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First Annual Meeting of the
Berkeley Linguistics Society

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PREFACE

The Proceedings of the First Annual Meeting of the Berkeley Linguistics Society represents the first of an anticipated series of volumes publishing the papers presented at the Annual Meeting. The BLS is a student-run organization designed to provide a national forum in the Berkeley area for presentation of current linguistics papers and a means for their wider dissemination. Our efforts could not have been better rewarded than by the set of papers collected in this volume.

The format for this publication owes an obvious and acknowledged debt to the Chicago Linguistic Society volumes. The photo-offset technique, coupled with the willingness of the authors to provide us with camera-ready copy almost immediately after the meeting, makes it possible to publish the proceedings within four months of the meeting, and at a reasonable cost.

A three-day conference and publication of the proceedings is never just the work of a handful of people; it represents a great deal of work and cooperation by many—from the members of the abstracts committees to the members of the Linguistics Department staff. In particular, we would like to single out the following people for their help: George Lakoff, who originally suggested that we form BLS, Claudia Corum, who gave invaluable advice, and Chuck Fillmore and Karl Zimmer for their patience and understanding. We thank the Chicago Linguistic Society. Not only have its staff members, past and present, been a source of advice and encouragement, but they also provided a loan to cover a substantial portion of the publication costs. The Berkeley Linguistics Department helped arrange the additional financing needed to print the proceedings and has been generous with its help and cooperation. We'd like to express our special appreciation to LaRue Seegmiller and Eileen C. Odegaard, not only for their continual help, but also for putting up with numerous inconveniences that we caused. In addition, we would like to thank the large number who are never directly given credit and who contributed a great deal of time and effort. Finally, of course, the linguists themselves who gave
and listened to the papers should be thanked since it is they who ultimately made the effort a success.

We look forward to organizing future conferences and publishing more volumes in the hope that they will prove to be valuable contributions to the linguistics community.

The Berkeley Linguistics Society
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The following authors did not present their papers at the Annual Meeting, but are published in this volume: J. Carroll and J. Hennessey, P. Jacobson, and L. Talmy.
THE PROPER FORMULATION OF THE SPURIOUS-SE RULE IN SPANISH*

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Harvard M.I.T.

0. Introduction
There is a set of oblique pronouns in Spanish which are clitics on the verb; that is, they are phonologically subordinate to the verb to which they are attached. The following examples illustrate the dative singular and accusative, masculine, singular, third person clitics:

1a. Lo pagué. 'I paid for it.'
    acc

1b. Le pagué. 'I paid him.'
    dat

(1a-b) show that pagar can take a single clitic pronominal object, either dative or accusative. However, if both clitics appear and both are third person, then the dative may not appear as le but must appear as se. Thus, the expected forms (1c) are ungrammatical; instead (1d) is the correct form:

1c. *Le lo pagué. ('I paid him for it.')
    *Lo le pagué.

1d. Se lo pagué. 'I paid him for it.'

In general, when third person dative and third person accusative clitics cooccur, the dative surfaces as se instead of its form in isolation le/les.1

Paradigms like (1) provide evidence for a rule which Perlmutter (1971:22) called the Spurious-se rule and formulated as follows:

2. \[
\begin{array}{cc}
\text{[PRO]} & \text{[PRO]}
\\
\text{III} & \text{III}
\\
\text{DAT} & \text{ACC}
\\
1 & 2
\end{array}
\] \rightarrow \text{se}, 2

On the basis of other facts, it has been more recently claimed that the spurious se morpheme may replace not only the dative in the sequence [3-DAT 3-ACC] but also the first accusative in the sequence [3-ACC 3-ACC] (see García, Roldán, Timberlake).2 Thus, García would formulate the Spurious-se rule more generally as follows:

3. \[
\begin{array}{cc}
\text{[PRO]} & \text{[PRO]}
\\
\text{III} & \text{III}
\\
\text{ACC} & \text{ACC}
\\
1 & 2
\end{array}
\] \rightarrow \text{se}, 2

The immediate purpose of this paper is to argue conclusively
against the generalized formulation of the Spurious-se rule in (3). Of more general interest is the fact that this investigation exposes facts which are of considerable relevance to the development of a theory of clitics. In section 1 below, we present some facts about Spanish to facilitate understanding the examples. In section 2, we proceed with the argument.

1. Some Facts about Spanish

1.1 Subject Pronouns
Subject pronouns are omitted when serving no contrastive or emphatic function:

4a. Lo quiero yo, no él. 'I want it, not him.'
   I  he

   b. Lo quiero ahora. 'I want it now.'

1.2 Personal a
Indirect objects are preceded by the preposition a. Certain direct objects also require a preceding preposition a. The conditions for the appearance of a are complex (see Isenberg 1968) but for our purposes it is sufficient to observe that definite, animate, direct objects require a:

5a. Vimos a Juan. 'We saw Juan.'
   b. *Vimos Juan.

The most straightforward way to determine the function of an a NP in a given S is by pronominalization, since dative and accusative pronouns are distinct.3

1.3 Clitics
We present here a chart of the clitics:

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1.4 Clitic Copies
NPs cooccur more or less obligatorily with a clitic copy in several environments:
1. Indirect objects obligatorily cooccur with a clitic.\(^4\)

6a. Le compré un saco a Marta. 'I bought Marta a coat.'
b. *Compré un saco a Marta.

2. In Platense, the Spanish spoken in the Río de la Plata region of South America, there is a class of direct objects which obligatorily cooccur with a clitic copy. This is the class of definite, specific, animate, direct objects (see Roldán 1971 for some discussion). For example:

7a. Las invitamos a las chicas. 'We invited the girls.'
b. *Invitamos a las chicas.

(7b) is grammatical in dialects which do not require a clitic copy of the direct object.

1.5 Enclisis and Proclisis

A fact of Spanish morphology is that while clitic pronouns procliticize to finite verbs,\(^5\) they encliticize to nonfinite verbs (infinitives, gerunds, affirmative imperatives). Orthographically, enclitics form a word with the verb; proclitics do not. For example:

8a. Lo quiero. 'I want it.'
b. *Quiérolo.
c. Quiero verlo. 'I want to see it.'
d. *Quiero lo ver.

1.6 Clitic Climbing

There is a rule in Spanish, generally known as Clitic Climbing, which moves clitic(s) from a dependent infinitive to a higher verb:

9a. Quiero verlo. 'I want to see it.'
b. Lo quiero ver. " "

Note that a clitic may climb to a verb higher than the immediately dominating verb:

10a. Quiero poder aprobarlo. 'I want to be able to pass it.'
b. Quiero poderlo aprobar. " "
c. Lo quiero poder aprobar. " "

If any clitic associated with an infinitive climbs, all clitics associated with that infinitive must climb:

11a. Quiero cosérmelo. 'I want to sew it for myself.'
b. Me lo quiero coser. " "
c. *Me quiero coserlo.
d. *Lo quiero coserme.

We will speak of the embedded infinitive as releasing the clitic and of the matrix verb as attracting the clitic. Whether a verb releases or attracts clitics seems to be lexically governed,
although there are subregularities. That is, not all verbs release clitics and not all verbs attract clitics.

2. The Spurious-*se* Rule

2.1

The Ss in (1) illustrated the effect of the Spurious-*se* rule operating on clitic sequences associated in underlying structure with a single V. The same rule applies to the output of Clitic Climbing:

12a. Le permití comprarlo. 'I permitted him to buy it.'  
\[
\text{dat} \quad \text{acc}
\]

b. *Lo le permití comprar.

\[
\text{*Lo le permití comprar.}
\]

c. Se lo permití comprar. " "

2.2

Paradigms like the following led García to propose that the Spurious-*se* rule operated not only on the sequence [3-DAT 3-ACC] but also on the sequence [3-ACC 3-ACC] which she must assume to arise here by Clitic Climbing.

13a. Lo vi hacerla. 'I saw him make it (fem).'

\[
\text{acc} \quad \text{msg} \quad \text{fsg}
\]

b. *La lo vi hacer.

\[
\text{La lo vi hacer.}
\]

c. Se la vi hacer. " "

We present three arguments against an analysis of (13c) which derives it by Clitic Climbing and the Spurious-*se* rule.

3. Verb Raising is a Possible Derivation

3.1

We argue first that (13a) and (13c) are related not by Clitic Climbing as proposed by García, but rather that they are examples of two fundamentally different constructions in which ver participates. Thus (13c) is not transformationally derived from (13a) although the two may share a common deep structure.

The argument that (13c) is derived by Clitic Climbing followed by the generalized Spurious-*se* rule depends crucially on there being no source for (13c) where *se* is the reflex of a dative NP. If there were such an S then this could of course be the source of *se* in (13c); then (13) would not constitute evidence in favor of the formulation adopted by García. As García herself observes, such Ss exist. Alongside of Ss like (14a) which is like (13a) except that the embedded object is nonpronominal, we have (14b)

14a. Lo vi hacer la casa. 'I saw him make the house.'

\[
\text{acc}
\]

b. Le vi hacer la casa. " "

\[
\text{dat}
\]
3.2
In order to account for the alternation between dative and accusative in (14), it is necessary to look further. The crucial fact for our purposes is that this alternation correlates exactly with possible positions of the NP which is cross-referenced by the dative or accusative clitic. The following examples represent the Platense dialect in which datives and animate accusatives obligatorily cooccur with a clitic copy. We use this dialect because the form of the clitic reveals immediately the grammatical function of the NP (see section 1.4), but this analysis is intended to hold for all dialects:

15a. Lo vi a Juan hacer la casa. 'I saw Juan make the house.'
    acc
b. *Lo vi hacer la casa a Juan.

In (15), a Juan is direct object. In this function, it must occur between vi and hacer. On the other hand, in the following Ss, a Juan is indirect object and occurs only after vi hacer:

16a. Le vi hacer la casa a Juan. 'I saw Juan make the house.'
    dat
b. *Le vi a Juan hacer la casa.

3.3
In (15a) a Juan functions as direct object of vi. We do not know at present whether (15a) is derived by a kind of Subject-to-Object Raising rule or by Equi-NP-Deletion. In the former case, a Juan would be raised to become object of vi. Thus it is accusative and occurs immediately after vi. If the Equi derivation is correct, a Juan would start out in its surface position. We will refer to this construction as the complement causative construction, a term which is intended to be neutral as to whether Raising or Equi is the crucial rule. We assume an underlying structure as in (17a) and a derived structure as in (17b):

3.4
(16a), on the other hand, is the output of the rule of Verb Raising (see Aissen 1974a,b). While the formulation of this rule is not clear, the effect of the rule is to incorporate an entire embedded S into the matrix S resulting in a simplex-like structure.6
Verb Raising has the following effects in Spanish:

18a. the old embedded V appears in infinitival form to the immediate right of the matrix V.

b. the old embedded direct object (if there was one) appears as accusative to the right of [V+infinitive].

c. the old embedded subject appears as accusative if there is no embedded direct object, and as dative if there is.

