FEASIBILITY

This paper discusses the feasibility of applying a model of language use based on a modification and extension (to be discussed below) of the generative semantic (transformational) theory of language competence recently developed by Paul Postal, George Lakoff, John Robert Ross, James D. McCawley, and others, to problems of computational linguistics.

The theory of generative semantics, to be discussed in section II, is an outgrowth of, and reaction to, Chomsky's 1965 theory of transformational linguistics. It is a radical theory which deals with a very great range of problems with very abstract methods. Those working in this paradigm hold that there is a linguistic level reflecting conceptual or semantic structure which is directly convertible into surface syntax by a single set of garden-variety transformations, with no significant intermediary level, that is, no "deep structure". Those of us working in generative semantics believe that methods substantially those long familiar in linguistics can achieve very absract, very general results which treat semantics in a more serious and enlightening way than ever before. I do not, I think, support this very strong claim very well in section II, but I provide summaries of several studies and a lengthy bibliogrpahy of works which when consulted will hopefully give some feeling for what is being attempted, I think not without results.

But generative semantics is a model, or rather, a theory, of competence, like most serious theories of language now held to by American linguists. Even if, as might be claimed, our semantic structures are to be merely variants of the structures long familiar from formal logic, so that if our assumptions are correct, we will ultimately be able to directly transform surface structures into underlying semantic structures, the majority of actual sentences, as well as all hyper-sentential structures, the treatment of which has been swept under the rug of "performance", will remain unhandle-able.

Accordingly, I propose initially certain extensions and modifications of the theory to make it in some sense

a model of performance. But if we are to apply it to the
computer, a major component must still be added. The impe-
tus to this application is the possibility of creating
an understanding machine, described in section IV below.
Since the actual human interpretation of language depends
on past knowledge (consider which of these sentences is
good and why:

As for Albuquerque, the Eiffel Tower is pretty.
As for Paris, the Eiffel Tower is pretty.

And these:

Shirley is a blonde and Susan is Nordic-looking too.
Shirley is a linguist and Susan is Nordic-lloking too.)

the   old split between semantics, syntax, and pragmatics
must be revised, and   our model closely linked with a
memory and possibly a logic component as well.

Obviously this defines a very difficult task, but insofar
as such goals as MT, artificial intelligence, and machine
reading of handwritten material or writing of spoken
material involve comprehension on the part of the machine,
of which there seems to be no doubt,   these important
goals will continue to elude us until such time as we
can devise such an understanding machine as I have described
below.

I   believe that generative semantics lays the
foundation for studies relevant to such a development,
and it is in this context that my proposals are made.

* * *

In section II I will discuss generative semantics.
In section III I will discuss the body of my proposals
here.
In section IV I will discuss what should be required
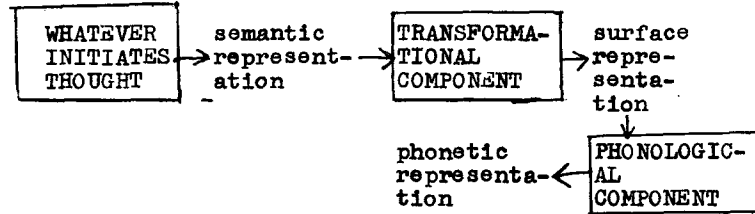of a generalized "understanding" machine.

Part II. The theory of
Generative Semantics.

The theory of generative semantics is an out-growth and reaction to the theory of transformational grammar as represented in Chomsky's 1965 book, Aspects of the Theory of Syntax (MIT Press). To a very large extent, this theory has been the development of a small group of former students of Chomsky's or their close colleagues. John (Haj) Ross has said that the theory is really just an attempt to explicate Paul Postal's work of five years ago to date. If Postal was the founder of this school, if you can call it that, its main workers have been Haj Ross and George Lakoff, who between 1965 and 1968 swept aside most of transformational linguistics as it then was. But perhaps best known of the group is James McCawley, who graduated from MIT in 1965 with a Ph.D. based on work in phonology, not syntax or semantics. He promptly amazed Lakoff and Ross by some very substantive work in the latter areas as well as phonology. As a student of McCawley's I will be emphasizing his contributions here, and those of my colleagues at Chicago, Jerry L. Morgan and Georgia M. Green, but it should be kept in mind that people like Ross, Lakoff, Postal, Arnold Zwicky, David Perlmutter, Emmon Bach, Robin Lakoff, and several others, have made the current theory possible, and that many others, such as Robert Wall, Lauri Kartunnen, Ronald Langacker, and others, have contributed as well. It should also be kept in mind that the Case Grammar of Fillmore and the work done by Gruber, while differing from generative semantics, have contributed a great deal to it.

The basic theory of generative semantics is built upon an attempt to relate the underlying semantic structure of language to the surface, phonetic manifestation of that underlying structure. That is, a phonetic reality is recognized, and a semantic reality is recognized. But unlike other versions of transformational grammar, this theory assigns no special status to syntax; syntax is subsumed in the semantics. McCawley has jokingly referred to his theory as being one of either "semantax" or "synantics". The name generative semantics is not a particularly good one, since it implies that the goal of the theory is, as with the work of Chomsky, to "separate the grammatical sequences" of a language "from the ungrammatical sequences." (Chomsky, Syntactic Structures, 13.) In

