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AUTHOR   Gleitman, Lila R.
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ABSTRACT

        The author raises the question of how to relate
modern transformational grammars to a body of empirical fact, and
suggests why paraphrasing might legitimately be considered a feature
of behavior relevant to linguistic competence. This study is
introduced by a discussion of the empirical basis of descriptive
linguistics, followed by sections discussing empirical evidence for
grammatical organization and linguistic performance and competence.
Section III presents a description of the author's experimental study
of the use of compound nouns, involving the free generation of
paraphrases, forced-choice, and some replication. The final section
discusses the perception of paraphrastic relations in the light of
individual differences, grammaticalness and paraphrasability, and
semigrammaticalness and error-type. General comments on linguistic
method and innovation and creativity in language use conclude the
study. Appended are a list of stimuli, tables indicating distribution
of errors in the generating task and a new scoring technique, and a
bibliography. (AMM)

Compound Nouns and English Speakers[1]

Lila R. Gleitman
Eastern Pennsylvania Psychiatric Institute

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Introduction: The Empirical Basis of Descriptive Linguistics

> For a small group of specialists, knowing about language is an end in itself. These specialists call themselves linguists, and the organized body of information which their investigations produce is called linguistics.

This definition of linguists and linguistics, given by Charles Francis Hockett (35) would seem to contain little that could be regarded as controversial. Yet it is by now abundantly clear that there are serious differences among linguists about what is meant when we say 'language', and thus about what the data for linguists' inquiries are or ought to be. Even within the last fifty years there have existed at least three distinguishable approaches to the problem of describing living languages, each with its own methods, each set of methods dictated at least in part by a different view of what the subject-matter was: what, in the world, was being described. Historically, each school has had its moment of dominance: the first, Traditionalist linguistics, the second, Empiricist linguistics, the third, Introspectionist linguistics. I derive these terms, of course, from the general tone of the polemics that accompanied the decline and fall of the first two schools (the third has not yet had time or cause to fall).

Each new generation of linguists has vigorously objected to the methods of its immediate predecessors. It seems to me, however, that in each instance it was the empirical basis

of preceding work, even more than the method used to order
and codify it, that was crucially being brought into question.
Each time the consensus of linguists has swung toward a dif-
ferent methodological approach, the change was accompanied
by (and in part attributable to) a shift in the subject-
matter under study. To clarify this point, I briefly review
here the theoretical stance (as seen both by practitioners
and critics) of each of these schools of thought. In each
case, the linguist is criticized at least as much for what
he chooses to observe as for the way he observes it.

The classical grammarians distinguished a special kind
of usage: the 'grammatical' or 'correct' or 'proper' kind
of speech and writing. They derived examples of such correct
usage, along with the conviction that it existed, from a
minute examination of their own and other respectable
intuitions. A strong reaction against traditionalist
grammars, so devised, came from the Bloomfieldian empiricist
school, whose members decried the classicists' preoccupation
with 'prestige' dialects. The empiricists believed that the
distinction between 'correct' and 'incorrect' speech, drawn
by the classicists, was at bottom but a distinction between
the dialects of social classes. Thus the traditional
grammarian was accused primarily of pettish moralizing. It
was said that he described language 'as it ought to be',
when he ought to be describing language 'as it was', that

he extolled the dialect of the ruling class as though it had
some intrinsic linguistic virtue. In fact, it is easy to
find in the work of grammarians at the turn of the century
and before a perhaps irrelevant emphasis on mere etiquette:

> A peculiar kind of vagabond language, always hanging
> on the outskirts of legitimate speech, but continually
> straying or forcing its way into the most respectable
> company, is what we call slang. The prejudice against
> this form of speech is to be encouraged....In the first
> place, all human speech is intended for the ears of
> others, and must therefore have a certain dignity, a
> certain courtesy, out of respect to one's hearers if
> not to one's self. Now slang, from the very fact that
> it is slang...has a taint of impropriety about it
> which makes it offensive. Again, the very currency of
> slang depends upon its allusions to things which are
> not supposed to be universally familiar or generally
> respectable; and hence it is vulgar....Finally, the
> unchecked and habitual use of slang (even polite
> slang) is deleterious to the mind....

<div align="right">Greenough and Kittredge (27)</div>

As a consequence of this concern with style and nicety,
traditional grammars often display a lopsided preoccupation
with just those points in the language where class dialects
subtly diverge. Yet in fact this cannot sensibly have been
the cause of the enormous reaction against traditional
grammars by its detractors. Harangue aside, the objection
of the empiricists to traditional grammars is to those
prescriptive statements that are independent of dialect
distinctions, i.e. judgments of inequality concerning
utterances even within an idiolect. The real problem the
empiricists reacted to was this: on what basis, in fact,
can one distinguish 'vagabond' from 'legitimate' speech

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when the former is admittedly found even 'in the most
respectable company'? That is to say, certain expressions,
modes of speech, structures, etc., were rejected by class-
icists even though they had broad currency. The traditional
grammarian never could or would say where the empirical
authority for his rules came from; the substitute for such
authority was too often facile certainty:

> The bewildering variety of our language, and in
> particular the lawless and fantastic coinages which
> we have just now been studying, may well suggest the
> question: 'Is there any criterion of good English?
> What principle of selection is one to follow who
> wishes to speak and write his mother tongue with
> purity and without affectation?' It is the business
> of grammar, rhetoric, and lexicography to answer
> this question.

(Greenough and Kittredge, op. cit.)

Among American linguists of the 1930's and 40's it was wide-
ly believed that no nonarbitrary empirical basis for the
classicists' prescriptions existed.

The empiricists in their turn tried to develop a study
of language that had a clearly observable and selfevident
data source: obtained speech. Their doctrine was that lan-
guage would be describe more or less directly as a codifica-
tion of some large corpus of utterances; that language 'is'
no more nor less than a collection of utterances, real and
potential. Eugene Nida (1949) below expresses the general
consensus of this era:

> The linguist records the actual forms employed, rather
> than regularizing the data or evaluating utterances
> on the basis of some literary dialect. In other words,

it is what people say rather than what some people think
they should say that is important to the descriptive
linguist....If any judgments are to be passed upon the
acceptability or so-called correctness of some usage,
these are left either to the anthropologist or sociolgist
for an objective statement of the factors in the society
which make certain persons more socially prominent and
hence make their speech more acceptable, or to the man
on the street, who is thoroughly accustomed to forming
judgments upon the basis of his own egocentric attitudes
and limited knowledge.

(65)

A similar program of research, and a similar objection to the

traditionalists, is given by Hockett:

It may come as a shock to learn that the linguist
is not particularly interested in...questions [of
correctness]...[This] does not mean that the linguist
is an advocate of incorrect forms, or that he denies
the reality of the distinction between correct and
incorrect. As a user of language, the linguist is
bound by the conventions of his society just as every-
one else is -- and is allowed the same degrees and
kinds of freedoms within these conventions....But
this has little if any relationship to his special
concern, which is analysing language. As an analyst of
language, the linguist is bound to observe and record
'incorrect' forms as well as 'correct' ones -- if the
language with which he is working makes such a
distinction.

(35)

Hockett's concluding comment reveals the position: he doubts

even that the distinction between 'correct' and 'incorrect'

exists independently of dialect difference, for every language.

Bloomfield himself writes:

The discrimination of elegant or 'correct' speech is a
by-product of certain social conditions. The linguist
has to observe it as he observes other linguistic
phenomena....This is only one of the problems of lin-
guistics and, since it is not a fundamental one, it
can be attacked only after many other things are known.

It will have been noticed that the empiricists tended
to equate two senses of usage that nowadays are distinguished:
there is the question of social (and regional and temporal)
dialect difference, for which caveats of correctness are no
doubt inappropriate; then there is the question of formal,
or written, vs informal styles of expression, where the
matter is more complex. Although even in the heyday of
scientific linguistics this distinction was not wholly for-
gotten, the issues of 'acceptability' and 'correctness' as
valid perceptual experiences were minimized, if not ignored.
The question for linguistics became simply: 'What is said?'
Thus did American descriptive linguistics react against
the doctrinaire, ill-formulated and, at the time, essentially
untestable pronouncements of traditional grammarians. Every
utterance was now said to have equal status as a linguistic
datum, the fact of having-been-uttered the definition of
grammaticalness.

The most serious effort to follow these principles in
writing a grammar was made by Charles Fries (24). He tapped
a large number of telephone conversations in the belief that
language could be described by beginning with a corpus of
spontaneous speech. From these data there were eventually
distilled class-lists equivalent in intent to exhaustive
co-occurrence matrices, and corresponding very grossly
to the traditional parts of speech. A great step was
thereby taken: the new grammars described the regularities

of language rather than emphasizing 'exceptions' and stylistic
minutiae; the episodic character of classical grammars was
counteracted.

Various questions remain, however. It has been objected
that 'intuition' is not evaded by such a method, i.e. that
the lists of classes were not mechanically derivable from
the raw data, as these linguists supposed. Far more
seriously, it has been objected that the resulting grammars
were not 'interesting', 'comprehensive', 'illuminating',
etc. It is a curious philosophical excess of the empiricist
movement that this last kind of criticism could, as a kind
of literary conceit, be accepted as a compliment:

> Children want explanation, and there is a child in
> each of us; descriptivism makes a virtue of not
> pampering that child.

So wrote Martin Joos in 1958 (40), justifying the replace-
ment of phonological explanations by 'a sober taxonomy'.
Then in this revolutionary period we find a clear gain in
justifiability and freedom from arbitrariness but, some say,
a comparable loss in compactness and 'explanatory power'.

During the Bloomfieldian period, there was at least
one linguist of stature who remained aloof from prevailing
opinion concerning the study of language. This, of course,
was Edward Sapir, who was suggesting a new frame of reference
for descriptive linguistics. His overriding interest was in

the perceptual organization that gives rise to overt language
behavior -- in fact, just that description of 'underlying
mental processes' that Bloomfield was at pains to banish.
In those days of aggressively scientific linguistics
(matched rather neatly in neighboring psychological circles
by mechanistic behaviorism), Sapir's views were regarded as
somewhat subversive -- a dangerous backsliding. Martin
Joos, again, summing up Sapir's contribution, wrote:

> [Sapir's] contribution was not the developing of any
> method, but rather the establishing of a charter for
> the free intellectual play of personalities more or
> less akin to his own. If their wits happen to be
> dimmer (and here he had few equals), their blunders
> may betray the essential irresponsibility of what
> has been called Sapir's 'method'. We welcome the in-
> sights of his genius, which allowed no scrap of evidence
> to escape at least subconscious weighing; where it is
> possible to check up, we normally find him right;
> thus we seem captious when we point out that he also
> said many things which are essentially uncheckable
> ('invulnerable') and thus not science.

Ten years later, the views of Sapir do not sound so odd or
mystical. He argued that a really explanatory theory of
language would be found in a higher-level construct of
mental patterning; his view is exemplified in this dis-
cussion of phonetics:

> I found it difficult or impossible to teach an Indian to
> make phonetic distinctions that did not correspond to
> 'points in the pattern of his language', however these
> differences might strike our objective ear, but that
> subtle, barely audible, phonetic differences, if only
> they hit the 'points in the pattern', were easily and
> voluntarily expressed in writing. In watching my
> Nootka interpreter write his language, I often had the
> curious feeling that he was transcribing an ideal
> flow of phonetic elements which he had heard, inadequate-
> ly from a purely objective standpoint, as the intention
> of the actual rumble of speech. (68)

The difference between Sapir's and Bloomfield's 'methods'
was not in the recognition of phonetic patterning (for
phonology, at least, the difference between physical and
perceptual fact was acknowledged), but in Sapir's "mentalistic"
assumption that the perceptual systematization at times
transcended present acoustic cues -- that the speaker
heard what wasn't there, that he had interna⊥ızed and
understood much that he had never heard. A most beguiling
example reported by Sapir is recorded by Bernard Bloch in
an editorial note (3):

> Sapir was working once with a bilingual Navaho informant,
> an old woman who patiently recited paradigms for him.
> When he had heard several dozen forms, Sapir ventured
> to create new forms by extrapolation. The old woman
> shook her head in wonder, and looked at him respect-
> fully. 'You're a funny man,' she said, 'You say
> things in my own language that I never heard before,
> and they're right!'

In 1957 Noam Chomsky carried Sapir's position to its
implied conclusion: Chomsky outlined a new linguistics
which had as an avowed aim the description of the speakers'
intuitions concerning the forms of language. This represent-
ed a radical departure from the strict equalitarian attitude
toward utterances that by now had become the establishment
view within the field, and an angry dialogue has persisted
until today. Chomsky claimed not that the speaker could
reliably verbalize rules or regularities of his native
tongue, but rather that the speaker could make judgments

and construct utterances that were explicable only in terms
of an implicit systematization of the language. Although
Chomsky writes that a 'corpus' of utterances underlies his
own linguistic theory construction, it should not be
supposed that what is meant is the kind of corpus the
empiricists thought they were using. Chomsky writes:

> In order to set the aims of grammar significantly
> it is sufficient to assume a partial knowledge of
> sentences and nonsentences....For the purposes of this
> discussion...suppose that we assume intuitive know-
> ledge of the grammatical sentences of English and ask
> what sort of grammar will be able to do the job of
> producing these in some effective and illuminating way.
> We thus face a familiar task of explication of some
> intuitive concept, in this case, the concept 'grammatical
> in English,' and, more generally, the concept
> 'grammatical.'
> (14)

The corpus (and thus the subject-matter) is now redefined:
While the empiricist uses 'some obtained utterances',
Chomsky selects some utterances judged to be grammatical
and some (obtained or not, indifferently) utterances
judged not to be grammatical.

   In the context of the present discussion, it is
important to notice that 'correctness', 'acceptability',
and 'grammaticalness' -- far from being excluded as secondary
or irrelevant -- become in the theories of Sapir and Chomsky
central concepts. Thus the question has shifted from what
the grammarian thinks the speaker ought to say, toward what
the speaker says, to what the speaker thinks he ought to
say; from what should by rights be said to him toward what
is actually said to him to what he hears.

Much of the attack and counterattack in the scholastic war that has ensued seems to converge around transformations as a formal device, but it is easy to see that the struggle pivots around Chomsky and his group. Zellig Harris, who, after all, introduced the concept of transformations (sentential relations) into syntactic study, was not engaged in the combat. Harris' work was not vitally controversial: in his published work on transformations (28, 29) he appears to tap the same empirical sources as his predecessors. Chomsky, on the other hand, draws from a hypothetical set of nice judgments by speakers.

Many American linguists persist in judging transformational grammars by their observational accuracy in describing speech (see, e.g., Hill (32)). If such were the intention of generative grammars, or of any transformational grammars, then surely they fail to meet the grossest test. But a theory cannot sensibly be faulted by reference to a set of data it never set out to pacify. Nevertheless, the assumption of classicists that the empirical issues could be passed over by edict seems untenable. Traditionalists and transformationalists can make common cause in the concern with judgmental features of linguistic behavior, but the need for external validation can hardly be questioned.

The dispute between empiricist and transformational linguists involves a disagreement concerning the very subject-matter of linguistics. The empiricist contention that

language can be described simply by classifying and arrang-
ing the elements of overt speech seems inadequate. This
argument has been made so well, so frequently, and so
vociferously in the literature (14, 15, 66) that it is
superfluous here to add a new version. Yet the central
question the empiricists raise requires a response: In
what nonarbitrary manner are certain utterances removed
from consideration? In other words, what are the empirical
correlates of transformational linguistics? So long as
this question is left unanswered, transformational grammars
remain descriptions in search of a phenomenon.

We therefore raise here the question of how to relate
modern transformational grammars to a body of empirical
fact; similarly, the question of what aspects of reality
will serve to justify or validate these grammars, aspects
of these grammars, details of these grammars, most important,
relevance of these grammars to the totality of the study of
language.

A systematic organization of language, along the lines
of a generative and transformational grammar, impinges on
the activities of a human being when he speaks or comprehends
his native tongue. This matter is hardly in doubt. I am
concerned here only with the place of such grammars in a
description of language and language use. In the empirical
work described here, I examine certain rockbottom factual

assumptions of the transformational grammarians, and
describe some tests of the adequacy of these assumptions.
From such work I believe something can be contributed to the
understanding of the methodological bases of current lin-
guistic research, and to the question of where transforma-
tional grammars fit into a description of language.

A note: Earlier I quoted from writers who felt that
issues such as the perception of 'correctness' lay outside
the linguist's competence and rightful concern. They are
clearly in again. It is only one more argument that the
borders between sciences are not sensibly subject to
juridical decision. Hockett's definition of linguists and
linguistics is ultimately the only tenable one, for it is
the only one vague enough, for scientific purposes; lin-
guistics is what linguists do. It is perhaps unfortunate
that Chomsky's recognition of covert aspects of linguistic
competence has opened a Pandora's Box of psychological and
psychophysical problems. His own earlier assumption was
that 'simple tests of acceptability' could be designed to
validate transformational grammars; it has not happened so.
Outside the cloisters of the grammarians there are masses
of people who seem as little able to make judgments as to
speak correctly, as little able to display their 'competence'
as to perform stably. If we must therefore enter into a
period of empirical investigation, some traditional academic

boundary lines will surely be overrun. Psychologists have
contributed little enough to the study of language that we
need surely have no qualms at invading their domain. I
raise the issue here because expectably little derives
from this work to controvert Chomsky's position: that
transformational grammars are ample and able to problems of
syntax and semantics; but I will suggest that the place of
syntax and semantics in the study of language needs clar-
ification. This may be cause for argument, but not for de-
classification, or even reclassification, of either linguists
or linguistics. Perhaps especially in the study of language,
we have to accept a continual overlapping with all the sciences
of man. Sapir is here again instructive:

> It is clear that the interest in language has in recent
> years been transcending the strictly linguistic circles.
> This is invevitable, for an understanding of language
> mechanisms is necessary for the study of both historical
> problems and problems of human behavior. One can only
> hope that linguists will become increasingly aware of
> the significance of their subject in the general field
> of science and will not stand aloof behind a tradition
> that threatens to become scholastic when not vitalized
> by interests which lie beyond the formal interest
> in language itself....
> (69)

Section I: A Historical Review: The Empirical Evidence for
             Grammatical Organization

I have suggested that the choice of subject-matter

schismatizes linguists into 'schools' even before the problem

of treatment of the data is raised. While the subject-matter

of American empirical linguistics (speech) is relatively easy

to define and justify, the fragment of reality described by

transformational (and traditional) grammarians is not altogether

clear.

In 1957, in Syntactic Structures, Chomsky set as the

fundamental aim of grammars the description of projective

features of language, i.e. of an unbounded set of grammatical

expressions. The 'creative aspect' of language was viewed as

a psychological as well as a formal reality; the ability of

speakers to form indefinitely many novel sentences was taken

as the basic phenomenon requiring descriptive counterpoint

in a grammar:

> The fundamental aim in the linguistic analysis of a
> language L is to separate the grammatical sequences which
> are the sentences of L from the ungrammatical sequences
> which are not sentences of L and to study the structure
> of grammatical sequences.
>
> A grammar mirrors the behavior of the speaker, who, on
> the basis of a finite and accidental experience with
> language, can produce or understand an indefinite number
> of new sentences. Indeed any explication of the notion
> 'grammatical in L' (i.e., any characterization of
> 'grammatical in L' in terms of 'observed utterance of
> L') can be thought of as offering an explanation of
> this fundamental feature of linguistic behavior.
>   (14)

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If it is the case that humans can say and understand new utterances, failure to account for this feature of linguistic behavior, at least in a psychological, or 'mentalist', grammar of the Chomsky variety, can by itself be taken as proof of the inadequacy of a proposed grammar:

> [Empiricist] speculations have not provided any way to account for or even to express the fundamental fact about the normal use of language, namely the speaker's ability to produce and understand instantly new sentences that are not similar to those previously heard in any physically defined sense or in terms of any notion of frames or classes of elements, nor associated with those previously heard by conditioning, nor obtainable from them by any sort of 'generalization' known to psychology or philosophy.
>
> Chomsky (Aspects of the Theory of Syntax, 1965)

There is in fact no doubt that novel utterances can be elicited even from very young children. On the other hand, Pavlov's dog will salivate to previously unheard bells and the energetic creativity of this latter act is open to some question. The special assertion of Chomsky concerning language is that no physical dimension can be found to account for the production or comprehension of new sentences that are in certain ways 'similar to' previously encountered sequences. It follows, in Chomsky's view, that humans are 'prewired' to recognize certain kinds of linguistic relations and not others. A grammar is then a neurophysiological hypothesis based on an examination of certain response-types.

It should be noticed that even for animal behavior it is often no simple task to specify a physical dimension

defining similarity (a rat will respond more emphatically
to a tone an octave away from his accustomed tone than to
one but a few notes away (α), even though pure tones are
used to minimize the chance of audible overtones). 'Similar-
ity' even here is a hindsight notion, dependent on identity
of response type. The perception of octaves must be explained
neurophysiologically for the animal, i.e. the dimensions of
physical similarity that are valid and immediate for ratkind
are tied up with intrinsic cognitive functioning. So, for
man, the tendency to notice certain kinds of patterning
and not others must be at least in part neurophysiologically
determined. To this extent certain organizational contours
of language learning and use are pregiven in the organism.
But it must be recognized that this general fact does not
come as a shock to the (post 1930) psychological community,
nor does it preclude the role of learning in language, nor
does it by itself point to some transcendental feature of
human cognition that is in principle irrelevant to the
description of other forms of life. However, as we shall
later discuss, the existence of anatomic and physiologic
correlates of verbal behavior (for one thing) make it extremely
unlikely that attempts to describe human language and cognition
as mere extensions of animal cognitive functioning will
succeed. In fact, disillusionment with the attempt to

extrapolate complex thinking processes from models of
animal behavior accounts for much of the interest among
psychologists in Chomsky's hypotheses.

What is not selfevident about the assertion that speakers
can deal with novelty is the generality of this skill. Even
in a loose qualitative way we know nothing of the limits,
extent, pervasiveness over the linguistic community, or
conditions for acquisition and use of novel linguistic be-
havior. Then though it is factually incontrovertable that
certain projective features must be written into language
descriptions, it is by no means clear which (and how far and
to whom and when) productive devices of various sorts are
available. It has been the Chomskian view that any restrictions
on these manipulations are only the general factors limiting
all sorts of mechanical and cognitive activities (e.g. limita-
tions of memory span, degree of attentiveness, motivation,
interference).

A good deal of experimental ingenuity would be required
to design a compact and feasible test of the extent of the
ability to comprehend and produce sentences, for it is obvious
that limitless outpourings of new sequences do not necessarily
tap the limits of the structuring postulated in a grammar.
Not only must all structural types be sampled, but the
relations among sentences and sentential types must be examined;
for it is the interweave among structures that lies at the

heart of the transformational hypothesis.

No one has ever undertaken to elicit sentences from individuals in a way systematic enough to point to an effective procedure for producing or understanding 'all grammatical' sequences (even within the constraints of memory, etc.). No such heroic procedure is necessary or useful, since the fabric of transformational theory is sufficiently rich and detailed to suggest a host of feasible corroborative tests. Since psychological validity is regarded as the central external buttress supporting proposed grammars, various evidences from behavior are adduced to justify these grammars both in general format and in detail. In this section I will consider the kinds of psychological evidence that have been brought forth to show the adequacy of this approach to grammar. In each instance, I attempt to show why such empirical evidence would support transformational theory, and examine also the results of attempts to make this evidence plausible, i.e. the fate of attempts to validate these assertions experimentally.

The major empirical claims of the transformational grammarians have been: (A) that speakers can judge between grammatical and ungrammatical sequences of English words; (B) that formal definitions of phrase-structure and trans- formational organization and complexity are mirrored in behavior; (C) that language acquisition is more-or-less

uniform in the human species, i.e. it is acquired inde-
pendently of level of intelligence and of specific environ-
mental features; and (D) that specific paraphrastic (meaning-
preserving) relations among sentential types are known to
the speaker of the language.

A. The ability to classify sentences as grammatical: A
central aspect of transformational grammars, and indeed all
grammars, is the specification of all the 'legitimate'
sequences in the language. If the grammar mirrors human
behavior in this respect, then within certain statable
limitations, e.g. length, a speaker of the language ought
to recognize an illegitimate sequence when he hears it. In
effect, the speaker would realize that his 'internalized
grammar' does not specify this sequence. Chomsky suggests a
test of this ability in Syntactic Structures:

> One way to test the adequacy of a grammar proposed
> for [a language] is to determine whether or not the
> sentences that it generates are actually grammatical,
> i.e., acceptable to a native speaker, etc. We can take
> certain steps toward providing a behavioral criterion
> for grammaticalness so that this test of adequacy can
> be carried out...
>
> (14)

Until fairly recently it was assumed by most grammarians of
Chomsky's general persuasion that such an empirical test
would so selfevidently succeed that there was little reason
for systematic experimental investigation. Formal models of
language are constructed with this datum accepted as proven.
Hilary Putnam, a philosopher, speaking before the American

Mathematical Society in 1960 (67), showed that this hypo-

thetical ability, "makes it legitimate to seek recursive

function-theoretic structures which could serve as models

for grammars." These are the facts given:

> ...that speakers can presumably classify sentences
> as acceptable or unacceptable, deviant or nondeviant,
> et cetera, without reliance on extra-linguistic cues.
> There are of course exceptions to this rule, but I am
> more impressed with the multiplicity of nonexceptions.
> I imagine, for example, that if I were on any number of
> occasions presented with a _list_ of sentences and asked
> to say which ones I thought were grammatical, I would on
> each occasion and without any information on the supposed
> context of use of the individual sentences classify
> 'Mary goed home' as an ungrammatical sentence and 'Mary
> went home' as a grammatical sentence. This act of
> classifying sentences as grammatical or ungrammatical
> seems to be one I can perform given no input except
> the sentences themselves. In short, it seems that in
> doing this job of classifying, I am implicitly relying
> on something like an effective procedure.

Empirical investigation of this topic has been attempted.

Critics of the general approach were the first to set it to the

test, but these investigations were carried out with such

astonishing experimental naivity as to render the results

inconclusive. Notice that Chomsky indicated that some

'behavioral criterion' could perhaps be developed for testing

the speaker's ability to make these judgments. The assump-

tion that classification could be performed by lay subjects

in the strictly schematic version outlined by, e.g., Putnam,

is obviously too strong. Consider merely the difficulty of

knowing in what way the subject understands the instructions,

and the complexity of actual experimental verification begins

to be obvious.

Nonetheless, rediscovering psychological methodology
from its beginnings, linguists such as A. A. Hill (𝟹.𝟷)
submitted to subjects lists of sentences, asking which were
acceptable, or grammatical. The results, as expected, are
far short of reliable. There seems to be evidence of
highly idiosyncratic behavior. However it is almost
impossible to interpret these results because the instructions
were so vague.

Sleator and Maclay (𝟻.𝟷) attempted a more systematic
study of the same question. In this version, an attempt is
made to control for the effects of 'nonsensicalness' and to
separate these from the effects of 'grammaticalness'.
Nonetheless, there was at minimum a good deal of statistical
smear in the outcome: although there was a discernible
indication of preference for grammatical sequences, the
result was either imperfect, or else masked by extraneous
effects of the testing conditions.

Hill concludes that speakers cannot judge sentences in
isolation. Sleator and Maclay conclude that empirical
criteria cannot be, because they do not here succeed, used
to justify grammars. Both conclusions are highly arbitrary.
Failure to provide an existence proof in a given experimental
situation can scarcely be regarded as disconfirmation;
failure of an empirical technique to elicit the desired

result -- or the opposite -- hardly justifies giving up
investigation. Within linguistics, it was left to Chomsky
to point out that, in all human activity, there are deficits
in performance that may be wholly extraneous to the topic
under experimental review. Here it suffices to say that
'acceptability tests' in this form give garbled results,
probably for any of a number of external reasons.

B. The perception of phrase-structure and transformational
complexity: Where 'acceptability tests' failed to provide
reliable evidence for grammatical structuring, more sophisti-
cated psychological techniques have provided more revealing
data.

George Miller and his associates (Miller, 1962; Miller
and Isard, 1963) set out to study the relevance of syntactic
structure to perception and recall. They presented subjects
with grammatically and semantically acceptable sentences,
sentences that were grammatical but semantically anomalous,
and sentences that were both ungrammatically and semantically
anomalous. They then asked subjects to listen to these
sentences in the presence of a masking noise. Later,
(Marks and Miller, 1964) they asked subjects to memorize the
sentences. In both experiments, the results are the same:
the grammatical and semantically sensible sentences are most
easily dealt with; the scrambled semantically anomalous
sentences are the most difficult; the other case falls in
between.