In either case, it stands to the right of [V+infinitive] relative order of accusative and dative being somewhat free. 7

The input and output structures of Verb Raising may be represented as below:

19a.        19b.
     S
    /   
 NP   VP
  yo   
 I V
 vi saw

NP
Juan

V
hace-

la casa

make

the house

S
NP
yo

VP

vi

hacer

la casa

make

the house

3.5

Consider the paradigm in (13) again:

13a. Lo vi hacerla. 'I saw him make it.'
    acc  acc

b. *Lo la vi hacer.
    *La lo vi hacer.

c. Se la vi hacer. 'I saw him make it.'

In these Ss both subject and object of the old embedded S are pronominal. (13a) is an instance of the complement causative construction; the old subject is accusative. Crucially (13c) can be derived by Verb Raising yielding the clitic sequence [3-DAT 3-ACC] -- a sequence which is then subject to the Spurious-se rule.

(13c), then, has an independently motivated derivation in which se la is derived from [DAT ACC]. Thus, these facts do not force a revision of the Spurious-se rule along the lines proposed by García. This, by itself, does not show that the proposed revision is wrong; it simply means that it is by no means forced by these facts.

4. Verb Raising is a Necessary Derivation

In the previous section we pointed out that an independently motivated rule of Verb Raising exists which would provide a derivation
for Ss like 'se lo vi hacer', in which the _se lo_ sequence could be
derived from a [DAT ACC] sequence. In this section, we show that
such Ss have properties that can only be accounted for if they are
assumed to have a derivation by Verb Raising, and cannot be account-
ted for if they only have a derivation by Clitic Climbing and Spu-
rious-__se__. It will remain open, until section 5, whether such Ss
have both derivations.

4.1
Ss like the following show that Clitic Climbing may have the ef-
fekt of joining two clitics which started out in different clauses:

20a. ?Le permití comprarlo. 'I permitted him to buy it.'
b. *Se lo permití comprar.

21a. ?Le ordené comprarlo. 'I ordered him to buy it.'
b. *Se lo ordené comprar.

22a. ?Le prometí comprarlo. 'I promised him to buy it.'
b. *Se lo prometí comprar.

The following Ss show, in addition, that all the matrix verbs
above (permitir, ordenar, prometer) all release _their_ clitics to
a higher clitic-attracting verb.

23a. Le debo permitirle besarla. 'I must permit him to kiss her
b. *Le debo ordenar besarla. 'I must order him to kiss her.'
c. Le debo prometer besarla. 'I must promise him to kiss her

4.2
However, a rather unexpected fact is that two clitics which are
joined by an application of Clitic Climbing will not climb further:

24a. Debo permitirle manejarlo. 'I must permit him to drive it.'
b. Debo permitírselo manajar. " "
c. *Se lo debo permitir manajar. " "

Clitics which start out as clitics to the same verb underlyingly
can climb together as shown by (25-26), which have the readings
given (corresponding to underlying structures with both clitics
in the same clause), and not the readings marked with '***', corre-
sponding to underlying structures with clitics originating in dif-
ferent clauses:

25. Se lo debo ordenar hacer. 'I must order PRO to do it
   for him.'
   ***'I must order him to do it.'

26. Se lo debo prometer comprar. 'I must promise to buy it
   for him.'
   ***'I must promise him to buy it.'

Thus it appears that we must impose the following constraint:

27. Clitics joined by Clitic Climbing may not climb further.
4.3
However, clitics joined by Verb Raising can climb further. This can be seen by considering the causative verb hacer 'to make, to have'. This verb, unlike the other causative verbs oir, ver, and dejar enters only into the Verb Raising construction and not into the complement causative construction:

28a. le hice manejar el coche a Juan. 'I had Juan drive the car.'
     dat
b. *lo hice a Juan manejar el coche. ('I had Juan drive the car.')
     acc

Clitics joined in this construction do climb further:

29a. se lo hice manejar. 'I had him drive it.'
     b. se lo quiero hacer manejar. 'I want to have him drive it.'

Since there is no possibility that se lo in (29a) were joined by Clitic Climbing, we may conclude that:

30. Clitics joined by Verb Raising may climb further.

Notice now that the joined clitics in examples like (31-33) can climb further:

31a. se lo vi comprar. 'I saw him buy it.'
     b. se lo quiero ver comprar. 'I want to see him buy it.'
32a. se la of cantar. 'I heard him sing it.'
     b. se la quiero oir cantar. 'I want to hear him sing it.'
33a. se lo dejé manejar. 'I let him drive it.'
     b. se lo quiero dejar manejar. 'I want to let him drive it.'

Following an analysis which uniquely derives (31a, 32a, 33a) by Clitic Climbing (and it is not clear that this is Garcia's analysis), we would not expect the clitics to be able to climb further; thus Ss (31b, 32b, 33b) should be ungrammatical.

Under our analysis, however, the clitics in the (a) Ss above end up together as a result of Verb Raising, not Clitic Climbing. Thus, we predict that the clitics will be able to climb further, as they do.

We have shown then that Ss of the type se lo vi comprar must have a derivation by Verb Raising, whether they have a derivation by Clitic Climbing or not. In the following section, we will show that these Ss cannot in any case be derived by Clitic Climbing and that the Verb Raising derivation is the only derivation they have.

5. Verb Raising is the Only Derivation
5.1
We have seen above numerous examples where Clitic Climbing applies to the following construction and joins the two clitics (see (20-22)).
34. DAT-V V-ACC → se-ACC-V V

If the generalized Spurious-se rule were correct, we would expect to find cases of Clitic Climbing applying to the following construction, joining the clitics and feeding Spurious-se:

35. ACC-V V-ACC → se-ACC-V V

Of course, Ss like (31a, 32a, 33a) were proposed to be Ss of exactly this type. But we have already shown that there must be an alternate derivation for these Ss which does not involve Clitic Climbing. A convincing case for the generalized Spurious-se rule can only be made by showing that there are derivations like the one sketched in (35) which must involve Clitic Climbing; i.e., derivations which have no alternate Verb Raising derivation.

On the other hand, if we can show that there are underlying structures of the form:

36. ACC-V V-ACC

which must be assumed to undergo Clitic Climbing, but which never surface as [se-ACC-V V] we will have a direct argument against the generalized Spurious-se rule, since it should apply in such cases to complete the derivation in (35).

In this section, we do exactly this. We show that there are structures like that of (36) which must be assumed to undergo Clitic Climbing but which never surface. The crucial link, then, will be to establish that Clitic Climbing must be assumed to apply in such derivations.

5.2

The Ss below have the structure of (36):

37a. Lo obligué a comprarla.10 'I made him buy it.'
   acc acc
b. Lo persuadí a comprarla. 'I persuaded him to buy it.'
   acc acc

If the downstairs clitic could climb and remain in its original shape, we would expect one of the outputs (38a,b):

38a. *Lo la obligué a comprar.
    *La lo obligué a comprar.

b. *Lo la persuadí a comprar.
   *La lo persuadí a comprar.

But all the Ss of (38) would be discarded by the surface filter on the order of clitics proposed in Perlmutter (1971). The proposed filter has the form:

39. se II I III
where I, II, III stand for first, second, and third person clitics. The filter is interpreted to exclude any sequence of clitics which does not conform to (39) where any element may be optionally included. This filter restricts the number of third person clitics (other than se) in a surface clitic sequence to one. While Perlmutter did not specifically justify allowing only one third person slot, there appear to be no occurrences in Spanish of more than one such clitic. Thus, we assume that (39) blocks the generation of the Ss in (38).

If the generalized Spurious-se rule were correct, however, we would expect the first of the accusative clitics to turn into se and to yield one of the following Ss:

\[ \begin{align*}
40a. & \text{ *Se la obligué a comprar. } \\
& \text{ *Se lo obligué a comprar. } \\
b. & \text{ *Se la persuadí a comprar. } \\
& \text{ *Se lo persuadí a comprar. }
\end{align*} \]

5.3
If we can establish now that (37) can undergo Clitic Climbing then we will have our argument against the generalized Spurious-se rule since, if that rule were correct, such Ss should surface as (40a,b). The restricted Spurious-se rule would, on the other hand, only generate outputs like (38) which are disposed of as noted above.

It is clear that the demonstration that (37) can undergo Clitic Climbing will have to be done indirectly. We will argue as follows: we noted above that whether a matrix verb attracts clitics from an embedded infinitive is lexically governed. That is, some verbs accept clitics, others do not, and this government seems to be in part idiosyncratic. Thus, while the verbs permitir, prometer, ordenar attract clitics, the verbs sugerir, enseñar, and pedir do not:

\[ \begin{align*}
41a. & \text{ Le sugerí romperla. 'I suggested to him that he break it.' } \\
& \text{ b. *Se la sugerí romper. } \\
42a. & \text{ Le enseñé a cantarlo. 'I taught him to sing it.' } \\
& \text{ b. *Se lo enseñé a cantar. } \\
43a. & \text{ Le pedí verlo. 'I asked him to see it.' } \\
& \text{ b. *Se lo pedí ver. }
\end{align*} \]

Since all of the (a) Ss above take a dative clitic, Clitic Climbing followed by the Spurious-se rule should result in possible clitic sequences. Since all of the (b) Ss are ungrammatical, we assume that Clitic Climbing is lexically blocked.

We will show that whether a verb allows Clitic Climbing or not correlates with one other syntactic property. We will then show that the verbs in (37), that is obligar and persuadir exhibit the behavior of a clitic climbing verb. We will conclude that obligar and persuadir are clitic climbing verbs and that Ss with obligar and persuadir may undergo Clitic Climbing. The fact that such Ss do not surface is evidence that the generalized Spurious-se rule is wrong.
Rivero (1970:640) posits a constraint which "allows only one negative particle no to appear as constituent of each simplex sentence in surface structure". She argues that Equi-NP-Deletion reduces a complex structure to a simplex one. Thus, while the (a) Ss below which have not undergone Equi are grammatical, the (b) Ss are ungrammatical:

44a. No creo que yo no lo haga bien. 'I don't believe that I don't do it well.'
   b. *No creo no hacerlo bien.

45a. No quiero que no comas pescado. 'I don't want you not to eat fish.'
   b. *No quiero no comer pescado. ('I don't want not to eat fish.')

There are speakers of Spanish who accept (44b, 45b). These same speakers, however, do make distinctions in other infinitival constructions allowing double no's in some, but not in others.13,14

46. *No le permití no salir. ('I didn't permit him not to leave.')
47. No le sugerí no salir. 'I didn't suggest to him not to leave.'