other words, to generate all and only grammatical sent-
ences of a language. This is not at all the goal of
generative semantics. Rather, what we want to do is
in some rigorous way specify the correlations of under-
lying semantic entities and surface phonetic entities:
to specify for any underlying semantic structure what
its possible phonetic realizations in some language are,
and for some phonetic structure what underlying semantic
structures it can represent. Naturally, some descript-
ive ability is predicated as well, that is, we want to
be able to define ambiguity in some algorithmic fashion,
we want to be   able to define levels or classes of
ill-correlation between structures on different levels,
etc. Chomsky would say that a sentenceslike "Golf plays
John" is eminently deserving of a star; we would say
(1) if it's supposed to mean'John plays golf', it doesn't
succeed in conveying the message; (2) if it's supposed
to mean 'John loves Marsha', then it's really bad; and
(3) if Golf is a man's name and John the name of a
game or role, it's a good sentence --- indeed, one
can very well imagine arcane circumstances under which
one might utter that sentence with the intent of
saying that the game plays John, that the tail wags the
dog, as it were. Suppose, for example, that John's
wife were tired of him spending all his free time
playing golf and she grumbled to a neighbor about
it, and the neighbor rather unfeelingly replied,
"Oh well, John plays golf." I can very well imagine
John's wife complaining bitterly,"Oh no, golf plays
John." In any case, it is for hus unimaginative
approach to language that Chomsky has been jokingly
called a "bourgeois formalist". Even when we use
stars, we try to keep in mind that just about
any valid phonological string of a language conveys
one or more meanings in some context, and that it is
artificial to take a string out of context and declare
it good or bad. So "generative semantics" is a bad
name.

The following diagram of the components of the
theory is based on McCawley's paper in the proceedings
of the 4th Regional Meeting of the Chicago Linguistic
Society (1968). A theory very similar is discussed
in Ronald Langacker's book Language and its Structure
(Harbrace, 1968), pp. 114-34.

WHATEVER INITIATES THOUGHT	semantic represent-ation	TRANSFORMA-TIONAL COMPONENT	surface repre-senta-tion

phonetic representa-tion ← PHONOLOGIC-AL COMPONENT

The above diagram comes from a report prepared by myself, Jerry Morgan, and Georgia Green, called the Camelot Report, which attempted to describe the current state of transformational research in the Summer of 1968, particularly in reference to the LSA Summer Linguistic Institute at the University of Illinois, where Haj Ross, George Lakoff, and Jim McCawley had lectured to large groups on a huge number of very "hairy" (i.e., difficult and tickleishly novel) topics.

In that report (which was prepared for Victor Yngve), we raised several questions concerning the above representation. We asked:

1. What will an adequate semantic representation have to include? What form will it have?

2. What can a transformation do? What does one look like?

3. At what stage and in what manner are semantic representations converted into words of real languages?

These were by no means all of the questions asked. Needless to say, the answering of these questions has hardly begun and will undoubtedly guarantee linguists a few good centuries of work at least. It is only in the last decade that syntax has been the subject of serious work, and we are still only discovering how ignorant we are. Semantics is even newer, less than a decade old. If anyone doubts that this is true, consider a) what the above 3 questions would have meant to a linguist in (say) 1955, and b)why he would have been wrong, in his (lack of) comprehension of them. One of the great contributions of Postal and Ross has been

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their constant critical look at transformational grammar. One of the things they saw was that our transformations were (and are) extremely powerful devices, with practically no constraints placed on their formulation.

What I will do here is summarize some of the attempts at partial answers to the three above questions. In this way I can delimit and explicate generative semantics best.

I will start by abstracting parts of two papers by McCawley that deal with the nature of semantic representation. In a paper in the Japanese journal Kotoba no Uchu (World of Language) in 1967, McCawley argued that semantic representation would be similar to syntactic representation as familiar from Aspects-type grammar, but that it would also be quite similar to symbolic logic as familiar from the tons of work that have followed Principia and such studies. That semantic representation should resemble syntactic representation makes sense if only because we are arguing for a single set of rules that transforms (i.e., relates) the underlying structure into (to) the surface structures. There will be more about that later.

McCawley argues as follows: the following devices have all had a role in symbolic logic:

1. propositional connectives: 'and', 'or', 'not'.

2. constants denoting individuals.

3. predicates, denoting properties and relationships.

4. set symbols and the quantifiers 'all' and 'there exists'.

5. descriptions of sets and individuals.

&x The following devices play a role in natural languages:

1. all lgs. have words for 'and', 'or', and 'not'. (he notes however that these words in natural lgs. may connect more than sentences)

2. "indices" denoting individuals; John loves John might be represented as x_1 loves x_2, but John loves

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<u>himself</u> is x_1 loves x_1.

3. predicates are expressed in natural lgs. (by verbs, adjectives, nouns, etc.)

4. "Words such as <u>all</u> and _∧<u>at least one</u> are two members   expressioms such as
of a rather large class of expressions which are
used to indicate not only the existence of an
individual or a set but the absolute or relative
number of members in that set."

5. sets and individuals can be expressed as descriptions
using modified noun phrases.

McCawley then gives further reasons for supposing symbolic
logic representation to be proper for semantic representa-
tion. (See the bibliography to this section where this
and other papers that can be consulted for these arguments
in detail are listed.)

In a paper prepared for the symposium on "Cognitive
Studies and Artificial Intelligence Research" held by the
Wenner-Gren Foundation at the University of Chicago in
March of this year, McCawley discussed semantic representa-
tion at length. Some of what he had to say there should
be noted. He claimed, "semantic representation must indicate
the immediate constituent structure of the elements involved
in it (i.e. examples showing that different meanings can
comsist of the same semantic elements combined in different
ways [are easy to come by])" (p.1) He gave the example of

John doesn't beat his wife because he loves her.

If the negation applies to <u>John beats his wife</u>, the
sentence means 'the reason that John doesn't beat his wife
is that he loves her', whereas if it applies to the
<u>John beats his wife because he loves her</u>., the mg. is 'the
reason that John beats his wife is not that he loves her.'
Notice that here a surface form represents at least two
different underlying structures which nonetheless contain
precisely the same semantic elements-- grouped differently,
however.

