguistic Institute, including, among other burlesques and cruelties, an entry for a review by Henry Lee Smith of a book attributed to George Trager, *How to Publish and Perish*.