Furthermore, the possibility of two no's in an infinitival construction correlates with the impossibility of Clitic Climbing in that construction. Thus, if a matrix verb $V_m$ does not allow the configuration [no $V_m$ no $V$], it does accept clitics. On the other hand, if a matrix verb does allow this configuration, it does not accept clitics.15 Corresponding to (46) and (47), then, we get (48) and (49) showing that permitir accepts clitics but not sugerir:

48. Se lo permití comprar. 'I permitted him to buy it.'
49. *Se lo sugerí comprar. ('I suggested to him that he buy it.'

The following Ss further exemplify this correlation. The (a) Ss show the possibility or impossibility of Clitic Climbing while the (b) Ss show the possibility or impossibility of double no's.16

50a. Se lo prohibí tocar. 'I forbade him to touch it.'
   b. *No le prohibí no tocarlo. ('I didn't forbid him not to touch it.')

51a. Se lo ordené comprar. 'I ordered him to buy it.'
   b. *No le ordené no tocarlo. ('I didn't order him not to touch it.')

52a. *Se lo pedí tocar. ('I asked him to touch it.')
   b. No le pedí no tocarlo. 'I didn't ask him not to touch it.
53a. *Se lo insistí en comprar. ('I insisted to him on my buying it.')
   b. No le insistí en no tocarlo. 'I didn't insist to him on my not touching it.'
5.5
Thus, for some speakers there is a clear generalization that any verb which forbids double no's does allow Clitic climbing. The crucial fact for our argument is that there are V_m's of the structure [ACC-V V-ACC] which do not allow double no's. Such verbs are obligar and persuadir:17

54a. *No lo obligué a no irse. ('I didn't force him not to go away.')
   b. *No lo persuadí a no irse. ('I didn't persuade him not to go away.')</p>

If this generalization is correct, it must be the case that obligar and persuadir are [+Clitic Climbing] verbs.18

Now if obligar and persuadir are [+Clitic Climbing], we must explain why Ss like (56) in which Clitic Climbing has applied to (55) never surface:

55. Lo obligué a comprarla. 'I made him buy it.'
   Lo persuadí a comprarla. 'I persuaded him to buy it.'
56. *Se la obligué a comprar.
   *Se la persuadí a comprar.

The generalized Spurious-se rule predicts that (56) should be grammatical since the rule applies to the sequence [ACC ACC]. Thus, (56) counterexemplifies the generalized rule. The restricted version of the Spurious-se rule, however, only applies to the sequence [DAT ACC]. Thus, it will not apply to the output of Clitic Climbing on (55), and (56) are predicted to be ungrammatical. We conclude that the generalized Spurious-se rule is wrong.

6. Conclusion
We have established that the original formulation of the Spurious-se rule -- that is, the formulation in (2) -- is the correct formulation. The investigation which led to this conclusion has uncovered regularities in the behavior of clitics in infinitival constructions. These regularities are themselves of considerable interest and of potentially greater significance than the conclusion itself. However, they remain unexplained here. To reach a full understanding of them will clearly require much further investigation which will hopefully lead to the development of a theory of clitics.

FOOTNOTES
*We are indebted to Jorge Hankamer for his careful reading of an earlier version of this paper and to Eugenia Kálnay de Rivas for her help as an informant.

1 se also serves as the dative third person reflexive:
   (i) se pagó. 'He paid himself.' (not: Hi paid him)
All gender and number distinctions are neutralized in se. Thus, (ld) is multiply ambiguous.
2 García (undated: 14-16, 87-89) seems to be the first to argue for such a formulation. As her discussion proceeds, it becomes less clear how strongly she would want to defend this proposal. Roldán (1974: 134) says nothing explicit about the formulation of the Spurious-se rule but it is clear from her discussion that she assumes a formulation like (3). Timberlake (1970: fn. 11) is following García. We will refer to the proposal as García's. The proposal was made in an unpublished xerox and we do not know whether García would still wish to defend the proposal.

3 Dative and accusative clitics are distinct in Latin American Spanish (so-called loísta dialects). In some dialects in Spain, (so-called leísta dialects), le/les is used both for dative and animate, masculine, accusative forms. The forms cited in this paper all correspond to the loísta dialect.

4 Perlmutter (1971: 33) reports that there are some dialects for which this doubling is optional.

5 In non-colloquial speech, clitics may encliticize to finite verbs in certain situations. (8b) is starred in the text because in colloquial speech, the form is never used. However, Ss like (8b) are used in newspapers, speeches, etc.

6 In Spanish, this rule applies not only to structures with the matrix verb ver but also to other verbs of perception (oir 'to hear'), and to the causative verbs hacer 'to make' and dejar 'to let'. See Aissen (1974a, b) and Bordelois (1974) for discussions of this construction.

7 This rule of Verb Raising appears to have the effect that its output assumes the features of a simplex S; this accounts for the derived position of the infinitive (next to the matrix V) and the case marking and derived positions of the accusative and dative NPs on the basis of principles of word order and case marking in simplex Ss.

8 The (a) Ss below are not perfect. Inanimate clitics appear to prefer to climb rather than to remain with the infinitive.

9 The reader will note that we are using deber 'must' in the following examples as a clitic attracting verb rather than querer. The reason is that verbs form a hierarchy according to their power to attract clitics; querer is somewhat further down the hierarchy than deber, and in particular does not attract clitics in Ss like (23).

10 The preposition a alone does not block Clitic Climbing. For example: (i) Empecé a hacerlo. 'I began to do it.'

(ii) Lo empecé a hacer.

See Bordelois (1974) for a discussion of the construction in (37).

11 To the extent that Clitic Climbing is lexically governed, we expect to find that speakers may vary with respect to which verbs are [+Clitic Climbing] and which [-Clitic Climbing].

12 The judgments in (44) and (45b) are Rivero's. In her dialect, apparently, Equi applies optionally to creer and obligatorily to querer.

13 We are aware of the fact that there is some dialectal variation in the double no construction.
Contreras and Rojas (1972) make this observation. The facts presented by Contreras and Rojas and below suggest that for at least some speakers the constraint as formulated by Rivero is not correct.

This is true for matrix verbs which require a personal object, like the verbs we are considering here. The implication: "if a matrix verb $V_m$ allows the configuration [no $V_m$ no $V$], it does not accept clitics" is not true for verbs like querer which both accept clitics and allow double no's. Nonetheless, the implication "if a matrix verb does not allow this configuration, it does accept clitics" seems to be correct, and it is this implication that we need for the argument.

Note that the clitic does not climb in the (b) examples. Clitics may never climb over an intervening no. See Rivas (1974) for an explanation.

There are also verbs of structure [ACC-V V-ACC] which do allow double no's. For example:

(i) No lo forcé a no irse. 'I didn't force him not to leave.'
(ii) No lo impulsé a no casarse. 'I didn't force him not to get married.'

We are making an additional assumption here which is that all verbs which enter into infinitival complement structures are marked as either [+ Clitic Climbing] or [- Clitic Climbing] even if the effect of Clitic Climbing is never observed. We are aware that this assumption requires justification. We present justification in a forthcoming paper.

The ungrammaticality of Ss like(i) and (ii) below, which will be generated under our analysis must also be explained, for these Ss are not excluded by Perlmuter's surface filter:

(i) *Me lo obligó a comprar. ('He made me buy it.')
(ii) *Me lo obligó a comprar. ('He made you buy it.')

A more complicated filter than Perlmuter's, one which refers to case and blocks any clitic sequence containing more than one accusative clitic, must exist if our analysis is correct. The following filter was proposed by Dinnsen (1972) to operate in addition to Perlmuter's filter:

(iii) Reflexive Benefactive Dative Accusative

This filter is interpreted as outlined above for Perlmuter's filter. Dinnsen justifies the relative ordering of clitics dictated by this filter, but not the inclusion of only one slot for each case. That is, he does not, for example, argue against the filter in (iv):

(iv) Reflexive Benefactive Dative Accusative Accusative

If our analysis is correct, (iv) must be wrong. Unfortunately, we cannot provide independent evidence for (iii) as against (iv). The
only source of two accusative clitics in Spanish that we know of is in Ss like (i) and (ii) above.

We can show that two dative clitics may not cooccur and this lends some plausibility, though nothing more, to the assumption that two accusative clitics are excluded. Two dative clitics arise by applying Clitic Climbing to the structure [DAT-V V-DAT]. The output of Clitic Climbing, however, is +Cl.Cl.

ungrammatical:

(v) Te ordené pegarle, 'I ordered you to strike him.'
    dat          dat

(vi) *Te le ordené pegar.

REFERENCES


VOICE -- BEYOND THE PASSIVE
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When American linguists think of the term voice, as applied to
the verbal system of a language, they generally think only of
active and passive, since those are the only well-developed voi-
ces in modern English. The Indo-European system from which Eng-
ish was derived, however, was quite different. To judge from
the daughter languages in which the PIE system was most fully
preserved (ancient Greek and Sanskrit, in particular), a more
fundamental distinction in PIE was that between active and middle
voices, the middle including within its functions, without fur-
ther formal distinction, the relationship that we would call pas-
active. Because it is often not possible to tell the difference,
formally, between passive and middle in these languages, these
two categories must have been viewed as having something im-
portant in common. (For this reason it would seem worthwhile to
preserve the term voice for the broader grammatical phenomenon
which encompasses all three distinctions—active, passive, and
middle.) The purpose of this paper, then, is to explicate how
the active/middle distinction differs from the active/passive
one; how languages like English handle whatever is included in
the middle that is not included in the passive; and at what lin-
guistic level the passive can be lumped with the middle, when
that is what has been done.

The passive has been argued to be fundamentally a strategy to
move NP's in and out of subject position. In an accusative-type
language, the subject of most active sentences is agentive—that
is, the subject is automatically assumed to be the agent of the
action, if that reading is semantically possible. Furthermore,
the subject is in general a sine qua non of the sentence. So if
the agent is to be de-emphasized or outright avoided (whether be-
cause it is unknown, irrelevant, or to be suppressed), some
strategy must be available to remove the agent subject and re-
place it with either a dummy or an NP having some other function
in the sentence proposition. The nonagentive passive fulfills
these needs. Thus, for example, we can neatly avoid confessing
who broke the window by saying The window was broken. Since sub-
ject position is a more or less highly "privileged" position (a
subject's reference and quantifiers are independently, rather
than dependently, evaluated; and the subject is generally easier
to relativize, to topicalize, etc.) (Keenan, 1974 and Dec. 1974),
the speaker will need a strategy for "promoting" nonagentive NP's
to subject position so that they can carry syntactic and semantic
structures which are unavailable or awkward in other positions.
The nonagentive passive also fulfills these needs; and the agen-
tive passive fulfills them without requiring that the agent be
dumped altogether.