Another point made is that "semantic representations
must include ..., some indication of presupposed coref-
erence." (p.2) That is, the following sentence in neutral
(i.e. null) context is ambiguous three-ways:

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John told Harry that his wife was pretty.

Whose wife? John's? Harry's? or a third's? It could be
any. However, if we know who his refers to, there
is no such ambiguity. This may seem trivial, but
it is a point often ignored.

McCawley then gives an argument for referential
indices being different from expressions used to describe.
The sentence

Max denied that he kissed the girl he kissed.

is not contradictory if "the girl he kissed" is the
speaker's description.

Another notion is that of presupposed set members
ship. In

Max is more intelligent than most Americans.

said with primary stress on most, the sentence is good
if and only if Max is presupposed to be American, that
is, the sentence implies Max is American. With primary
stress on Americans, however, Max is presupposed not
to be American. Presupposition is in general a very
hairy topic which was recently the subject of an entire
conference (at the Ohio State University). We know
very little about the nuamces of implication and
are only beginning even to identify the problems. But
if a machine is ever to read Catcher in the Rye catching
all the nuances of the italicized words, we had better
find out how stress is used to alter the presupposition-
al set of a sentence. I need not be so unsubtle as to
suggest the extreme value of such researches to psycholo-
gy. Perhaps they already know about all this, for all
I know. In any case I cannot restrain myself from
including McCawley's beautiful example

CIA Agents are more stupid than most
    Americans.

He had primary stress on the most, but I prefer to think
of it as going on the Americans.

I would like to interject at this point a minor
apology. I have been rather fan-clubish here and
have waved my hand a lot. Frankly I see no value

in rehearsing here all the arguments available elsewhere.
But I would like the reader to bear in mind my skimpy
resume in no way reflects the quality of the original.
Let me also note, lest I seem unduly credulous towards
the thoughts of Chairman Quang [mild-mannered linguist
J. D. McCawley is in reality Q. P. Dong, Chairman of
Unamerican Studies at an unknown university], that most
of us working within the paradigm of generative
semantics would be the first to admit that our theories
haven't a prayer of being right, that is, they hardly
approach even a partially realistic and naturalistic
theory of language. If we like it better than other
paradigms it is because we   believe that no
other current theory is any better and that this one
at least has a good chance of self-improvement. (End
of apologia.)

        If semantic representation looks much like
logical representation, it also differs from it.
In the Kotoba no Uchu paper McCawley noted the follow-
ing differences:

1. "It is necessary to admit predicates which assert
   properties not only of individuals but also of
   sets and propositions."

2. "In mathematics one enumerates certain objects
   which [one] will talk about, defines other objects
   in terms of these objects, and confines [oneself]
   to a discussion of objects which [one] has either
   postulated or defined.... However, one does not
   begin a conversation by giving a list of postu-
   lates and definitions. ....people often
   talk about things which either do not exist or
   which they have identified incorrectly. Indices
   exist in the minds of the speaker rather than
   in the real world; they are conceptual entities
   which the individual speaker creates in interpret-
   ing his experience."

In the Wenner-Gren symposium, McCawley had more to say about
the difference between logic and language.

1. Immediate constituent structure (trees) rather than
   parentheses are basic. First, "semantic representations
   are to form the input to a system of transformations
   that relate meaning to superficial form; to the

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extent that these transformations have been formulated
and justified, they appear to be stateable only in
terms of constituent structure and constituent type,
rather than in terms of configurations of parentheses
   and terminal symbols." Secondly, "it may be necessa-
ry to operate in terms of semantic representations in
which symbols have no left-to-right ordering...."

2. There will have to be more 'logical operators', such as
   most, almost all, and many.

3. "And and ... or ... cannot be regarded as just binary
   operators but must be allowed to take an arbitrary
   number of operands."

4. The quantifiers must be restricted rather than
   unrestricted as in most logical systems. Some
   quantifiers imply existence: All dogs like to
   bite postmen. involves the presupposition that
   dogs exist, whereas the unrestricted quantifiers
   logicians use have no such presupposition.

5. "Adequate semantic representation of sentences
   involving 'shifters' (Jakobson, 1957) such as
   I, you, here, now, ..., gestures and deictic
   words like this and that, and tenses, will have
   to include reference to the speech act. The most
   promising approach to this aspect of semantic
   representation ... is Ross's (1969) elaboration
   of Austin's (1962) notion of 'performative verb'."
   (See now too Searle's book, Speech Acts, CUP, 1969
   ---RIB)

6. "The range of indices will have to be enormous.
   In particular, it will have to include not only
   indices that purport to refer to physical objects,
   but also indices corresponding to mythical or
   literary objects, so that one can represent the
   meaning of sentences such as

        The Trobriand Islanders believe in Santa
        Claus, but they call him Ubu Ubu."

7. McC. rejects "the traditional distinction between
   'predicate' and 'logical operator' and treat[s]
   such 'logical operators' as quantifiers, conjunctions,
   and negation as predicates...."

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To clarify the relationship of semantic to syntactic representations let me quote here from McCawley's Kotoba no Uchu paper:

Since the rules for combining items into larger units in symbolic logic formulas must be stated in terms of categories such as 'preposition', 'predicate', and 'index', these categories can be regarded as labels on the nodes of these trees. And since ... these categories all appear to correspond to syntactic categories, the same symbols (S, V, NP, etc.) may be used as node labels in semantic representations as are used in syntactic representations. Accordingly, semantic representations appear to be extremely close in formal nature to syntactic representations, so close in fact that it becomes possible to catalogue the conceivable formal differences and determine whether those differences are real or apparent.