Lest it seem obvious or trite to have performed such experiments at all, it should be recalled that even in linguistics today there are those who question the assertion of linguistic organization in the individual speaker, while recognizing regularities in the language community. Even more, in psychology, there have been many attempts to describe language using only the most primitive grammatical and statistical organizing hypotheses. Miller's series of experiments established beyond further doubt that word-sequences with certain kinds of grammatical and semantic properties are perceived wholly differently from haphazard and semistructured sequences. There remains to provide the detail of the organization, and the psychological processes this organization implies.

Bever and Fodor (1963) showed that speakers can recognize internal features of organization (phrase-structure) within the sentence. They interpolated a clicking noise during the recorded presentation of a sentence, and asked subjects to judge at what point in the sentence the click occurred. Judgments tended to be displaced toward major constituent boundaries. In a crucial test, syntactically ambiguous sentences whose alternate interpretations had different major constituent boundaries were presented after the subject was 'set' by prior sentential context to hear one interpretation or the other. Although the stimuli were physically identical the displacement was predictable from the set.

In most transformational grammars, it is possible to
distinguish on formal grounds between 'simpler' and 'more
complex' constructions. If the grammar mirrors behavior
in this sense, the individual will find the simpler construction
more immediate and comprehensible than the complex.[1]
Miller (1964) reviews a series of attempts to study the
relative comprehensibility of affirmative (grammatically
simpler) and negative (more complex) sentences, when semantic
content is controlled. Briefly summarizing this work:
Smoke (1932) and Hovland and Weiss (1953) showed under a
variety of experimental circumstances that negative sen-
tences are more difficult to deal with than affirmative
sentences. Wason (1961) showed that the affirmative-
negative difference was more important than the difference
between truth and falsehood, thus suggesting that a
grammatical factor beyond the conceptual, or logical, factor
accounted for some of the difficulty with negation. McMahon
(1963) showed that this difficulty could in part be described
as a measurable difference in time required to unscramble
the transform. This last explanation depends, of course, on
the assumption that sentences must be transformed to -- or
understood in -- their simplest syntactic form as a step

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[1]Such effects are noticeable only when a way is found to
tax the individual because, presumably, the language patterns
are so well established in an adult that all 'normal' sentence
types will be grasped very easily. There are any number of
ways (e.g. masking noise, memory loading, interpolated tasks)
to make difficult perceptions and activities that are so easy
under normal circumstances as to make any error or even delay
unlikely.

in comprehension.

The general attempt to measure the times involved in 'making grammatical transformations' was begun by Miller (1962). In the early experiments, subjects were shown by example the kind of transformation they were to effect, and the time to response was measured. Rough estimates of the time involved suggested that the effects of complexity were additive (i.e., transforming a negative-passive to a simple active positive took about the time of transforming a negative to a positive plus the time of transforming a passive to an active). It should be remembered, however, that such conscious manipulation of syntactic forms may be an activity quite alien from the normal use of language. In so far as it may, however, be relevant, the result seems consistent with formal definitions of syntactic complexity.

A more natural and powerful technique for measuring relative complexity among syntactic forms has been developed by Savin and Perchonock (70) in the context of studies of immediate-memory. Savin requires subjects to recall a sentence, and in addition to the sentence, as many as possible of an arbitrary list of unrelated words or digits. The number of arbitrary words that are remembered varies inversely with the complexity of the sentence that is to be recalled; again, the relationship seems to be additive. The rationale of these experiments is this: it is presumed that the sentence

'takes up space' in a small 'immediate-memory bank'; a certain amount of space is now left over and can be 'loaded' with the arbitrary material. The simpler the sentence, the less space it should have taken in the memory bank, and the more space should be left for the filler words. In fact, assertions of a generative grammar concerning relative complexity of structural types correctly predict differences in the number of filler words that are recalled by the subjects.

The work summarized above admittedly is relevant only to the more gross aspects of English syntactic structure; the simplest and most general transformational relationships are dealt with under certain very restricted circumstances. The overall result is a display of the fruitfulness of grammatical patterning as a concept in describing linguistic behavior.

C. Faculte de langage: Man is innately endowed with the ability and the propensity to learn to speak. No other animal is so endowed. That language ability is species-specific has rarely been seriously doubted. That the ability to speak is a cognitive function <u>qualitatively</u> different from anything available in the repertoires of other animals is a common supposition. Occasionally there have appeared revolutionary thinkers, such as B. F. Skinner (14), who have tried to construct a psychologically tenable theory of language by extrapolation from the laws of animal thought. However, the existence of anatomic and physiologic correlates of language (i.e., morphological and functional

specializations such as cerebral dominance and special co-
ordination centers for motor speech) make it unlikely in the
extreme that this faculty of mind will be explained as a mere
elaboration of functions available to the lower animals. Attempts
to teach animals the rudiments of speech are uniform failures
(31, 44). On the other side of this coin, severe physical handi-
caps such as congenital deafness or blindness, damage to the
articulatory mechanisms, even feeble-mindedness, fail to suppress
or even significantly to retard the acquisition of language in
humans.[1]

In recent years, Chomsky and his coworkers have argued
that the 'faculte de langage' represents a large instinctual
pattern of predispositions and behaviors in mankind, relative-
ly immune to environmental or cultural control or direction,
and more-or-less independent of other aspects of cognition:

> ...the structure of particular languages may very well
> be largely determined by factors over which the individual
> has no conscious control and concerning which society
> may have little choice or freedom. On the basis of the
> best informat:on now available, it seems reasonable to
> suppose that a child cannot help constructing a particular
> sort of transformational grammar to account for the data
> presented to him, any more than he can control his per-
> ception of solid objects or his attention to line and
> angle. Thus it may well be that the general features of
> language structure reflect, not so much the course of one's
> experience, but rather the general character of one's
> capacity to acquire knowledge -- in the traditional sense,
> one's innate ideas and innate principles. It seems to me
> that the problem of clarifying this issue and sharpening
> our understanding of its many facets provides the most
> interesting and important reason for the study of
> descriptively adequate grammars and, beyond this, the
> formulation and justification of a general linguistic
> theory that meets the condition of explanatory adequacy....
>
> (Chomsky, 1965)

---

[1]In these points, we are following Lenneberg (1964), who
presents current evidence concerning the biological bases of
language ability.

Some linguists have expressed mystification at the
Chomskian interest in the biological bases of linguistic
ability. However, once it is accepted that linguistic
theory is a description of the verbal activities and
capacities of people rather than a formalism responsible
only to its own elegance and consistency, it follows that
language acquisition becomes at least coequal with other
issues of verbal behavior as a phenomenon that linguistics
will have to explain; more precisely, linguistic theory must
be able to support a plausible description of language
acquisition, though perhaps remaining neutral in the
question of the processes and neural structures involved
in learning.

The preeminence of language learning as a crux of
linguistic theory comes about in the search for 'linguistic
universals', hypothesized features of structure that are
necessary and fundamental to natural language, hence,
possibly, 'innate' organizing principles rooted in the
cognitive functioning of human beings:

> A theory of linguistic structure that aims for
> explanatory adequacy incorporates an account of
> linguistic universals, and it attributes tacit knowledge
> of these universals to the child. It proposes, then,
> that the child approaches the data with the presumption
> that they are drawn from a language of a certain
> antecedently well-defined type, his problem being to
> determine which of the (humanly) possible languages is
> that of the community in which he is placed. Language
> learning would be impossible unless this were the case.
> The important question is: What are the initial assumptions
> concerning the nature of language that the child brings

to language learning, and how detailed and specific
is the innate schema (the general definition of
"grammar") that gradually becomes more explicit and
differentiated as the child learns the language?
For the present we cannot come at all close to making a
hypothesis about innate schemata that is rich, detailed
and specific enough to account for the fact of language
acquisition. Consequently, the main task of linguistic
theory must be to develop an account of linguistic
universals that, on the one hand, will not be falsi-
fied by the actual diversity of languages and, on the
other, will be sufficiently rich and explicit to account
for the rapidity and uniformity of language learning,
and the remarkable complexity and range of the generative
grammars that are the product of language learning.

(Chomsky, 1965, op.cit.)

It would be beyond the scope of our discussion to relate

in detail the form of the language-acquiring-device that

Chomsky postulates. But the kind of phenomena and theoretical

hypotheses that lead him to the innatist position are well

characterized in the following:

It seems plain that language acquisition is based on
the child's discovery of what from a formal point of
view is a deep and abstract theory -- a generative
grammar of his language -- many of the concepts and
principles of which are only remotely related to
experience by long and intricate chains of unconscious
quasi-inferential steps. A consideration of the
character of the grammar that is acquired, the
degenerate quality and narrowly limited extent of the
available data, the striking uniformity of the resulting
grammars, and their independence of intelligence,
motivation, and emotional state, over wide ranges of
variation, leave little hope that much of the structure
of the language can be learned by an organism initially
uninformed as to its general character.

(Chomsky, 1965, op. cit.)

Chomsky then is hypothesizing very definite neuro-

physiologically determined organizing principles for discover-

ing and selecting among grammars. The claim is for a faculty

of mind more-or-less independent of the principles and pro-
cesses that are thought to underly thinking and that are
related to 'intelligence' in a more general sense. The
human is viewed as a structure-seeking organism; linguistic
behavior as a speciated instinct, in much the sense of the
animal behavior patterns studied by the ethologists.

The following kinds of evidence would seem to be
relevant to testing the correctness, or at least the
utility, of this view: (1) the existence of anatomic and
physiologic correlates of language behavior; (2) 'critical
periods' for language-learning, or certain stages of
language-learning, and, in general, a regular developmental
onset; and (3) asymptotic similarity in the adult behavior
of nonpathological individuals.

1. anatomic and physiologic correlates: Specific
areas of the brain basic to speech have been isolated al-
though the relation of these areas to each other and their
specific functions are not yet well known. Lenneberg
(op. cit.) lists as examples of the specific biological
endowments of man necessary to speech: cerebral dominance,
specialization of cerebrocortical topography, special coordin-
ation centers for motor speech, specialized temporal pattern
perception, special respiratory adjustment and tolerance
for prolonged speech.

The existence of these special biological endowments is
the more suggestive because it implies some separation of

linguistic and other intellective functions. The literature
on asphasia and other congenital and traumatic brain
pathologies also (26; 38; 71) suggests that various cognitive
functions may remain unimpaired although language facility has
been severely damaged.

2. the onset of language: critical periods: A
major factor in instinctual patterns as described by ethologists
is the 'critical period' during which the behavior is catalyzed
by aspects of the external environment which are ineffectual
at other maturational periods. The behavior emerges only
when a certain stage of neurophysiological maturation is
reached. If the external environment is sufficiently ab-
normal to preclude the emergence of these patterns at the
maturationally appropriate time, the behavior never appears
in the organism at all. Thus, the study of language
development in feral children would be of critical interest
in this connection. Roger Brown (10) has reviewed the
rather vague literature on this subject, but with the in-
formation available no conclusive statements are possible.
In general, it seems practically impossible to teach language
to those feral children who were outside human society between
the ages of one and six; yet there is evidence that those
children who were cast out or 'lost' were abnormal in the
first place.

On the other hand there are fairly well-documented cases
of children who were isolated from human contact for the

normal period of language development without permanant

linguistic impairment. For example, Davis reports on the case of

a child who was tied to a chair and maintained in silent

isolation for over six years. This child was able to

achieve normal speech (and a high I.Q. score) only two

years after rehabilitation (18).

On the basis of studies of language development in the

normal child, (1, 7, 11, 12, 19, 41, 58), which are now proliferat-

ing, Chomsky and some other transformational linguists are

concluding that the onset of language is sufficiently regular

to support the maturational hypothesis. Below are remarks

from Eric Lenneberg, in support of a biologically determined

description of language:

> The onset of speech is an extremely regular phenomenon,
> appearing at a certain time in the child's physical
> development and following a fixed sequence of events,
> as if all children followed the same general "strategy"
> from the time they begin to the period at which they
> have mastered the art of speaking...
>
> (Lenneberg; op. cit.)

Lenneberg at this point cites some general aspects of the

learning process (e.g., 'the first things that are learned

are principles -- not items') but the precise evidence is

unclear from his report, although he cites a number of

studies in a general way. From our own work on language

development (Shipley et al. 1965), the conclusion of the

34

grammarians on this issue seems questionable. Our subjects

appear to show variation in onset, and seem to display vary-

ing "strategies" of acquisition. It may be that this

variation is 'small' in the context of a lifetime, or in

the context of abstractly possible variation; but it does

not seem to me that there is as yet any basis at all on which

notions like 'small and expectable variation' vs 'huge and

astonishing variation' could sensibly be defined.

Yet so well entrenched is the belief in a regular and

environment-proof onset in development that Chomsky and

Lenneberg have seen fit to cite it as an argument for

innate endowments. Some of the (very paltry, to be sure)

evidence that is now in our possession suggests, on the

contrary, that environment is a powerful factor at least in

affecting developmental rate (63, concerning the retarding

effect of impersonal handling and lack of linguistic con-

tact in an orphanage situation; 31, concerning the accelerating

effects on development of story-reading in lower-class

environments; 53, concerning the low but consistent correla-

tion of language onset with IQ; etc.).

3. asympototic equivalence in adult linguistic
competence: A commonplace among the linguists and philos-

ophers is that, in non-pathological individuals, adult

linguistic competence is uniform. Given the enormous variation

of intelligence, environment, and culture, a finding of adult

equivalence would give credence to an innatist hypothesis.

For example, Katz (42) writes:

> Variation in performance with intelligence [in non-linguistic tasks] contrasts with the performance of speakers with respect to some purely linguistic skill, where no significant individual differences are found...

In a similar vein, Hilary Putnam asserts the independence of language ability from intelligence as an argument for 'a model of [the speaker] as a Turing machine who is processing each new sentence with which he is provided according to some mechanical program.':

> Even a person of very low-grade intelligence normally learns both to speak his particular dialect grammatically and to recognize deviations from grammaticalness...a moron whose parents happen to speak the prestige dialect may have serious vocabulary deficiencies but he rarely has grammar deficiencies. He too learns to speak the prestige dialect, and to feel that there is something wrong with sentences that deviate from the grammatical regularities of the prestige dialect, even if he does not have the extremely complicated skill (parsing) which is required to say what is wrong. But an ability of this kind, which can be acquired by practically anyone or which can be utilized by practically anyone independently of intelligence level, is almost certainly quasi-mechanical in character.
>
> (Putnam, op. cit.)

Experimental evidence for this view of uniform adult linguistic competence is extremely thin. In tests of 'acceptability' (cf. Hill; Sleator and Maclay), reading comprehension (53), and other similar skills presumably related to language facility, we do not find uniformity at all. It is argued by Chomsky that external factors such as

performance decrements and differences in cognitive abilities
account for any apparent difference in underlying competence.
The question of competence vs performance will be taken up
in a later section (Sec. II); here it suffices to say that
the notion of uniform competence is not, to my knowledge, sup-
ported by any conclusive systematic evidence. That a tremen-
dous degree of systematic organization of linguistic data
is a crucial element in the language perception and use of
all normal speakers is well established, particularly by the
studies of Miller and his coworkers (op. cit.); but, as
is obvious, such results are compatible both with hypotheses
of individual difference or hypotheses of equality in degree
of underlying competence.

An argument often cited in favor of the view that all
adults, over a broad spectrum of intellectual ability,
come to a similar level of competence is the appearance of
speech in the feeble-minded. Lenneberg (op.cit.) writes:
'Children whose IQ is 50 at age 12 and about 30 at age 20
are completely in possession of language although their
articulation may be poor and an occasional grammatical
mistake may occur.' Thus as measured IQ decreases with age,
something like normal speech nevertheless develops.

In the main, the evidence for equal competence is
anecdotal (anyone can and does read the Daily News, ergo
everyone is linguistically competent). Anecdotal evidence

for inequality in linguistic competence is even easier to provide, and it is just as unconvincing (only the very best people can read Dylan Thomas, or make a good pun, or write a grammar, and therefore some are more linguistically competent than others). Even where systematic data are cited (Lenneberg, above), the measure defining 'complete possession of language' is left totally unstated. In the development of transformational grammars, in all their variety and richness, we must make use of the most exquisite judgments of the sentences and semisentences of the language. It is claimed that such judgmental precision is necessary in describing languages, and that the possibility of fine judgments is itself evidence of the remarkable competence of the speaker. Were such delicate decisions excluded in the process of writing or justifying a grammar,[1] grammars could not sensibly be devised or tested nor would there be as much reason to exault the complexity of syntax; for this complexity would not be discovered. But the mere production of a few, or a few thousand, dull but 'previously unheard' sentences by the average speaker is taken as sufficient reason to

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[1]Those structural linguists who have no grammatical theory to account for these judgments do, indeed, exclude them on the grounds that they are 'not grammar', and the transformationalists are quick to point out the emptiness of this protest.

endow him with all the wealth of the English language.

Presumably the mere fact that a linguist's butcher can say

"Good morning, Dr. Chomsky; the liver is fine today." serves

as proof that in every butcher there have emerged the subtlest

features of English syntactic structure. It seems to me

selfevident that the matter cannot rest on such flimsy

evidence.

D. Paraphrase: The ability 'to say it in one's own

words' is intuitively taken as an index of understanding.

Some grammatical transforms are simple paraphrases; the

surface structure of the sentence or phrase is changed, but

its deep structure is unchanged; the information conveyed may

intuitively seem to be the same. Transformations effecting

(or describing) sentential forms in this general relation are

often called 'meaning-preserving' transformations; sentential

structures so described are paraphrases of each other.[1]

---

[1]Whether or not such paraphrases are the 'best' para-
phrases intuitively, is another question, i.e. there may be
differences of opinion about whether the best paraphrase
of glass[1] house[3] bird[2] would be:

    (a) a bird who lives in a house made of glass

or (b) a bird who lives in a green[1] house[3]

or (c) a bird who shouldn't throw stones

Without approaching such questions, we can agree that certain
paraphrases rather uniformly acceptable to speakers are described
as transforms in transformational grammars.

Zellig Harris first developed the notion of transforma-
tional grammars when studying the structure of discourse (⌐⁹);
in this work he became aware that a fundamental inadequacy of
traditional grammar was the failure to make explicit clear
intuitive relations among sentences of certain types (e.g.,
active/passive; declarative/interrogative). In order to
normalize a text so that the same information was always
presented in the same way he proposed a grammar that had as
an integral feature the description of certain kinds of
paraphrase intrinsic to the structure of languages:

> Transformations are applicable in various studies
> or utilizations of systems of a generally linguistic
> type...The chief outside uses of transformations, how-
> ever, depend upon their special meaning status. Mean-
> ing is a matter of evaluation and cannot be fitted
> directly into the type of science that is developed in
> structural linguistics or in transformation theory.
> Still, for various purposes it may be possible to set up
> some practical evaluation of meaning; and with respect
> to most of these evaluations, transformations will have
> a special status. That many sentences which are trans-
> forms of each other have more or less the same meaning,
> except for different external grammatical status...is
> an immediate impression. This is not surprising, since
> meaning correlates closely with range of occurrence,
> and transformations maintain the same occurrence range.
> When we have transformations which are associated
> with meaning change, it is usually possible to
> attribute the meaning change to the special morphemes...
> in whose environment the transformation occurs.
> (Harris, 29)

One of the goals of transformational theory at all hands
has been to explain how people perceive systematic relations
among sentences. Since the transformational relationships
are in large part paraphrastic, though of course there are

paraphrastic relationships that are not transformational,
a transformational grammar is a partial answer to the question
of why people think that certain drastically different
sequences of phonemes 'mean about the same thing.'

There is no explicit 'method' described in a grammar
for generating or recognizing paraphrastic transforms, or
anything else. However, transformational grammars do char-
acterize such relationships in a way sufficiently explicit to
support a description of the production and understanding
of paraphrastic pairs. Certainly it is a fundamental
empirical claim of transformational grammarians that
relationships among sentential types of a transformational
and paraphrastic character are in some sense 'known' or
'available' to the speaker; i.e. while the speaker may not,
in general, be able to state the principles of such
relationships -- in this special sense, he does not 'know'
anything about grammar -- his behavior points to an implicit
awareness of these relationships.

Certain theoreticians, notably Henry Hiz (33), have
argued that intuitions concerning paraphrastic relationships
may provide a more pervasive and reliable empirical basis
for the study of syntax than intuitions concerning degrees
of grammaticalness in a single sentence. In fact, the claim

that people can in certain circumstances say the same thing in different ways, or recognize that two 'different' sentences mean the same thing seems to be less open to dispute than the claims concerning the ability to classify sentences according to their grammaticalness.

In 1961, W. H. Livant reported (*50*) on the basis of an experiment with three individuals that adult English speakers could paraphrase or 'interpret' (in effect, give transforms of) previously unheard compound-nouns regardless of semantic bizarreness, given only that they understood the individual words of the compound.

McNeill in a recent article in Science (*55*) studied the use of compound nouns in a different but related way. He asked engineers to derive compounds given relative clauses that might be assigned some technical meaning within engineering. Similarly, he asked them to provide the relative clause, given the compound. The subjects were to some degree able to perform both these tasks, succeeding more uniformly with the conversion to the compound nominal. However, McNeill noted that these subjects did not seem aware of the ambiguity involved in the compound, i.e. in the possibility of retrieving more than one (and meaningfully different) relative clause from a given compound.

Blumenthal (*5*) asked Harvard-Radcliffe students to paraphrase sentences that were instances of triple self-embedding. Most responses not only failed to be paraphrases

but showed that subjects considered them to be jumbled
instances of multiple conjunction.

The evidence for an effective procedure for paraphras-
ing, then, is also thin at this point.

Section II: Performance and Competence

It is immediately obvious that there are systematic
differences between the set of sentences described in a
linguistic grammar and the set of spoken and written sentences,
both current and potential: Some sentences described in a
grammar are too long or complex to be managed by the speaker;
these are in principle unutterable.   There are other
expressions too foolish or farfetched ever to gain any
currency anywhere, anytime; these are in practice unspeak-
able.   On the other hand, many observed sentences are not
described in a linguistic grammar.   Through inattention,
interference, interruption, and so forth, the speaker
produces sentences that the linguist has (presumably)
reason to exclude.   These differences between what the gram-
mar describes and what is said may at least in part be
characterized as performance difficulties fundamentally
distinguishable from the individual's competence in linguistic
matters.   Generative grammars, and, to a large extent, all
transformational grammars, purport to describe competence;
they are not direct descriptions of speech or comprehension:

> Linguistic theory is concerned primarily with an ideal
> speaker-listener, in a completely homogeneous speech-
> community, who knows its language perfectly and is un-
> affected by such grammatically irrelevant conditions
> as memory-limitations, distraction, shifts of attention
> and interest, and errors (random or characteristic) in
> applying his knowledge of the language in actual
> performance.
> (Chomsky, 1965)

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Though linguistic theory therefore is intended as no more nor less than a second-order construct whose relations to actual linguistic behavior may be neither direct nor simple, it should be emphasized that the notion of competence is put forward by its proponents as a concrete psychological reality. Chomsky (1964) writes that "On the basis of a limited experience with the data of speech, each normal human [emphasis ours] has developed for himself a thorough competence in his native language." 'Competence' should be understood to refer both to the speaker's capacity for acquiring (any) natural language under reasonable conditions, and also the product of that learning, i.e. the internalized generative grammar in terms of which the speaker speaks and understands the language.

Because only performance is directly observable, the problem of giving external justification for grammars is very complicated:

> Obviously, every speaker of a language has mastered and
> internalized a generative grammar that expresses his
> knowledge of his language. This is not to say that he is
> aware of the rules of the grammar or even that he can
> become aware of them or that his statements about his
> intuitive knowledge of the language are necessarily
> accurate. Any interesting generative grammar will
> be dealing, for the most part, with mental processes
> that are far beyond the level of actual or even
> potential consciousness; furthermore, it is quite apparent
> that a speaker's reports and viewpoints about his be-
> havior and his competence may be in error. Thus a
> generative grammar attempts to specify what the
> speaker actually knows, not what he reports about his
> knowledge. Similarly, a theory of visual perception
> would attempt to account for what a person actually

> sees and the mechanisms that determine this rather
> than his statements about what he sees and why,
> though these statements may provide useful, in fact,
> compelling evidence for such a theory.
>
> (Chomsky, 1965)

If the analogy with visual perception is a valid one
(I will not attempt to argue this question) it is clear
that under no circumstances would the naive viewer come up
with the statements of optical and physiological fact that
indeed underlie his perception. Nevertheless, we expect that
a theory of visual perception will account for what the
viewer sees, once we have laid aside trivial or irrelevant
interferences (he is judging by his knowledge of the known
size of certain objects; he is not attending to the task;
he has water in his eye). Similarly, we must ask for
evidence from what the speaker says or understands, or
reports concerning his speech or understanding, to support
linguistic theory.

Then in order to relate language behavior to grammars,
extensive psychological investigation may become necessary.
At some point one must learn how to describe some of the
'false' or flawed sentences systematically, or the linguistic
grammar can never be brought back to the level of data.
In brief, there must be some relatively fixed relations
between a linguistic grammar and language behavior if there
is to be any psychologically valid explication of the notion

'linguistic competence'. Ideally, when the linguistically
trivial interferences are excluded, we ought to see the
grammar bare.

Because of the highly abstract and covert quality of this
postulated underlying knowledge, and because of its continual
interlacing in practice with other aspects of reality (i.e.
those linguistically external matters affecting performance),
it will turn out to be -- if it turns out to be -- singularly
elusive; in fact, Chomsky says "...although one might propose
various operational tests for acceptability, it is unlikely
that a necessary and sufficient operational criterion might
be invented for the much more abstract and far more important
notion of grammaticalness."

Thus it is claimed (1) that there exists a covert
knowledge of language describable as a generative grammar
that accounts for the human use of language; (2) that each
normal individual is in possession of this grammar, neuro-
physiologically; (3) that a general display of these facts
will not, in all probability, be forthcoming; however that
(4) there exists sufficient, in fact overwhelming, evidence
from partial displays of competence both to support the
theoretical account and to render further demonstrations
somewhat superfluous. These claims are very strong indeed,
and require from the believer a certain act of faith. The
difficulty in approaching this theoretical stance is
intensified when we read that, at least for some of the

Chomskian linguists, the issue has been reduced to one of

definition:

> A necessary condition for something to be part of the
> subject matter of a linguistic theory is that each
> speaker be able to perform in that regard much as
> every other does.
>
> (Katz, 42)

We need not consider the validity of this statement for

matters of performance, for there it is surely in error;

if every speaker must actually perform as well as the next

before we decide that a linguistic skill is being tested,

then we may never discover any act that is linguistic. In

fact it can be admitted in advance and without reservation

that we will never encounter any task that is purely

linguistic. That is not the point. It is clear from Katz'

statement here (and elsewhere, cf. page 35) that his claim

derives from the belief that competence is universal and

equal in the normal population. If, when individual dif-

ferences are found -- and they will be, as I shall show --

we are to be told, by virtue of this very finding, that they

must therefore be extralinguistic, then indeed linguistic

theory has become invulnerable.

Section III: An Experimental Study of the Use of
               Compound Nouns

     In a restaurant in Philadelphia, named The Pub, there is

a plaque that reads:

                    VOLUME FEEDING MANAGEMENT
                        SUCCESS FORMULA
                            AWARD
                            1963

a tribute, not only to the managers of The Pub, but also to

the productivity of the noun-compounding process in English.

Our admiration is only slightly damped when we discover that

the search for a success formula was made by a trade-journal

named Volume Feeding Management. Yet this finding makes it

appear less likely that the six-word complex above arose

whole-cloth as a paraphrase of:

          the award given for discovering a formula for succeed-
          ing at managing the feeding of people in volumes

Since neither phrase nor paraphrase comes trippingly off

the tongue even after decipherment, we think it plausible

to consider how far the use of such recursive devices is

open to the individual.

     Specifically, we give here a grammatical sketch of

compounding processes in current English, and an experimental

investigation of the knowledge and use of these processes

by English-speaking adults. More generally, we are pursuing

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the attempt to make explicit certain relations between the
language (or its grammar) and linguistic behavior. In the
preceding sections, we have seen that there have been phenome-
nological linguists who deny any distinction between language
and its users. More recently, there has developed a detailed
and sophisticated model which embeds the language in the
speaker. In the grammatical sketch (section <u>A</u> following)
we provide in outline a linguistic description of compound
nominals, from a transformational point of view. In the
empirical work I present evidence concerning the tacit
knowledge of this grammar in speakers.