The new subject, of course, is not the expected agent of the
action; and in order to avoid intolerable ambiguity, the deviant function of the subject with respect to the action expressed in the verb must be marked in some way. Presumably it could be marked in the NP itself; or it could be marked in the verbal complex (that is, by an affix to the verb or by an auxiliary). Most commonly we find it marked in the verbal complex somewhere; and so we have an opposition in the verb system which shows how the subject NP is to be understood. In short, we have a system of verbal voice. Theoretically, however, there are other ways of showing the voice relationships than by marking them in the verbal complex, so I shall retain the term verbal voice for whatever gets marked in the verb and use the term grammatical voice for the broader set of voice relationships, wherever they may be indicated in a given language.

By this analysis, then, English can be said to have a verbal voice system which includes active voice (The cat scratched it) and passive voice (The cat was scratched). In the passive voice we can either specify the agent (The cat was scratched by the dog) or exclude it; and the nonagentive subject can correspond either to the direct object of the active (A dollar was given to the man by Snodgrass) or—and this is less common among languages—to a more "inaccessible" direct object (The man was given a dollar by Snodgrass). Note that, although only one contrast is marked in the verbal complex, namely active against passive, provision is also made to mark the nonsubject nouns in such a way that their functions can be unscrambled without intolerable ambiguity, for whatever combination of promoted and demoted nouns arises. Thus the agent, which is marked only by word order in the active, gets by as a relational marker in the passive, and the indirect object (unless promoted to subject) must keep with it the to which it could often drop in the active.

The passive, then, is a device for promoting the NP's in and out of subject position. The middle voice, by contrast, seems to function fundamentally as a strategy for marking identities between the surface subject and other NP's in the sentence proposition. Since linguistic literature on the middle voice is almost nonexistent, let us take some examples from ancient Greek (Sanskrit works in almost exactly the same way) to see how it behaves.

When a Greek author chooses the middle over the active voice for a particular verb, he is expressing what is usually described by Greek teachers as the "involvement" of the subject in the action—the fact that the agentive subject is also affected in some way by the action. What does this mean? In English, the most "middly" kind of expression we have is a colloquial sentence like I bought me a new hat or I fixed me a sandwich, containing what a classical grammarian would have called a "dative of interest." A transitive Greek middle, indeed, has much this semantic structure:
In each of these examples, the middle voice is expressing the fact that the subject is not only performing the action, as agent, but receiving some benefit from it as well. This is the basic sense in which the subject is seen to be "involved" in the action. We could also say that the middle voice is signaling that what would otherwise have to be expressed as the indirect object is identical to the subject.

Viewing the middle in this way, as a means of signaling that some nonsubject NP in the sentence proposition is identical with the surface subject, we find that this analysis accounts for and even predicts the various uses of the middle in Greek. We have begun with a full-dress transitive sentence, in which the direct object is overtly expressed. There, a middle voice in the verb is found to imply that what would logically be the indirect object is identical to the subject. What if no direct object is expressed? If the verb is known to be an intransitive verb, which never takes a direct object, then the implication is still that the identity holds between subject and indirect object:

<table>
<thead>
<tr>
<th>Active</th>
<th>Middle</th>
</tr>
</thead>
<tbody>
<tr>
<td>αἱρ-ῶ</td>
<td>αἱρ-οῦμαι</td>
</tr>
<tr>
<td>hair -ῶ</td>
<td>hair-oumai</td>
</tr>
<tr>
<td>take act. share</td>
<td>take mid. share</td>
</tr>
<tr>
<td>&quot;I take a share.&quot;</td>
<td>&quot;I choose (take for my own benefit) a share.&quot;</td>
</tr>
<tr>
<td>ἤγαγ-ον γυναῖκα.</td>
<td>ἤγαγ-ὀμην γυναῖκα.</td>
</tr>
<tr>
<td>ēgag-on gynaika</td>
<td>ēgag-omen gynaika</td>
</tr>
<tr>
<td>led act. woman</td>
<td>led mid. woman</td>
</tr>
<tr>
<td>&quot;I led a woman.&quot;</td>
<td>&quot;I married (led to myself / for my own benefit) a woman.&quot;</td>
</tr>
<tr>
<td>πράττ-ῶ χρηματα.</td>
<td>πράττ-ομαί χρηματα.</td>
</tr>
<tr>
<td>pratt-ῶ khrēmata</td>
<td>pratt-omai khrēmata</td>
</tr>
<tr>
<td>do act. things</td>
<td>do mid. things</td>
</tr>
<tr>
<td>&quot;I accomplish (manage) things.&quot;</td>
<td>&quot;I accomplish (manage) things for myself; I make money.&quot;</td>
</tr>
</tbody>
</table>

If, on the other hand, the verb is known to be transitive but no direct object is overtly expressed, the direct object is taken to be identical with the surface subject. The identity can be taken either reflexively or reciprocally, according to context and inherent semantic likelihood:

<table>
<thead>
<tr>
<th>Active</th>
<th>Middle</th>
</tr>
</thead>
<tbody>
<tr>
<td>πολίτευ-ω</td>
<td>πολίτευ-ομαι</td>
</tr>
<tr>
<td>politeu -ŏ</td>
<td>politeu -omai</td>
</tr>
<tr>
<td>be citizen act.</td>
<td>be citizen mid.</td>
</tr>
<tr>
<td>&quot;I am a citizen / have civic rights.&quot;</td>
<td>&quot;I act as a citizen / carry out my civic rights for myself.&quot;</td>
</tr>
</tbody>
</table>
λούω τὰ ἱμάτια.  
lou -ō ta himatia  
wash act. the cloaks  
"I wash the cloaks."

λοῦ-ομαί  
lou -omai  
wash mid. (1 sg.)  
"I wash myself."

λοῦ-ὀμεθα  
lou -ometha  
wash mid. (1 pl.)  
"We wash ourselves." OR "We wash each other."

στεφαν-ὀμεθα σέ  
stefan-ōmetha se  
crown act. you  
"We crown you."

στεφαν-ὀμεθα  
stefan-ōmetha  
crown mid.  
"We crown ourselves." OR "We crown each other."

It does not take much thought to see that what is expressed in Greek by the middle marker in the verb is generally expressed in English by special pronouns. Thus we have two reciprocal forms (each other, one another) and a whole set of reflexive pronouns (myself, yourself, etc.) which we can tuck into the proper grammatical position to show identity to the subject. English, in short, has settled on marking identity relations in the slots for the affected NP's, rather than in the verb. And since the function of these pronouns is the same as that of the Greek middle marker—to show identity with the subject—it comes as no surprise that these pronouns are barred from occurring in subject position.

What is more curious is that Greek also has a full set of (nonnominative) reflexive and reciprocal pronouns. If our analysis up to this point has been correct, we would expect that the middle voice and the reflexive/reciprocal pronouns are two different strategies to achieve the same end: namely to mark identities between the surface subject and some other NP in the sentence proposition. So why would a language want both of them? Evidently, to obtain a higher degree of logical expressiveness than a single, compact verbal voice marker can provide.

All that the middle marker does is to put an identity flag into the verb. It is up to the hearer to decide, by whatever means available (by context, by semantic appropriateness, by grammatical elimination of alternatives), which slot is to be filled in with an NP matching the subject, and whether it is to be understood distributively (reciprocally) or nondistributively (reflexively). By marking the NP itself, however, in the guise of a special pronoun substitute, all of these distinctions can be made overt. Many languages go only halfway, of course, not distinguishing direct object pronouns from indirect object pronouns. But the full set of distinctions can generally be achieved more readily by marking the NP's than by developing a fully differentiated set of markers for the verb. Greek, in fact, uses both noun-marking and verb-marking systems. The middle can be and is used alone to indicate that an identity relation exists; or an active verb with a reflexive or reciprocal pronoun can be and is
used to specify the exact nature of the identity relation, if its specification is important;\textsuperscript{10} or--since both possibilities exist in the language--the middle can even be reinforced by a reflexive or reciprocal pronoun to lay exceptionally heavy stress on the identity (e.g. Xenophon, Anabasis 1.8.29, οἱ μὲν φοι βασιλέα κελεύοι τινα ἐπισκόπαν αὐτόν..., οἱ δὲ αὐτοῦ ἐπισκόπασθαι. hoi men (some) phasi (say) basilea (king) keleusai (to have ordered) tina (someone) episphaks-ai (to kill, active) auton (him), hoi d' (others) heauton (himself, reflexive pronoun) episphaks-asthai (to kill, middle). "Some say the king ordered someone to kill him..., others (say) he killed himself." [Smyth, 391 ]). The fact that Greek (not to mention the other Indo-European languages) slowly reorganized its voice system so that the role of identity-marking was taken over more and more completely by the pronouns, and so that the verb markers were specialized to act as NP promotion flags, is probably not unrelated to the obvious redundancy of the Greek system and to the greater specificity of the pronominal over the verbal marking system.

By now we have discovered how the active/middle distinction differs from the active/passive one--namely by being a strategy to handle a different underlying problem--and how languages like English dispose of the problem to which the middle voice is addressing itself--namely, by the use of pronouns rather than a verbal marker. But we have yet to discover how it comes that a language like Greek can use its middle voice to include the functions for which we use the passive. The passive, after all, has nothing to do with NP identities, being instead a strategy to move NP's in and out of subject position.

Such an amalgamation of functions seems to be explainable only in terms of a semantic restructuring of the middle and passive that has occurred fairly near the surface--a reanalysis which is probably possible only if, for both strategies, the option has been chosen to set a single flag in the verb instead of marking the appropriate NP's or NP slots. Of all the relationships that we have isolated so far for discussion, there is only one in which the subject is not doing the action expressed in the verb. That relationship is what is traditionally called the passive; and English pits the passive against all the other situations, which it lumps under "active" (plain active, plus reflexive and reciprocal). But similarly, of all these relationships, there is only one in which the subject is not affected by the action. That is what we might call the "plain" active (nonreflexive, non-reciprocal); and the Greek system of verbal voice pits this plain active against all the other situations, which it lumps under "middle." Since the terminology rapidly becomes confusing, thanks to the lumping of functions, the argument is more clearly presented in diagrams showing the relationship of the surface subject to the action expressed in the verb (which is, incidentally, the classic definition of voice):
PLAIN ACTIVE \[\text{SS} \rightarrow \text{Subject does action to someone or something else}\]
PASSIVE \[\text{SS} \leftarrow \text{Action is done to subject by someone or something else}\]
REFLEXIVE \[\text{SS} \leftrightarrow \text{Subject does action to itself}\]
RECIPROCAL \[\text{SS} \leftrightarrow \text{Members of plural subject do action to other members of pl. subj.}\]
FULL-DRESS MIDDLE \[\text{SS} \leftrightarrow \text{Subject does action to someone or something else, in such a way as to affect self also}\]

English groups the relationships according to an active/passive system as follows:

ACTIVE (outgoing arrows):
- Plain Active \[\text{SS} \rightarrow \]
- Reflexive \[\text{SS} \leftrightarrow \]
- Reciprocal \[\text{SS} \leftrightarrow \]
- Full Middle \[\text{SS} \leftrightarrow \]

PASSIVE (no outgoing arrows):
- Passive \[\text{SS} \leftarrow \]

That is to say, the active subsumes all the cases in which the subject is agent (including reflexive, reciprocal, and full middle), and the passive takes care of the remaining case, the one in which the subject is not performing the action. Greek, on the other hand, groups the relationships according to an active/middle system as follows:

ACTIVE (no incoming arrows):
- Plain Active \[\text{SS} \rightarrow \]
- Reflexive \[\text{SS} \leftrightarrow \]
- Reciprocal \[\text{SS} \leftrightarrow \]
- Passive \[\text{SS} \leftarrow \]

MIDDLE (incoming arrows):
- Full Middle \[\text{SS} \leftrightarrow \]
- Reflexive \[\text{SS} \leftrightarrow \]
- Reciprocal \[\text{SS} \leftrightarrow \]
- Passive \[\text{SS} \leftarrow \]

That is, the active represents the one case in which the subject is not specified as being affected by the action; whereas the middle subsumes all cases in which the subject is affected by the action—and in this way quite logically includes the passive as well as the reflexive and reciprocal.