Among such differences he lists:

1. "The items in a syntactic representation must be assigned a linear order, whereas it is not obvious that linear ordering of items in a semantic representation makes any sense."

2. "Syntactic representations involve lexical items from the language as their terminal nodes, whereas the terminal nodes in a semantic representation are semantic units rather than lexical units."

3. "There are many syntactic categories which appear to play no role in semantic representation, for ex., verb-phrase, preposition, and prepositional phrase." (At the 5th Regional Meeting of the CLS, April of this year, A. L. Becker of the University of Michigan presented a paper in which he argued prepositions are underlying predicates; prepositional phrases are accordingly verb-phrases.)

McCawley concluded nonetheless that these differences do not provide an argument that semantic representations are different in formal nature from syntactic representations. Again, I will omit his reasons for that conclusion.

I might summarize all this by saying:
1. Semantic representation is a modification of the representations long familiar from formal logic.

2. Such representations do not radically differ from
the surface syntactic representations of Aspects-
type grammar.

Let me close by posing more problems. McCawley asks
the following questions at the end of his Kotoba no
Uchu paper. While they do not specifically relate
to semantic structure, I include them to give some idea
of what we believe to be the sort of questions that
a serious theory of language should provide justifiable
answers for:

1. How do the mgs. of words change as a language evolves?

2. How does a child learn mgs. in learning to speak his
native language?

3. What mechanisms are involved in phenomena such as
metaphor....? (Dorothy Lambert has written a
Ph.D. thesis at Michigan on the subject of
metaphor within the paradigm of Case Grammar.
This 500 page dissertation is probably one of
the best studies of the subject to date from a
linguistic point of view.)--RIB)

4. To what extent are the units of semantic representationq
universal?

5. To what extent does the lexicon of a language have
a structure?

6. Can all languages express the same ideas?

7. To what extent doe's one's language affect his thinking?

8. To what extent is one's ability to learn lexical items
conditioned by his knowledge of the world?

I will now turn to the second question raised
above on p. II-3. This question has as yet received
little study. It is a very difficult topic, but a
very important one. I will confine myself here to a
few brief comments and a few references.

One of the important studies underway now is about syntactic variables. This was the subject of Ross' 1967 dissertation. Variables such as X and Y are familiar from transformational grammars, but no one had attempted before to specify in general what the notion of syntactic variable entailed. While Ross' study was important, and he came up with several important constraints on the form of transformations, much work remains. Lakoff and Postal are also working on related questions. Let me list here some of the constraints Ross gave in his thesis:

1) The complex NP constraint.
    No element contained in a sentence dominated
    by a noun phrase withxa lexical head noun
    may be moved out of that noun phrase by a
    transformation. (p, 127)

2) The cross-over condition.
    No NP mentioned in the structural index of
    a transformation may be reordered by that
    rule in such a way as to cross over a
    coreferential NP. (p. 132)

3)The coordinate structure constraint.
    In a coordinate structure, no conjunct may be
    moved, nor may any element contained in a
    conjunct be moved out of that conjunct.
    (p. 161)

4) The pied piping convention.
    Any transformation which is stated in such a way
    as to effect the reordering of some specified
    node NP, where this node is preceded and followed
    by variables in the structural index of the
    rule, may apply to this NP or to any non-
    coordinate NP which dominates it, as long as
    there are no occurences of any coordinate node,
    nor of the node S, on the branch connecting
    the higher node and the specified node.

(That is,:

        ...any NP above some specified one may be reorder-
        ed, instead of the specified one, but there are
        environments where the lower NP may not be moved,
        and only some higher one can, consonant with the
        conditions imposed /In the convention./) (p.206)

5) The sentential subject constraint.
    No element dominated by an S may be moved
    out of that S if that node S is dominated
    by an NP which itself is immediately
    dominated by S. (p. 243)

6) The frozen structure constraint.
    If a clause has been, extraposed from a noun
    phrase whose head noun is lexical, this noun
    phrase may not be moved, nor may any element
    of the clause be moved out of that clause.
    (p. 295)

7) Definition of identity.
    Constituents are identical if they have the
    same constituent structure and are identical
    morpheme-for-morpheme, or if they differ only
    as to pronouns, where the pronouns in each of
    the identical constituents are commanded by
    antecedents in the non-identical portions
    of the phrase-marker. (p.348)

A very important constraint occurs on p. 480 of the
thesis, but I omit it here because it contains many
terms I would not care to define here. I reccomend
Ross' dissertation for anyone with doubts about
any deep principles of language organization emerging
from our studies in transformational grammar. He will
be cured.

      Recently George Lakoff has studied the notion of
"derivational constraint". This study is quite recent
and still very very hairy, but hints in his 1969 CLS
paper, and comments by Postal on it suggest that rule
odering is merely a special case or manifestation
of a deeper principle of grammar organization. The
next revolution effected by generative semantics may
well be to drop rule ordering from our canons.

      For various reasons (partly that it interests
me more) I will have much more   to say here about
lexical insertion than I will about constraints on
transformations, although undoubtedly the   latter
is ultimately of much greater importance.

      Until 1965 or so, it was assumed that the terminal
symbols of a P-marker are lexical items; the lexicon merely
assigns properties to these items. Gruber in his 1965

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dissertation argued that certain transformations had to occur before lexical items entered trees: that is, that there were pre-lexical transformations.