A. A linguistic description of compound nominals

The term 'compound-noun' refers to certain colloca-
tions of two or more nouns pronounced as a single constituent
under a particular intonation contour. The same term in-
cludes as well certain other ordered collocations of words,
one or more of which may be other than nouns. The term is
often extended to include certain apparently related con-
structions which are adjectival or adverbial in character.

1. <u>Nominal transforms</u>: In English, there are many
sentences containing nominals whose internal structure re-
flects that of independent sentences, e.g.:

<u>That he was happy</u> was clear to us.
<u>The resumption of nuclear testing</u> was greeted with hostility.
<u>Flying planes</u> is dangerous.
<u>Flying planes</u> are dangerous.

We call sequences such as those italicized above 'nominals' because, for one thing, they occupy the same positions in sentences as do simple noun phrases, e.g.:

> The point was clear to us.
> Macnamara was greeted with hostility.
> The lion is dangerous.
> Lions are dangerous.

Furthermore, the complex nominals, just like simple noun-phrases, determine other features of the constructions in which they appear, i.e. if in subject position, they determine the present-tense affix of the verb; they have selectional effects on the choice of the following verb and object, etc.

In short, it is convenient to suppose that these complex entities play a constructional role similar to that of noun-phrases in the phrase structure of the sentence. However, the sentential character of these phrases is also apparent to the speaker, e.g.

> that he is happy seems related to: He is happy.
> The resumption of nuclear testing seems related to:
>   Nuclear testing was resumed.
> Flying planes seems related to Planes fly or, alternatively,
>   to (Someone) flies planes.

In a transformational grammar, sequences like these, having sentential character but phrasal function in the sentence in which they appear, are considered to have 'derived from' underlying sentential constructions, through formal operations that 'transform' them into nominals and

embed them into other constructions. When these complex
forms are treated, on the one hand, as elaborations of the
phrase-structure notion noun-phrase, and on the other hand, as
reflexes of the notion sentence, the description of sentence
structure is kept simple, and selectional restrictions that
are identical for a large number of sentential and nominal
forms need not be restated, e.g.:

> Macnamara was greeted with hostility.
> Wasn't Macnamara greeted with hostility?
> Macnamara's being greeted with hostility amuses me.
> The greeting of Macnamara with hostility is understandable.
> It is treason to greet Macnamara with hostility.

Compound-nouns represent in such a grammar a further
method for 'converting' or 'restructuring' a sentence as a
nominal phrase, e.g.:

> Macnamara-greeting is a foolish hobby.
> Test-resumption would be folly.

2. Compounding: The particular 'process' (or relation)
called compounding is a familiar one in English. In tradition-
al terms, we speak of a compound, or of compounding, when-
ever two or more words or constructions are joined together
in such a way that their independent status is not altogether
obliterated. Thus there is said to be copulative compounding,
e.g.:

> lions and tigers

in which the elements are coordinate; dependent or attributive
compounding, in which one element stands in, e.g., genitive,
or instrumental, relation to the other, e.g.:

the lion's foot

and <u>descriptive compounding</u>, in which one element qualifies
or describes the other, e.g.:

    houseboat
    peapod

It is this last process, <u>descriptive compounding</u>, that is
described below.

Descriptive compounding is not limited to nouns in its
elements, e.g.:

    black-berry
    money-back-guarantee
    car-wash

nor in its 'transformed' phrase-structure status, e.g.:

    boot-black   (a noun-phrase)
    lovelorn   (an adjective)
    down-grade   (a verb)
    onyx black   (an adjectival phrase)

3. Identification and pronounciation of compounds:

Compounds are centrally characterized by intonational pattern.
The actual physical properties of the defining intonations
are not easy to describe. Spectrographic study shows that
the relations between the acoustic and perceptual facts
are very complicated, in part because higher-level grammatical
considerations interact with acoustic cues to determine
the perception of what was said. I will speak here in terms
of certain perceptions about relative pitch and stress that
are more-or-less reliably associated with such constructions;
but the reader should bear in mind that what the English-
speaking listener reports as distinctions of relative volume

(stress, or accent) and frequency (pitch) are often objective-
ly something else, e.g. tempo changes, changes in vowel
quality, elision (slurring) at the boundaries between words.
Thus it is in a rather ill-defined sense that I will be
discussing two major perceptual cues for compounds: (a) a
pitch feature, i.e. pitch rises ($\nearrow$) on the first word of
the compound and falls ($\searrow$) on the last; (b) a stress feature,
i.e. stress is strong or primary ($\underline{1}$) on the first word of
the compound and weak or tertiary ($\underline{3}$) on the second. I will
show (and it has been shown before) in the empirical work
that follows that these loosely defined features in fact
reliably describe the perception of compounds. For the
simplest case of a two-word compound composed of monosyllabic
nouns, we can write these features as follows:

[1] $\overset{\nearrow}{N}{}^{1}\ \overset{\searrow}{N}{}^{3}$

for example: $\overset{\nearrow}{\text{mail}}{}^{1}\ \overset{\searrow}{\text{man}}{}^{3}$

Other grammatical constructions in English often con-
sist of the same sequences of words as compound nouns. For
example, the sequence:

    lady killer

may be a compound (with the meaning -- metaphorical or not --
slayer of ladies), or it may be a phrase (with the meaning
slayer who is a lady). But if this latter sense is intended,
the pronunciation will be characteristically different from

[i] above:

[ii] $\nearrow N^2$ $\searrow N^1$

While the pitch feature is similar -- or perhaps the same -- the stress here is intermediate or secondary ($\underline{2}$) on the first word, and primary ($\underline{1}$) on the second. Such change in meaning as a function of change in stress is a pervasive feature of English, occurring within single words (e.g. $\underline{con\overset{1}{s}ort}^3$ vs $\underline{con\overset{3}{s}ort}^1$) and in constructions, e.g.:

$lady^2bug^1$	(= a bug lady)
$lady^1bug^3$	(= a species of bug, both male and female)
$lady^2bird^1$	(=a bird lady)
$lady^1bird^3$	(= a First Lady)

When the stress pattern of such constructions is essentially that of the adjective-noun sequence in English (the pattern $\underline{21}$), we call the collocation $\underline{phrasal}$; then the term 'phrase' applies to $\underline{lady^2bug^1}$ just as it does to $\underline{old^2bug^1}$. When the primary stress appears instead on the initial word of the sequence, e.g. $\underline{lady^1bug^3}$, we call the sequence a $\underline{compound}$.

The reader will by now have objected that the notion of fixed stress in words or constructions is something of a fiction. There are certainly occasions on which the stress patterns specified here for given meanings are not necessary, or are even wrong, e.g.

I said $eye^2 brow^1$, not $eye^2 lash^1$.

There are, then, overriding considerations of sentential stress (ordinarily called 'emphatic' or 'contrastive' stress patterns) that cause the usual, or 'colorless' or 'normal', stress patterns to be superceded and effectively submerged. As I will show by responses of subjects, the colorless stress pattern is ordinarily assumed by the speaker in the absence of contrastive contest.

Another difficulty in specifying the stress of compounds arises because there is a very definite and very small number of distinctions of stress and pitch that can be maintained reliably by listener and speaker. Then as soon as the compound becomes longer (has polysyllabic elements or has more than two elements) these features become confusing and distinctions wash out. There are three, just possibly four, distinctions of stress that are produced and perceived reliably; there is rising, steady, and falling pitch; there are in addition a few perceptible changes in tempo and vowel quality. The objective details of this intonation system are not altogether well known. What is clear is that as the compound grows longer there are insufficient distinctions available in the language for keeping some sequences apart. For example, while we can distinguish between the two cases of <u>lady bug</u>, we cannot so easily tell whether a <u>lady</u>^{1} <u>bug</u>^{3} <u>house</u>^{2} is a house for the (species) lady bug, or a house for female bugs. Occasionally, even in this simple case, a

speaker may report the paraphrase <u>asylum for females</u>,
suggesting that he heard <u>lady^2 bug^1 house^3</u>. These difficulties
for the hearer rapidly become insurmountable; it is impossible,
without further context, to know what an <u>old lady bug house</u>
<u>keeper</u> does for a living.

<u>4. Grammar of compound nominals</u>: In modern grammars,
the kinds of phrases and compounds of which we have been
speaking are ordinarily described by what are called
transformational rules. These are, very roughly, state-
ments describing relations among sentential structures. In
generative grammars, more complex structures are derived
from simpler sources, and further 'derived structure' is
assigned to the complex expression. Presumably the speaker
is in some tacit sense aware both of the immediate ("surface")
structure of the sentences he hears or says and also aware
of the ("deep structure") sources of these sentences, i.e.
the simpler structures from which they are derived, and the
mode of that derivation. For example, the listener who
hears the sentence

John is approached by spies.

is aware that <u>John</u> is the subject of that sentence, in the
sense that the singularity of the word <u>John</u> determines that
the following word must be <u>is</u> and not <u>are</u>. At the same time,
he knows that <u>spies</u> is in some deeper sense the subject of

the sentence, i.e. that the Passive is related in a very
definite way to the Active structure underlying:

Spies approach John.

The fragment of a transformational grammar describing
compound nominals thus attempts to explain the sense in which

a wood^1 box^3 (a compound-noun)

is palpably akin to such apparently different constructions
as:

a box for wood   (a prepositional phrase)
and   a box that holds wood   (a relative clause)

Further, a complete grammar would seek to explain some
rather more distant relatives such as:

A box holds wood.

and even:

Does a box hold wood?
Is wood what a box holds?
A box does not hold wood.

The network relating all these forms does service, of course,
for an unbounded number of sequences differing morphologically,
but not structurally, from this example, e.g. brick house;
teakwood box, thus -- if successful -- describing at once
the wealth of possible utterances of the language and the
organizational principles explaining their rather uniform
comprehensibility.

a. Compound Nouns[1]: We have noted that the immediate
paraphrastic kin of compound nouns seem to be relative
clauses and prepositional phrases. The relationship can be
roughly stated as follows:

$$[\text{iii}]. \; N_1 \left\{ \begin{array}{c} P \\ \left\{ \begin{array}{c} \text{that} \\ \text{wh-} \end{array} \right\} \;\; (\text{be}) \;\; V \;\; (\text{+en by}) \end{array} \right\} \;\; N_2 \;\; (\text{+plural}) \rightarrow N_2^1 \; N_1^3 \;\; [=N]$$

where N = noun; V = verb; P = preposition; + indicates an
affix; braces indicate a choice; parentheses indicate optional
elements; → is the relation said to hold between the two sides
of the formula. The bracketed note following the formula
indicates the structure assigned to the right-hand (output,
derived) expression.

For example:

        a gear that drives a wheel - a wheel gear
        a gear for wheels   - a wheel gear

The source or prior structure for compound-nouns is thus
said to be a noun-phrase containing a relative (that), or a
preposition. The structure of outputs (the right-hand form)
is again a nominal structure, a unit: N. This will mean

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[1]The grammatical description given here is neither
complete nor wholly original. Much is owed to R. B. Lees (45)
who is nevertheless not responsible for errors here. The
material is presented to provide context for the experimental
work and is in no sense a "definitive" grammar.

that the rules are reapplicable, using their own outputs in further inputs. For example, if <u>wheel gear</u> is an output of [iii], that output is a noun, so it may appear in the left-hand of [iii], e.g.:

        a wheel gear for trains - a train wheel gear
        a train that uses a
          wheel gear   - a wheel gear train

Thus a single formula can describe all the (infinitely many, arbitrarily complex) expressions of this type, at the same time providing a statement of the syntactic properties of all its outputs, i.e. predicting the constraints on its relative occurrence within sentences.

Notice that if N_2 is plural, the plural affix is lost in the compound form. This will mean, in the general case, that nonfinal elements of compound nouns have -- like adjectives -- no inflexional affixes.[1] The mechanism of compounding, essentially, is inversion of the two nouns, marked by the addition of a characteristic intonation pattern.

---

[1]Because the plural affix is in part homophonous with the genitive affix, certain examples (e.g. <u>calves liver</u>) appear to be exceptions. However, particularly in compounds with genitive sources (which will not be discussed here), there are unambiguous exceptions, such as <u>women's club</u>. There may be isolated real exceptions when [iii] is in fact the syntactic relation involved, e.g. <u>purple people eaters</u>. At any rate, such exceptions seem to occur only in case the noun involved does not take the morph <u>+s</u> as plural, but has instead an 'irregular' plural. Thus <u>purple person eaters</u> (in either reading) is permissible, if stodgy, while <u>purple persons eaters</u> is ungrammatical.

It is clear from formula [iii], that the 'conditions'
or prior structures associated with compounds are many and
varied. Then very different nominal expressions are matched
by the same compound, e.g. in principle a <u>horse cart</u> might be:

a cart that is shaped like a horse	(as in <u>box car</u>)
a cart that is pulled by a horse	(as in <u>dog sled</u>)
a cart that a horse rides in	(as in <u>hay wagon</u>)
a cart for a horse	(as in <u>dog house</u>)
a cart that is as big as a horse	(as in <u>horse radish</u>)

Further, there may be 'set' or 'frozen' meanings of the same
phrase that are only metaphorically -- if at all -- related to
the independent meanings of the element words of the com-
pound as in, e.g. <u>horse play</u> (boisterous play), <u>horse sense</u>
(common sense), <u>cartwheel</u> (an acrobatic trick), <u>Charley horse</u>
(stiffness from muscular strain). We shall have more to
say concerning both the ambiguity of compounds and the character
of set phrases after further "rules" for regular compounding
are discussed.

    <u>b. compound nominals with verbal components</u>: Further
compounds are formed from the same source structures by
essentially the same mechanisms (inversion and addition of
the characteristic intonation pattern) by deletion of a
nominal rather than the verbal or prepositional elements:

$$[iv] \quad N_1 \quad \begin{Bmatrix} that \\ wh- \end{Bmatrix} \quad V+tense \quad N_2 \rightarrow \overset{\searrow}{N_2^1} \overset{\searrow}{V^3} \quad [=N]$$

for example:

a gear that drives a wheel - a wheel drive

$$[\text{v}] \quad N_1 \quad \begin{Bmatrix} \text{that} \\ \text{wh-} \end{Bmatrix} \quad V\text{+tense} \quad N_2 \rightarrow V^1 \, N_1^3 \quad [=N]$$

for example:

a gear that drives (a wheel) - a drive gear

c. compounds with copulative verbs: Certain other compounds seem to be associated in a very similar way with relative clauses containing the copula be:

$$[\text{vi}] \quad N_1 \quad \begin{Bmatrix} \text{that} \\ \text{wh-} \end{Bmatrix} \quad \underline{\text{be}}\text{+tense} \quad N_2 \rightarrow N_2^1 \, N_1^3 \quad [=N]$$

for example:

a friend who is a girl - a girl friend

d. naming: Other compounds are paraphrastically related to clauses that provide names, e.g. Leyden jar, Main Street; and to species names, e.g. elm tree, hound dog, blueberry pie. These are perhaps most easily described as an extension of [vi], which will be restated below.

e. compounds with adjectival elements: Noun phrases with adjectival modifiers can be described through their relationship to predicative phrases, i.e.:

$$[\text{vii}] \quad N \quad \begin{Bmatrix} \text{that} \\ \text{wh-} \end{Bmatrix} \quad \text{be+tense} \quad A \rightarrow A^2 \, N^1 \quad [=N]$$

for example:

　　　a man who is stout - a stout man

Notice that the stress is phrasal, not compound. However,
there are very many cases of adjective-noun collocations which
have instead the compound stress patterns, e.g. blackbird,
White House. These constructions have obvious relations to
the naming processes sketched in section d above. The mean-
ings of these phrases are frozen in rather specific ways.
Thus, for example, while

　　　a pure^2 blue^2 bird^1

must certainly be blue from tip to tail,

　　　a pure^2 blue^1 bird^3

may (and does) have a distinctly reddish breast. Certainly
the blue^1 bird^3 was so named because it is a bird that is
blue, but the expression blue^1 bird^3 does not refer to all
birds who are blue, but only to a particular species. No
rule of grammar can predict whether an adjective-noun pair
is to be pronounced as a compound or as a phrase. In effect,
this is what is said by lexicographers when they list and
define the compound blue^1 bird^3, but do not list the phrase
blue^2 bird^1. The latter can be understood by reference to
its element words and rule [vii]; the special meaning of the
former must be given lexically. The difficulty in this
situation is the openness, or productivity, of the naming
operation. Where elements, or even constructions, are

finite in number and relatively fixed in membership, simple
listing is more or less sufficient descriptively. However,
recursively defined constructions cannot in principle or in
practice be listed exhaustively. Hence, for Language and
for speaker, it is assumed that some rule such as [vii] is
involved. In this instance, what are not described are the
semantic conditions that may give rise to the compounding
stress pattern. Lees (op. cit.), in a very revealing dis-
cussion of these matters, points out that there are naming
operations that maintain phrasal intonation without being
in any structural or semantic way obviously different from
those taking compound intonation, e.g. <u>Near East</u>, <u>Tiny Tim</u>,
<u>common cold</u>, <u>Big Ben</u>. It suffices here to provide the
general syntactic picture by generalizing rule [vi]:

$$[\text{viii}] \quad N_1 \left\{ \begin{array}{c} \text{that} \\ \text{wh-} \end{array} \right\} \text{be+tense} \left\{ \begin{array}{c} \text{named} \\ \text{like} \end{array} \right\} \left\{ \begin{array}{c} N_2 \\ A \end{array} \right\} \rightarrow \left\{ \begin{array}{c} N_2^1 \ N_1^3 \\ A \end{array} \right\} \quad [=N]$$

    <u>f. frozen compounds and the 'appropriate' verb</u>: In
section <u>a</u> above, I listed a number of theoretically possible
meanings for the compound <u>horse cart</u>. However, a horse cart
is just a cart that is drawn by a horse. We may know this
either conventionally -- because the compound has become a
lexical item with a set or frozen meaning -- or because of
certain semantical relations between the elements of the

compound that lead to only one or a very few 'sensible' or 'likely' things that it could mean.

1) set compounds: We know that fly paper is paper for catching flies, not paper that flies write on or paper that flies. This is in no sense given in the syntax of the compound. A particular meaning has been 'frozen onto' the compound, even though its underlying structure is still apparent. We know why fly paper is called fly paper, but it will contine to be called fly paper even if we catch spiders in it; further, we will probably avoid calling paper that flies write on fly paper. Such set compounds must be entered into a grammar as lexical items, and they may be expected to be handled psychologically in ways quite different from compound constructions. Work with children (Livant ; Berko) shows that frozen compounds are, in general, per- ceived initially as simple lexical items, and their con- structional features are only 'discovered' later in development, and very possibly by reasoning processes that are largely extralinguistic in character. (My children tell me that a black board is called a black board because you write on it.)

2) appropriate verbs: Zellig Harris (3c) has pointed out that there are cooccurrence relations in the language that serve to disambiguate compounds formed on the basis of rule [iii]. Your mail man is the man who brings your mail; the man who burns your mail is not your mail man,

unless, perhaps, you are with the CIA. The man who burns
your leaves in the autumn might conceivably be called your
leaf man, but the vandal who burns your leaves in the spring
is not your leaf man. Certain verbs are very generally
associated with compounds (e.g. bring, yield, look like), and
certain other expressions will always 'do', in a general way,
although they are not very revealing (e.g. 'in some way
characteristic of'). The apparent generality of some
of these associations has led to some attempts to choose some
such verb or phrase as a specific to the compounding process,
i.e. to substitute for the V of formula [iii] a specific item.
This enterprise does not succeed very well, yet it fails to
fail altogether; this state of affairs is awkward. I re-
frain here from any attempt to characterize the notion of
appropriate verbs syntactically; we shall see, however, that
it has a great deal of vitality as a psychological concept.

g. exocentric and endocentric constructions:
A dog house is a house, of a kind. It is just that kind of
house that dogs live in. The cooccurrence restraints (or
privilege of occurrence) for dog house overlap considerably
with the restraints on the cooccurrence of house, though there
are some gross differences. On the other hand, the cooc-
currence range of dog is not so nearly like the cooccurrence
range of dog house as is house. In this sense, house is the

'head' of the phrase, <u>dog</u> a limiting modifier.[1] Compounds whose elements stand in this modifier-head relationship are called <u>endocentric</u>.[2]

On the other hand, the cooccurrence range of <u>cut throat</u> is close neither to that of <u>cut</u> nor that of <u>throat</u>. It is close to that of <u>hoodlum</u> or <u>thug</u> or <u>hood</u>. This kind of compound is called <u>exocentric</u>. There are compounds on most of the models we have described that are exocentric, e.g. <u>mouthpiece</u> (a lawyer), <u>deadbeat</u> (a sponger). Exocentric constructions are by definition frozen, because their meaning

_____

[1]There are additional reasons to call the second element, rather than the first, in such constructions the head. The second element will determine, for example, whether the compound requires an article (<u>the</u>, <u>a</u>), e.g.:

<u>We prefer the rice package.</u>	but not <u>We prefer rice package.</u>
<u>We prefer the package rice.</u>	and also <u>We prefer package rice.</u>

This distinction can be accounted for by assuming (1) that <u>rice</u> is the kind of noun that occurs freely in the singular without an article, e.g. <u>We prefer rice.</u> and also <u>We prefer the rice.</u>, while <u>package</u> is not; and (2) that the second member of the compound is its head.

[2]In describing the distinction between exocentric and endocentric compounds I am following Lees (op. cit.).

cannot be reconstructed by reference to the lexical items
they contain and internal features of the construction.
That this is so is a matter of degree. An ingenious speaker
who had never heard the words <u>cutthroat</u> or <u>deadbeat</u> might
conceivably figure out the senses in which these words are
intended in speech. But these meanings are not 'pregiven'
by reference to a regular paraphrase. It is because the
constructional features that (at some time or other) gave
rise to the compound are retrospectively clear (i.e., if we
know that a <u>cutthroat</u> is a <u>hood</u>, it is obvious that a
cutthroat was called a <u>cutthroat</u> because he cut people's
throats) that grammarians are reluctant to demote these
constructions to the status of mere words. It is a
psychological question, open again for each such presumed
compound and open again for each speaker, whether the
compound has any perceived underlying constructional
stature.

All exocentric constructions are treated here as mere
words. This decision is a strategic one, thus in part
arbitrary. The problem of treating these items as constructions
is that we are then faced with an immense and unmanageable
continum. Consider, for example, the problem of deciding
which of the following compounds with <u>dog</u> are real
constructions (only exocentric) and which are frozen com-
pounds (i.e., lexical entries): <u>dog ape</u> (a baboon), <u>dog bane</u>

(genus <u>Apocynum</u>), <u>dog cart</u> (a cart drawn by a dog), <u>dog days</u> (the sultry part of the summer), <u>dog fight</u> (a melee of air-planes), <u>dog fish</u> (any of several small sharks), <u>dog paddle</u> (a swimming stroke similar to that used by dogs), <u>dog rose</u> (<u>Rosa canina</u>), <u>Dog Star</u> (<u>Sirius</u>), <u>dog tooth</u> (the tooth of a dog), <u>dog trot</u> (a gentle trot, a dance), <u>dog wood</u> (<u>Cornus</u>), <u>dog watch</u> (a watch of two hours on shipboard). I consider a compound a compound only in case (1) the transformational paraphrastic relations for the item are appropriate and (2) the usual alternative interpretations are appropriate and available. Thus <u>dog cart</u> is a compound only in case it may be a cart in which a dog rides as well as a cart which a dog draws.

<u>h. moribund processes</u>: In the preceding sections, I have described what I believe to be the major productive processes of compounding current in American English. There are, in addition, certain classes of apparent compounds constructed according to models that are no longer in active use. Major examples are verb-object compounds (such as <u>cut-throat</u>, <u>pickpocket</u>, <u>shunpike</u>) without the inversion of order common to all the active processes described. I will contend that English speakers no longer make use of this model for the creation of new nominals in the language.

1. comparative modification: A further construction is described here, though it is perhaps unrelated to the constructions under consideration. Its outputs are, however, often strings identical to compounds and nominal and adjectival phrases simply because of the extensive lexical ambiguity in English between common nouns and adjectives. Such constructions will show up in the empirical work sufficiently often so that we ought to have a name for them:

[ix] as A as N → N^2 A^1 [=A]

for example:

    as cold as ice - ice cold
    as red as a rose - rose red

Notice that these adjectival phrases are subject to nominal interpretation (a Communist rose).

j. parts of speech: Lexical assignments are represented in a grammar as, e.g.:

    [x]   N → bird; house; man;...
    [xi]   A → glad; wry; sick; ...
    [xii]   V → eat; sing; speak;...

As we have seen, there are ways of adding members to lists like [x], e.g. through rule [iii]. A recursive rule is used rather than simply adding, e.g. horse cart, to [x] because, as [iii] implies, there are arbitrarily many complex members of [x].

Some simple words are cross-classified, i.e. they appear on more than one lexical list. For example, dry is used

both as adjective and as verb. There are also cases of
words that appear on one list and appear again with a fixed
addition (affix) on another list. For example, many verbs
act as nouns in sentences when they have the suffix -tion.
Since, to a limited extent, it is possible to add -tion to
words and constructions that never had them before, it is
not only convenient but necessary to describe this feature
of the language by rule; otherwise an exhaustive lexicon
cannot be provided.

There is a case intermediate between cross-classifica-
tion and change of class-membership by affixation: often
a word is used in more than one way without the addition of
an affix; yet one use seems intuitively 'basic', the other
less so. For example, yellow is clearly an adjective.
Yet it may be used as a nominal (Yellow is my favorite
color) and even as a verb (The parchments yellow). If the
grammar that is correct is just that grammar that describes
the speaker's intuition about the language, it would be
convenient to mirror the feeling of 'basic classification' as
a simple list-entry, and to derive other classifications by
rule (i.e. by assuming that yellow as a noun or verb was a
special case of affixation). There is no formal justification
for such a move, but we will see that there is ample evidence
of intuitive knowledge of basic classification.

B. An empirical study of the use of compound-nouns

Three general questions are raised in these experiments:

(1) In what ways is a linguistic grammar describing the capacities of individuals? Although the informal evidence for some projective device in individuals is overwhelming, a private grammar may be more limited than that describing the Ideal Grammarian. Certainly the view of man as algebraist is rather novel in psychological circles (see, however, recent formulations, e.g. Miller et al. 1963; Neisser, forthcoming).

(2) If people 'have' such grammars, what linguistic activities does this make possible for them? Can they make decisions about grammaticalness? Can they paraphrase? We know that a grammar is a description of something now called 'competence', but competence at what?

(3) How uniform are people in their organization of the language and in their linguistic activities? If there are no significant differences in adult linguistic behavior (as, e.g., Chomsky, Putnam, Lenneberg, and Katz suggest), then we can fix the relations between the grammar and speech in essentially simple ways.

It has seemed reasonable to many linguists to endow each person with all the wealth and richness of the language,

regardless of the fact that this undercuts any broadening

of the aims of linguistic theory: it would seem on the face

of it that the study of style, fluency, and articulateness

would be related at least in part to differing approaches

and capacities in matters of grammar. If all speakers must

be alike in some aspect of language use for that aspect

to be part of linguistic theory at all, then linguistic

theory cannot be counted on to provide insight into

features of language use that presumably concern us. This

seems a highly arbitrary and unjustified limitation to place

upon linguistics, one that would rob it of texture and

descriptive capacity.

The particular aspect of language use I study here

is that of compound-nouns, and certain related structures

in English. Compound-nouns offer a very convenient aspect

of English for study. Their complexity and internal structure

can be varied considerably without adding to the phrase

either in length or in vocabulary. Compounds are often

ambiguous as strings, overlapping with various other

nominal and adjectival structures, partially because of the

lexical ambiguity, in English, of most of the short words

in the open lexical classes. Compounding at a considerable

degree of complexity is a frequent and familiar productive

activity in English, one that shows up at a relatively early

stage in linguistic development, and one that is used without

restraint even in the most rudimentary discourse:

> I like to box.
> How I like to box!
> So, every day,
> I box a Gox.
>
> In yellow socks
> I box my Gox
> I box in yellow
> Gox box socks.

(Seuss, 1960)

With these materials, I have studied the way individuals

paraphrase under experimentally controlled conditions. In

earlier discussion (cf. Section I), I suggested why para-

phrasing might legitimately be considered a feature of be-

havior relevant to linguistic competence. It goes without

saying that the proponents of the notion 'competence' may

disagree with this estimate, and they are certainly free

to do so, although they have themselves rested much of the

claim for grammatical intuition on the presumptive ability

to paraphrase. In my opinion, the ability to perceive

sentential relationships of a formally statable kind -- and that

is what paraphrastic transforms are -- would be a basic

prediction of the theory of an internalized generative gram-

mar, once it is granted that this hypothetical competence

makes the individual competent to do anything at all with

language.