Thus embedded in the middle system, of course, the passive is no longer obvious as a strategy devoted to moving NP's in and out of subject position. Nonetheless, it continues to solve the NP-moving problem well enough; it might even be possible to view the middle as shifting NP's, in the special case in which two NP's are identical (i.e., an NP gets moved up to and merged with the subject). So perhaps no more is required of the middle system by the synchronic exigencies of communicating.

Diachronically, however, one might wonder how an active/middle system could come about—in particular, whether it could ever arise from an active/passive system. We have the opposite transformation (from a middle to a passive system) well enough at-
tested in the histories of practically all the Indo-European languages: it involves the development of special pronouns as logically more specific ways of handling NP identities, together with the gradual restriction (or total reworking from a separate stative form) of a single voice-flag in the verbal complex to signal the one remaining relation, passive. But how proto-Indo-European got its middle system we have no way of knowing.

It may seem at first glance to be unlikely that an active/passive system could ever turn into an active/middle system. Yet the trend in Romance (and in Slavic) to enlarge the use of one-time reflexive pronouns to distinctly middle and even downright passive uses, as in French Je me lave les mains "I am washing my hands" (compare Greek λοώ-οµε τὰς χειρὰς I wash, middle) τὰς (the) kheiras (hands), constructed in a closely equivalent way) and Les portes se ferment à deux heures "The doors are closed at two o'clock," seem to demonstrate the possibility of going full circle.

A sentence like Les portes se ferment... raises another question, however. After all, French does have a true, if little-used, passive, with which such "false reflexive" expressions are in contrast: Les portes sont fermées par nous (et pas par le concierge) "The doors are closed by us (and not by the doorman)."

As in a true passive, the subject of a false reflexive is not the agent of the action (SS←), but this form differs in the implication that the subject is somehow bringing the action onto itself (SS⇒). Roughly the same difference exists in English between the plain passive and the so-called "get-passive"--

| PLAIN PASSIVE | SS← | The cat was scratched.  |
| GET PASSIVE  | SS⇒ | The window was broken. |
| The cat got scratched. |
| The window got broken. |

The choice of a form like The window got broken over The window was broken seems to imply that the window somehow brought the catastrophe onto itself—if only by being in the wrong place at the wrong time. Its presence, as it were, catalyses the action performed by a quite separate agent. This observation is reinforced by the notable ease with which the reflexive pronoun can be added to the get-passive (The cat got itself scratched), contrasted with the impossibility of doing so to the true passive.11

What we have here, then, would seem to be a logical extension of the surface analysis of the voice relationships, filling in the hole in the surface pattern. For we now have the three kinds of arrows represented singly

| PLAIN ACTIVE | SS→ |
| PLAIN PASSIVE | SS← |
| REFLEXIVE | SS⇒ |
and in every possible combination of two:

R ECIPROCAL  SS
F UL MIDDLE  SS
C A TALYTIC PASSIVE  SS

This last link in the chain not only provides a more satisfying analysis of the passively used reflexive forms of Romance—

CATALYTIC PASSIVE  SS
Fr. Les portes se ferment à deux heures. "The doors get (themselves) closed at two o'clock."

but also provides a logical route by which an active/passive system could regroup to become an active/middle system. The reflexive/reciprocal (that is, NP-identity) marker can be seen extending into agent-demotion (passive) territory, and becoming ripe for reanalysis as a mark for any situation in which the subject is affected by the action, whether or not it is also the agent of the action. As such it is becoming not just a simple intransitive marker, but a genuine middle voice marker in a nascent active/middle system.12

NOTES
1
I am excluding any consideration of the voice systems of ergative languages, treating only accusative languages.
2
The only formal distinction between passive and middle voice in Homeric Greek, for example, is in the aorist. Even in Classical Greek, there is no formal distinction of passive outside of the aorist and future tenses. Sanskrit shows a similar history of a very slow spread of the passive, but here the distinction begins in the present tense.
3
David Perlmutter and Paul Postal, Linguistic Institute, 1974.
4
Cf. Keenan and Comrie. Some languages, it seems, can promote from still less accessible positions, but only at the risk of considerable grammatical ambiguity.
5
Except in those few dialects in which one can say such sentences as It was given him. Not all languages are so unambiguous in this regard, however.
6
Cf. Lyons, 373, or Smyth, 392 (sec. 1728); see Smyth also for numerous examples, including many of the standard ones used here.
7
Given the NP positions accessible to this identity-marking operation in Greek. These seem to be: direct object, indirect object, and genitive (possessor) of direct object (as in λού-ομεν)
τὰς χεῖρας lou-omai (I wash, middle) tas (the) kheiras (hands) "I am washing my hands").

8 Of course, we have other occasional tricks, such as substituting an entirely different verb with a different semantic structure (e.g. kill/die), or using the same surface morpheme sometimes transitively, sometimes intransitively (e.g. break).

9 French, for example, uses a form like se for reflexive or reciprocal direct or indirect object, indistinguishably. Special forms can be added to specify reflexive or reciprocal, if necessary, but are normally omitted.

10 At least, these seem to be the intentions. Unfortunately, there are no more native speakers of ancient Greek to verify such interpretations.

11 Pointed out to me by Edward Keenan.

12 Russian -сб / -сра, the so-called reflexive marker, has gone much the same route. Since this verbal suffix continues to contrast with the free-standing reflexive pronoun сеbara derived from the same PIE source, however, the resulting voice system is much more complex, not to say fragmented, than in Romance.

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Coordination Reduction and the English Comparative/Superlative: A Psycholinguistic Perspective
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Introduction
It is the concern of linguistics to develop general and revealing theories of language. The linguist seeks first to provide a theoretical structure capable of treating relevant data (e.g., acceptability intuitions, occurrence in given contexts, etc.) and second to select the most highly valued adequate descriptive theory (for discussion see Chomsky, 1957, 1965; and many other sources). As part of the latter goal the linguist may concern himself with the possible role of linguistic grammars in a general psychological theory of mind (cf., Chomsky, 1968).

If the linguist makes this commitment, it is incumbent on him to 'integrate' his theories of language with other relevant psychological theories (e.g., theories of perception, memory, social organization, etc.). For example, a general principle of perception need not be stated in grammar even if it can be shown to be the efficient cause of some fact about language behavior. This would be imparsimonious for and even deleterious to the development of a general theory of mind.

One independent line of argument for integrated approaches to the study of language is that very frequently we find that parcelling out the description of language phenomena leads to the discovery of generalizations which may have been obscured otherwise. In this paper we will specifically show that English graded adjective conjoined structures can be handled exceptionlessly within the universal syntactic schemata of coordination reduction proposed by Harries (1973). We will further argue that some of the data which have previously been considered problematic in the analysis of these structures (and which thereby obscured the simplest grammatical treatment) are accountable in accord with maximally general principles of speech production and perception and of conversation.

On the Grammar of Conjoined Graded Adjectives
The basic facts concerning graded adjectives, that is comparatives and superlatives, are well known. In English there are two types of grade markings, periphrastic (as in 1d, f) and inflectional (as in 1a,c). There are strong constraints on the form of the

1a. biggest  c. handsomest  *e. beautifullest
*b. most big  d. most handsome  f. most beautiful

grade marker which an adjective may take. These are largely governed by the phonological shape of the adjective itself (Jesperson, 1933). It is also known that there is an interaction between the form of the grade marker and the operation or non-operation of certain syntactic rules, specifically Coordination Reduction and Reduplication.
Zwicky (1969) noticed that coordinate reduced graded adjective structures occur only with a periphrastic grade marker. Thus, 2a, but not 2b. He also noted that reduplication is possible only

2a. That one is nice, but I want something more plain and simple.
b.*That one is nice, but I want something plain and simpler.

where the same form of the adjective grade appears throughout the reduplicate string. Thus 3a and b, but not c. Zwicky discusses

3a. The children got more and more and more noisy.
b. The children got noisier and noisier and noisier.
c.*The children got noisier and noisier and more noisy.

these facts as a case of apparently phonological constraints on the operation of syntactic rules. Specifically, the data would seem to require the ordering of a phonologically conditioned rule of Adjective Inflection prior to the syntactic rules of Coordination Reduction and Reduplication.

In order to avoid such a solution, Zwicky proposes that each of the potential inputs to Adjective Inflection, which he describes as a minor rule, is marked as such in the lexicon by a rule feature (placed on the final morpheme of the adjective). Coordination Reduction is also constrained by this feature, failing to operate if its input string includes an adjective marked for the rule. The constraint on Reduplication is accounted for in that the rule feature is copied along with the adjective it marks. Adjective Inflection can thus be ordered after the syntax and the desired separation between syntax and phonology can be maintained.

This analysis presents several difficulties. It is not clear that the solution is satisfactory theoretically, since it requires that a syntactic rule be permitted to be made sensitive to the occurrence of a lexical rule feature whose referant is ordered later in the grammar (in fact, in another component of the grammar). This would commit us to a theory of grammar which admits global constraints. Further, the analysis, even if accepted in principle, proves unworkable in fact. Consider the sentences of 4.

4a. That's the most red and juicy apple I've ever eaten.
b. That's the most red and succulent apple I've ever eaten.
c. Harry is the most polite and attentive student in class.
d. Harry is the most thoughtful and attentive student in class.

Of these, Zwicky's rules would generate only 4d. This is because "red", "juicy" and "polite" would all be marked to undergo Adjective Inflection. Yet 4b is at least marginally acceptable, and 4c is quite definitely acceptable.