Before Gruber, the system of semantics was one in which T-rules generated from deep structures surface structures and P-rules generated semantic representations for those deep structures. This was the theory of intepretive semantics (as in Katz and Postal, for ex.) Gruber proposed a derivational semantics. Gruber intended to "show various consistently recurrent semantic relationships among parts of the sentence and among different sentences, which can best be explained by the existence of some underlying pattern of which the syntactic structure is a particular manifestation." (p.1) He concluded that "a level at which semantic interpretation will be relevant will   ... be deeper than the level of 'deep structure' in syntax." (p.2) Later Lakoff showed evidence that in fact the level of semantic interpretation was that of deep structure, but argued that (as Gruber said) "syntax and semantics will have the same representation at the prelexical level"(p. 3): a single set of rules would transform semantic structures containing no lexical items into surface syntactic representations containing them.

The study of lexical insertion, the process by which the underlying semantic elements are grouped into units replaceable by surface lexical items has led to a large literature containing a great many questions, and some positive answers. An important paper was McCawley's 1968 paper, "Lexical insertion in a transformational grammar without deep structure."

There he started by assuming various points concluded in other papers of his. He very clearly presents some of the tehets of generative semantics, so with some repetition from above I quote these points here:

1. Syntactic and semantic representations are of the same formal nature....

2. There is a single system of rules ... which relates semantic representation to surface structure through intermediate stages.   ɪ

3. In the earlier stages of the conversion from semantic

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representation to surface structure, terminal nodes
may have for labels 'referential indices' such as
were introduced in Chomsky 1965.... In semantic
representation, only indices and 'predicates' are
terminal node labels....

McCawley then defined 'dictionary entry' as a
transformation which replaced part of a tree by a
surface lexical item. He expressed doubt these rules
could be ordered internally or external, since it
would hardly be possible, for example, that some question
would arise as to the relative ordering of the transforma-
tion introducing the word horse and that extraposing NP's
in two dialects, that is, the ordering could not possibly
matter.

He then raised several possibilities as to the re-
lative ordering of the lexical rules vis-a-vis other
rules. Are the lexical rules last,   first, or where?
McCawley argued for the lexical rules applying just
before the post-cyclic rules, and adduced evidence for
several rules, predicate-raising, equi-NP deletion, etc.,
being pre-lexical.

In his 1968 LSA paper, Jerry L. Morgan of the
University of Chicago added to this. He pointed out
"the rather strong assumption that lexical items only
'replace' constituents." (p.3) He wrote, "the process
of syntactic derivation begins with semantic representa-
tion in terms of trees containing very highly abstract
semantic terms, operating upon this by means of rules
permuting, deleting, and collapsing parts of the representa-
tion, finally deriving a structure whose constituents are
replaced by lexical items." (p. 3) He then stated a
very strong   claim of the theory:

> Given the set of universal pre-lexical
> rules, the set of universal semantic
> primitives, and the set of universal
> constraints on the operation of rules,
> such as those described by Ross 1967,
> these define the universal set of possible
> lexical items in their semantic aspect;
> that is, they rule out as impossible an
> infinitix classof a priori possible
> "meanings" a lexical item could have. (p.4)

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A second very strong claim of the theory is:

> Insofar as the selection from, and details
> of implementation of, the universal set of
> rules is language-specific, the idiosyncracies
> of a given language in this respect will also
> be reflected by systematic gaps in the lexicon.
> The same is true for the set of semantic
> primitives and the set of constraints on rules.
> .... (p.4-5)

Morgan came up with some restrictions on lexical items:

only
1) "lexical items ×an replace a constituent which
   is not labelled S." (p.6)

2) "verbs cannot incorporate referential indices."(p.6)

One further   point to be made is that lexical items
can only replace well-formed subtrees.

My own work has been concerned with specifying
classes of possible lexical items and accounting for
the syntactic properties of verbs in terms of their
semantics, thereby attempting to capture the intuition
long familiar from traditional grammar that certain
semantic classes of verbs, such as "verbs of giving
and taking" or "verbs of motion" also form syntactic classes
and hence their syntactic properties can be regarded as
derived from their semantics.

Georgia Green of the University of Chicago has
presented a paper (1969) which is also interesting
in terms of lexical insertion. She tends to regard
lexical insertion as fairly divorced from
morphology, and views lexical insertion as the replace-
ment of an entire sub-tree by a surface lexical item
which may contain more than one morpheme as classically
defined. This position is somewhat different from my
own, as I regard lexical insertion as primarily involving
the replacement of items on a 1-1 basis. However, this
is an empirical question and only future research will
decide which of us is more nearly correct.

So far I have discussed lexical insertion in
terms of sweeping, general principles of the organization
of the grammar. In order to more clearly specify what

lexical insertion is all about, I ought to present
some of the kinds of problems which have generated
all of this interest in the subject.

At the Texas Conference on Universals in 1967, the proceedings of which were
published in 1968 as Universals in Linguistic Theory, McCawley raised the
Universals in Linguistic Theory, McCawley raised the
question of dictionary organization anew. He opted for
a "Weinreichian" lexicon in which lexical items were
combinations of semantic, syntactic, and phonological
information. McCawley supported this with this evidence:
the reason John is sadder than that book. is bad is that
the two sads in the underlying structure of the sentence
are different lexical items. They therefore cannot
participate in comparison:

> *John is as sad as that book he read yesterday.
> *He exploits his employees more than the oppur-
> tunity to please.
> *Is Brazil as independent as the continuum
> hypothesis? (exx. of Chomsky's.)