In <u>The Structure of a Semantic Theory</u> (43), Katz and

Fodor comment concerning the understanding of complex

nominals:

> It is only because there is a systematic way to under-
> stand the meaning of such constructions in terms of the
> meanings of their parts that the infinite stock of
> strings produced by the grammatical mechanism for
> creating new modifier-head constructions can be employed
> by speakers to refer to familiar objects.

The assumption that will concern us in this is that

speakers are presumed to understand a compound (excluding

moribund and frozen forms) if they understand the separate

words comprising it and, of course, if they understand the

structure of the compound.

I have mentioned briefly an experiment by W. H. Livant

(50, op. cit.) with three individuals that purported to

show that English speakers could paraphrase or 'interpret'

previously unheard compounds regardless of semantic

bizarreness. A paraphrasing task of the kind Livant suggests

would provide the first kind of evidence that ought to be

looked for in support of the theoretical remarks of Katz and

Fodor.

Livant's stimuli were three-word sequences all contain-

ing the same words (<u>black</u>; <u>bird</u>; and <u>house</u>) in all six orders

and with two stress patterns common to compounds (<u>132</u>, <u>213</u>),

for a total of twelve stimuli. He reports that his subjects

were uniformly successful in providing paraphrases. The
implication of this result is that speakers are able to
generate at will sentential structures related (in the grammar)
in very specific ways to compound nouns and certain other
nominal and adjectival phrases.

It is worth submitting this issue to a more general and
rigorous test. Uniform success in this task would give
credence to the rather common view among transformationalists
that there is little individual difference in adult linguistic
ability. A finding of asymptotic similarity in adult lin-
guistic competence would strongly support the view of lan-
guage as 'instinct'. Such results would give detail and
texture to the notion of grammatical competence as a psycho-
logical phenomenon. It is plausible that the individual
'in possession of' a transformational grammar might recognize
meaningful and grammatical relationships among sentences
with closely related morphology and deep structure. An
empirical display of the fact that people can generate
paraphrases at will without contextual suuport would be a
result of great strength concerning the breadth and conscious
availability of grammar. That adults could uniformly pro-
vide a particular kind of paraphrase under rather unnatural
and difficult circumstances would be startling.

In this study, as in any laboratory situation, we can
expect the results to be confounded with all sorts of

interferences that can be called, for convenience, 'perfor-
mance errors' unrelated to what we set out to study. Al-
though we cannot exclude these factors, we can hope to pro-
duce a situation in which such extraneous effects can be
extricated from the phenomenon under investigation.

1. Pilot work: In the course of studying the use of
compound-nouns by children, I attempted to replicate Livant's
results with adults. Livant does not report the conditions
under which he presented the stimuli to his three subjects,
but I assumed that the only realistic test would be an oral
presentation (because some of the grammatical cues for these
constructions are not reflected in English orthography). I
was not able to reproduce his results: where his subjects
had uniformly succeeded in providing acceptable paraphrases
for the compounds, none of my twelve subjects performed
perfectly. Table I summarizes the results of this work.

Table I shows that where the compound can be derived
using only rules [iii] and [vii] or [viii] subjects give a
correct response over 80% of the time. Where the derivation
involves any other rule([iv] or [ix]), subjects are correct
only about 29% of the time.

Notice that errors are associated here most strongly
with the requirement to use rule [iv]. Is rule [iv] 'difficult'?
Not necessarily: it is only that neither bird, black, nor
house is a verb. It is possible to view black as a verb

as a verb (i.e. to add black as a member of rule [xii]),
but this requires at a minimum an extra computation, and at
a maximum an exercise of ingenuity.

The simplest description of these facts is: where black
can be viewed as Adjective, the responses are correct most
(90%) of the time; where black is viewed as Noun (color-name),
responses are correct less often (62%), and where black is
viewed as Verb, responses are rarely (12%) correct.

Each subject in this experiment did best when black was
used as an adjectival or nominal modifier; there was no
exception. Each subject did worst when black could be
interpreted as a verb. Those subjects who found a legitimate
paraphrase when black was used as a simple noun were the only
subjects who found a legitimate paraphrase for any instance
of the verbal use of black. All subjects who had a college
education found a legitimate paraphrase when black was used
as a simple noun; no subjects without a college education
found a legitimate paraphrase when black was used as a simple
noun.

My first supposition was that less sophisticated subjects
might have had trouble understanding or accepting this strange
task. Some of the responses called for are very bizarre
(a bird who blackens houses). All understood the task to
at least this extent: they gave responses that were noun-
phrases containing a relative clause. But not all matched

Stimulus	Sample correct response	Subjects who gave correct response		Rule describing:	
		#	%	intern. compnd.	extern. compnd.
1. $bird^2$ $black^1$ $house^3$	a black house for birds	10	83.3	viii	iii
2. $black^2$ $house^1$ $bird^3$	a pet bird that is black	12	100	iii	vii
3. $house^1$ $black^3$ $bird^2$	a bird who blackens houses a bird as black as a house	2	16.7	iv ix	viii viii
4. $bird^2$ $house^1$ $black^3$	a bird who blackens houses	0	0	iv	viii
5. $house^2$ $black^1$ $bird^3$	the black bird that lives in the house	12	100	viii	iii
6. $bird^1$ $house^3$ $black^2$	as black as a bird house stuff for blackening houses	8	66.6	iii iii	ix iv
7. $bird^1$ $black^3$ $house^2$	a house which is black as a bird a house where those who blacken birds live	2	16.7	ix iv	viii iii
8. $black^1$ $house^3$ $bird^2$	a bird who lives in a blackhouse	10	83.3	viii	iii
9. $house^2$ $bird^1$ $black^3$	stuff for blackening birds that is kept in the house, one who blackens birds who lives in the house	2	16.7	iv	iii
10. $house^1$ $bird^3$ $black^2$	that black that is characteristic of certain housebirds stuff for blackening the bird that lives in the house	7	58.5	iii iv	ix iii
11. $black^2$ $bird^1$ $house^3$	a birdhouse painted black	11	96.6	iii	vii
12. $black^1$ $bird^3$ $house^2$	a house for birds who are black; a house for blackbirds	10	83.3	viii	iii

Table I

Results of Replicating Livant's Study with 12 Subjects

the 'right' nounphrase with the stimuli.

As a first check on the possibility that some subjects simply lack the ingenuity to come up with an acceptable response, I retested two subjects; as follows. I told these subjects that there were 'right' and 'wrong' answers. I repeated the stimuli, giving the subject the right answer, and asking him to compare this with (his own) wrong answer. Though they were willing to agree that my answers were 'better', on a retest an hour later they made the same errors.

2. Experiment 1: the free generation of paraphrases: I was now persuaded that there might be a real difficulty, for some subjects, in understanding complex compound nouns without strong contextual support. At the same time, I had found no subject who had trouble with the task of paraphrasing itself: unless the stimulus was complex in some very special way, each subject had been able to provide legitimate paraphrases.

I therefore decided to test (1) the effect of educational level (hence, hopefully, intelligence) on performance in this kind of task, (2) the effect of structural complexity of the stimulus and, (3) the effect of semantic interference with a structurally determined response, i.e. the tendency -- in the pilot study -- to say bird[1] house[3] as part of almost any wrong response.

a. stimuli: In the pilot-study, it seemed that the subjects did best when black could be interpreted as a prenominal adjective.

Yet the stress and position of the word black in these compounds does not always permit this simple interpretation. In that case, either the subject is going to come up with a more complicated interpretation or he must give what I will call a 'wrong' response.

To test this general hypothesis, we chose members of various lexical categories permissible in compounds: noun, verb, adjective. Some words chosen belonged to more than one category, so that there was a choice of interpretations. For some of these, we could decide intuitively on a primary (basic) classification and a secondary (derived) classification. Presumably, the correct answer is harder to come by if the structure of the compound requires the use of one of these derived category memberships.

We chose twelve words to vary with bird and house (these, in the spoken language, unambiguous nouns). Each word is simple (according to Thorndike and Lorge (76)), monosyllabic (to avoid internal stress complications), and has no inconvenient morphophonemic interactions with bird and house. All stimuli contain the words bird and house, and one of the 'test' words. All six orders of three words and two stress patterns

(132, 213) occur. Then there are 144 stimuli. The stimulus list appears as Appendix A. All 144 were presented to each subject. Table II gives the twelve test words, and their presumed linguistic classifications. Here and there, there may be some disagreement with the classification given in Table II. The reader may, for example, think that the use of glass as noun is not as 'basic' as its use as adjective. Further, it is clear that glass and black, both classified here as adjectives, differ in some major respects syntactically.[1] There is no need to resolve these issues in advance of the experiment.

Of the 144 stimuli, there are grammatical interpretations for 132; the 12 stimuli containing the word eat are ungrammatical, or at best awkward.

We recorded the stimuli on tape so that all subjects received the identical acoustic information. The stimulus-tape was pretested to assure that the relevant acoustic information (stress, clarity, etc.) was there to be heard.

_____

[1] You can say a very black house but not a very glass house, a blacker house but not a glasser house or a more glass house.

Test word	Basic classifications	Derived classifications
black	adjective	noun, verb
stone	adjective, noun	verb
glass	adjective, noun	
dry	adjective, verb	
thin	adjective	verb
bright	adjective	
boot	noun	verb
foot	noun	
shut	verb, past participle	adjective
wash	verb	noun
kill	verb	noun
eat	verb	

Table II

Lexical classification of test words.

To this end we taught two of our staff (one clerical, one professional) how to indicate stress, and had them transcribe the stimulus tape. There was 95% agreement. The 5% of stimuli that were not described indentically by these judges were recorded and retested until complete reliability of transcription was achieved.[1]

b. order of stimuli and presentation: Stimuli were randomized by blocks of twelve according to test-word and also according to 'structural type' (i.e. stress and order of words).

Subjects in each experimental group (see below) were divided at random into three order-groups. For subjects in order-group λ , the tape was started at the first taped stimulus; for those in group $\underline{\beta}$, the tape was started at the 37th stimulus; for those in group γ at the 49th stimulus.

---

[1]There is more than one intonation contour associated with each of the stress patterns used here; probably they differ in tempo. We studied some of these contours to see whether subjects could distinguish between them. We found that subjects could distinguish between the pattern intended for $\underline{pea}^1$ $\underline{soup}^3$ $\underline{green}^2$ (as green as peasoup) and $\underline{pea}^1$ $\underline{soup}^3$ $\underline{green}^2$ (the vegetable you put in peasoup). Reliability in making this distinction was well over 80% for three respondents. The stress-pitch patterns we chose for study were those of peasoup green (the vegetable).

Stimulus $\underline{1}$ was unfamiliar and semantically odd but grammatically possible ($\underline{\text{foot}^1 \text{ house}^3 \text{ bird}^2}$), Stimulus $\underline{37}$ was partially familiar and semantically transparent ($\underline{\text{stone}^2 \text{ bird}^1 \text{ house}^3}$), and Stimulus $\underline{49}$ was unfamiliar, bizarre, and ungrammatical ($\underline{\text{eat}^1 \text{ bird}^3 \text{ house}^2}$). Varying the order of presentation allowed us to check whether a subject might be affected all through the test by particular features of the first stimulus he faced. Since the order of presentation was changed by starting the same tape in different places -- not by using other recordings -- all subjects nevertheless finally heard physically identical stimuli.

c. subjects: All subjects were female, essentially monolingual English speakers; most were mid-Atlantic and mid-Western in background, but one was Canadian. All were between the ages of 19 and 36.

Group A: Seven graduate students in various fields.
Group B: Seven undergraduates and college graduates
           who had no intention of doing graduate work.
Group C: Eleven high-school graduates who had no
           intention of going to college.(secretaries).

All subjects were paid at the same rate for participation in the study. All were told that they were 'participating in an experiment about the way people use English'. Four subjects from group C were later discarded because of possible failure to understand the directions. The criteria for discarding subjects are discussed later.

        <u>d. instructions</u>: The experimenter read the instructions to each subject. If the subject seemed confused, the experimenter was to repeat and amplify -- but not add other kinds of information. In fact, the subjects seemed to understand the directions as they were given:

      This is an experiment on English compound nouns. Examples of English compound nouns are <u>milkman</u> and <u>money back guarantee</u>.

      I will play some compound nouns on this tape recorder. When you hear one of these compounds, I would like you to give me a phrase that means about the same thing. For example, if I say <u>milkman</u>, you would say <u>a man who brings the milk</u>, or something like that. If I say <u>money back guarantee</u>, you would say, <u>a guarantee that you'll get your money back</u>. I do not want you to just use the same words in another sentence. For example, the sentence <u>Milk falls on the man</u> contains the words <u>milk</u> and <u>man</u>, but it does not mean the same thing as <u>milkman</u>. <u>A man who brings the milk does</u> mean about the same thing as <u>milkman</u>. <u>A guarantee that you'll get your money back does</u> mean about the same thing as <u>money back guarantee</u>.

      All the compounds you will hear will be three words long. To make sure you have no problems about hearing them, here is a list of the words that will appear in various combinations of three. [subject is here given a typed list of all the words]. No other words will ever be used.

      Some of the compounds you hear may be familiar to you, and you may know what they mean. Others may never even have been said before by anyone. Try to think what they might mean if they were ever used. Sometimes this may be something that strikes you as silly or even impossible. For example, I am sure you have never heard the words $\text{duck}^1 \text{ food}^3 \text{ man}^2$. Try to think what it might mean if it were used. I guess it would be <u>a man who makes duck food</u>, or <u>a man who brings food for ducks</u>. Is this clear?

      You will hear about 150 of these. Each time you will give me a sentence or phrase that means about the same thing as the compound you hear. If you want me to repeat one of them for any reason, just ask. Take as long as you like to answer.

Sometimes people ask me whether they should pay
attention to the way we say the compound. The answer is "yes",
because the way we say it may affect the meaning. If you
become tired, tell me, and we will take a break. We will
take a break after each 50 anyway.

Remember: I want you to give me a phrase or sentence
that means about the same thing as the compound noun you
will hear. I will remind you of this automatically after
each 12, so don't think that my reminding you indicates
that you have been doing anything wrong.

After the instructions are given, the experimenter

plays the first stimulus. Now the subject invariably asks

a question of this general form: "Now? Is this what?

Now I'm supposed to say what that means?" The experimenter

says "Yes". Now the subject gives a response, and then

usually asks: "Is that right? Is that what you want me to

do?"

The experimenter answers (regardless of whether the

subject got the right answer or the wrong answer):

Yes, if that is a sentence or phrase that means about
the same thing to you. I'm supposed to keep a straight-
face here, and not show whether or not I agree with your
answer, so don't think your answers are wrong because I don't
nod or say 'yes' or anything like that.

The following portion of the instructions is repeated

after every twelve stimuli, unless or until the subject gets

annoyed and says he understands perfectly well and is bored

with the repetition of instructions:

I am now repeating part of the instructions, as I told
you I would. Remember: I want you to give me a phrase or
sentence that means about the same thing as the compound noun
you will hear.

e. procedure: After reading the instructions,
the experimenter played the stimuli one-by-one, and the
entire experimental session was recorded on another tape-
recorder. After every 50 stimuli (or when the subject re-
quested) a 15 minute break was taken.

Subjects sometimes repeated the stimulus aloud before
or after giving a response. Occasionally the repetition was
inaccurate, i.e. the subject misreported the stress or the
order or even the word itself (foot was misheard as boot).
In these instances, the experimenter said "Let me play that
one for you again," and the subject was encouraged to
respond again.

After the experiment was done, the experimenter asked
the subject his age and educational background. These
questions were delayed until after testing because subjects
asked about their educational background may assume they are
taking an Intelligence Test (perhaps they were).

f. scoring:

1) transcription: Subjects' responses were
transcribed onto form-sheets from the tapes by two independent
judges. There were a very few instances in which one or the
other transcriber had trouble understanding what a subject
had said. Otherwise, the results of transcription were re-
liable. (Greater than 98%).

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2) correct vs. incorrect response: A linguist and a graduate student in English Literature were trained to score the responses, on the basis of the grammatical descriptions provided in Section and above and on the basis of their intuitions concerning the correctness of the response. Both judges scored the responses for three subjects so that reliability of scoring could be checked. There was greater than 95% reliability between the two independent scorings made by these judges. Item by item scoring was checked to see whether a response scored as correct for one subject was scored the same way for other subjects. There was a slight (not statistically significant) 'halo effect', i.e. a tendency to score 'good' subjects leniently, possibly because their responses were more fluent and literate. Inconsistencies of this kind were corrected. It is curious that the stimuli that we call ungrammatical (those containing eat) were not a source of unreliability in scoring: in these instances, it is difficult to provide any external description of what is called a correct or an incorrect response; yet judgments were reliable. The validity of such judgments is a separate question.

Subjects were considered to have made the correct response unless it was definite that they had made the wrong response.

In cases of ambiguity, the response was scored correct where one choice among the ambiguities was correct. For example, if $bird^2$ $black^1$ $house^3$ got the response: a house for a bird that is black, the answer was scored as correct. Little attempt was made during the experimental sessions to ask subjects to clarify such responses, because pilot work showed that any 'probing' tended to confuse and mislead the subject.

The particular verb or preposition chosen by the subject when giving responses related to rule [iii] (where the verb or preposition is not present in the compound) was not considered of particular relevance in deciding whether a response was correct or incorrect. For example, in response to $black^1$ $house^3$ $bird^2$, a subject might say:

    a bird who lives in a black house
    a bird from a black house
    a bird for black houses
    birds who hang around black houses
    birds who build their nests in black houses
    a bird of black houses

All of these responses are equivalently described by rule [iii], and are considered the identical response. Responses are often expanded, i.e. the internal compound is also paraphrased, e.g.:

    a bird that lives in a house that is black
    a bird that lives in a $black^1$ $house^3$, and it is called
        a $black^1$ $house^3$ because it is a $black^2$ $house^1$

Expansion of the internal compound was not required in scoring a response correct (for the simple reason that requiring

subjects to make this further expansion would have involved

more complicated instructions; the tendency to expand was

not related to the tendency to do well, or to educational

background). However, if the subject made an error in ex-

panding, his response was called incorrect. Then only if a

subject responds to the above stimulus:

```
     A bird who lives in a house and he, the bird, is black
or   A black bird who lives in a house
or   a house for a blackbird
```

is he scored as having erred.

We distinguish four kinds of error:

a. errors of order (E_o): If the response the subject

gave would have been correct had the order of the 13 to the 2

stress word in the stimulus been reversed, we say that the

subject made an (O_1) error of order, e.g.:

```
Stimulus:          bird¹ house³ boot²
```
Stimulus: $bird^1$ $house^3$ $boot^2$
Correct response: a boot you wear in a $bird^1$ $house^3$
Error of order: a $bird^1$ $house^3$ that has a boot in it
Stimulus for which
 this response would
 have been correct: $boot^2$ $bird^1$ $house^3$

Similiarly, when a single change of order within the 13

compound would make the response correct, this inversion was

considered an (O_2) error of order, e.g.:

Stimulus: $bird^1$ $house^3$ $boot^2$
Error of order: a boot for $house^1$ $birds^3$, pet birds
Stimulus for which
 this response would
 have been correct: $house^1$ $bird^3$ $boot^2$

However, if the change of order involves inversion of one
of the words joined under <u>13</u> stress, we consider that a
different kind of error was made, e.g.:

Stimulus:	$bird^1$ $house^3$ $boot^2$
Response:	a house for $birds^{,2}$ $boots^2$
Stimulus for which this would have been correct:	$bird^1$ $boot^3$ $house^2$ or $bird^2$ $boot^1$ $house^3$

b. errors of stress (E_s): If a single change in stress
would make the wrong answer the right answer, we say there
was an error of stress, e.g.:

Stimulus:	dry^1 $house^3$ $bird^2$
Correct response:	a bird who $lives^2$ in a dry^1 $house^3$ (or a dry^2 $house^1$)
Error of stress:	a $house^1$ $bird^3$ who is dry
Stimulus for which this would have been correct:	dry^2 $house^1$ $bird^3$

c. chaotic errors (E_{ch}): Where more than one change of
stress or order or both would have to be made for the wrong
answer to be a right answer, the error is called chaotic,
e.g.:

Stimulus:	$house^2$ $bird^1$ $black^3$
Correct response:	paint for blackening $house^1$ $birds^3$; as black as a $house^1$ $bird^3$
Error of chaos:	a bird house that is black
Stimulus for which this would have been correct:	$black^2$ $bird^1$ $house^3$

d. grammatical error (E_g): If the subject fails to
provide a nominal response at all, we call the error a
grammatical error, e.g.:

Stimulus:	bird^2 wash^1 house^3
Correct response:	a house where birds are washed a laundry for birds
Error of grammar:	somebody is telling the bird to wash the house wash the house^1 bird^3! an Indian is saying: 'bird: wash house!'

In other words, it may be difficult to say just what a compound noun is, but we can assume it is not an Imperative.

    3) discarding subjects: All subjects provided nominal and adjectival-phrase paraphrases most of the time (errors of grammar (E_g) were relatively infrequent). Ignoring E_g, then, we can see that there is a certain probability of providing a correct paraphrase by chance; if, for example, the 'test-word' is a noun, and the stress pattern provided is 132, the response pattern:

    a 2 for   13
       that is
       etc.

is going to be called a correct response, while the other five possible arrangements of the nouns (represented above by their assigned stress names) are going to be called errors. However, since more than one kind of rule can generally be invoked in defense of other paraphrases, since the probabilities of chance correct responses are somewhat different for verb and adjective test words, since any word in any compound can be reassigned to another lexical class, and since, in fact, E_g does sometimes occur, it is impossible to provide any reasonable estimate of the probability of a chance correct response for any stimulus.

We felt safe in assuming, after considering the various factors above, that a subject who was correct over 25% of the time was not responding at random. Further, any subject who was reasonably rational in his responses to certain selected 'easy' compounds could be assumed to be attempting to follow the instructions.[1] Any subject who failed to meet one of these two criteria was discarded. On this basis, results for four of the eleven C-group subjects were excluded. It should be noted, however, that the discarded subjects did not in fact seem to be qualitatively different in their response characteristics from C-group subjects just ahead of them. All were sufficiently uniform in responding appropriately to the easy stimuli to conclude that they represented, so to speak, simply the bottom of the same curve. Nonetheless, we 'chopped off' this bottom of the curve to ensure that population differences could not be interpreted as failure of some few subjects to understand the instructions effectively.

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[1]The chosen 'easy' compounds: $black^2$ $bird^1$ $house^3$; $bird^2$ $wash^1$ $house^3$, $black^1$ $bird^3$ $house^2$; $glass^1$ $house^3$ $bird^2$; $thin^2$ $house^1$ $bird^3$.

g. <u>results</u>:

1) <u>gross effects</u>:

(a) <u>The gross effect of groups</u>: The
result that stands out immediately is an overwhelming dif-
ference among the three groups. The mean number of errors
is 17.7 for Group A, 64.3 for Group B, and 89.1 for Group C.
Figure 1 presents the individual error scores for each subject
and shows that there is no overlap of groups A and C.

(b) <u>Statistical method</u>: It appears that,
for at least some aspects of linguistic performance, there
are individual and population differences. Given the dis-
persion of performance scores, we must adopt some techniques
of data analysis that can handle some of the more trouble-
some consequences of this dispersion. We must, in other words,
be able to cope with the problem of whether the effect of
some variable is genuine or might be attributed to chance
fluctuation. Some linguists may feel that this is regrettable;
and perhaps it is. But it should be noted that we are not
proposing a statistical model of language, or linguistic
behavior, but only a way of extracting some linguistic
events from the behavioral buzzing confusion in which these
phenomena are surely embedded. It is not quite clear why
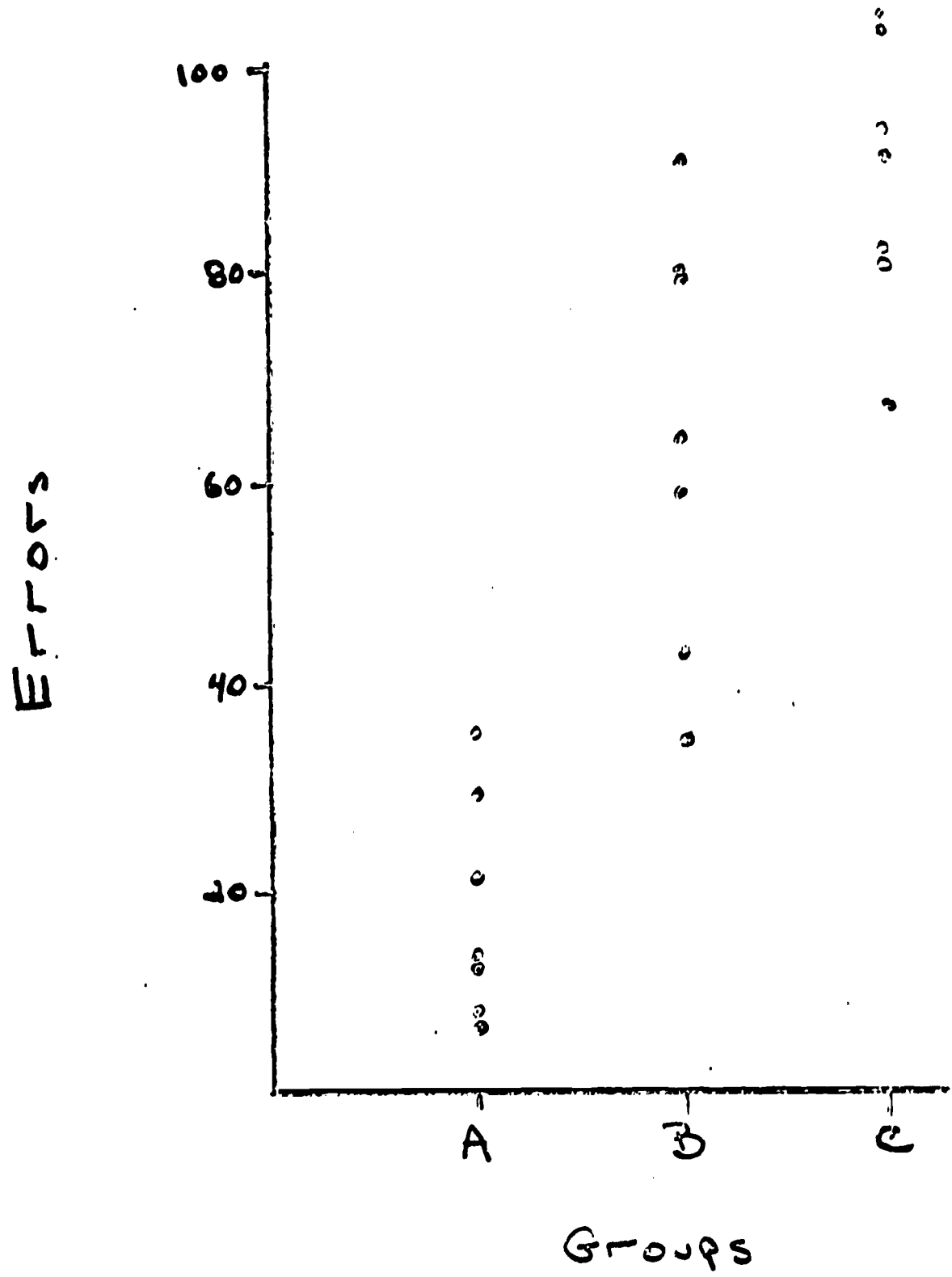linguistics should be offended by techniques that are

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Figure 1. Distribution of error scores by group

routinely used in such varied disciplines as astronomy,
genetics, agriculture, and psychology. At least some of
these fields have not been the worse for their brush with
the normal curve.

The primary statistical tool we will use is the
analysis of variance. This technique, developed by Fisher
(20), is particularly useful in multifactor experimental
designs. Its main feature is a method of partitioning the
variance and assigning it to various factors and the inter-
actions among them. Thus for each factor examined, one can
determine whether its effect reliably transcends chance
fluctuation. This is accomplished by comparing the
variance attributable to one or another factor with variance
estimates of residual error. The analysis of variance is
thus useful in separating out effects of the variables and
in determining the contribution of each to the experimental
results. Beyond assessing these "main effects", this
statistical technique also allows us to determine whether
the several factors produce their effects independently of
each other, or whether the effect of one factor is different
depending upon the value taken by the other. In the second
case, we speak of an "interaction" between the two factors.
Statistically speaking, this means that the difference due to a

variation in factor A will differ depending upon factor B.