In the case of Reduplication, the rules fail to properly constrain precisely those cases which motivated the analysis. If the adjective in question requires either one or the other form of the grade marker, these would be derived uniformly in any case. Those adjectives which may take either form of the grade marker
(eg. "noisy" in 3, "polite" in 4c) would have to be marked in the lexicon [\+ Adjective Inflection] or some such. Thus, after Reduplication operates there would still remain the possibility of wrongly deriving a mixed string, as in 3c.

It seems to us that a major source of these difficulties resides in Zwicky's initial assumptions. He works from the viewpoint that Adjective Inflection bleeds Coordination Reduction. There is some evidence that this is not the case. Zwicky claims that adjectives which undergo Adjective Inflection do not appear in coordinate reduced structures. But consider again the sentences of 4. Both 4b and 4c contain adjectives which are lexically marked to undergo Adjective Inflection, either optionally or obligatorily. Yet these sentences are acceptable.

Zwicky claims that his treatment of the relation between Coordination Reduction and Adjective Inflection is consonant with a general principle that Coordination Reduction does not apply to proper parts of words. While this principle appears to be a valid generalization for graded adjectives, it is not true in the seemingly parallel case of genitives, where coordinate reduced structures occur with both periphrastic and inflectional markers, as in 5.

5a. The coat-of-arms of the king and queen is on the gate at Buckingham Palace.

b. Bill and John's boat was sunk in a storm last week.

While this evidence is by no means definitive, it is at least suggestive that the direction of the rule conditioning is not necessarily the one Zwicky assumes. In our analysis Coordination Reduction constrains Adjective Inflection.

If such an analysis is allowed, the grammatical description becomes quite simple. Coordination Reduction is ordered before Adjective Inflection. Following Harries (1973) the universal syntactic schema of Coordination Reduction is a two-step process of 'deletion' and 'regrouping'. Deletion operates by removing all but the leftmost of the identical constituents of the conjuncts of a coordinate structure. In each reduced conjunct regrouping Chomsky-adjoints the lowest node that exhaustively dominates all lexical nodes to the corresponding node in the unreduced conjunct. In the cases under discussion this would yield structures of the type shown in 6. Adjective Inflection is constrained to operate only

![Diagram](image)

on inputs where the grade marker and the adjective are both dominated by a single node, structures of the type shown in 7. Where Coordina-
tion Reduction has already applied Adjective Inflection is blocked,

```
    Adj.
   /   \
Grade  Adj.
   ...   ...
```
because potential inputs no longer meet the structural description of the rule.

This analysis is desirable in that it orders Adjective Inflection after the syntax without appeal to global constraints. Further, it allows a major syntactical rule, that is, Coordination Reduction, to operate exceptionlessly, avoiding ad hoc constraints. However, it is not without apparent problems. While it allows the generation of 4c and 4d, it will also generate the other sentences of 4, affording us no basis for distinguishing among sentences which obviously differ in acceptability. We are also apparently left without an account of the observed constraint on Reduplication. However, at this point the analysis is incomplete.

A Psycholinguistic Perspective

To this point we have argued that the data for conjoined comparative and superlative structures is more complex than as originally countenanced by Zwicky. However, we have also proposed that these structures be generated exceptionlessly according to the universal schema of coordination reduction. This has led us to an apparent difficulty in that we predict the grammaticality of structures like 8b, which are in fact not fully acceptable for all speakers, and conversely that we fail to generate forms like 8d which we

8a.*This moon is the most big and most bright I've ever seen.
b.?This moon is the most big and bright I've ever seen.
c. This moon is the biggest and brightest I've ever seen.
d.*This moon is the big and brightest I've ever seen.

have observed in actual speech. We believe the difficulty to be only apparent. We propose to deal with these cases by appeal to the perceptual principle of 'gobbling' and the speech production principle of 'analogy'. Since these principles have already been independently motivated in papers by Langendoen and Bever (1973), Carroll (1974, 1975) and Bever, Carroll and Hurtig (1975), this approach allows us to simultaneously pursue generalizations both in our account of the grammatical structure of conjoined comparative and superlative forms and in our description of the structure of linguistic performance theory.

The principle of gobbling defined in 9 predicts the reduced acceptability of 8b. The periphrastic superlative in examples like

9 Modifying elements tend to be parsed as co-constituent with most adjacent potential constituent heads.
this is gobbled up by the most adjacent potential head, namely the first adjective of the conjoined structure. Of course, this sort of gobbling can obtain in forms like 10b as well. However, in 10b the parsed constituent 'more beautiful' is still perfectly accept-

10a? She is more beautiful and more graceful than I remembered.  
b She is more beautiful and graceful than I remembered.  
c* She is beautifuller and gracefuller than I remembered.  
d* She is beautiful and gracefuller than I remembered.

able whereas in 8b the resultant form 'most big' is not. In order for sentences like 8b and 10b to make sense, the grade marker must semantically distribute over all members of the conjunction it syntactically dominates. This assumption of 'distributivity' plus the general principle of gobbling will distinguish 8b and 10b vis-a-vis acceptability judgments. Gobbling renders 8b unacceptable but does not so mark 10b because 'more beautiful' is a possible constituent whereas 'most big' is not. Distributivity then saves 10b with respect to acceptability by providing the proper semantic reading in spite of perceptual gobbling. Distributivity provides a semantically sensible interpretation for 8b as well but does not mitigate its unacceptability (ie, since 'most big' is out whereas 'more beautiful' is in).

There are several independent facts which support the foregoing proposal. For example, consider the 'absolute' superlative paradigm in 11. These forms recruit the superlative morphology

11a? A most big and most bright silver platter fell on Mort.  
b A most big and bright silver platter fell on Mort.  
c* A biggest and brightest silver platter fell on Mort.  
d* A big and brightest silver platter fell on Mort.

but to different semantic ends. All inflectional forms are unacceptable (contrast 11c with 8c) whereas all periphrastic forms are acceptable (contrast 11a with 8a). Gobbling is predicted in 11b just as it is in 8b and 10b. However, as noted above, 10b is acceptable in that "more beautiful" is acceptable and 8b is questionable in that 'most big' is unacceptable. The sequence 'most big' is, however, acceptable in the absolute superlative whereas it is not in the ordinary superlative. The gobbling analysis, therefore can explain the acceptability contrast between 8b and 11b.

Returning now to ordinary comparative/superlatives we would like to explore a consequence of our hypothesized principle of 'distributivity'. Since the periphrastic marker in forms like 8b and 10b must distribute in order for them to make sense, we might expect a potential weak derived gobbling effect involving the grade marker and the second member of the adjective conjunction. This tentative principle does find support in contrasts like 12 versus 8b. Since the second adjective in 12 may only take the

12 This moon is the most big and brilliant I've ever seen.
periphrastic grade marker, any gobbling which may obtain in forms like 8b between the grade marker and the second conjunct will not obtain in 12, hence 12 should be more acceptable. We refer to this principle as 'distributive gobbling'. We turn now to conjoined genitives in order to demonstrate the generality of the preceding analysis of conjoined grade adjectives.

Consider 13. Our argument concerning gobbling predicts a per-
ceptual misparsing in which the scope of the genitive inflection in 13a and 13c is erroneously taken to be only the right-most NP. Similarly, we predict a tendency for the periphrastic genitive in 13b and 13d to be interpreted as only attached to the left-most NP of the conjunct pair. Of course, when gobbling does not prevail, the entire coordinate structure becomes the genitive head. In general, periphrastic genitives are more acceptable with 'heavier' NPs and inflectional genitives are more acceptable with smaller NPs. On these grounds, the gobbling interpretation should be more likely to obtain in 13c than in 13d. We find that this agrees with our judgments. Now contrast 13b with 13d. Since 'the King of France' is heavier than 'Jack' we would predict that the gobbling interpretation is also more likely in 13b than it is in 13d. We also find this to accord with our judgments. This fact also gives us a way to explore the validity of distributive gobbling with conjoined genitive forms.

Our proposal concerning distributive gobbling would predict that in so far as genitive gobbling obtains and extra-sentential context demands that the distributive semantic reading be made, then forms like 14a and b will be more acceptable than forms like

14a The mother of Jack and The King of France told them to wield power.
14b The mother of Jack and the Student Council President told them to wield power.

13d. The periphrastic genitive disfavors short NP heads like 'Jack'. Hence gobbling would be predicted in any of the forms, 14a, 14b or 13d, and this will result in some degree of unacceptability. If a distributive gobbling effect can be detected, it would presumably manifest an even greater unacceptability for forms like 13d. In 14a and 14b the second NP better comports with the periphrastic genitive. An analogous line of thinking would predict 15a to be more acceptable than 15b when perceptual gobbling obtains but the distributive reading is required. While
15a Bill and the Student Council President's mother told them to wash up.

b The football team captain and the Student Council President’s mother told them to wash up.

we find that our distributive gobbling predictions are borne out, the acceptability judgments required are so subtle that we regard this confirmation as tentative. 3

We have argued that 8b can be viewed as fully grammatical but reduced in acceptability in virtue of perceptual gobbling. We shall now propose that 8d may be viewed as ungrammatical but to some extent acceptable or utterable in virtue of 'analogy'.
Analog as defined in 16 can provide the speaker with acceptable

16 A 'grammatical' sequence X may be analogically replaced by (ie. derived into) an 'acceptable' sequence Y when, through the agency of some linguistically relevant but extra-grammatical system, X becomes marked as 'unacceptable'.
Condition: The replacement sequence which is selected by analogy will be (1) structurally most related to, and (2) semantically most similar to the original sequence X.

or utterable alternative forms for grammatical but potentially difficult strings. The sorts of difficulties that can motivate analogical replacements range from anticipated perceptual problems for the hearer, like ambiguity (cf. Carroll, 1974,1975) to articulatory and clause-planning problems for the speaker (cf. Bever, Carroll and Hurtig, 1975).

In the present case, the grammar generates forms like 8b although these forms are found to present some measure of perceptual difficulty due to gobbling (cf. Bever et al, 1975, for discussion of similar cases). The speaker's tacit knowledge of this difficulty puts him in a position to choose an alternative output form in accordance with the analogy principle. One alternative which seems to satisfy both conditions of the principle is that of blocking coordination reduction altogether, yielding a form like 8c. However, given that an analogical alternative to 8b is at issue, another possibility suggests itself. A form like 8d may satisfy condition 1 of the analogy principle better than 8c. The 'derivation' of 8d must violate the morphological spelling out rule which specifies that only periphrastic grade markers may obtain in coordinate reduced structures. However, since the syntactic derivation of 8d includes coordination reduction, which the derivation of 8c does not, the form 8d may therefore be structurally more similar to 8b than 8c is.