McCawley called for a theory of "implicational relations",
since in cases such as the ambiguity of warm the ambiguity
is not a property of the item itself but of a class of
items, and therefore such an ambiguity must be specified
in terms of general principles. McCawley was not clear
about the nature of these implicational relations, so
that the nature of the relationship of the various sads
was more or less left open. I have discussed the notion
of systematic ambiguity, where the ambiguities of an entire
class of verbs is specified in terms of the derivational
process underlying them all, not just   in terms
of a descriptive statement. Thus we are seeking to
explain lexical gaps in terms of statements such as

> "The reason some language L lacks a verb V
> glossing the verb W in the language M is that
> M, but not L, has the transformation T."

Anyone familiar with the lexicons of French, English,
and German, for example, knows that there are certain kinds
of verb which are not typical of one or another of these
languages which nonetheless readily occur in the others.
Such verbs are derived by processes occuring in one but
not another language, and our task is to discover and
describe such processes. Thus we may ultimately be able
to tell how the class of French verbs, say, differs from

the class of all possible verbs.

I have attempted in these few pages to present
a digest of some works in the paradigm of generative
semantics. I have not really attempted to provide
even an elementary guide to the methods of generative
semantics or to its conclusions, its findings, but I
hope I have explicated somewhat its goals and given
some insight into the direction in which it is moving.
Some very strong claims are forthcoming on the nature of
grammars and languages and hence of language itself. A
tremendous amount of work needs to be done, but one can
see clearly that one possible end point of this work will
be a very comprehensive, very strong theory of language
competence that has a great deal to say about human
beings.

One perhaps minor point, though, looms up large
here: generative semantics relates semantic structures
to surface sentences by a single set of rules. There
are several versions of transformational grammar that do
this, but generative semantics is perhaps the most-develop-
ed of these. But as the saying goes, what goes up must
come down: we may paraphrase this as: what can be generated,
can be analyzed. The theory permits, ideally, an
algorithmic translation of a surface string into one or
more underlying semantic structures. For computational
linguistics, that may be its most appealing feature.

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EXTENSION AND MODIFICATION OF
THE THEORY

Paul Postal, in a 1964 paper, "Underlying and super-
ficial linguistic structure", seemed to rule out any
principled approach to the study of performance. But it
seems clear to me that performance has merely been a
catch-all term used by linguists with a lot of nasty
facts on their hands they had no way of handling. In
section II I mentioned the treatment of semi-grammatical
sentences as they used to be called. Now I think we
should be able to treat so-called sentence fragments
as being part of language proper. I see no reason,
once we get over our hang-ups with sharp categorization
of grammaticality and judgments of grammaticality in
null context, why we cannot have a principled treatment
of sentence fragments.

Another area usually relegated to Never-never land
is that of the structure of discourse. Obviously the
sentence pairs

Harry is a fool. He voted for Richard Nixon.
Ha voted for Richard Nixon. Harry is a vote.

are not equivalent. Imagine if we take every other sentence
on a page, say, the beginning of Matthew 2. The result is
hardly a well-formed discourse.

Now the birth of Jesus came about in this way.
But her husband, Joseph, was an upright man
and did not wish to disgrace her, and he decided
to break off the engagement privately. "Joseph,
descendent of David, do not fear to take Mary,
your wife, to your home, for it is through the
influence of the holy Spirit that she is to be-
come a mother." All this happened in fulfill-
ment of what the Lord said through the prophet....
But he did not live with her as a husband until
she had had a son, and he named the child Jesus.
"Where is the newly born king of the Jews?"

To now, it has generally been held that the structure of
discourse is linear, that is, sentences are strung to-
gether one after the other and well-formedness is based
on how well these sentences string. But the context
is vital to the form of a sentence. Similarly, whether

two clauses are united or put into separate sentences de-
pends on context: by context we cannot mean merely the
two sentences on either side of the sentence in question,
nor can we mean the n sentences to either side. This is
quite as mad as the folly of the early 50's that syntax
was a matter of which words had what probability of
occuring n words to either side of a given word. What we
need is a grammar, a generative grammar, a transforma-
tional grammar, of discourse, based on the same methods
that have been developed in syntax over the last decade.
This work was pioneered by George Lakoff's 1964 study
of Russian folk-tales, in which he revised Propp's
phrase structure grammar of the "morphology" of Russian
folk-tales. I subsequently re-modified Lakoff's work
and programmed it in COMIT for a 7090-7094 machine to
generate plot outlines of Russian folktales. The results
were partly abominable and partly amusing, but the point is
that while hardly any discourse is as   stereotyped as
Russian folktales or US patents, that certain structures
nonetheless occur which are larger than the sentence. The
notions of subordination and coordination of sentences and
even whole discourses are quite valid and quite amenable
to investigation.

A third class of problems concern logic. The implica-
tions of a sentence may be quite as important as the state-
ments made by it. We linguists are only beginning to
investigate presupposition, implication, insinuation,
assertion, etc., but philosophers have been aware of these
problems for a long time and a large literature exists.
We want a machine to get as much information out of a
sentence as a human would.

A fourth class of problems concern memory. Any program
must involve knowledge. Humans do not use language in
vacuo. Suppose I know that Sherlock Holmes is a tall,
thin man. Suppose further that a fat, short man comes
up to me and tells me he is Sherlock Holmes. If my
memory and logic components are going full blast I
immediately suggest to the gentleman that a)he is
either lying, or b)could use a good psychiatrist,
or c)he has a bad sense of humor. W_e would not
like the computer to read a sarcastic sentence, such as
"Surely they have a right to do unto others what they
would not want others to do unto them" and file it away
neatly. We need to give the computer a certain amount
of linguistic sophistocation as far as irony, insinuation,

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and such go. This might seem overly optimistic, since
most human beings lack this ability, but let me suggest
that the goal of computational linguistics is to understand
human capabilities, not reproduce them, something which
can be done far cheaper by producing new human beings
thru natural means than producing software in our labs.
The only thing keeping us from programming computers
to, for example, have a sense of human, is our peculiar
delusion that we can't do it.