Technically, a number of preconditions have to be met
to satisfy the assumptions of an analysis of variance. The
variances of the subcells are assumed to be the same, the
distributions normal, and so on. It is often possible to
pre-test the data and, in case of nonhomogeneity of variance,
to transform the data until the assumptions of the analysis
are met (e.g. by chang⋯$\underline{x}$ to log $\underline{x}$, proportions to their
arc sine equivalents). However, it then becomes necessary
to justify the generalization from the result for transformed
scores to the raw data. A series of studies (Norton, as
reported by Lindquist, 47) has demonstrated that in practice
the analysis of variance is remarkably insensitive to such
violations of its assumptions, even where these are fairly
extreme; we have therefore not attempted to correct for
violations.

<u>(c) Statistical treatment of gross</u>
<u>effects</u>: It is hardly surprising that the group difference in
overall error scores is highly significant statistically.
A simple analysis of variance shows an immense effect of
groups [F = 37.1, $\underline{df}$ = 2 and 18, p < .001]. Further,
Duncan's Multiple Range Test showed that all groups dif-
fered significantly from each other [p < .01].

It will be recalled that in addition to the population
groups, subjects (within groups) were further subdivided
into 'order-groups' that received the stimuli in three dif-
ferent serial orders. Some subjects began with a syntactically
plausible and semantically familiar stimulus, others with a
syntactically plausible but semantically odd stimulus,
and still others with a syntactically opaque and semantically
odd stimulus. This difference did not affect the final
error-scores. An analysis of variance shows no trace of an
order effect [F < 1].

It is nevertheless possible that there might still be
an effect due to serial position. The question is: is an
item simpler when it comes late in the series; in short,
does the subject learn? To answer this question, we con-
sidered mean error scores for test-items 1-36; seven subjects
received these as the first 36 items, seven others received
them in position 96-132, seven others in position 10)-144.
The mean error scores do decline very slightly for groups
that received the items later in the test, but the effect
does not begin to reach significance. An analysis of
variance for the 36 stimuli shows the usual huge effect of
groups [F = 42.2, df = 2 and 12, p < .001], but neither the
main effect of serial position nor its interaction with the
group effect met anything like the usual criteria of statisti-
cal acceptability (both p values were larger than .25).

2. The relation of syntactic form to error:

In the previous analysis, we examined overall error scores for all 144 stimulus items. We did not ask what kinds of errors were made, or what kinds of stimuli led to errors. We turn now to these questions.

a. Categories of stimulus and response types:

1) Stimulus categories: Each stimulus item consisted of three words, two of which were always the same (bird and house), and a third that could be varied. We refer to the two constant terms as C, to the variable one as X. The first basis of classification of the stimuli is by the lexical classification of the X-term (see Table 2). For the moment, we restrict the analysis to X-terms that have unambiguous 'basic' lexical classification (Noun, Adjective, or Verb) in Table 2; excluded are stimuli containing stone, glass, dry, or shut as the X-term. The results for these ambiguously classified stimuli will be considered later. Their exclusion leaves 96 stimulus items.

Each X-term can occur in the 1st, 2nd, or 3rd serial position in the compound. We disregard, for the moment, the relative order of the two constant terms, thus for present purposes grouping house-black-bird with bird-black-house. The number of arrangements is doubled when we consider that for each of the three word-orders there are two possible

stress patterns ($\underline{132}$ and $\underline{213}$).

There are now three bases for stimulus categorization: (1) lexical class of the X-term; (2) position of the X-term; and (3) stress pattern of the compound. Thus there are 18 'phrase-categories'.[1]

2) Response categories: Just as we sub-categorized the stimulus-types, we now distinguish among the error-types yielded b the scoring method (see Section

). There are four kinds of error: E_o (errors of order)[2], E_s (errors of stress), E_{ch} (errors of chaos), and E_g (errors of grammar).

b. Conversion to ratio scores:

The preceeding analysis of group effects was based on the gross error scores for all subjects for all 144 stimuli. We are now asking about the distribution of the

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[1]Each stimulus-category is given a three-place name by (1) lexical class (N, A, or V); (2) position of X (1, 2, or 3), and (3) stress pattern. When the X-term has intermediate stress, the stress pattern is c alled positive (+); otherwise, negative (-). Intermediate stress occurs only in serial positions 1 and 3. In second position, X-terms with primary stress are called E, with tertiary stress, D. Thus N1+ includes $\underline{foot^2\ bird^1\ house^3}$. N1- includes $\underline{foot^1\ house^3\ bird^2}$; N2E includes $\underline{bird^2\ foot^1\ house^3}$; N2D includes $\underline{bird^1\ foot^3\ house^2}$.

[2]In this analysis, O_1 and O_2 errors are not distinguished. In experiments 2 and 3, this further distinction will be treated.

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errors. Is it still reasonable to work with gross error
scores? Suppose we want to know whether various phrase-
categories are associated with certain kinds of errors.
We would get our answer by summing up all of our subjects'
error scores within each of the 72 subcells (18 phrase-
categories and 4 error-types). But once we have done this,
each error counts equally, whether it is made by a subject
who errs very often or a subject who errs hardly at all.
It is obvious that in the final reckoning, the result must
be disproportionately affected by the subjects with higher
error scores. Since we have already granted individual
differences, we are no longer asking how many errors a subject
made, but rather how these errors are distributed by phrase-
category and error-type. We will therefore consider all
subjects and consider all equally: subjects, rather than
errors, will carry equal weight. This effect is accomplished
statistically by a simple ratio transformation of the
error scores: Each subject's error scores (within phrase-
categories and error-types) were divided by the total
number of errors made by this subject on the 96 stimulus
items. It is obvious that the sum of these adjusted
scores will now be equal (in fact, 1.00) for every subject;
thus each subject contributed equally to the overall pattern

of results.[1]

_____

[1]By the ratio transformation, we have accorded equal
status to all individuals, regardless of their differing
propensities to err. This makes excellent sense statistically.
It also is a more reasonable model of the linguistic behavior
of the individuals and groups we are studying. Given the
enormous differences in individual error scores, it is
plausible to suppose that there are different 'baselines'
for error, or general performance level, among subjects.
If this is so, an analysis of variance on the 'raw' error
scores will be very misleading. By analogy, suppose we are
studying the effect of very hot weather on the performance
of tennis-players; suppose further that one subject is a
professional, and another is a rank amateur. As the tem-
perature increases from 70 to 90 degrees, let us say, the
professional's missed shots increase from 2 to 4 in 20.
The amateur's increase from 8 to 16. If we use mean missed
shots as our measure, and treat these data in their raw
form, we will find an 'interaction' between temperature and
professional status, i.e. the effect of temperature is not
additively equal for the two subjects, since the professional's
errors increased by only 2 while the amateur's increased by
8. It is more probable, however, that the effect of tem-
perature is to double the number of missed shots regardless
of the basic propensity to miss the ball. The interaction,
then, is spurious, a result of the inappropriateness of the
counting method. If, instad, we had originally transformed
the scores to proportions (thus correcting for the differnt
baselines), we would now find that the effect of temperature
on mean adjusted error was additively equal, i.e. the dif-
ferences between the differences were identical. Granting an
overall difference of aptitude among our own subjects, it
seems more likely that some factor of difficulty added to cer-
tain stimulus categories would increase the proportion of error
more or less equally, than that it would increase the number
of errors per subject by some constant amount. Without a
model that actually predicts the relation of syntactic (and
semantic) form to error production, it is difficult to
speculate much further than the ratio model we have tentative-
ly accepted here.

But it should be pointed out that there are drawbacks to
a ratio analysis that are independent of the plausibility of
the model for these data. The A-group subjects make far fewer

c. Analysis by group, phrase, and error type:

Here we consider the relations among three main factors:
Groups (A, B, C), phrase-category (the 18 types described
above), and error-type (E_∂, E_s, E_{ch}, E_g). An analysis of
variance was performed based on the mean adjusted error scores
of each subject for each of the 18 phrase-categories.
Absolute differences between groups have of course been
eliminated by the score adjustment.[1]

Table 3 presents mean errors by group, phrase-category,
and error type. The most casual inspection indicates
that there are wide variations resulting from phrase
category, and that these are not distributed evenly over
types of error.

Footnote continued:

errors, thus yielding data that are less reliable, especially
when the results are broken up into numerous sub-categories.
For these subjects, one random error (and even A-group sub-
jects are prone to random lapses of attention) may have a
disproportionate statistical effect. There is no perfect
way of balancing the advantages of the absolute and the ratio
analyses. It is worth noting that, for the most part, the
analyses led to rather similar results whether carried out
on the raw scores or on the transformed ratio scores.

[1]A simple analysis of variance using raw scores was
performed for the 96 stimuli considered here (the ambiguously
classified stimuli have been removed) to determine whether
the group-effect for all 144 stimuli was still present. The
same massive effect was found [$F = 28.37$, df = 2 and 18,
$P < .001$].

Table 3. Mean adjusted errors by group, phrase-category, and error-type (adjusted errors x 1000).

Phrase-category	Errors of Order			Errors of Stress			Errors of Chaos			Errors of Grammar		
	A	B	C	A	B	C	A	B	C	A	B	C
N1+	9	0	14	0	23	14	0	7	10	0	0	12
N1-	10	13	11	0	36	14	0	3	3	0	0	3
N2E	0	22	9	0	19	13	0	6	6	0	0	0
N2D	0	8	7	15	10	34	0	7	6	0	0	0
N3+	0	12	3	10	0	6	0	3	3	0	7	0
N3-	38	36	15	0	42	48	0	16	7	0	0	0
A1+	0	0	4	0	0	12	0	0	2	0	3	0
A1-	0	10	5	96	52	70	0	0	0	0	0	0
A2E	26	23	12	10	17	8	6	10	10	24	0	0
A2D	36	19	9	0	33	55	0	10	31	0	0	2
A3+	18	68	79	10	0	0	0	10	5	0	0	0
A3-	164	42	32	16	0	3	44	64	58	0	0	2
V1+	140	8	4	0	9	15	0	7	2	89	25	53
V1-	25	31	18	46	35	8	0	10	18	10	17	35
V2E	19	16	6	10	3	8	6	24	18	0	7	16
V2D	46	5	0	0	13	10	6	21	19	0	4	15
V3+	0	22	17	30	9	2	0	22	10	0	2	16
V3-	34	47	20	6	7	22	0	18	23	0	6	10

To disentangle the various effects, let us first simplify by lumping results for all population groups. This is done in Table 4, which shows mean number of errors as a function of category. A consideration of total number of errors (Col. 5, Table 4) shows that some categories are fairly easy to deal with (e.g. A1+, N3+, N2E), while others are very hard (e.g. A3-, V1-, A3+). It thus appears that there is a difference in the difficulty of the various categories, quite apart from the kind of errors they produce. This main effect of phrase-category is highly significant [F = 7.87, $\underline{df}$ = 17 and 1,490, p < .001].

There is similarly a significant main effect of error-type [F = 23.4, $\underline{df}$ = 3 and 1,388, p < .001]. Errors of order and stress occur more often than errors of chaos and grammar.

That this last main effect has no independent interest is shown by the fact that phrase-category and error-type interact; thus by manipulating the phrase-categories, we can affect the relative frequency of various kinds of error. Errors of grammar occur most frequently when the X-term is a verb (particularly in first position), errors of order with postnominal adjectives (A2D, A3), and errors of stress with noun X-terms. This interaction of phrase-category with error-type is significant [F = 2.79, $\underline{df}$ = 51 and 1,490, p < .01], and has certain obvious syntactic correlates that we will discuss later.

Table 4. Mean adjusted errors by phrase-category and error-type (adjusted errors x 1000).

	Errors of Order	Errors of Stress	Errors of Chaos	Errors of Grammar	All Errors
N1+	8	12	5	4	29
N1-	11	17	2	1	31
N2E	10	11	4	0	25
N2D	5	19	5	0	29
N3+	5	5	2	2	14
N3-	30	30	8	0	68
A1+	2	4	1	1	8
A1-	5	73	0	0	78
A2E	20	12	9	8	49
A2D	21	29	13	1	64
A3+	55	3	5	0	63
A3-	79	6	55	1	141
V1+	51	8	3	56	118
V1-	25	30	9	21	85
V2E	14	7	16	7	44
V2D	17	8	15	6	46
V3+	13	14	10	6	43
V3-	34	12	14	5	65
All phrase-categories	405	300	176	119	1000

We now ask how group-membership interacts with the effects of phrase-category and error-type. Table 5 presents mean adjusted error scores for groups A, B, and C for each of the four error-types. Of considerable interest is a significant interaction between group-membership and error-type [$F = 2.60$, df $= 6$ and $1,490$, $p < .02$]. The A-group makes proportionally more errors of order, and fewer errors of chaos and grammar; the reverse is true of the C-group, while the B-group falls in between.[1] Referring back to Table 3, we can attribute this effect primarily to two very difficult phrase-categories (A3- and V1+). In both these cases, the A-group responds with E_o. For A3-, Groups B and C give E_{ch}. For V1+ group B and especially group C give mainly E_g.

While there is an interaction between group and error-type, there is little evidence of an interaction between group and phrase-category [$F = 1.16$, df $= 34$ and $1,490$, $p > .05$]. It appears that phrase-categories that are difficult for the linguistically inept are difficult also

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[1] This effect can be enhanced by use of a more refined scoring technique which will be explained later (App. B).

for those who are more facile; the absolute level of difficulty
(i.e. the baseline for error) varies, but the relative order
is pretty much the same. In a way this is reassuring:
granted that there are some major differences among indi-
viduals (the raw-score effect of groups), and differences in
how they cope with difficulty (the interaction of groups
with error-type), at least they all speak the same language
(there is no interaction of groups with phrase-category).

       d. The relations between phrase-category and
number and kind of error: The preceeding analysis indicated
that the phrase-category affects the number of errors com-
mitted, and that different phrase-categories are associated
with different kinds of errors. Several further analyses
were undertaken to examine these effects more closely.

       1) The effect of lexical class: Inspection
of Table 4 suggests that one determinant of difficulty and
of kind of error is the lexical class of the X-word. To
test this suggestion more directly, we grouped the 18 phrase-
categories by the three kinds of X-term (Noun, Adjective,
Verb).

    The effects of lexical class on performance are shown in
the bargraph of Figure 2. Recall that E_o is much the most
frequent error-type committed. Figure 2 shows that for both
Nouns and Adjectives, E_o occurs more often than any other

Table 5. Error-type as a function of group. (Mean adjusted errors x 1000)

Error of:	Group:		
	A	B	C
Order	566	381	266
Stress	248	310	340
Chaos	62	238	231
Grammar	124	71	163

error. But E_s is very much greater for Nouns than for

Adjectives, and E_o is greater for Adjectives than

for Nouns. E_g is almost absent except with Verb X-terms.

We reserve for later discussion an examination of what these

associations (and also the scatter of E_{ch} over lexical classes)

imply. An analysis of variance showed a significant main

effect of lexical class [F = 10.85, <u>df</u> = 2 and 1,457, p < .001]:

Nouns lead to fewer errors than either verbs or adjectives.

More important, there was a significant interaction between

lexical class and kind of error [F = 4.44, <u>df</u> = 6 and

1,457, p < .01]. Nouns are associated with errors of

stress, adjectives with errors of order, and verbs with

errors of grammar.

<u>2) The effect of position</u>: The very power-
ful effect of phrase-category (on both number and kind of

error) can be accounted for in part by the lexical class of

the X-term, as the previous analysis has shown. But it is

quite obvious that a good deal of variation is still left

unaccounted for. Some of the variance that remains is

clearly attributable to the position of the X-term. For

example, phrase-categories with Adjectives in first position

lead to little error, while phrase-categories with Adjectives

in last position are very difficult. Such effects are im-
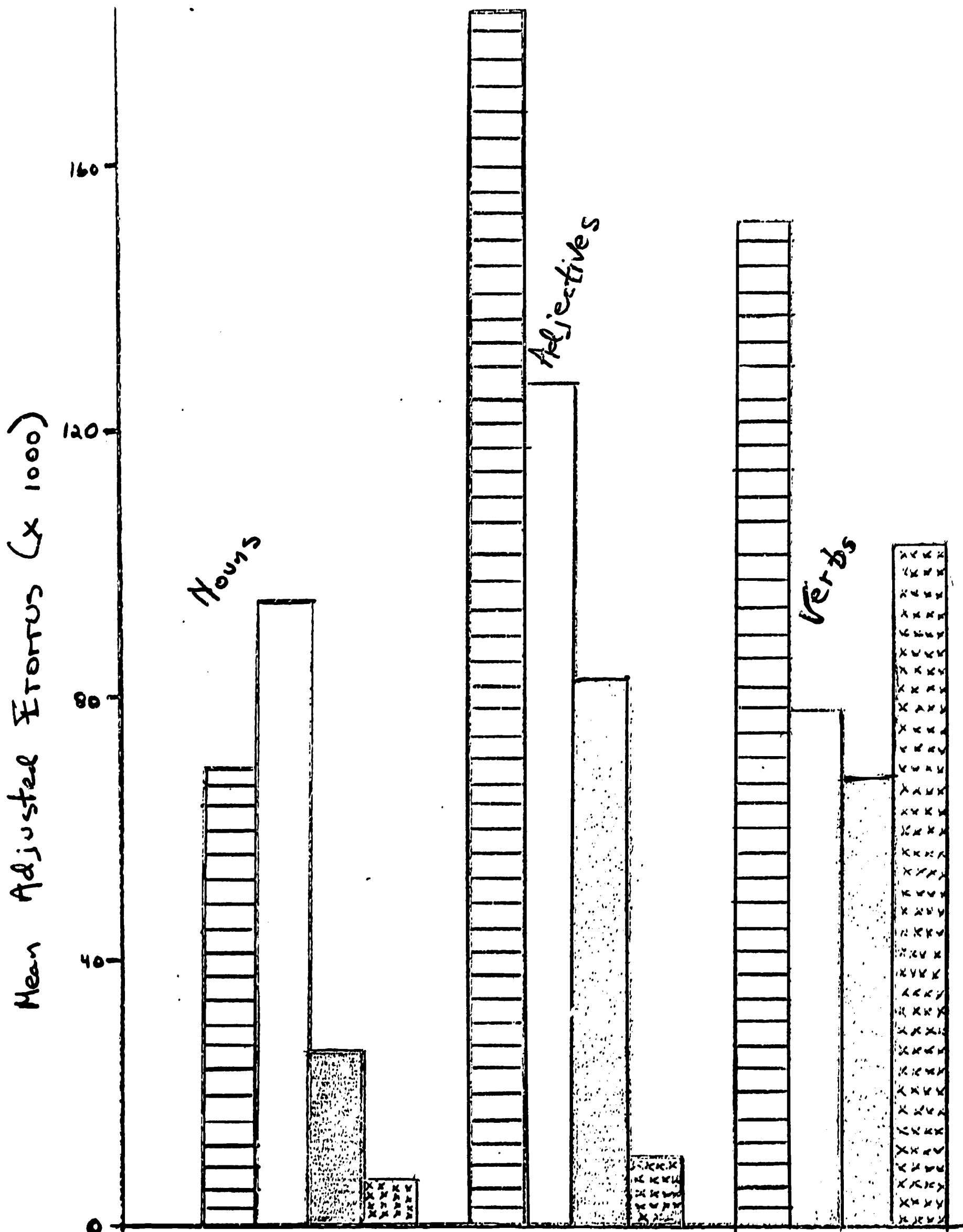mediately apparent upon inspection of Table 3; they should be

Figure 2. Error type and lexical class.

even more apparent upon inspection of the English language:
Adjectives often begin phrases, but rarely end them. Given
the enormous importance of sequential patterning in English,
the fact that lexical class must interact with position
need hardly be demonstrated. (As an exercise in pedantry,
we did perform the statistical demonstration, performing
an analysis of variance on overall adjusted error scores as
a function of lexical class and position. We are happy to
announce a highly significant interaction between lexical
class and position of the X-term [$F = 6.82$, $\underline{df} = 4$, and
1,482, $p < .001$]).

        <u>3) The effect of 'unity'</u>: The differences
among the phrase-categories are not, however, abolished even
if we (statistically) equate for lexical class, the position
of the X-term, and the interaction between them. A major
source of variation still remains. Inspection of Table 3
suggests that this factor is connected with the stress
pattern, especially in case the X-term is in first or
third position. Several further analyses were undertaken
to isolate this factor and see how it operates.

      Consider stimulus items with serial order XCC and CCX.
Two stress patterns (<u>132</u>, <u>213</u>) occur with both these
serial orders in our stimuli. The stress patterns have the
effect either of 'unifying' the two constant (C) terms,

(i.e., $c^1c^3x^2$, $x^2c^1c^3$), or 'disunifying' them (i.e., $c^2c^1x^3$, $x^1c^3c^2$). In other words, the 13 stress subpattern (whether it preceeds or follows the secondary (2) stress) conjoins two words into a subcompound (e.g. $\underline{bird^1 \ house^3}$).

Suppose there is a tendency for subjects to 'expect' that the two C-terms will be a subunit (i.e. will have the 13 subpattern). Given the C-terms we used, this would not be surprising. $\underline{Bird^1 \ house^3}$ is a familiar unit, known to speakers as a lexical item; $\underline{house^1 \ bird^3}$ is not familiar, but it requires no great flight of the imagination to connect it, by analogy, with known items (e.g. $\underline{jail\text{-}bird}$, $\underline{house\text{-}cat}$, $\underline{field\text{-}mouse}$, $\underline{barn\text{-}owl}$). At any rate, neither of these pairs, when 'unified', violates cooccurrence restraints in the language or semantic plausibility. When, on the other hand, a C-term is unified with an X-term, a cooccurrence restriction may or may not be violated (e.g. $\underline{black^1 \ bird^3}$ and $\underline{wash^1house^3}$ are familiar and plausible; $\underline{bird^1 \ boot^3}$ and $\underline{dry^1 \ house^3}$ are unfamiliar but plausible; $\underline{house^1 \ foot^3}$ and $\underline{bird^1 \ thin^3}$ are unfamiliar, implausible, and just possibly violations of real cooccurrence restraints in English). We would expect fewer errors where cooccurrence restrictions are respected. Since for 'unified' CC, there is never a violation, and for 'nonunified' CC, there is often such a violation, unified CC ought, on average, to be easier. Notice that if CC is nonunified, and in fact CX or XC does violate a cooccurrence

restriction, this phenomenon might be further enhanced:
not only must the subject deal with an eccentric semantic
event, he must take care not to be beguiled by a normal
semantic event that he can bring into existence by just a
wee flim-flam with the stress pattern. Presented with foot[1]
bird[3] house[2] (clearly a home for livery birds) one might be
tempted to respond 'a basement apartment for birds', al-
though that is a foot[2] bird[1] house[3], as we know, and, in
all fairness, not an altogether unextravagant alternative.

To determine whether such a phenomenon actually exists,
we compared mean adjusted error scores and error-types
obtained in which the C-terms were unified with those ob-
tained for items where the C-terms were not unified. For
these purposes we of course do not consider items in which
the X-term is in second position (where the C-terms cannot
be unified). Again, we exclude the lexically ambiguous stim-
uli. Then there are 64 stimulus-items for the following
analyses.

Table 6 presents mean adjusted scores for unified and
nonunified stimulus items by error-type. As the table
indicates, the stress difference indeed produces enormous
differences in the tendency to err. The number of errors
for items with nonunifying stress is almost double that for
items with unifying stress. As expected, the main effect of
the unity factor is highly significant [$F = 11.25$, $\underline{df} = 1$
and 964, $p < .001$].

Table 6.   Errors as function of stress. (Mean adjusted errors x 1000)[1]

	Error-type				All errors
	Order	Stress	Chaos	Grammar	
Unified stress	133	47	27	69	276
Non-unified stress	184	167	88	28	467

[1]Figures do not total to 1000 since they only include data from X-term positions 1 and 3.

If the unity factor can in fact be interpreted as we say, then the errors produced by this stimulus factor should be primarily errors of stress and chaos.[1] As Table 6 suggests, this turns out to be so: The unity factor interacts significantly with error-type [F = 5.88, $\underline{df}$ = 3 and 964, $p < .01$], and the interaction is attributable to a sharp increase in E_s and E_{ch}.

Figure 3 graphically presents the unity factor as a function of groups. There is no trace of an interaction between unity and group membership, $(F < 1)$. Apparently, A-group subjects are as prone to such interference (in proportion to their baseline for error) as are C-group subjects.

4) The effect of familiarity: It is possible that overall difference in the familiarity of the sub-compound (the item under 13 stress) may affect the tendency to err. We have mentioned above a very obvious difference between the well known compound bird-house, and the (probably) unknown, though plausible, compound house-bird. House-bird can be understood either by reference to the compounding rules of the language or by some other more direct

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[1]It will be recalled (see Section II B 2 f) that an error of chaos is said to occur when a response reflects both an error of stress and an error of order. Since we therefore expect some covariance of E_s and E_{ch}, we here expect a rise in both.
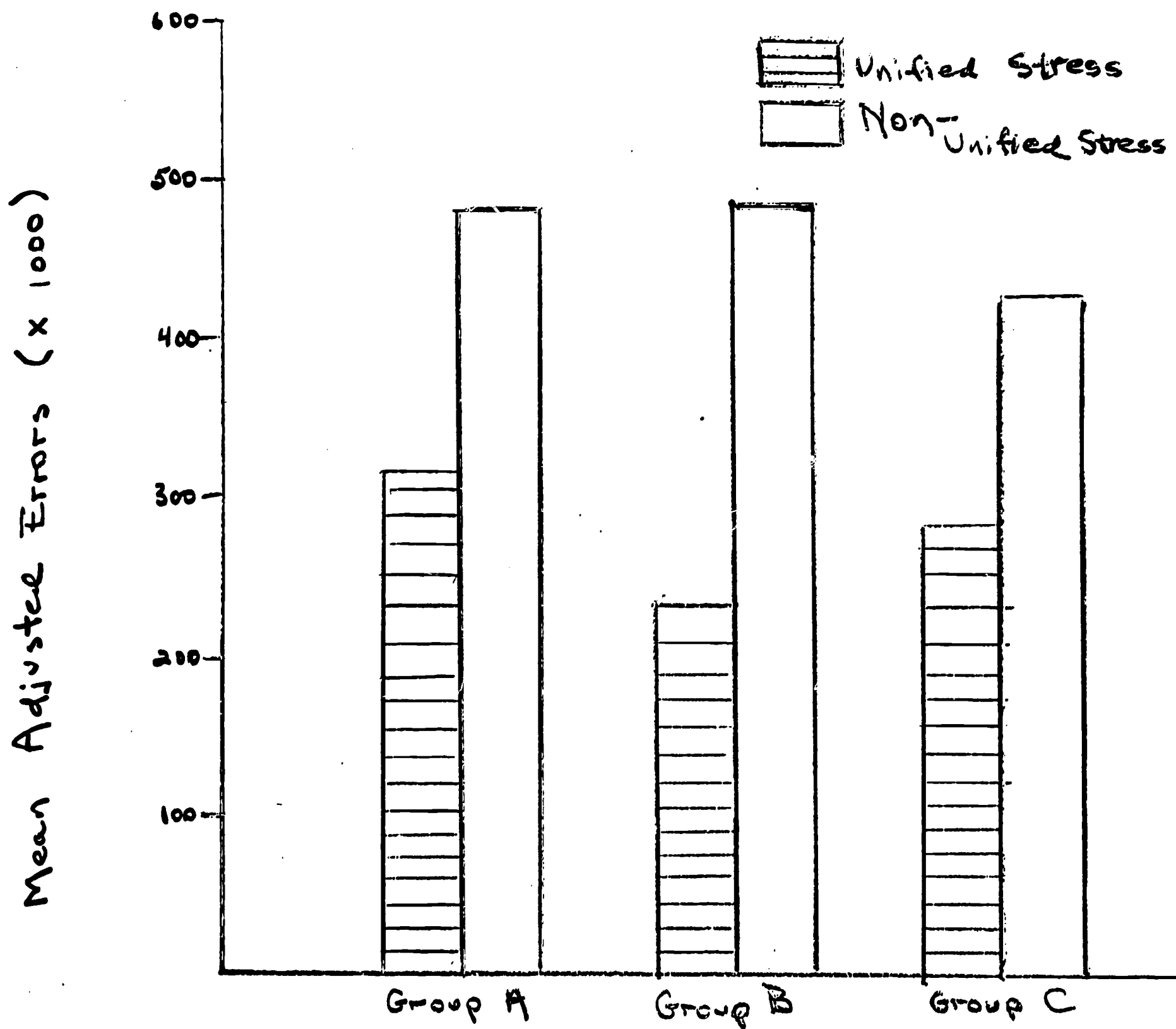
Figure 3. The unity factor by group.

mode of analogy (e.g. to house-cat). But a bird-house is
just a nest. It is known as a lexical item; if you also know
it is called a bird-house because it is a house for birds,
so much the better, but this step is not necessary.