The same left-ward semantic distributivity operating in strings like 13c permits the analogical replacement of 8d to also satisfy condition 2 on the analogy principle. In so far as distributivity obtains forms like 8d are acceptable but ungrammatical (cf,Langendoen and Bever, 1973, for discussion of this category). However, on our judgments this distributivity applies only imperfectly. In practice, forms like 8d are likely to violate
condition 2 and resultingy are not particularly good analogical forms. They are clearly not as acceptable as the 'not un-' form discussed by Langendon and Bever (1973) and the 'good and' form discussed by Carroll (1974, 1975). These two forms are exemplified in 17 below.

17 a. A not unhappy man entered the room.
b. The waiters here aren't good, but they're good and greedy.

Despite the gobbling problem, we find that forms like 8b are more commonly used in our dialects.

In considering analogical replacements it is important, however, to distinguish between 'utterability' and 'acceptability'. While the latter datum represents a more selective criterion for well-formedness in the language, the former also provides insight into the dynamic structure of language. Bever et al (1975) contrast 'utterable' analogical forms like 18 with fully 'acceptable' analogies like 19. (See Bever et al, (1975) for a discussion of

18 a. I really enjoyed flying in an airplane that I understood how it worked.
b. Both John, Harry and Mike stopped over yesterday for some cold meatballs.
c. Sheila and the guard wanted each other to meet in Miami.

19 a. Lewis wanted to try and scale the mission wall.
b. Everyone forgot their coat.
c. That Herbie is boring disturbs Aunt Alice has been a joke in our family for years.

this distinction in linguistic performance theory.) Forms like 8d seem to be analogical forms which are merely utterable and usually not fully acceptable. One reason for this might be that the left-ward semantic distributivity which must necessarily obtain in order for the form to meet condition 2 does not in practice reliably obtain. Another reason may be that various preferable forms do exist such as 8b and 8c (this view is also considered by Bever et al)

We have observed other utterable but unacceptable grade forms such as 20.

20 The Post Office in La Jolla is clearly the most .""(pause)"".big.

The effective instigator of cases like this seems to be speech production planning error. The grade marker is realized prematurely in its most general form, the periphrastic. However, the adjective (when it comes, usually after a pause) doesn’t fit. Perhaps forms like 8d and 20 should be viewed as complementary speech production processes. In 20 through a planning error the speech production system codifies the grade marking when it should
have waited for the adjective stem (in order to affix an inflectional marker). While in 8d the grade marking is not codified soon enough and must be analogically adjoined to the final conjunct.

The foregoing analysis constitutes a very strong claim in terms of the coding trade-off between wholistic linguistic representations and the linear sequence of sounds by which they are realized. Since there is presently no relevant data with which to evaluate the claim, we must simply reserve judgment.

Finally, we return to Zwicky's observation concerning strings like 3. Grammatically we appear to be forced to generate structures like 3c or else to write a context sensitive morphological spelling out rule which blocks them very late in the grammar. Here again we believe the difficulty to be only apparent. A more general principle appears to be at work. We propose that forms like 3c are grammatical but unacceptable because they violate a general conversational principle (cf. Grice, 1968 for a discussion of conversational rules) governing the relevant formal symmetries of conjoined structures (cf. Kuno (1974) Schachter (1974); Bever et al, 1975). We claim, then, that 3c is unacceptable on the same grounds that 21 and 22 are. By treating 3c in this way

*21 a That girl is like a child but sphinxlike
    b That child is childlike but like a sphinx.
*22 a Mary like cooking and to clean
    b Mary likes to cook and cleaning

we make a generalization which would be obscured if the facts were dealt with by Zwicky's rule feature solution or context-sensitive spelling out.

Moreover, there is independent motivation for such a claim. Kuno (1974) has noted the following examples:

23 a. John likes flying airplanes and murdering cossacks.
    b.?John likes chopping firewood and flickering campfires.

24 a. The dinner is ready to eat and so is the salad.
    b.?The dinner is ready to eat and so are the guests.

25 a. John expected Mary's departure and Jane's arrival.
    b.?John expected Mary's departure and Jane would arrive.

26 a. John likes Mary and Bill.
    b.?John likes Mary and singing songs.

Kuno observed that coordinate conjunction is most acceptable with conjuncts having parallel structures. He suggests further that when ambiguity is involved parallel structures are more acceptable under parallel interpretations as in 23.

Schachter (ms) has detailed this proposal in his discussion of the "Coordinate Constituent Constraint" (the CCC):
The constituents of a coordinate construction must belong to the same syntactic category and have the same semantic function.

In his consideration of this principle, he notes the following contrasts:

27 a. John ate quickly and greedily.
   b. John ate quickly and a grilled cheese sandwich.

28 a. It's odd that John is busy and that Helen is idle now.
   b. It's odd for John to be busy and for Helen to be idle now.
   c. It's odd that John is busy and for Helen to be idle now.

29 a. Bobby is the man who was defeated by Billie Jean and who beat Margaret.
   b. Bobby is the man defeated by Billie Jean and who beat Margaret.

Schachter suggests that the CCC is itself perceptually motivated. He accepts the speech perception model of Bever (1970), Grosu (1972) and others which rests on 'perceptual principles'. These principles provide segmentation and mapping hypotheses for input verbal material. Schachter appeals to Grosu's (1972) 'perceptual conflict' principle (which is related to Bever's (1970) 'double function' principle). This principle asserts "that complexity arises when two sets of cues assign contradictory values to a stimulus in terms of some parameter." (Grosu, 1972, p. 2) Such complexity can presumably render linguistic forms perceptually unacceptable. Schachter notes that the principle would therefore have the effect of disfavoring or blocking conflict forms in speech.

Schachter points out that coordinate conjunction implicitly assigns 'equal rank' to conjuncts. However, if the coordinate conjoined constituents differ either in syntactic category or semantic function, they are simultaneously being 'equated' and 'contrasted'. The result of this is perceptual conflict and ultimately speaker/hearer judgments of unacceptability.

**Conclusion**

The analysis we have given here is, of course, in no sense a decisive argument for integrated approaches to the study of language. Phenomena we would analyze by appeal to the functional interaction of theoretically separable verbal systems will always have corresponding analyses in 'all-inclusive' grammars. The explanation of this resides in the formal power of the grammars in question (cf, Peters and Ritchie, 1974). This issue might become empirical if grammars could be constrained in ways that would reduce their enormous expressive power. We have explored one such constraint here, namely that phonological rules are not ordered within the syntax in the transformational grammars of natural languages (cf, Zwicky and Pullum, forthcoming, for a more complete
discussion). The fact that we are led to this analysis by adopting an integrated model we take as a prima facie argument in favor of this approach to linguistic research.

In addition, we find the intuitive appeal of the integrated approach enormous. As it is obvious that the speaker of a language knows something complex and abstract, it is obvious that much that is not uniquely linguistic is involved in speaking and understanding sentences. Phenomena that are, e.g., fundamentally perceptual ought to be treated in a perceptual theory. We propose that linguistic grammars should describe not all that is relevant to language, but just that which is specific to it.

Footnotes

1 We would like to thank Thomas Bever, Lucia Kellar, Geoffrey Pullum, Rick Wojcik and especially Arnold Zwicky for helpful comments on this research. They should not, of course, be implicated in any blunders we made in spite of them.

2 One way to avoid this misparsing would be to repeat the genitive inflection as in i, ii, iii and iv. While these forms are per-

i The King of England's and King of France's mother...
ii The mother of the King of France and of the King of Eng-
land....
iii Jack's and Bill's mother...
iv The mother of Bill and of Jack...

spicuous, they may be stylistically awkward. This observation also bears on the status of 10a and 11a above.

3 Note also that the support we can derive from an example like 15 is confounded by word length.

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ACCEPTABLE AMBIGUITY
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In a recent article entitled "Unacceptable Ambiguity" (Linguistic Inquiry, 1973, IV/1) Hankamer proposes the No-Ambiguity Condition (NAC), which states:

I. Any application of Gapping which would yield an output structure identical to a structure derivable by Gapping from another source, but with the "gap" at the left extremity, is disallowed. (p.29)

The intent of this condition is to restrict Gapping in such a way that the deletion site must be as far to the left as structurally possible. Thus, the interpretation of 1a must always be 1b, and not 1c:

1.(57)a. Jack calls Joe Mike and Sam Harry.
    b. ... and [Jack calls] Sam Harry.
    c. ... and Sam *[calls Joe] Harry.

The first part of the article (pp.17-35) is devoted to exposition and justification of the NAC. It is clear that Hankamer takes it as being valid for English, and, although he stops short of a categorical assertion that it is a universal condition, he comes as close as possible to that position:

II.a. I consider it established, then, that the NAC is empirically supported, and must be included in an adequate account of natural language. (p.36)
    b. If the NAC on Gapping is a universal constraint, as I assume, ... . (p.37)
    c. Although I have largely restricted myself to examples from English in demonstrating the operation of the NAC, exactly the same condition restricts Gapping in other languages, and there is every reason to believe that it is universal. (p.38)

This paper takes issue with these claims. In particular, I shall attempt to show that the NAC as formulated is certainly not a universal condition, and that its validity even for English is not as uncontroversial as one might suppose.

Looking briefly at English, it seems clear that there are several problematical cases which arise. The first of these is a set of three types of counterexamples which Hankamer himself raises in his article:

3. (86) Max writes plays in the bedroom, and Harvey in the basement.

4. (89) Paul Schacter has informed me that the basic order in Tagalog and related languages is VOS; Ives Goddard that the unmarked order in Algonkian is OVS; and Guy Carden that the basic order in Aleut is OSV.

In the case of 2/(68) and 4/(89) Hankamer finds arguments to get around these possible counterexamples. He says that in 2/(68) Congressman is not an NP (he does not say what else it might be) and that in 4/(89) me is not an NP (it has become a clitic of the verb); therefore they do not violate the NAC. But these examples cannot be dismissed so easily. In an earlier paper (1975), I have given arguments -- which cannot be repeated here for lack of space -- showing that Congressman is in fact an NP, and that sentences like 4/(89) exist with undisputed noun phrases as well as with possible-clitic pronouns. As for 3/(86), Hankamer connects the problem with strict subcategorization and the generic nature of plays, but ultimately concedes that this sentence does violate the NAC as formulated and that an ad hoc condition will have to be placed on it:

III. In the absence of some general explanation, it will be necessary to modify the NAC in some way to allow the derivation of sentences like (86). (pp.34-35)

It seems, then, that sentences of these three classes stand as counterexamples to the NAC.

A second difficulty for the NAC in English is that there seem to be gradations of acceptability among sentences which the NAC rules out in an undifferentiated way. Thus, according to the NAC, 1/(57)b, 5/(60)b, 6/(59)b, 7/(38)b, 8b, and 9b — with a left-peripheral gap — are possible, while the corresponding c-readings — with an internal gap — should all be equally unacceptable. This, however, is not the case.

5. (60)a. The press characterized Agnew as colorless, and Nixon as low-keyed.
   b. ... and [the press characterized] Nixon as low-keyed.
   c. ... and Nixon *[characterized Agnew] as low-keyed.