So these are the problems that have not been the
subject of serious research. Note that I do not mean
by this that no one has ever looked at them and
found anything out. Even Newton was Platonian enough
to realize that nothing new is ever discovered under
the sun. But no linguist operating in terms of a
formalized or quasi-formalized system has studied these
problems very much. This is not to s$_a$y that certain
conclusions about the future construction of a theory of
language use cannot be drawn from our present ignorance.
T$_h$e rest of this section will be devoted to how we
with our Neanderthalic knowledge of language can outline
a decent formal theory of 'la parole', something that
         we would want to do, I think, even had the computer
never been invented. (End of sermon.)

One question which arises here is what the nature of
underlying semantic structures is. Do people think in
trees? McCawley in his article on the base rejected the
notion of derivation. Instead he instituted a system of
"node-admissibility conditions". These are actually
conditions on the well-formedness of trees. Any object
meeting these requirements is a w ll-formed tree, other-
wise it is not (although I have yet to settle in
my own mind whether an ill-formed tree is still a tree,
just as I have been confused about whether an ill-formed
sentence of English is still a sentence of English at
all.) Each NAC has the form

$$< A; \ B \ C >$$

which is read, "a node A is admissible if   it immediate-
ly and exclusively dominates a node labelled B and a
node labelled C." NAC's generate trees directly, as opposed
to rewriting rules which, in Chomsky's system, first
go through a derivation, from which trees are then construct-
ed. But the important point here is "Grammars are written
by fools like me, but only God can make a tree": meaning

that linguists need not concern themselves with the origin
of trees to discover their properties.

Of course, if we are to be manipulating semantic struct-
ures, we are going to have to be concerned with where
trees come from. A more basic question is whether the
kinds of trees generative semantics claims to be semantic
are reasonable semantic structures, that is, whether the
investigator in artificial intelligence, for example,
could live with them. I think there is a very good
chance that this is the case. The basic elements of these
trees are as follows. We have referential indices referencing
individuals. I think that in any system we will need a
device such as this. Both these indices and larger entities
called senetences or S's can be dominated by the category
N. I think again that any system will heed to consider
sentences recursive in this way. Then we will need predicates
of arbitrary "weight", tho' in natural language the number
of N's associated with any predicate V will undoubtedly be
rather small. One possible counter to this is obviated
if we assure that we have ways of referring to sets. Then
we can define S as a V and associated N's. This is not
really a bad scheme.

Where it does fall down is in its failure to reflect
hyper-propositional relations. The conceptual universe
of a person is not a bunch of unrelated trees or
sentences (propositions). We will want ways to connect
the Napoleon of "Napoleon ate cheese" with that of "Napoleon
hated Elba". Thus the conceptual universe is a network,
with a far more complex structure than our underlying semantic
trees. We therefore need some set of rules for isolating
part of this network to serve as the underlying tree for
some surface sentence or set of sentences, since it may turn
out from our study of the structure of discourse that
the unit of generation is larger than the sentence.

More will be said on these matters in section IV.

PART IV. The understanding
machine.

One basic goal of research into computational linguistics might be to investigate how information is extracted from linguistic source data. (ultimately this ties into such questions as that of automated abstracting.) That component of our projected understanding machine which will model the information abstracting process let us dub the "info grabber".

The info grabber of course is not isolated. It will have to be connected with a logic component and a memory with which it will interact.

Nor is this the whole picture. As shown below one needs also a way of encoding the semantic output of the logic component for later output as linguistic data. Therefore the whole system will look like:
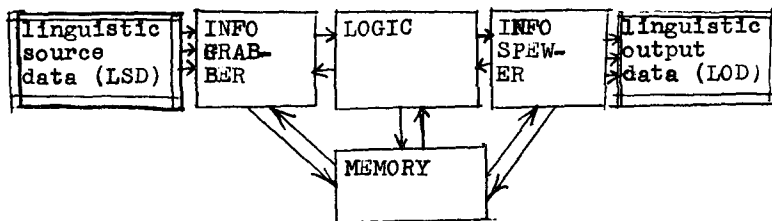
Notice that I have dubbed that component which synthesizes the LOD the "infox spewer".

We can regard the above as a reasonable model not only of an understanding machine, but of the speaker. The above model would certainly be of use in the study of the use o f a natural language as a computer input-output language both for programming and for other applications, such as interaction between student and teaching machine in an educational program. I have made some study of such a system, which I called EASIOL (English as an Input-output Language), taking into account the results of the two studies I know of which approximated what I was after, namely Daniel Bobrow's STUDENT program, reported on in "A Question Answering System for High School Algebra Word Problems" Proc. FJCC 25, 1964, and in Scientific American September 1966, pp. 252-260, which was Bobrow's
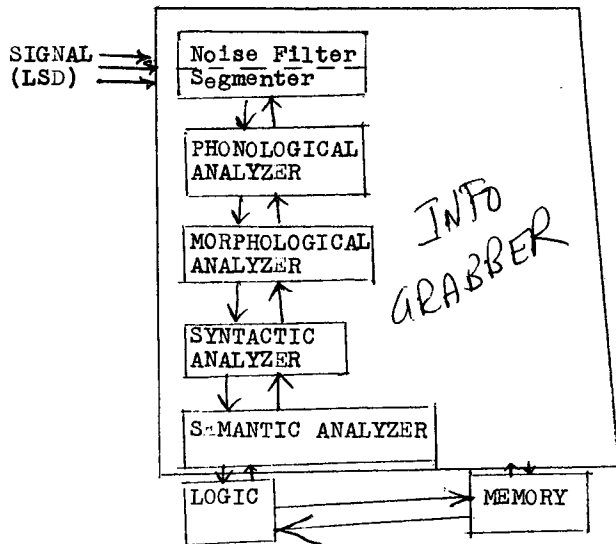
research for his doctorate. Bobrow modified LISP in the
direction of COMIT, calling the hybrid METEOR. The
system he evolved has a fair amount of flexibdlity
and generality, and can   deal with many kinds
of problems expressed in stylized language. I might
criticize Bobrow for his naivete over natural language,
but since I am even more naive about information
processing I will not do so.