The 'computation' or comprehension of three word com-
pounds containing bird-house might then be expected to be
somewhat easier than the comprehension of those containing
house-bird. Should this mean that, in our test, the se-
quence bird, house will lead to fewer errors than the se-
quence house, bird? Not necessarily. We already know of the
power of the unity factor. What we expect is that familiar-
ity should facilitate a correct response if the stress
pattern demands that the two C-terms be responded to as a
unit ('+' stimuli); but if the stress pattern is such
('-' stimuli) as to disunify the C-terms (i.e. demand that
they be taken as non-units), then the effect of familiarity
of the compound should pull against the demands of the test
situation. If bird-house is a better unit than house-bird,
by the same token it ought to be a worse nonunit (e.g.,
$bright^1$ $bird^3$ $house^2$ ought to be harder than $bright^1$ $house^3$
$bird^2$). In short, we expect that the two factors of
familiarity and unity will interact. Given unifying stress,
familiarity should help; given disunifying stress, it should
hinder. If both effects are about equally strong, there

should be no main effect of familiarity, for it will be cancelled out.

To test these speculations, the same 64 items were considered again, but now grouped by both familiarity of compound and by stress pattern. The results are presented in Figure 4. As the figure indicates, the two factors did interact and in the expected direction. An analysis of variance shows the interaction to be highly significant $[F = 11.69, \underline{df} = 1 \text{ and } 53, p < .001]$. The same enormous effect of unity already discussed was found again $[F = 20.3, \underline{df} = 1 \text{ and } 53, p < .001]$, but there was, as expected, no trace of a main effect of familiarity $[F < 1]$. It is worth noting that the interaction between familiarity and unity occurred rather uniformly for all three groups. There was no triple interaction $(G \times F \times U)$, nor any simple group by familiarity interaction.

e. Mixed lexical items: We have thus far excluded from analysis stimulus-items containing the words glass, stone, dry, and shut, which said (Table 2) had more than one 'basic' lexical classification. We turn now to an examination of these mixed items. In all, we are dealing with 48 stimulus items.
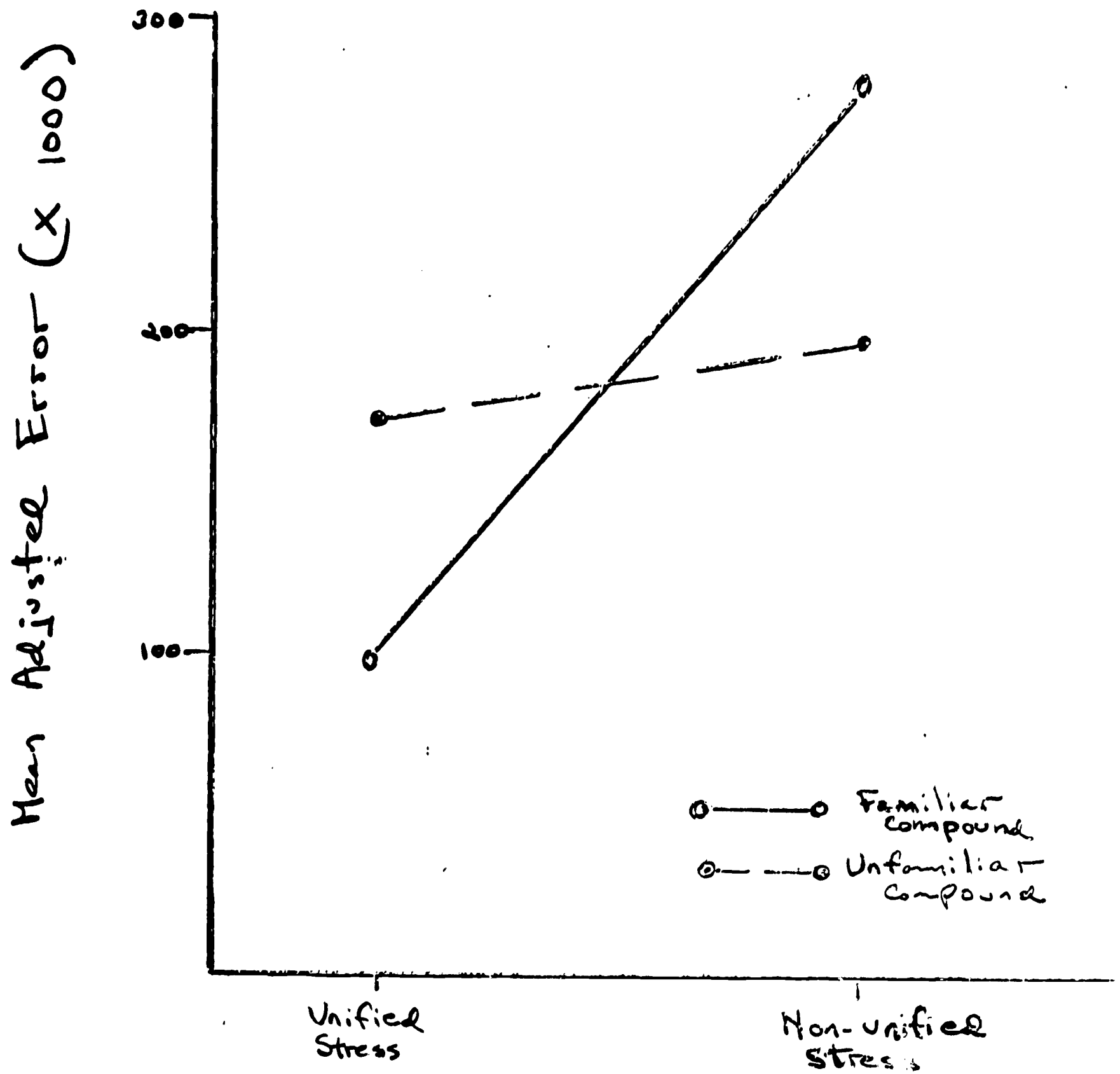
Figure 4.   The effect of familiarity of compound under different conditions of stress.

1) Stimulus subcategorization: Without
reawakening the argument of whether store and glass are indeed
adjectives (see footnote 1, p. $\mathcal{J}$I), we can agree that words
like these are comfortable in prenominal position with
phrasal (21) intonation, while other nouns are not (e.g.,
stone^2 house^1 vs *dog^2 house^1). In this sense only we say
that stone and glass are 'mixed', having positional privileges
in common with both adjectives (as in glass^2 house^1) and
nouns (as in house^1 glass^3); we group these two words into a
category "AN". Dry and shut have the positional privileges
of both verbs and adjectives; we group them as "VA".

2) Comparison of mixed and simple lexical items:

(1) Gross error: Our initial supposition
was that stimuli with mixed X-terms ought to lead to less
error than stimuli with simple X-terms. After all, it is
difficult to deal with an adjective in third serial position,
and easier to deal with a noun in third position. This being
so, an adjective that may alternatively be viewed as a noun
ought to present less of a problem than a simple adjective.

To test this notion, we compared the mean number of errors
when the X-terms were simple (that is N, A, or V) and when
they were mixed (AN or VA). These means are presented in
Table 7. As the table shows, our initial notion may have
been plausible, but it turns out to be false: mixed X-terms

Table 7. Mean gross errors for simple and mixed X-terms.[1]

| Error-type | lexical class of X term | | | | |
	N	A	V	AN	VA
Order	1.0	2.2	1.4	1.5	2.2
Stress	1.6	1.7	.9	1.8	1.6
Chaos	.5	1.3	1.2	.7	1.2
Grammar	.1	.	1.6	.0	.7
All errors	3.2	5.2	5.1	4.0	5.7

[1]Mean gross errors per X-term.

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do not lead to fewer errors than simple ones. The AV X-terms
lead to more errors than either the A or the V X-terms. (How-
ever, neither the difference between A and AV nor that be-
tween V and AV achieves statistical significance; for the
first, t = .94; for the second, t = 1.41. With $\underline{df}$ = 20
in both comparisons, p > .10). The AN X-terms lead to a
level of difficulty midway between that of N X-terms (t =
2.52, $\underline{df}$ = 20, p < .01) and that of A X-terms (t = 2.52,
$\underline{df}$ = 20 , p < .01).

These effects held for all groups. Consider the AN
items, which had a mean error score midway between that ob-
tained for A and N items. In all, 14 subjects found these
items harder than those having N X-terms; of these 14 sub-
jects, five were in group-A, four in group-B, and five in
group-C. On the other hand, only three subjects found these
AN items harder than items with A X-terms; of these three
subjects, one came from each of the groups. The pattern is
similar for the VA items. Only seven subjects (three from
group-A, one from group-B, three from group-C) made more
errors on A than on AV items; only eight subjects (three from
group-A, one from group-B, four from group-C) made more
errors on V than on AV items.

(2) Error-type: We already know that cer-
error-types are associated with the different lexical classes

of the X-terms. Errors of order are associated with adjectives, errors of stress with nouns, and errors of grammar with verbs (see Figure 2). Perhaps by looking at the kind of error made for the stimuli with mixed X-terms, we can discover how they are interpreted by the subjects.

Inspection of Table 7 suggests that AN words were, overall, interpreted as nouns. However, in third position (where -- of all places -- it is dangerous to interpret them as adjectives) they were surprisingly often taken as adjectives, even though this rendered them artificially difficult. Similarly for VA, there are many instances in which the mixed X-term is evidently viewed as an adjective when that would be most difficult to interpret (in third position), and as a verb when that would be most difficult (in first position), thus increasing both errors of order and errors of grammar beyond the number for simple V or A. It is as though the subject, rather than looking for ways of being correct, was seeking out ways of getting to a wrong answer.

3. Experiment 2

We have seen that there are gross differences between the behavior of more educated and less educated populations in responses to this kind of test. It is highly improbable that there are social-class or educational-class dialect differences that bear on the structure of compounds of the

kinds we studied.[1] Then it is likely that our results reflect global differences in the handling of linguistic materials by recognizable populations within the same dialect group.

In reporting the results of Experiment 1, we assumed that the less educated groups in fact understood the instructions. The assumption seems warranted because (1) we discarded subjects whose scores were not overwhelmingly beyond chance expectations (see Section $III\,B3$); and (2) responses of all subjects -- regardless of baseline for error -- were found to be systematically related to phrase-category in the same way, i.e. all subjects seemed to be responding similarly to the same features of the stimulus situation.

Yet even if all subjects did understand the instructions, it is possible that they differed in 'imaginativeness' or 'ingenuity' in dredging up semantically plausible interpretations for some of the compounds. There is no doubt that since a large number of the compounds were semantically

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[1]The social or dialect-group interpretation of the results will be shown to be unacceptable in Experiment 3 in which we finally do find a few secretaries whose performance is fairly close to that of the graduate students.

bizarre, a large number of correct paraphrases had to be
bizarre also. Ingenuity aside, perhaps the less-educated
subjects were uncomfortable (at being wrong, at seeming
foolish) so they did not verbalize semantically odd
responses.

Furthermore, the inability to generate paraphrases
spontaneously need not necessarily bear on the question of
whether the subjects understood the phrases. Although our
intuition was that the measure of understanding is 'being
able to say it in one's own words', that requirement might
well be too strong.

For these reasons, we had to consider whether the task
had been well designed to display the subjects' maximal
comprehension. We therefore designed a forced-choice
experimental situation in which the subject's task was to
indicate his preference between two proposed paraphrases.
Since the subject is now asked merely to choose between
equally bizarre alternatives, the issues of verbal imaginative-
ness and temerity are removed, and the task becomes in some
sense a more passive one.

a. Subjects: Nine months elapsed between
the first experiment and this one. Six of the graduate
students and three of the secretaries were no longer
available, so some new subjects were added. Thus for some

subjects, nine months to a year elapsed between tests,
while for the new subjects only a few weeks elapsed between
tests. This large difference in test-interval was found to
be irrelevant to the subject's consistency in performing
the two tasks. The college group ("B") was not retested.

Subjects were:
    Five graduate students from the population pre-
        viously tested.
    Twelve secretaries from the population previously
        tested.

Results for two secretaries were dropped from the analysis
because these subjects failed to provide relative-clause
responses frequently enough for us to believe they under-
stood the instructions (see p. 92). We no longer, however,
required a minimum score of 25% correct, because internal
features of poor subjects' responses seemed no different
from those of better subjects. Two secretaries(S4 and S8
in this new group get only 20% of the stimuli right in
the original task.

        b. Materials: The same taped list of stimuli
was used. Two decks of response-cards were developed.
The first listed the modal correct response of the new A-
group to each of the items in the spontaneous generation
task. The second listed the modal error (in most cases
this was the modal response of the C-group) of the new C-
group for each of the original items.

The two decks were randomized, using a random number table, so that the 'correct' response now might appear either in the left-hand or the right-hand pack.

c. Instructions: The subject was reminded of the earlier task, and told that this time she was asked merely to choose the card that 'meant most nearly the same thing' as the compound she heard. Stress was indicated on the card by hyphenation, as in ordinary English orthography, and the subject was taught and pretested to assure that she understood how to pronounce the responses listed on the cards; this seemed to create no difficulty. The instructions from the spontaneous-generation task were now reread to the subject, with appropriate changes for the new task.

d. Method: The two randomized decks were laid face-up before the subject (i.e. the correct and the incorrect responses to stimulus 1 were first exposed). The stimulus was then played on the recorder, and the subject handed to the experimenter the card she preferred. As in the previous experiment, replay of the stimulus was allowed at the subject's request. The experimenter now removed the card that had not been chosen (thus exposing the choices for the next stimulus), recorded the response, and played the next stimulus.

b. results:

   1) Gross effects: Our first question is per-
haps the most important here: Did the subjects do better
when their task was to recognize the correct alternative
rather than to produce it? If they did markedly better, we
would suspect that the results for Experiment 1 were arti-
facts of the procedure. As it happens, subjects' behavior
was substantially unaltered.

   Table 8 presents the percentage of correct responses
for each of the subjects in Experiment 1 ("generation")
and Experiment 2 ("choice").[1] At first glance, the table
seems to suggest that there has been a marked improvement for
all but one C-group subject.[2] But this improvement results
from an artificial inflation of the recognition score by
chance factors. After all, the subject now has only two
alternatives: if she did not understand a word of English,
she would come up with a score of 50% "correct", a fact well
known to all who have taken true-false objective examinations.
We must obviously correct for this chance factor.

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   [1]N is less than 144 for most subjects because we ex-
clude trials on which the subject's spontaneous paraphrase
was not intelligible on the tape.

   [2]For A-group subjects there is also perhaps an improve-
ment, but an obvious ceiling effect makes it difficult to
evaluate this result.

Suppose we had some means of detecting how many of the correct choices made by the subject are "genuine", i.e. reflect "what she really knows". Let us call the number of genuine correct responses M. If there are N stimuli altogether, there are of course N - M cases for which the subject does not "really know" what she is doing. Given that she does not know, she can do no better than guess. Under these circumstances the chances of guessing correctly are .5; hence 1/2 (N - M) "correct" choices will be added to his score. The total number of correct choices (T), genuine and chance, is thus:

$$T = M + 1/2 (N - M)$$

Can we find some way of estimating M? In fact we can, for we have the number of correct paraphrases given by the subject in Experiment 1. The results of that experiment suggest that the probability of generating a paraphrase correctly by chance is essentially zero. Substituting appropriately we can now derive what T should be on the assumption that the subjects perform identically on both tasks. These predicted T-values are presented in the last column of Table 8. It is clear that, once the choice scores are corrected for chance, Experiment 1 leads to more correct responses than does Experiment 2. This outcome is the more surprising because we would really have assumed subjects would be correct more than half the time even for

Table 8

Group	Subject	N	Generated correct	Obtained chosen correct	Expected chosen correct
C	S1	143	56	77	99.5
C	S2	141	78	94	109.5
C	S3	143	60	88	101.5
C	S4	142	26	63	84.0
C	S5	143	59	89	101.0
C	S6	117	41	68	79.0
C	S7	140	84	82	112.0
C	S8	125	28	58	76.5
C	S9	144	57	79	100.5
C	S10	135	50	81	92.5
A	S11	144	138	138	141.0
A	S12	144	110	136	127.0
A	S13	144	128	137	136.0
A	S14	144	126	133	135.0
A	S15	144	137	138	140.5

Comparision of number of correct responses
for the "generation" and "choice" conditions

stimuli which they had not generated correctly, because the
correct paraphrase -- which the subject might not previoulsy
have 'thought of' -- is presented for consideration.

Since Experiment 2 did not improve the performance of
group-C subjects, we can reject the hypothesis that their
ineptitude in generating paraphrases was an artifact of the
procedure. But they did not merely fail to improve, they
seem to have gotten worse. Why?

> 2) Choice performance as a function of
the alternatives: The number of predicted correct choices
(T) was estimated on the assumption that when the subjects
did not really understand the compound, they would respond
by chance. The fact is that they did not do so.

Table 9 displays the results in terms of the item-by-
item consistency of each subject in the two experiments, i.e.
whether the subject chose correctly just in case he had
generated a correct paraphrase for a given item. It is
obvious from this table that the subject is least likely to
choose wrong if she had generated a correct paraphrase.
But does the subject prefer to choose wrong if she had
generated an incorrect paraphrase? This matter is unclear
from Table 9: six out of ten C-group subjects seem to show

this preference, while four go in the opposite direction.[1]
For the A-group, the number of wrong responses is too small
to support any conclusions, but they appear in general to
choose correctly even when they had generated unacceptable
paraphrases.

In order to clarify the question of whether subjects
in fact prefer to choose the paraphrases they had previously
generated, we can sensibly consider only those instances in
which either the correct paraphrase or the error available
as choices was equivalent to the paraphrase actually gen-
erated by the subject in Experiment 1, i.e. we exclude
now all trials in which the subject did not have the
alternative of effectively repeating her original response.[2]

_____

[1]The ambiguity of these results compared with the
model tested in Table 8 implies that that model was in error
in assuming that the probability of a chance correct spon-
taneous paraphrase was close to zero. Lenient scoring and
the benefit of a doubt in constructionally ambiguous responses
no doubt account for this fact.

[2]Judging whether the subject's response was in fact
available was a difficult matter, for we could not require
word-by-word identity. For example, a bird who lives in a
black house and a bird who lives around black houses were
considered identical for these purposes. However, a bird of
the species "black-house" was considered a different answer.
All questionable cases were excluded. Judgments were made
without knowledge of the subject's actual forced-choice
response. The subject's responses were available as choices
between 42% of the time (S9) and 81% of the time (S13).

Table 9

Group	Subject	N	Gen. correct Chose correct	Gen. correct Chose wrong	Gen. wrong Chose correct	Gen. wrong Chose wrong
C	S1	143	41	15	36	51
C	S2	141	56	22	38	25
C	S3	143	44	16	44	39
C	S4	142	14	12	49	67
C	S5	143	49	10	40	44
C	S6	117	26	15	42	34
C	S7	140	64	20	18	38
C	S8	125	13	15	45	52
C	S9	144	31	26	48	39
C	S10	135	43	7	38	47
C	Total	1373	381	158	398	436
A	S11	144	133	5	5	1
A	S12	144	109	1	27	¯7
A	S13	144	123	5	14	2
A	S14	144	119	7	14	4
A	S15	144	135	2	3	4
A	Total	720	619	20	63	18

Consistency of subjects between test and retest

Table 10 consists of contingency tables for C-group subjects, indicating that they tend to choose as they had generated. Each subject shows this same effect, seven of the ten significantly so. The sum of chi-squares for all C-group subjects is highly significant, yielding a chi-square of 113.9 which with <u>df</u> = 10 is significant well beyond the .001 level. Very clearly, subjects tend to persevere in their correct responses. For eight of the ten subjects, the tendency also to persevere in errors enhances this result, and only one subject (S2) shows a weak effect in the other direction. Over all C-group subjects, the probability of choosing a modal error in Experiment 2 if it was generated in Experiment 1 is considerably higher than .5. In table 11, the responses of A-group subjects are also indicated as contingency tables, although the paucity of errors does not allow us to make a realistic statistical test of significance. Notice, however, that the apparent tendency of A-group subjects to choose correctly even when they had generated incorrectly is (Table 9) minimized, just as it is for the C-group, when we consider only cases when the subject had his own generated response available as a choice: apparently, it is <u>other people's errors</u> that subjects correct, but they are satisfied to persevere in their own errors if those are available. It seems safe to conclude that it is not the case

Table 10

S1

	R	W
R	33	17
W	9	31

$x^2 = 16.90$

$p < .001$

S2

	R	W
R	35	18
W	11	16

$x^2 = 3.63$

$.10 < p < .05$

S3

	R	W
R	37	31
W	12	32

$x^2 = 7.94$

$p < .01$

S4

	R	W
R	11	16
W	7	33

$x^2 = 5.49$

$p < .02$

S5

	R	W
R	45	18
W	9	32

$x^2 = 24.36$

$p < .001$

S6

	R	W
R	19	11
W	9	18

$x^2 = 5.11$

$.02 < p < .05$

S7

	R	W
R	34	5
W	10	26

$x^2 = 27.23$

$p < .001$

S8

	R	W
R	11	22
W	11	33

$x^2 = .64$

S9

	R	W
R	23	10
W	18	10

$x^2 = .20$

S10

	R	W
R	35	15
W	4	30

$x^2 = 27.40$

$p < .001$

Σ of x^2 = 118.9, with 10 <u>df</u>, $p < .001$

Contingency tables for C-group subjects
The rows represent right ("R") and wrong ("W") choices for each subject.
Columns represent right and wrong spontaneous paraphrases.

Table 11

S11

	R	W
R	101	0
W	3	1

S13

	R	W
R	106	5
W	5	1

S12

	R	W
R	87	9
W	0	2

S14

	R	W
R	65	1
W	4	4

S15

	R	W
R	105	0
W	2	1

Contingency Tables for A-Group Subjects

that subjects simply "know nothing" and are guessing, under
this condition; quite the contrary, their errors (as we
label them) seem to be systematic features of their organiza-
tion of the task we set.

We now ask what happens when the subjects originally
generated an error other than the modal error, and now have
to choose between the correct paraphrase (which they did
not generate) and an incorrect paraphrase which they also
did not generate. Now the false alternative is someone
else's error. Under these conditions, subjects are more like-
ly to opt for the correct response, as Table 12 shows. For
stimulus items on which a subject made a non-modal error
in Experiment 1, the proportions of correct choices in
Experiment 2 were generally over .5. The corresponding
figures for stimulus items on which the subjects made a
modal error in Experiment 1 were generally under .5.
Every C-group subject shows a difference in the same
direction.[1] Again, the A-group seems to show the same
effect, but the numbers on which proportions for the A-
group were calculated are ludicrously small. Thus the sub-
ject prefers his own response, right or wrong by our own criteria,
and tends to our own definition of what is correct only when
his own preferred response is not available to him.

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[1]That this result is highly significant needs no
statistical proof.

Table 12

% Correct Choice

Group	Subject	Where subject had generated the modal error	Where subject had generated a non-modal error
C	S1	35.4	48.7
C	S2	52.9	68.9
C	S3	49.2	65.0
C	S4	32.6	49.2
C	S5	36.0	64.7
C	S6	37.9	66.0
C	S7	16.1	52.0
C	S8	40.0	54.7
C	S9	50.0	56.7
C	S10	34.1	57.5
A	S11	0.0	100.0
A	S12	81.8	78.3
A	S13	83.3	90.0
A	S14	14.3	100.0
A	S15	0.0	50.0

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3) Distribution of errors in the forced-
choice situation: Because there were many new subjects in
this condition, scores on the generating task were rescored
for all subjects to see whether the results described for
Experiment 1 were repeated with the new samples. Some
refinement of the scoring technique was also attempted.
The pattern of results was very much the same, so they will
not be described here again. Appendix B explains the re-
scoring and gives the major results of this replication.

4. Experiment 3:

I mentioned briefly when introducing Experiment 2
(p.123) my conviction that the differences obtained in these
experiments were not dialect-group distinctions. This
conviction was based on three facts: (1) the secretaries
have, in general, the same regional pronunciation (mid-
atlantic urban) as the graduate students, by my observation;
(2) to my knowledge, the distinctive stress features of
compounds are very similar across the United States, perhaps
excepting the Southern dialects; (3) the same syntactic
structures were difficult for all populations tested, i.e.
there was no interaction, in the spontaneous generation task,
between group-membership and phrase-category. Nevertheless,
in view of the nonoverlapping scores of A-group and C-group
members, and the undeniable fact that there must exist
secretaries who are more verbal, or more intelligent, than

some graduate students, it seemed sensible to look for such
secretaries and examine the structure of their responses.
If we found secretaries who were as consistent as graduate
students, but whose error-type preferences turned out to be
similar to those of secretaries, we might conclude that
there existed a dialect difference, at least in the mode of
relating semigrammatical sentences to the underlying grammar.

At this writing, we have been able to find only two
secretaries whose overall generation scores were at all
close to those of the graduate students, although there was
still no overlap: S16 had 55 errors, S17 had 47, while the
maximum number of graduate-student errors was 34. Still,
these two subjects err far less often than the thirty other
secretaries we have so far tested. Table 13 shows the
distribution of these subjects' errors by error-type; these
distributions resemble A-group distributions (see Table 5)
much more closely than they do C-group distributions.

In the forced-choice situation, these subjects again
resemble the graduate students: S1 had 22 wrong choices;
S2 had 19.

		Error Type:				
		Order	Stress	Chaos	Grammar	All
S16	No.	38	1	7	9	55
	%	69.1	1.9	12.7	16.3	
S17	No.	35	2	8	2	47
	%	74.6	4.2	17.0	4.2	

Table 13

Errors and distribution of error-types (in percentages) for two
high-scoring secretaries.

Section IV: Discussion

There are three major findings of the experimental
studies: (1) there are very large differences in the behavior
of the three populations sampled in providing and recogniz-
ing paraphrases; (2) for all individuals, the same kinds of
phrase-categories lead to difficulty in providing a para-
phrase; and (3) the kinds of error 'preferred' differ for
the various phrase-categories and for the three populations.
In Section A of the discussion, I take up these topics in
turn. Part 1 (Individual Differences) discusses the dif-
ferences among people in their linguistic organization;
part 2 (Grammaticalness and Paraphrasability) deals with the
effect of syntactic structure in determing the nature of a
paraphrase; part 3 (Semigrammaticalness and Error-Type)
discusses modes of coping with linguistic material perceived
as structurally deviant.

In Section B of the discussion, I take up the question
of the relevance of this work for linguistic research. First
the methodological implications of the results for linguists
are discussed; second, the relevance of the work to the study
of creativity and innovation in language use.

A. The Perception of Paraphrastic Relations

1. Individual Differences: Inescapably, these pop-
ulations differ in their responses to the experimental
questions. Graduate students paraphrase compounds different-
ly than High-School graduates.

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a. But are there really individual differences?
We have seen that the generative grammarians suppose that
individuals are equally competent in linguistic matters.
Since individual differences seem to exist, we must consider
lines of argument by which these findings could be explained
away, leaving the theory of equal competence intact. The
first kind of argument would be that the experiment itself
was at fault, i.e., that the apparent linguistic differences
among the subjects are artifacts of the experimental situa-
tion. If the differences are facts, not artifacts, then
the second position of the grammarians seems to be (cf. p. 4୬)
that the experiment must therefore have tested performance,
not competence. I consider both these matters here, not
only because my results seem to be at variance with an
influential theoretical position, but because the same
questions ought to be answered about any experimental situation.

   1) Fact or artifact? Let us ask once again:
are the results an artifact of the experimental situation?
Consider two possible artifacts that have some a priori
plausibility:

   (a) Perhaps the compounds were rather
difficult to hear and to process, because of the absence of
sentential context and determiners. This interpretation is
not too likely. To begin with, it is virtually impossible

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that there were acoustic problems. Two judges reliably
interpreted the stress-relations of the stimulus tapes be-
fore the experimentation began. Furthermore, there is clear
evidence that the A-group subjects had no problems of this
sort: they hear the stress and the separate words of the
stimulus correctly in at least 35 of every 36 instances.[1]
There is no reason to suppose that C-group subjects differ
in their hearing from A-group subjects. I might add that,
apart from errors of grammar, the paraphrases provided by the
C-group do not seem to differ in content or ingenuity from
those provided by the A-group: what differs is the match
of phrase to paraphrase. There is then no reason to
suppose that some part or attribute of the stimulus was not
detected by the subjects, who then somehow had to fabricate
that which they had missed. What differed from group to
group was the decision of what to process or how to process
it or the nature of the processing itself.

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[1]Stress and chaos errors may result from mishearing
of the stress, or they may involve an error in processing
rather than encoding. However, all correct responses, and
errors of other types imply (except for possible correct
'guesses' here and there) a correct hearing of the stress-
relations. Thus if we consider all errors of stress and
chaos to be encoding errors, we get the maximum number
of mishearings of stress that could have occurred. On this
basis, we conclude that A-group subjects mishear the stress
no more than one time in thirty-six. Misreports of individual
words occur in less than .001 of trials.

(b) Perhaps the subject did not understand the instructions. To be sure, the 'free' and 'forced-choice' situations produce essentially the same results, but after all, the instructions for both tasks were almost alike, so this point is not conclusive.

We can rule out this interpretation with fair assurance. The internal pattern of the results makes it certain that subjects understood well enough to behave systematically: (1) for all subjects, and for all stimuli, fewer than one response in thirty fails to elicit the syntactic format (a nominal or adjectival phrase) required for a correct response; (2) such format errors (errors of grammar) as did occur were almost totally limited to compounds with verb X-terms, for which there is often no conceivable grammatical interpretation; (3) for all subjects, the same stimulus types account for the same proportion of errors (e.g., there is no interaction between group-membership and phrase-category). All of these findings suggest that all subjects were responding to the same characteristics of the same stimuli.

There is a final variant of this hypothesis: perhaps the situation is so artificial that only the 'test-wise' individual can credit the kind of answer he is called

upon to give. Perhaps it takes a sort of perverse sophistica-
tion to suppose that the right answer (to any question)
could be 'one who blackens housebirds'. The results of the
forced-choice study do much to remove this possibility,
for here the subject has only a choice between eccentricities;
yet he does not choose haphazardly between them.

It would appear, then, that the results reflect genuine
group differences rather than artifacts of the experimental
situation. In a way, this is not too surprising: individual
differences in response to (apparently) linguistic tasks
have been found before, at least where the tasks are not
too simple (cf. Sleator and Maclay; Hill). An exception is
the study by Livant (op. cit.) who found no variation at all
among the three subjects he asked to paraphrase compound-
nouns. Considering the size and reliability of our group
effects, I can only conclude that Livant's three subjects
were more linguistically sophisticated than the subjects
in our A-group. If so, his study hardly constitutes a fair
test of linguistic uniformity.

2) Performance or competence? These data
make it impossible to deny that there are individual dif-
ferences in the performance of linguistic tasks; Katz'
assertion (cf. p.35) to the contrary is thus disposed of.
But then there are few linguists who would not have granted

performance inequities from the beginning. To most of the
linguists whose work is here relevant, the issue is not
whether people differ in their performance, but whether such
differences reflect any underlying differences in their
linguistic competence.

It is obvious that the student of language initially
confronts certain facts of performance; competence can only
be inferred. On what basis can such an inference be drawn?

In order to make the distinction,the transformational
linguist provides criteria of competence that are independent
of such obvious facts about performance as the utterances a
speaker happens to produce. Thus if a speaker can distinguish
between a well-formed sentence and a deviant sentence, if he
can recognize constructional homonymity, etc., we can infer
a kind of organization not necessarily reflected in
spontaneous speech.[1] Yet these procedures (so happily
tailored to the special talents of professional linguists)
cannot be the only ones allowed. Since at the heart of the
transformational hypothesis is a description of the relations
among sentences or sentence-types, it would seem that the
ability to provide paraphrases (or at least to distinguish

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[1]I have previously noted that there have as yet been no
general tests of competence -- for various populations --
using these procedures.

paraphrases from nonparaphrases) would be the most direct and compelling evidence for this theory. But if the theory of equal competence is correct, it should apply to secretaries no less than to graduate students. If, as Chomsky asserts, "each normal human has developed for himself a thorough competence in his native language", then in principle at least we should be able to extract this evidence from anybody. Necessarily I have asked subjects to 'perform' because I could not ask them to 'be competent.' Their paraphrastic performances varied widely. I now examine the Chomskian supposition that such enormous differences in performance are compatible with the hypothesis of equal-competence.

Performance deficits might reflect some kind of statistical noise in the system: random errors due to lapses of attention, lowered motivation, and the like; or they could be attributed to a failure to attend to the salient syntactic features of the task. I do not believe that these factors adequately account for the failure of some of our subjects to cope with the task presented to them.

There is no evidence from the results that C-group subjects were less reliable or systematic in their responses than A-group subjects. In the forced-choice situation, C-group subjects were very likely to choose a correct paraphrase if they had earlier generated one, and were very

likely to choose an incorrect paraphrase if they had earlier

generated it. Thus they do not make random errors: they

choose systematically, but differently, from A-group subjects.

Furthermore, sheer difference in the overall tendency to

err will not account for the differences among groups.

When we equate statistically for differing baselines for

error, significant differences in the kind of error pre-

ferred by the various populations are not eliminated. It

is thus difficult to argue that obtained differences between

the groups represent merely different aptitudes in responding

stably in test-situations.

It might be supposed that the C-group subject is more

likely to be misled by semantic features of the situation

that are essentially extraneous to the syntactic problem.

The instructions for both experiments ask the subjects to

disregard semantic anomalies ("many of these compounds may

seem odd or silly to you..."), but this does not mean that

all of the subjects were able to oblige us by disregarding

them. One could suppose that, while the same underlying

grammatical organization holds for all subjects, some of them

are more prone to semantic seduction, more likely to desert

the syntactic Super-Ego for the semantical Id. This sup-

position cannot be maintained in the face of the experimental

findings.

In the first place, there is the evidence provided by the enormous effect of 'unity', the tendency to perceive a semantically plausible subunit even if the stress feature of the compound is thereby rendered syntactically deviant (e.g. $\underline{black^1house^3bird^2}$ is harder to paraphrase than $\underline{black^2house^1bird^3}$). Here surely is an instance of semantic seduction. But it is no more apparent in C-group subjects than in A-group subjects, once the overall differences in error-rate are adjusted: the unity factor affects all groups equally (see Figure 3).

Another indication that C-group subjects were no more prone to semantic interference than A-group subjects comes from the results for the stimuli with 'mixed' lexical assignment. The notion 'a bird-house made of stone' is no more extravagant than the notion 'a stone used to make a bird-house'. Both phrases require a belief in the possibility of stone bird-houses. Yet given the stimulus $\underline{bird^1house^3stone^2}$, only the C-group is inclined to respond 'a bird-house made of stone'. This tendency surely turns on the essential 'prepositionalness' of the word stone and not on the semantic character of the phrase.[1]

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[1]I have earlier noted that stone is not even a 'good' adjective, i.e. we do not say 'a very stone house'. If it is a noun, then, it should be thought of as post-positional, and $\underline{stone^2bird^1house^3}$ should seem at least as difficult as $\underline{bird^1house^3stone^2}$.

3) The limits of competence: Except for members of the A-group we have found little evidence of a general ability to recognize transformationally related phrases. Although, for each phrase, we provided in the forced-choice situation a legitimate grammatical paraphrase, C-group subjects did very poorly in choosing the appropriate paraphrase from a pile of two.

Then if competence is competence at anything, what is competence competence at? I have quoted various linguists and philosophers who, defining competence as the 'tacit knowledge' of the speaker, project into each normal adult what comes to an effective procedure for saying, classifying, and understanding an indefinitely extendable set of new sentences. . In part, this claim is based on the selfevident fact that speakers can create 'new' sentences, physically different from any they have heard, by the use of certain iterative procedures, such as coordinating conjunction. An unbounded set of grammatical sentences is thereby brought into existence, but the ability to employ such devices does not imply the ability to understand complex embedded constructions of the language, nor the ability to classify sentences, nor the ability to recognize sentential relations in a principled way. These latter skills differ from person to person.

Whether the differences among people are so large that they interfere with reading The Daily News, we cannot know

from what has been done here, and cannot really know at all
until the role of grammatical organization in linguistic
behavior is assessed. The linguistic entities I have
studied are reasonably complicated, but three-word compounds
are not computational monstrosities, and they are created
and used all around us. Context and knowledge of the world
no doubt resolve most of the confusions that inhere in
these structures when they are encountered in everyday life.
Nevertheless, the results of these experiments cast doubt
on the ability of less-educated (or less linguistically
able) individuals to deal with syntactic complexity when
there is no contextual recourse.

In sum, there is little doubt that speakers differ in
how well they can paraphrase difficult speech fragments.
The effect cannot be dismissed as an artifact, nor can it
be explained away as a 'mere' performance deficit. It
is hard to maintain the position that competence is equal
for all speakers unless one finds a more restricted and
genuinely meaningful definition of that term.

2. Grammaticalness and Paraphrasability

We have by now ascertained that subjects and populations
differ considerably in their ability to deal with the ex-
perimental stimuli. However, we also find effects that point

to qualitative similarities in the behavior of all of the subjects.

a. Phrase-category: Phrase-category is a highly significant factor in determining response-type. For every stimulus item in the test, there is strong consensus about an appropriate response among A-group subjects, and this consensus persists in a rambling dwindling fashion down into the C-group, for all but the most difficult phrase-categories. In this kind of situation, with these instructions, compounds quite generally elicit as paraphrases relative clauses with the underlying structure of their 'transformational sources'; that is, right-hand structures in the formulas of Section III are related to their corresponding left-hand structures in a very special way. These rather uniform effects of phrase-category on the structure of responses must reflect something of what we mean when we say that all of our subjects belong to the same linguistic community.

Thus the significant effect of phrase-category is one more demonstration of the grammatical organization of linguistic behavior. I have mentioned various other behavioral correlates of syntactic structure: certain sequences are uniformly judged to be grammatical (cf. Chomsky, 1957; but

also Sleator and Maclay) are easy to repeat (Johnson)
and to recall and hear in noise (Marks and Miller; Miller
and Isard). We can now add that the linguistic structure of
a phrase has a powerful effect on the kind of paraphrase it
will elicit without context, and often in spite of semantic
or even syntactic deviance.

b. Phrase-category and the tendency to err:
Some phrase-categories are significantly more difficult to
paraphrase than others (see Table 4), i.e., they elicit
fewer identical paraphrases, and indeed, elicit certain
paraphrases that we reject. I have shown that the rank-
order difficulty of the various phrase-categories is approxi-
mately the same for all populations, i.e. there is no
interaction between group-membership and phrase-category
measured against mean-error.

Discussed below are two possible kinds of explanation
for the fact that certain phrase-categories are more dif-
ficult to paraphrase than others for all populations:
(1) there may be differences in the grammatical status of
the phrase-categories, and (2) there may be differences in
the difficulty of the various types arising from the
methods subjects use to encode linguistic material.

(1) Paraphrasability and the grammatical
status of the compound: Table 7 shows that the lexical-class

of the test word has a highly significant effect on the
tendency to err. Thus phrase-categories with noun test-
words lead to fewer errors than phrase-categories with
adjective and verb test-words. A possible explanation of
this finding is that ungrammatical or semigrammatical
sequences lead to much less uniformity in response-type:
while the subject can deal with grammatical sequences by
reference to a grammar, there is no such (or less of an)
internalized 'semigrammar' that describes deviant sequences.

Referring to the grammatical sketch of Section III, we
see that there are indeed many and confusing restrictions
on the participation of verbs in compounds: verbs with
certain kinds of complements are excluded; unaffixed forms
of verbs are blocked in compounds by the existence of com-
peting suffixed forms, etc. We know, in fact, that one
of the verbs chosen (eat) results in uniformly unacceptable
compounds.

However, if the grammatical status of the phrase-
category is the factor determining its paraphrasability,
we are left with some unanswered questions: The verb eat,
which results in ungrammatical compounds, is associated
with no more error than the verbs wash and kill, which
almost uniformly form grammatical compounds. Further, com-
pounds with post-nominal adjectives also lead to more error

than compounds with noun test-words; yet all of these yield technically grammatical compounds.

It is clear that we cannot use paraphrasability as a criterion of grammaticalness, for that would involve us in a fruitless tautology. Criteria for grammaticalness have to do with systematic formal considerations. On the other hand, it is true that grammatical sentences (so defined by reference to precisely such formal criteria) very often are just those that are uniformly acceptable, paraphrasable, repeatable, recallable, etc. It would seem that such behavioral correlates of formal grammaticalness might give us some way of fixing a core of data for which the grammar is to account. However, when we adopt such an approach, we automatically build into grammatical description accounts of rather distant features of cognitive functioning (e.g. rules of grammar that were previously recursive are now to be limited by constraints on immediate memory). Furthermore, by accepting what is essentially Katz' position (cf. p. 47) we become incapable of describing aspects of linguistic behavior that are limited to certain individuals and to certain circumstances (we will discover as a trivial tautological consequence that people differ not at all in their perception of grammatical expressions in the language).

The failure to find that every compound we call grammatical is as easy to paraphrase as every other compound we call grammatical raises no obvious problems for grammatical description. But we still do not know why some compounds are easier to paraphrase than others. Consider, for example, the very large differences in difficulty between phrase categories with prenominal adjectives; e.g.:

$$\text{black}^2\text{bird}^1\text{house}^3 \qquad (\text{C1})$$
$$\text{thin}^2\text{bird}^1\text{house}^3$$

and phrase-categories with postnominal adjectives, e.g.:

$$\text{bird}^1\text{house}^3\text{black}^2 \qquad (\text{C2})$$
$$\text{bird}^1\text{house}^3\text{thin}^2$$

While C1 is easy to paraphrase, C2 is very hard. It scarcely needs showing that C1 represents a constructional type that occurs more frequently in everyday speech than C2, but why should that matter? If the subject 'knows' this fragment of syntax, and if the examples are not more unreasonably complex or nonsensical in the one case than in the other, why should subjects fail only with C2? Surely they have heard similar forms (midnight blue, peasoup green, ice cold, ruby red, sparking bright, dirt poor). Notice that even if in a grammar of English such transforms were 'generated later' or were 'further removed from the kernel' (and we have no reason to say they are), we would have no way of accounting for the fact that a person who 'knew the

whole grammar' would have trouble with instances of such a
constructional type if they did not strain his memory or
his credulity.

Thus apparently we cannot turn to internal matters
of grammatical complexity to decide why some phrase-
categories lead to more errors than others. An obvious
interpretation of the results, however, is that certain
features of the grammar are known less well to speakers
than are others. Postnominal use of adjectives is a case
in point (phrase-categories A2-, A3+, and A3-) and compound-
ing with verbs is another (phrase-categories V1, V2, and V3).
Put another way, rules [iii] and [viii] of the grammar are
very well assimilated by speakers, while rules [iv], [v]
and [ix] are not.

If we accept this hypothesis, we must look at the re-
sults in absolute rather than relative terms, as we would
for most questions of expertise: A-group subjects are a
bit shaky on these frills of the grammar, but C-group
subjects hardly seem to know them at all. Thus we can also
explain the persistence of C-group subjects in repeating
their errors systematically in the forced-choice situation,
when the correct alternative is presented to them.

To account for a host of phrases constantly coming into
the language in a regular and, to many, a comprehensible
fashion, a grammar of English must describe postnominal

adjectival phrases. By admitting this 'productive' process, we do not say exactly how productive it is, and it is not easy within grammatical description to make this proviso. If we say: "this transformation has strong cooccurrence restraints" we merely beg the question, for we haven't any notion how to describe those constraints. Yet it is intuitively clear that C1 and C2 phrases are examples of syntactic types of wholly different status within the language: C1 is almost totally productive, over the whole adjective-noun range, while C2 is obscurely limited to ill-understood adjective subcategories.

In general terms we can admit that there are "frills of grammar" of restricted status that must be accepted technically as legitimate -- because they are, to some extent, productive -- but which may be less well assimilated by speakers, perhaps just because of their highly restricted domain. However, we may find some more specific difficulties with the phrase-categories that give rise to nonuniformity in paraphrasing when we consider the possible encoding problems a speaker faces when he hears these stimuli.

(2) Paraphrasability and the encoding process: Another kind of explanation for the differential paraphrasability of various 'grammatical' compounds lies in a description of the process of encoding, which may mirror the grammar only remotely.

Chomsky (1961) has once suggested that a generative grammar "supplemented with a hierarchy of categories" may be the kind of device that best describes 'levels of grammaticalness' of utterances. The general schema Chomsky proposed is this:

> Suppose that we have a grammar that generates an infinite set of utterances with structural descriptions. Let us call the units in terms of which these utterances are represented by the neutral term formatives...[e.g., words, morphemes]...Suppose, in addition, that we have an m-level hierarchy of categories of formatives....On each level, the categories are exhaustive in the sense that each formative belongs to at least one....We might also require that each level be a refinement of the preceding one, i.e., a classification into subcategories of the preceding level.

Thus, for example, a sentence like Golf plays John has a grammatical representation at some level in the hierarchy of categories (NVN), but fails to be represented at another level when the noun-class is subcategorized into animate and inanimate, and the verb-class into verbs that can take inanimate subjects and verbs that cannot ($^{*}N_{in}V_{an}N_{an}$). The category-level at which the sentence 'fails' is the 'degree' to which it is ungrammatical. Thus, in the process of understanding a sentence, there may be a stage at which the utterance is projected as a sequence of various-level categories.

Suppose, as we listen to a phrase, we develop certain "expectancies" about the sequence of categories at some level

that will occur within the phrase; violations of these
expectancies may increase the probability of an error even
though there are other situations in which we would be able
to deal with such violations.[1] Thus at some level of
representation, after N, or NN, A is unlikely. After N, or
NN, unaffixed V is similarly unlikely. This does not mean
that these sequences of categories are not grammatical, only
unexpected.

This interpretation of the special difficulty of com-
pounds with postnominal adjectives and verbs is not altogether
at variance with syntactic description, for the assumption
of syntactic organization of linguistic material does not
preclude the existence of parallel heuristic procedures
and computational devices in the encoder that are probabilistic
and serial in nature. While left-to-right encoding of sen-
tences fails to account for many linguistic phenomena (cf.
Fodor and Bever), it is still quite likely that serial

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[1]Left-to-right parsing devices employing phrase-structure
grammars incorporate some such notions of expectancy.
Yngve (81) relates difficulty for the hearer to the degree
of embeddedness of an element, i.e. to the number of left-
branches that connect it to higher-level constituents. There
have been some experimental studies of this hypothesis
(e.g. Martin and Roberts, 57).

organization has its own relevance in the encoding process.[1]
Should this be so, then violations of certain expectancies
will interfere with comprehensibility.

This interpretation of the results helps to explain
why compounds with postnominal adjectives and verbs, although
grammatical, often fail to be paraphrased correctly: they
are not economically encoded, and thus error arises.

The same hypothesis, on a lower level of the 'hierarchy
of categories', explains the unity factor, the tendency to hear
the two constant-nouns of the stimuli as though they were a
unit. This is the most powerful determinant of error,
aside from group-membership, in the test. We have shown
that the prior familiarity of $\underline{bird^1house^3}$ as a lexical item
would not account for this factor, since the effect for the
'unknown' item $\underline{house^1bird^3}$ was essentially the same (see p. 115).
Yet both these sequences of words obey the cooccurrence
constraints of the language, and both are semantically

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[1]Johnson (op. cit.) observes that the first few words
of sentences are repeated more often than the last few in
an experimental situation designed to produce some errors
in recall and repetition. This is a significant and stable
effect. However, a larger effect, for the same sentences, is
the tendency for all words of a major constituent to be
repeated if one of them is repeated, i.e., the transitional
error probabilities for words in the sentence are not inde-
pendent. Thus while a phrase-structure description accounts
for Johnson's results in large measure, an effect describable
as serial organization in memory is also discernible.

plausible. It may well be that once morphological as well
as syntactic constraints are met in a contiguous pair of
nouns within a nominal phrase, the hearer gets set to hear
them as a unit. Violation of this expectancy seems to in-
terfere systematically with comprehensibility: disunified
sequences of this sort are reorganized and understood
(or misunderstood) as unified sequences. Errors with these
disunified sequences are twice as frequent as with unified
stimuli.

The relation of habit or mental set to comprehen-
sibility is noticable in the everyday use of language; I
believe that a descriptive mechanism of this sort will be
required to explain many facets of the relation between
language-user and formal grammar. Not every pun is under-
stood. We would not expect the speaker to realize that
his intent was unclear every time he said 'Time flies',
nor would we expect the listener to recognize it. What we
might expect, at least in a large number of cases, is that
both encoder and decoder would be able to recognize
the ambiguity under certain

circumstances; this much is a reasonable prediction from
the theory of grammar. Thus we would expect the ambiguity
to be recognized more often if we said: "Here is an
ambiguous sentence: 'Time flies'". And we would expect it
to be recognizable more often if, in addition, we paraphrased
it: "I mean you should time flies, you should keep the time
of flies."

In the spontaneous-generation task, we possibly have
made it difficult for the subject to notice such problems
with these constructions, given that their normal expectancy,
or set, would interfere with their noticing them. Thus by
the time the subject heard house - bird he might have formed
a rather tight set (1) for house^1 bird^3, and (2) for a
nominal construction, i.e., a following noun.

In the forced-choice task, we tried to break or weaken
this set by presenting the subject with an alternative
paraphrase. Should one expect this method to break the set?
The answer depends on the kinds of assumptions that are made
about the encoding process. If the subject hears the phrase,
encodes it, and then simply looks for his chosen version
on the response-cards, the situation is no different from
the spontaneous-generation task. If, on the other hand,
the choice of paraphrases is still open to the listener when
he reads the response cards (and he is allowed to read them

even before he hears the stimulus), the set ought to be
weakened. The overall finding (see Table 10) is that sub-
jects in the forced-choice situation tend strongly to do
whatever they did in the spontaneous-generation task.
Whether this is because their knowledge of the grammar dis-
allows one of the alternatives, or whether it is because
their encoding procedures preclude a change of mind, cannot
be decided from these experiments.

3. Semigrammaticalness and error type

Let us ask, in terms of the expectancy hypothesis,
what courses of action the subject may pursue when his set
is violated. If, after a pair of constant nouns, the sub-
ject hears an adjective or a verb, rather than the expected
noun, he has essentially the following choices:

(1) recast the word as the expected noun, e.g.:

$house^1bird^3thin^2$ - a cracker shaped like
a house-bird;
a diet food used by house-
birds

(2) find a rule different from the one expected
to be applicable, e.g.:

- as thin as a housebird

(3) a combination of (1) and (2):

- an annual festivity at
which everybody tries
to thin their house-
bird

(4) recast the phrase as though the ex-
pectancies had been met, i.e., make an
error, e.g.:

- a housebird who is thin

I adopt here the simplifying assumption that the subject will provide a correct and grammatically defensible paraphrase whenever he can. Given that a subject makes an error (chooses a grammatically indefensible paraphrase) I believe that he has perceived the stimulus as somehow other-than-grammatical and is attempting to cope what he conceives to be a 'semi-grammatical' sequence. I temporarily leave out of account the question of whether this perceptual organization arises during the encoding process, or thereafter.

As the scoring technique implies, we have noticed that subjects make different errors on different occasions. Sometimes they behave as though they had heard a stress pattern different from the one provided (E_s) and on other occasions they act as though they had heard the words in an order different from the actual one (E_o). If a subject 'commits' both these errors simultaneously, we say he has committed an error of chaos (E_{ch}). Further, there are other occasions on which the subject recasts the phrase (contrary to all sorts of grammatical cues, let alone the instructions for the task) as some other structure entirely, most notably an imperative (E_g). If the subject cannot see through to the implied nominal or adjectival structure of the compound as given, it follows that he must treat the stimulus as

semigrammatical and make one of these adjustments.

To understand why subjects make different errors in response to different stimulus types and why different subjects make different errors, we must examine in some detail the question of how a person copes with perceived semigrammaticalness. This question has been the subject of much linguistic inquiry. By an ungrammatical sentence, I mean a sentence that a grammar does not describe. But it is obvious that the situation is not as black and white as that: very many sequences fail to be grammatical only by a little, and many such sequences are readily comprehensible. Chomsky, as discussed earlier, proposed a method of assigning 'degrees of grammaticalness' to such sequences by reference to a finite hierarchy of categories that are related to sentences at various levels of representation. Paul Ziff (82) has proposed that a speaker is equipped with a small set of procedural rules, in addition to the rules of grammar, that enable him to 'find his way back' from the partly grammatical (semigrammatical) sentence to a sentence described in the grammar. The length or tortuousness of the route back is Ziff's measure of the ungrammaticalness of the original sequence. Jerrold Katz (42) takes strong exception to Ziff's model, partly because Ziff's analysis leaves open the question of how the speaker recognizes the grammatical sentence to which the ungrammatical one was ultimately related --

his question is how the speaker knows what routes to choose.
The answer, perhaps, is that Ziff's description takes up
where Chomsky's leaves off. But Katz' major objections turn
on the failure of Chomsky's proposal and Ziff's to parti-
tion the set of all nonsentences into two mutually exclusive
and collectively exhaustive sets: the set of semisentences
(which are partially structured and thus comprehensible)
and the set of nonsentences (incomprehensible strings of
words without recognizable underlying structure). Katz'
own model involves an underground semisentence grammar,
complete with its own semiphrase-structure, semiderivations,
semitransformational rules, 'traffic rules' that limit the
number and kind of semiderivations that can be undertaken
in place of derivations and, further, rules that relate the
resulting semistrings to the strings generated by the
grammar.

The details of these models are beyond the scope of
this discussion. However, our data provide some empirical
evidence of the paths people in fact travel when they per-
ceive a sequence as semigrammatical. It seems plausible
that such evidence can provide a basis on which to narrow
down the choice among theoretical models, if psychological
validity is to be required of the model chosen.

To explain these subjects' activities with deviant
sequences, we can equip them with the hierarchy of categories

notion, and a set of simple heuristic devices such as Ziff

proposes:

[sg i] Class extension: Word-class membership can be extended, or subcategorizations ignored.

[sg ii] Permutation: Sequences which are ungrammatical in the order presented can be re-ordered.

[sg iii] Change of contour: Sequences which are un-grammatical with the stress features presented can be repronounced.

[sg iv] Addition and deletion of constants.

Notice that the devices suggested here, and by Ziff, are

just those operations (permutation, deletion, etc.) that

appear in the structure of the grammar itself. We can closely

predict a subject's paraphrases by means of these semi-

grammar 'rules' if we know (a) the sequence of categories

that is being treated as semigrammatical, and (b) the popula-

tion to which the subject belongs.

a. Phrase-category and error-type: The strength

of the association between error-type and phrase-category

(Figure 2) establishes the fact that subjects behave

systematically, even when they perceive the stimulus as a

grammatically deviant one. The choice of error, or 'route

back', depends very heavily on the phrase-category of the

stimulus: extension of class membership [sg i] occurs for

all phrase-categories, but errors of order [sg ii] are associat-

ed with post-nominal adjectives, errors of stress [sg iii]

with nouns, and errors of grammar [sg iv] with verbs,
particularly in first-position. Thus the choice of response
depends on how the phrase fails to be grammatical.

Generally speaking, subjects seem to isolate the
anomaly, in these stimuli, by viewing the phrase as a
sequence of elements in a phrase-structure representation,
much as Chomsky suggests. Given that the sequence fails
at this level, the subject makes selective changes, much
in the way Ziff proposes.

For the kinds of stimuli we used, subjects[1] seem to
prefer to find a higher-level of phrase structure at which
the sequence has a grammatical representation, i.e. they
ignore violated subcategorization rules [sg i]. We have
scored such extensions as correct responses. Thus, over-
all, subjects 'correctly' paraphrase over half of the
stimuli even though most stimuli violate cooccurrence
restrictions or subcategorization rules.

_____

[1]We have suggested earlier that [sg i] is almost totally
limited to group-A subjects. When we pool the data for the
various populations, [sg i] is a very frequent device. We
consider the population restriction in the next section.

When subjects do not make use of this procedure
either because they cannot, or because it does not work
in the particular instance, they prefer to permute the
sequence [sg ii] until it can be represented correctly at a
high phrase-structure level. Thus errors of order are the
most frequent for all populations.

When the test-word is an adjective, rearrangements of
the elements, in this situation, will result [sg ii] in a
sequence representable as a noun-phrase derivable with the
'well assimilated' rules [iii] and [viii]. When the test-
word is a noun, rearrangement will help only if the
resultant sequence now obeys cooccurrence restructions that
were previously violated. The same result can often be
achieved by a shift in stress [sg iii], and this route is
very often selected when the sequence includes a noun test-
word, i.e., disunified sequences are unified.

In a certain number of cases, subjects' responses
can be characterized only by assuming that they applied
both [sg ii] and [sg iii] to the same sequence. We have
called such responses errors of chaos, since they seem to
play so fast and loose with the stimuli as presented.