A second system which I have heard later evolved into
a more general system, is the BASEBALL program reported
on by Green, Wolf, Carol Chomsky, and Laughery in the
Feigenbaum-Feldman volume, Computers and Thought. This
system bases itself on a rather stylized type of data
structure. I have not followed the progress of
either of these projects, but both betray inherent
faults that made it unlikely that either could form
the basis for a more general system operating on
actual discourse. Nonetheless, these systems are very
convincing for those who think that language is the
sacrosanct birthright of human beings and that computers
will never be able to handle such tasks as writing abstracts
of articles.

The above model is also a reasonable model of human
speakers (if we forget that people differ from machines
in essential ways -- vive la différence!)

The first part of the "info grab" is the read-in. Hope-
fully this will someday be done by the machine itself, via
optical reader or speech   . analyzer. I think that
research on readers and analyzers has in general been
unhappy because of a failure to realize how complex
recognition by humans is. Recognition is not simply
an optical or auditory problem. All levels of language
must interact in the process: it is well-known that real
speech is   ᵤ more easily handled than approximants
to speech, ᵪ this can only be due to the recognition
process being cyclic and operating simultaneously on
all levels. The simplest recognition routines would
involve something like:

SIGNAL →
(LSD) →

| Noise Filter |
| Segmenter |

↓↑

| PHONOLOGICAL |
| ANALYZER |

↓↑

| MORPHOLOGICAL |
| ANALYZER |

INTO GRABBER

↓↑

| SYNTACTIC |
| ANALYZER |

↓↑

| SEMANTIC ANALYZER |

↓↑

| LOGIC | ⟷ | MEMORY |

Indeed, we have to connect up the logic and the memory
to this system. Below is a real sample of my hand-
writing when writing rapidly.

of automated alerting) that component of our projected understanding machine which will model ~~the~~ the information

No recognition routine, not even my own human one
can at all times decipher this garbage. Redundancy is
pretty near nil and such words as "of", "as", "a", and
"or" tend to be homologous. What a human reader can't
do, we can hardly   expect a machine to do.
But humans can guess from context what a word must be,
and then see if the squiggle on the page is close
enough. This involves both syntactic and semantic
recognition, and if we ever want machine reading of
handwriting, we must give the machine this capability.
But suppose the reader still can't handle the
writing? I suppose then we want it to get the logic
component to intiate a question such as," What is that?"

That is, we want the computer to   be able to go
thru the whole set of levels. This will necessitate
a much more complicated program than those around
today, incorporating a greater amount of linguistic
expertise, but undoubtedly it is necessary.

Let us assume that the info grabber has grabbed the
info, it will have (1) to store this information in the
memory, and (2) let the logic component examine the
information. Suppose I know that Richard Daley is the
mayor of Chicago, and I read in a Chicago newspaper that
the Mayor of Chicago is the greatest man in the world.
The LSD must somehow be so stored that I can retrieve
from my memory the fact that Richard Daley is thought
to be the greatest man in the world by that newspaper.

This raises .   the question of how to convert
underlying semantic trees into subnets of the semantic
network of which memory probably consists. Many of the
features incorporated into .   Sidney Lamb's conceptu-
al networks will, I think, be incorporatable into the model.
In particular, all occurences of a particular entity (con-
cept) will have to be linked in some way or identified.

In a sense info grabbing starts by analyzing the LSD
into semantic structures, and ends by synthesizing these
structures and those already in memory into a new memory
network.

One point that should be made clear is that all informa-
tion will have to be represented on the same level. That
is, both the program and the data will reside in the same
memory net, as in a computer. Reading an algorithm in
a book, the machine will store this in its memory just
as it stores part of its own program, and it will be
able to either quote the algorithm later as linguistic
material as part of info retrieval, or use that algorithm
as part of its own logical operations. There is some
question tho as to whether this quite ideal machine could
actually function in this way. But human beings are like
this in some ways, and it is part of their language capa-
bility that they should or could.

The process of info spewing is a reverse of the
info grab. The logic component will initiate the
spew, using part of the memory net and selecting
one or more underlying trees to spew out. It will then

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go through the derivational process and ultimately generate
an actual string of sentences. Perhaps feedback will
enter here, so that the machine can utilize part of its
own spewings   as immediate LSD, although it is hard
to see why the machine would need to do so, altho humans
are constantly correcting themselves mid-sentence.

An obvious question is what the role of generative
semantics in all this. I think the experience of CL
has been in general that ad hoc programs don't
work. We need a basic linguistic theory. I think
generative semantics is the best bet. But as
I noted, it is a theory of cometence. We will need
to modify it. I think we need to

1) admit rules of non-recoverable deletion,

2) admit rules for hypersentential constructs,

and

3) build strong interactions with logic and memory
   components.

In particular, the relationship of underlying semantic
structures to conceptual networks will have to be
investigated in depth.

If the hypotheses of the GS linguists are correct, then
we have a simple but powerful basis for programs directly
transforming language source materials into semantic
information usuable by programs. For example, if
the semantic structures turn out to be universal, they
can serve as a pivot or intermediary for the currently
out of fashion goal of MT.