When the test-word is a verb, rearrangement of the
elements or shift in stress, or both, will very often fail
to affect the semigrammatical status of the sequence. Under
this circumstance, some subjects seem willing to conclude
that certain 'function words' and transformational constants
may be missing from an imperative sequence. This is rather

surprising, since the deletion of function words occurs
only under very restricted and easily specifiable cir-
cumstances (in headlines; when Tonto speaks; when babies
babble). Nevertheless, with other paths blocked, some
subjects seemingly resort to an insertion of elements that
make it possible to view verb-containing sequences as
imperative sentences [sg iv].

Then adjective test-words are associated with [sg ii],
nouns with [sg iii], and verbs with [sg iv]. Errors of
chaos [sg ii + sg iii] are more uniformly distributed,
although they are expectably more frequent for adjective
test-words.[1] The use of [sg i] to extend word-class
membership does not show up as an error in our scoring.
The weight of evidence necessary in each instance to decide
whether a subcategorization rule was violated (thus necessit-
ating [sg i]) precluded reliable judgments by the scorers.
We can only estimate the use of [sg i] by examining clear
instances in which stimuli violated cooccurrence restrictions,
and from our knowledge of the number of other kinds of error.

---

[1]The distribution of E_s, E_o, and E_{ch} is distorted
slightly by the initial scoring technique, in which E_g
[sg iv] superceded others, i.e. an error that involved
E_g and E_o was scored as E_g.

b. Group-membership and the response to
semigrammaticalness: Although error-type is related to
phrase-category independently of group-membership, error-
type also interacts with group-membership. That is, dif-
ferent populations prefer different kinds of error.

In the previous section, I noted that from these data
and these scoring techniques one cannot decide at what point
the subject decided that he was dealing with a semigrammatical
sequence: we have had to score [sg i] corrections as correct
responses. At any rate, inspection of the responses shows
[sg i] to be largely restricted to A-group subjects, for
clear instances. Inspection of the results for lexically
ambiguous stimuli (Table 7) also shows a very large element
of classificational rigidity for non-A-group subjects;
this is another indication that C-group subjects are un-
likely to use [sg i].

The massive relation between group-membership and the
tendency to err (Figure 1) indicates that the point at which
subjects decide to use the other [sg] rules differs sig-
nificantly for different populations. The overall assumption
is that the point at which subjects give up on the stimulus
as a grammatical one and turn to [sg] rules differs
significantly. Subjects also differ in the kind of error
they then choose to make.

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I have argued that errors of grammar [sg iv] do
violence to the overt nominal structure of the sequence:
they imply a set of missing determiners, affixes, etc.,
in a fairly arbitrary fashion. This being so, the use of
[sg iv] - that is, an error of grammar - must be consider-
ed a serious error. One might also argue plausibly that
the use of two [sg] rules is somehow worse and more serious
than the use of one [sg] rule; thus errors of chaos can
reasonably be considered 'serious' errors.

On these grounds we group together errors of stress
and order as less serious errors, and errors of chaos and
grammar as more serious errors. We find that, not only
do A-group subjects err less often than C-group subjects,
but they are significantly more prone to make less serious
errors when they err at all. C-group subjects contribute
almost all the errors of chaos and grammar. When the data
are converted to relative numbers, thus correcting for the
difference in the overall tendency to err, this difference
between the groups continues to hold (Table 5). These
differences are enhanced when a more refined scoring technique
is adopted (see Appendix B).

The use of more serious errors by C-groups subjects for
the same stimuli for which A-group subjects chose less serious
errors implies either: (1) the C-group sujbect sees the

stimulus as 'less grammatical' and so his 'route back' has to be more tortuous, or (2) the C-group subject is not economical in his use of [sg] rules, or fails to recognize that some [sg] routes are preferable to others.

We have already shown that, on an absolute basis, C-group subjects are significantly more prone to err than A-group subjects: they fail to perceive as many of the stimuli as grammatical, and they fail as well to make use of the acceptable [sg i] in case the stimulus is ungrammatical. We now see that they are also more prone to the serious [sg] alternatives than are the A-group members. From all these findings, we conclude that the paraphrases of the C-group are not merely different from those of the A-group, but qualitatively inferior.

I have not begun to suggest a formal description of the fact that people cope with semigrammaticalness systematically; I have here proposed one kind of technique for eliciting behavior that must constrain any such theory. The 'rules' or procedures postulated here are contextless, and perhaps specific to the particular set of stimuli I have chosen, though they are very similar to those proposed by Ziff on theoretical grounds. The results of the experiment seem to be a display of the kinds of systematic approach to semigrammatical material presupposed by both Ziff and Chomsky.

Katz might very rightly argue that the loose and vague collection of activities characterized by these [sg] rules is hardly the starting point for 'a theory of semsentences' that will explain what to do under what circumstances, which way to go first, how far to go, which sequences will be tractable and which not, why everybody understands the same semisentences in the same ways, why everybody distinguishes semisentences from nonsentences more or less identically.

However, there is really no obligation to meet the requirements of Katz' theory because it appears that people do different things under the same circumstances, go different ways first, go to different extents, find different sequences tractable, understand the same semisentences in different ways, and distinguish semisentences from nonsentences wholly differently.

The need for Katz' theory hangs on his belief that "Semisentences are comprehensible to each speaker according only to his linguistic abilities" (42) and on his already cited statement that "variation in performance with intelligence [in nonlinguistic tasks]...contrasts with the performance of speakers with respect to some purely linguistic skill, where no significant individual differences are found."

I believe on the basis of these experimental results that Katz' elaborate semigrammar may be too much apparatus with which to encumber the already overburdened normal speaker.

B. Some more general comments

1. Linguistic method

a. The Ideal Speaker-Hearer: Transformational linguists have ordinarily worked with sophisticated informants, choosing among them until they find one with 'a good ear'. To most linguists, this procedure needs little justification: the desirable subject is one who by some previous experience or innate talent comes equipped to answer questions of well-formedness reliably. On the other hand, this selection bias has sometimes been cause for dismay among critics (usually psychologists) more sensitive to matters of representative sampling who could not help asking what the population was to which the sample results were to apply.

A very similar issue has long been debated by psychologists concerned with problems in perception and psychophysics. In this case, the point is to relate certain psychological qualities (e.g., hue, form, perceived depth) to the physical attributes of the stimulus. The classical psychophysicists argued that normal individuals do not differ in the structure of their perceptual systems but do differ in the stability of their reports. Their solution was to train observers until their judgment became solid, and they assumed that the training did not 'change anything' about the character of these percepts themselves. The trained subject thus was

supposed to be better able to get to the 'pure' percept,
for his report would be uncontaminated by many of the random
and systematic distortions his training had taught him to
disregard. Whether this procedure is appropriate for
psychologists is for them to decide. What is relevant here
is the striking similarity between assumptions that are
explicit in classical psychophysics and implicit in lin-
guistics. Consider the assumptions necessarily made by a
linguist who avoids subjects to whom the word 'grammatical'
is obscure or provocative.

It is obvious that the method of asking only oneself
(and perhaps for good measure one or two linguist friends)
about linguistic questions is justifiable if and only if all
normal persons are alike in their fundamental approach to
grammar. However, we did find massive individual differences,
both in the ability to paraphrase and in the way semigrammatical
material is paraphrased. The notion that all normal in-
dividuals perform equivalently in linguistic tasks (which I
take to mean, e.g., paraphrasing, classifying, punning,
writing poetry) was not supported. Given these results,
one may well suspect that grammars written by linguists
may lack descriptive adequacy for normal populations: the
linguists' informants are A-group members (that is, latent
grammar-writers). The linguist has succeeded in setting up
a mirror and has described himself.

If people differ, the failure to sample the linguistic
community cannot but lead to a biased view of how people at
large deal with language. The belief in equal competence is
very possibly the upshot of assuming that linguists and
philosophers provide a representative sample of the popula-
tion: Speakers of English. Until we account for the fact
that secretaries seem to be different in their linguistic
competence from graduate students, we must reserve judgment
on the hypothesis of equal competence -- as well as on all
the nativist ramifications and neurophysiological suppositions
that hang on it.

b. Data for a theory of language: There is no
disagreement about the fact that linguistic inquiry begins
with a list of obtained utterances. Hardly anyone seriously
denies that this list must be accompanied by reports that at
least give us the speakers' opinions about whether these
utterances are reasonable representatives of the language
under study, i.e., the utterances should not be those of
an aphasic or a madman or a foreigner, nor utterances that
were interrupted or poetic or humorously intended. Both
the utterances and the introspective reports are facts
about the performance of speakers, and from these we can
begin to infer certain underlying abilities of speakers
that may account for these performances in a systematic and
illuminating way.

The utterances that people produce and their opinions about the legitimacy of these utterances already limit the kind of description we can adopt concerning linguistic organization. However, we can obviously choose more sensibly among models when we add to the stock of performances that we examine. Thus, taken together, the findings that people can create novel sentences, carry out paraphrasing, classifying and parsing tasks, and recognize constructional homonymity (even if they can do none of these things perfectly, or at all times) become in sum very powerful evidence for something like an internalized generative grammar. The 'scientific' assumption of certain empirical linguists that most of these kinds of data are irrelevant or unreal, that spontaneous utterances have some special status among performance types, hardly deserves serious consideration. I have discussed at great length, however, the problem of confusing these performances with the underlying abilities we set out to describe: failure to perform may not reflect the absence of an ability. This problem is not unique for questions of language. Psychologists have also pointed out (see in this connection Hochberg (34)) that the elicitation of introspective reports in such a way that the reliability of the interpretation may be evaluated requires varied and sophisticated techniques, convergent procedures, and internal validation. I am arguing in favor of such a

pluralistic approach in linguistics.

The development of a systematic body of data by precise
and sophisticated procedures seems to me to be a central
step in the construction of an explanatory linguistic
theory. The empiricist linguists never succeeded in learn-
ing how to collect usable data from human subjects. The
generative grammarians, on the other hand, have simply not
agreed that there is the need:

> The social and behavioral sciences provide ample
> evidence that objectivity can be pursued with
> little consequent gain in insight and under-
> standing...At a given stage of investigation, one...
> must ask whether or to what extent a wider range and
> more exact description of phenomena is relevant to
> solving the problems that he faces. In linguistics,
> it seems to me that sharpening the data by more ob-
> jective tests is a matter of small importance to the
> problems at hand....many questions that can realistically
> and significantly be formulated today do not demand
> evidence of a kind that is unavailable or unattainable
> without significant improvements in objectivity of
> experimental technique.
>
> (Chomsky, 1965)

Is there a current need for more objective data? I
think there is, because I think that the generative gram-
marians have begun to draw conclusions from the data at
hand, and the theory in hand, that could be justified only
were some extensive further empirical confirmation provided.
I believe that generative grammarians have been too quick
to throw away disconfirmatory evidence simply by pointing

to the performance-competence distinction. After all,
failure to perform may sometimes be an indication of the lack
of an ability, or a limitation on an ability. This is the
other side of the coin.

It seems to me a strategic error to assume that what
is in Chomsky must therefore be in his butcher; and that what
is in the grammar must therefore be in the mind of the user.
Perhaps grammarians leave too much out of account the fact
that there are formal conditions on grammars such as
internal consistency and simplicity that need not be re-
quirements on the user. When individuals seem to differ,
or fail, in displaying competence in apparently linguistic
activities, we cannot categorically conclude that the test
must therefore have been an irrelevant 'operational pro-
cedure', nor that the finding of difference itself is proof
that whatever was tested lies outside the province of lin-
guists (cf. Katz, p.47). Individual difference and limita-
tions on capacity enter the province of linguistics at least
at the point when linguists begin to argue from their formal-
ism to the nature of the human mind. They enter well be-
fore that if questions of style, fluency, and articulate-
ness in language use are linguistic questions.

c. The relevance to linguists of current
empirical studies: Empirical predictions from the theory
of grammar seem to be categorically confirmed when the grosser

aspects of linguistic organization are studied. For example,
Marks and Miller (56) and Miller and Isard (62) have de-
monstrated convincingly that speakers of English perceive
some simple well-formed sentences wholly differently from
simple non-well-formed sentences. Bever and Fodor (21) and
Johnson (39) have shown that people perceive some high-
level phrase-structural relations as well, and there do
not seem to be any significant individual differences in
responses to such tests. When the simple noniterative
transformations are studied (e.g. Savin and Perchonock (70)),
the result again seems to be in accordance with the predictions
of linguistic theory, and free of individual difference.

However, when the response to more complex linguistic
stimuli is examined (as here, and also cf. McNeill (55) and
Blementhal (5)) or when further skills and intuitions
related to grammatical organization are tested (as here,
the ability to paraphrase, and also cf. Maclay and Sleator
(52), the ability to classify sentences), individual dif-
ferences in response-type begin to be found. And just as
this more complex knowledge seems better developed in some
individuals than in others, it seems to be better developed
for certain fragments of the grammar than for others.

Thus while the psychological reality of phrase-structure grammars has been amply attested, the perception of transformational structure -- apart from the simple noniterative sentence-to-sentence transformations -- is not as easy to display.

Is this a technical accident? It is possible that the procedures involved in eliciting confirmatory evidence are here harder to develop. It is also possible that there is something real in the 'smear' or error-variance of these data. A forthcoming article by Martin Braine (8) describes work on language acquisition suggesting that phrase-structural organization of the language ontogenetically precedes transformational organization. Perhaps this re-working and resynthesis of the matter of language occurs in a different degree in different individuals. Perhaps, also, it fails to take place at all for certain kinds of constructions that we, nevertheless, have formal reasons for describing with transformational rules in a grammar.

I imagine most adults have had the experience of suddenly recognizing that some compound they had always con-sidered a simple lexical item had a recognizable substructure never before noticed. Thereafter, is the decrypted item not restructured in perception? It seems plausible that the restructuring from immature phrase-structure representations

of linguistic material (in which, presumably, there are many kinds of 'simple' sentences, i.e., there are many primitive symbols '$\underline{S}$') into consolidating transformational representations takes place only for some of the material, and for some of the people. The hundreds of attested instances of certain postnominal uses of adjectives (e.g., onyx black) may be insufficient to achieve rule [ix] in the mind of the speaker.

Empirical work of the sort cited here at least gives direction to such inquiry, and it reveals details of phenomena that are not easy to come by intuitively or logically. These studies are interesting not because they test or affirm or make more objective generative grammars; they are interesting because they help us assess more realistically what generalizations about linguistic processes and the nature of the mind can be drawn from formal grammars.

2. Innovation and creativity in language use: Generative grammar enables us to describe novelty or creativity in the use of language through the speaker's ability to make use of recursive procedures that are inherent in language structure. Only when we understand these laws or rules of language can we explain the everyday existence of forms like Volume feeding management success formula award, and their apparent comprehensibility. To attempt to explain

the behavior these entities imply without reference to a

systematic grammar of the language is a self-defeating

enterprise, although it has been tried often enough with-

in linguistics and within psychology. As Postal (66) has

put it:

> As if one were to suggest studying the behavior of a
> computer without knowing anything about its program,
> or the activity of chess players without having
> studied the rules of chess, or the performance of a
> symphony without considering the score.

At the same time, if we simply equate the speaker's

behavior, or even his underlying competence or knowledge,

with the rules of language themselves, we erase the dis-

tinction between speaker and system, between language

and speech, as surely as do the linguists and psychologists

who denied the distinction in the first place. Language

is no more a projection of possible perceptual judgments than

it is a projection of possibly obtainable utterances. It is

fairly obvious that few, or no, individuals could on a

single occasion paraphrase:

> the award given for succeeding in finding a formula
> for managing the feeding of people in large volumes

by its compound-nominal equivalent. The compound itself is

the momument, no doubt, of a long entrepreneurial history,

in which the establishment of the journal Volume Feeding

Management was an epochal event. Since the Language, in

this sense, is bigger than any of us, and since none of us, in this sense, is wholly competent to use it, it does little enough harm to recognize that among us there will be differences in the extent of our approach to the abstraction.

We have seen then that the abstract property of recursiveness in language structure explains certain creative activities of the speaker, although the Language itself is not limited by the limitations, whatever they are, on the speaker's creativity at any one moment. Yet there is a further distinction between the 'rules' of an abstraction like Lanugage and the rules of an abstraction like Chess:[1] should my daughter steal my queen or move her king two squares (these are by no means merely hypothetical events), the rules of chess are not thereby affected. Similarly, if the whole second-grade fails an arithmetic test, the structure of arithmetic is left unaltered.

This is not the case for language. If the second-grade persists in saying chess-wise, where we have always said in a chess-like fashion, that decision will eventually rebound upon the language itself.

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[1]I leave aside here the question of 'conscious' vs. 'tacit' rule-following behavior.

The problem of the ways we learn and use what we know
about language thus become an interesting linguistic
question, for it is to a certain extent the question of the
mutability of language structure. If there are aspects of
English syntax which, by virtue of their own internal logic,
or by virtue of their relations to other aspects of the lan-
guage, or by virtue of the demands they make upon the mind
of the user, are imperfectly learned, or learned differently
by those who are less capable than others, this imperfect
knowledge may give rise to a systematic set of utterances
("errors", to us) that are synthesized into new ("correct")
rules by the succeeding generation.

Thus, I think, when we turn to a consideration of how
people know what they know about linguistic structure, and
how they cope with what they do not know (how they deal with
perceived semigrammaticalness), we begin to approach another
source of creativity and innovation, beyond recursivity, in
language use. I believe this source of creativity is close
to what is usually called analogic change. Poets make
analogic extensions when it suits their purposes; so, in
quite another fashion, do those whose lack of capability
may force them to deal with nondeviant sequences as though
they required such extension.

For the case of compound-nouns, we are fortunate in
being able to chronicle the life-cycles of productive

processes that have at least temporarily ceased to operate
in English. For the disused compounding processes leave
morphological fossils in the lexical stock of the language:
we see <u>cut-throat</u> and <u>pick-pocket</u> where we would now
'construct' <u>throat-cutter</u> and <u>pocket-picker</u>. Indeed, what
is productive in compounding seems to change in English
at a relatively quick rate, if we compare these changes to
innovations, let us say, in processes for sentence-negation
or modes of affixing clauses to nouns. Could this be
because of the inherent difficulties in processing or under-
standing compounds? When we consider how our subjects deal
with difficult compounds, we seem to get a hint about their
preferences in analogic extension, whereever they are forced
to make them: thus the sequential pattern of the English
compound is seemingly more often ignored, or ignorable,
for the speaker, than its internal stress relations. This
last, of course, is a hunch based on a small sample of very
artificial data, and nothing really can be made of it: I
offer it only as a suggestion for the line that research
into such questions might profitably follow.

In sum, the Language seems to have an existence of its
own, independent of the abilities and disabilities of those
who bend it to their uses. Its creative quality derives
in part from its recursive properties, but these very pro-
perties tax qualities of the speaker's mind and intention.

It is at this point that new forms are analogically
created, again in a systematic way. Thus perhaps language
may develop and change through constant and strenuous
interaction between its own genius and the speaker's
attempt to cope with it within the structure of his limited
perceptions.

Appendix A

List of Stimuli

1. foot^1 bird^3 house^2
2. house^2 shut^1 bird^3
3. black^1 house^3 bird^2
4. bird^1 stone^3 house^2
5. house^1 wash^3 bird^2
6. bird^1 house^3 boot^2
7. glass^2 house^1 bird^3
8. house^2 bird^1 eat^3
9. house^1 bird^3 kill^2
10. bird^2 house^1 bright^3
11. thin^2 bird^1 house^3
12. bird^2 dry^1 house^3
13. kill^1 house^3 bird^2
14. bird^2 boot^1 house^3
15. house^2 glass^1 bird^3
16. house^2 bird^1 dry^3
17. bird^1 house^3 foot^2
18. house^1 bird^3 stone^2
19. wash^1 bird^3 house^2
20. shut^2 bird^1 house^3
21. eat^2 house^1 bird^3
22. bird^1 bright^3 house^2
23. house^1 black^3 bird^2
24. bird^2 house^1 thin^3
25. house^2 thin^1 bird^3
26. bird^2 eat^1 house^3
27. wash^1 house^3 bird^2
28. shut^2 house^1 bird^3
29. bird^1 house^3 dry^2
30. house^1 bird^3 black^2
31. house^1 foot^3 bird^2
32. bird^2 house^1 stone^3
33. bird^1 kill^3 house^2
34. bright^2 bird^1 house^3
35. house^2 bird^1 glass^3
36. boot^1 bird^3 house^2
37. stone^2 bird^1 house^3
38. dry^1 bird^3 house^2
39. house^2 bird^1 shut^3
40. house^1 bird^3 wash^2
41. foot^1 house^3 bird^2
42. house^1 boot^3 bird^2
43. house^2 bright^1 bird^3
44. bird^1 black^3 house^2
45. bird^2 glass^1 house^3
46. bird^2 house^1 kill^3
47. thin^2 house^1 bird^3
48. bird^1 house^3 eat^2

49.	eat^1	$bird^3$	$house^2$	76.	$bird^2$	$house^1$ $foot^3$
50.	$house^2$	$stone^1$	$bird^3$	77.	$bird^1$	$boot^3$ $house^2$
51.	$boot^1$	$house^3$	$bird^2$	78.	$wash^2$	$bird^1$ $house^3$
52.	$bright^2$	$house^1$	$bird^3$	79.	$house^2$	$black^1$ $bird^3$
53.	$kill^2$	$bird^1$	$house^3$	80.	$kill^2$	$house^1$ $bird^3$
54.	$bird^1$	$wash^3$	$house^2$	81.	$bird^1$	$house^3$ $thin^2$
55.	$bird^2$	$shut^1$	$house^3$	82.	$house^2$	$bird^1$ $stone^3$
56.	$house^1$	$bird^3$	$foot^2$	83.	$house^1$	$glass^3$ $bird^2$
57.	$house^1$	dry^3	$bird^2$	84.	eat^1	$house^3$ $bird^2$
58.	$bird^2$	$house^1$	$black^3$	85.	$house^1$	$shut^3$ $bird^2$
59.	$house^2$	$bird^1$	$thin^3$	86.	$black^2$	$house^1$ $bird^2$
60.	$bird^1$	$house^3$	$glass^2$	87.	$bird^2$	$house^1$ $boot^3$
61.	$bird^2$	$thin^1$	$house^3$	88.	$glass^1$	$house^3$ $bird^2$
62.	$bird^2$	$house^1$	$wash^3$	89.	$house^2$	$wash^1$ $bird^3$
63.	$glass^1$	$bird^3$	$house^2$	90.	$bird^2$	$stone^1$ $house^3$
64.	$stone^2$	$house^1$	$bird^3$	91.	$house^2$	$bird^1$ $kill^3$
65.	$bird^1$	$foot^3$	$house^2$	92.	$bird^1$	$house^3$ $bright^2$
66.	$bird^1$	$house^3$	$shut^2$	93.	$thin^1$	$bird^3$ $house^2$
67.	$house^1$	eat^3	$bird^2$	94.	$house^1$	$bird^3$ eat^2
68.	$black^2$	$bird^1$	$house^3$	95.	$bird^1$	dry^3 $house^2$
69.	$house^1$	$bird^3$	$boot^2$	96.	$foot^2$	$bird^1$ $house^3$
70.	dry^1	$house^3$	$bird^2$	97.	$house^2$	$foot^1$ $bird^3$
71.	$house^2$	$kill^1$	$bird^3$	98.	$bird^1$	eat^3 $house^2$
72.	$house^2$	$bird^1$	$bright^3$	99.	$house^1$	$bird^3$ $glass^2$
73.	$house^1$	$bird^3$	dry^2	100.	$shut^1$	$house^3$ $bird^2$
74.	$shut^1$	$bird^3$	$house^2$	101.	$house^1$	$thin^3$ $bird^2$
75.	$bird^2$	$bright^1$	$house^3$	102.	$boot^2$	$bird^1$ $house^3$

103. wash^2 house^1 bird^3
104. bird^2 house^1 dry^3
105. bird^1 house^3 stone^2
106. bird^2 kill^1 house^3
107. bright^1 bird^3 house^2
108. house^2 bird^1 black^3
109. foot^2 house^1 bird^3
110. house^2 boot^1 bird^3
111. house^1 bird^3 shut^2
112. bird^1 house^3 kill^2
113. thin^1 house^3 bird^2
114. stone^1 bird^3 house^2
115. bird^2 black^1 house^3
116. bird^2 house^1 eat^3
117. dry^2 bird^1 house^3
118. bird^1 glass^3 house^2
119. house^2 bird^1 wash^3
120. house^1 bright^3 bird^2
121. house^1 bird^3 thin^2
122. bird^2 house^1 glass^3
123. house^1 stone^3 bird^2

124. eat^2 bird^1 house^3
125. bird^1 shut^3 house^2
126. boot^2 house^1 bird^3
127. bird^1 house^3 black^2
128. bird^2 wash^1 house^3
129. house^2 bird^1 foot^3
130. kill^1 bird^3 house^2
131. house^2 dry^1 bird^3
132. bright^1 house^3 bird^2
133. bird^1 house^3 wash^2
134. glass^2 bird^1 house^3
135. house^1 kill^3 bird^2
136. house^2 bird^1 boot^3
137. bird^1 thin^3 house^2
138. dry^2 house^1 bird^3
139. house^1 bird^3 bright^2
140. bird^2 house^1 shut^3
141. black^1 bird^3 house^2
142. bird^2 foot^1 house^3
143. house^2 eat^1 bird^3
144. stone^1 house^3 bird^2

Appendix B

Distribution of errors in the generating task, and a
new scoring technique.

Distribution of errors:

1. Rescoring the results: When describing the original
scoring technique (P.90), we mentioned the possibility of
distinguishing errors that invert the elements of the
13 ("primary") compound (substituting bird-house for house-
bird) and errors that invert the elements of the "secondary"
compound (substituting black bird-house for bird-house black).
This latter error seems intuitively less serious than the
former. We now distinguish between a primary order error
(O_p) and a secondary order error (O_s).

There was one further refinement we now tried to make
in the scoring technique. The subjects sometimes gave
sentential responses where relative clauses were called for;
in all these cases, the response was scored as $\underline{E}_g$, a gram-
matical error. But sometimes this error seemed to be trivial --
in cases where it was otherwise a sensible paraphrase, e.g.
a subject might say "The bird-house was black" instead of
"the bird-house that was black". In other cases, the error
seemed more serious because it included, as well, a stress,
order, or chaos error. In this version, we double scored
sentential responses, so that we could separate those that

were otherwise correct from those that included a further error.

Table I shows the distribution of error scores in genera-
tion using the old and the new techniques. It will be recall-
ed that the population used in Experiment 1 (where we employ
the original scoring technique) differs in part from that used
in Experiment 2 (where we employ the refined scoring technique).
It can be seen immediately that the results are very similar
despite changes in the population and the scoring. The dif-
ference in distribution of error-types among the groups is
slightly enhanced by the new scoring technique, for O_p dis-
tributes itself fairly evenly among the three population-
groups, thus increasing the differences in the proportion of
O_s errors among groups. Since grammar errors (E_g) very gen-
erally incorporated some further internal error, (generally
an <u>order</u> or <u>chaos</u> error), the double scoring of results does
not significantly decrease the absolute number of errors.[1]

Table II shows the distribution of error-type by lexical
class for the populations of Experiment 2, using the new
scoring. A comparison of this table with the bargraph of
Figure 2 shows that these relations remain very much the
same.

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[1]Inspection of Table I does show a significant drop in the
proportion of grammar errors for the A-group. The two A-
group subjects who contributed these errors were not available
for the second experiment, and no new A-group members made
more than one or two such errors.

Table I

| | Group A | | Group B | | Group C | |
| | Scoring | | Scoring | | Scoring | |
	old	new	old	new	old	new
Secondary Order		52.94		31.83		23.95
Primary Order		12.74		13.90		15.35
Order (combined):	56.6	65.68	38.1	45.73	26.6	39.30
Stress	24.8	23.52	31.0	31.39	34.0	30.07
Chaos	6.2	10.78	23.8	22.86	23.1	30.61
Grammar	12.4	3.92	7.1	4.03	16.3	17.56

Effects on error-type distribution of rescoring and some change in population (percentages sum to more than 100 where E_g is double-scored).

Table II

Lexical Class	O_s	O_p	S	Chaos	Grammar
N	24.88	11.96	38.75	25.40	6.2
A	31.26	17.63	28.25	22.84	4.0
V	32.37	10.79	18.28	38.54	28.85
AV	30.44	13.68	28.49	27.37	14.80
NA	21.11	18.51	41.48	18.88	2.22

Distribution of error-type for each lexical class using the new scoring and new population (percentages sum to more than 100 where E_g is double-scored).

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