6. (59)a. Max wanted to put the eggplant on the table, and Harvey in the sink.
   b. ... and [Max wanted to put] Harvey in the sink.
   c. ... and Harvey *[wanted to put the eggplant] in the sink.

7. (38)a. Max gave Sally a nickel, and Harvey a dime.
   b. ... and [Max gave] Harvey a dime.
   c. ... and Harvey *[gave Sally] a dime.

8. a. Alice made the teacher a knitted scarf, and Eric a
charm bracelet.
b. ... and [Alice made] Eric a charm bracelet.
c. ... and Eric [made the teacher] a charm bracelet.

9.a. Tom told Margaret that Sally was coming, and Bill that she wasn't.
b. ... and [Tom told] Bill that she wasn't.
c. ... and Bill [told Margaret] that she wasn't.

Sentences 1/(57), 5/(60), 6/(59), 7/(38), and some others were tested on a group of informants. For 1/(57) and 5/(60), while there were sporadic c-interpretations, the subjects always made the b-interpretations, and the results correlated well with the prediction of the NAC. For 6/(59), all subjects made the b-interpretation, but a large number also made the c-interpretation; this, however, I attribute to semantic interference, since the b-reading is semantically anomalous, and therefore I shall assume that this example, too, is in accord with the NAC. For 7/(38), however, all subjects still made the b-interpretation, but in addition over half also made the c-interpretation. Here I see no appeal to interference from outside syntax, and I conclude that, at least for some speakers, the NAC does not make the correct predictions about the interpretation of 7/(38). Sentences 8 and 9 were not in the test corpus, but I suspect that those who accept 7/(38)c will also accept 8c and 9c, since the sentence types are quite similar. Furthermore, even some of the speakers who found all of the c-interpretations unacceptable in the absolute sense felt that 7/(38)c and some others similar to it were somehow not as bad as 1/(57)c or 5/(60)c. The precise rankings and cutoff point of acceptability vary from speaker to speaker, but the very fact that such variation is possible indicates that we need more than just a blanket prohibition against structural ambiguity.

A third problem for the NAC in English is that the acceptability of certain gappings which are ruled out by the NAC seems to vary with the heaviness -- and perhaps also other properties -- of what is left behind, at least for some speakers. Sentence 3/(86) cited above probably belongs to this category, which is further illustrated by 10-14.4

10. ??Tom reads the newspaper quickly, and Sandra carefully.
11. ??Tom reads the newspaper over breakfast, and Sandra over lunch.
12. Tom reads the newspaper in the morning, and Sandra in the afternoon.
13. Tom reads the newspaper on the way to work, and Sandra after she gets there.
14. Tom reads the newspaper in the morning on the way to work, and Sandra over coffee after she gets there.

Here again, simple comparison of structures as envisioned in the
NAC is inadequate to differentiate among the examples.

The facts which we have just been looking at cast some doubt on the validity of the NAC for English; it may hold for some speakers, but it does not hold for others, and therefore an unqualified claim that English obeys the NAC is too strong.

In discussing the English data above it has been necessary to rely on delicate differences in acceptability which vary from speaker to speaker. In addition, as was pointed out earlier, it is sometimes necessary to motivate an interpretation by supplying stress, intonation, or context. Because of this, it may seem that these arguments are weak or marginal. I would therefore like to turn to some Russian data which show indisputably that the NAC cannot be a universal condition.

Sentence 15 shows an example of a verb which is subcategorized for three noun phrases, and the sentences in 16 show gapped sentences constructed from sentences like 15:

15. Vanja predstavil Sαšu Maše.
   V.-NOM introduced S.-ACC M.-DAT
   Vanya introduced Sasha to Masha.

   V.-NOM introduced S.-ACC M.-DAT and L.-ACC S.-DAT
   ... and [Vanya introduced] Lenya to Serezha.

   b. Vanja predstavil Sαšu Maše, a Lenja Sereža.
   V.-NOM introduced S.-ACC M.-DAT and L.-NOM S.-DAT
   ... and Lenya [introduced Sasha] to Serezha.

   c. Vanja predstavil Sαšu Maše, a Lenja Serežu.
   V.-NOM introduced S.-ACC M.-DAT and L.-NOM S.-ACC
   ... and Lenya [introduced] Serezha [to Masha].

16a has a left-peripheral gap, 16b has an internal gap, and 16c has a discontinuous gap including the right periphery. All three are normal, unimpeachable sentences of Russian; 16b and 16c also are counterexamples to the NAC. Hankamer states, referring to the NAC,

IV. In short, the condition insures that if the output of Gapping is structurally interpretable with a left-peripheral gap, it will be so interpreted, even if this interpretation forces a reading on which the sentence is ungrammatical because of a selectional or agreement violation... (p.30)

16a shows that there is structurally a left-peripheral interpretation for the sentences in 16, but no such interpretation is assigned to 16b or 16c. It may be thought that the morphological information contained in 16b and 16c accounts for their failure to obey the NAC by disambiguating otherwise-identical structures. Hankamer does not specify just which level of structure is to be
compared under the NAC, but he makes reference only to configurations of major-category nodes like V and NP, and he does make it clear that he is not talking about terminal strings. Further, he seems to rule out morphology when he says:

V. The fact that it is structural ambiguity which is prohibited, and that semantic or morphological disambiguation cannot force the suppressed structural interpretation, will be of significance later in distinguishing the kind of ambiguity avoidance observed here from other phenomena of a quite different nature. (p.31)

On the basis of sentences like those in 16, it would seem that at the very least the NAC must be modified so as to be sensitive to morphological information, since it is precisely that information which allows the sentences to be interpreted in only one way, and therefore makes them not ambiguous. Such a modification could be made without too much difficulty, but even this step will not save the NAC, because the morphological information contained in the surface structure is not always unambiguous. Sentence 17/(84) is put forward by Hankamer as an example which is ruled out by the NAC; however a similar Russian example, 18, is perfectly acceptable.

17.(84) *The chess club elected Tony treasurer, and the outing club president.
18.a. Literaturnyj kružok vybral Sašu predsedatelem, a ekonomičeskij kružok sekretarem.
   literature club elected Sasha chairman and economics club secretary
   The literature club elected Sasha chairman, and the economics club secretary.
b. *...a ekonomičeskij kružok sekretarem
   and economics-ACC club-ACC secretary-INST
   ...*and [the literature club elected] the economics club secretary.
c. ...a ekonomičeskij kružok sekretarem,
   and economics-NOM club-NOM secretary-INST
   ...and the economics club elected Sasha] secretary.

Here the morphology does not help, since ekonomičeskij kružok can be either nominative or accusative. The accusative reading 18b has a left-peripheral gap and is the one predicted by the NAC; although structurally possible, it is of course semantically absurd, and is rejected by speakers of Russian. The nominative reading 18c has an internal gap and is explicitly ruled out by the NAC, since a left-peripheral gap is structurally possible; but this interpretation is the normal one, and is the only one which is commonly made. Further cases where Gapping produces both structural and semantic ambiguity in Russian -- and where
the morphology cannot help — are given in 19-22. Examples 19 and 20 are simple sentences of the same type as 15.

19. Smirnova predstavila Ivanova Petrovu.
   S.-f-NOM introduced I.-m-ACC P.-m-DAT
   Ms. Smirnov introduced Mr. Ivanov to Mr. Petrov.

20. Smirnov predstavil Donskoy Kostrovu.
   S.-m-NOM introduced D.-f-DAT K.-f-ACC
   Mr. Smirnov introduced Ms. Kostrov to Ms. Donskoy.

If we make 19 and 20 into right conjuncts, we can gap them and get 21 and 22; but these sentences are now structurally and semantically three ways ambiguous.

21. Smirnova predstavila Orlovskogo Maksimovoj,
    a Ivanova Petrovu.
   S.-f-NOM introduced O.-m-ACC M.-f-DAT
    a. and I.-m-ACC P.-m-DAT
    b. and I.-f-NOM P.-m-DAT
    c. and I.-f-NOM P.-f-ACC
   Ms. Smirnov introduced Mr. Orlovsky to Ms. Maksimov,...
    a. ... and ✓[Ms. Smirnov introduced] Mr. Ivanov to Mr.
       Petrov.
    b. ... and Ms. Ivanov ✓[introduced Mr. Orlovsky] to Mr.
       Petrov.
    c. ... and Ms. Ivanov ✓[introduced] Ms. Petrov [to Ms.
       Maksimov].

22. Smirnov predstavil Orlovskogo Maksimovoj,
    a Donskoy Kostrovu.
   S.-m-NOM introduced O.-m-ACC M.-f-DAT
    a. and D.-f-DAT K.-f-ACC
    b. and D.-m-NOM K.-m-DAT
    c. and D.-m-NOM K.-f-ACC
   Mr. Smirnov introduced Mr. Orlovsky to Ms. Maksimov, ...
    a. ... and ✓[Mr. Smirnov introduced] Ms. Kostrov to
       Ms. Donskoy.
    b. ... and Mr. Donskoy ✓[introduced Mr. Orlovsky] to
       Mr. Kostrov.
    c. ... and Mr. Donskoy ✓[introduced] Ms. Kostrov [to
       Ms. Maksimov].

The three interpretations of 21 and 22 are all normal, acceptable readings for these sentences, and correspond to three different sources for the reduced clauses. Only one of these in each case is the sentence from which we started: 19 lies behind 21a and 20 lies behind 22a. The b and c sentences come from other sources, which have internal and right-peripheral gaps, respectively. This is in direct contradiction to the NAC, which asserts that 21a and 22a are the only allowable readings and that 21b, 21c, 22b and 22c are consequently impossible.
The existence of sentences like 21 and 22, as well as 18, also contradicts a further claim made by Hankamer. Generalizing from the NAC, he proposes the Structural Recoverability Hypothesis (SRH) and a Structural Recoverability Condition (SRC) based on it. These attempts to limit the application of deletion rules by prohibiting them from introducing structural ambiguity, since structural ambiguity introduced by deletion rules, according to Hankamer, "is not tolerated in natural language." (p.31)

VI.a. SRH: Deletion rules involving variables are universally subject to a transderivational condition which prevents them from applying in such a way as to introduce structural ambiguity. (p.40)

b. SRC: If a deletion rule operating over a variable would introduce structural ambiguity by yielding the same output upon application to two different sources, both applications of the rule are blocked. (p.41)

It is apparent from 18, 21 and 22 that Gapping in Russian does operate in such a way as to introduce ambiguity, both structural and semantic, and that no application of the rule to the possible sources of those sentences is blocked.

Counterevidence to the NAC can also be found in other languages. Cases from Polish where the NAC (and also the SRH and SRC) encounter difficulty have been cited by Fedorowicz-Bacz (1973). She points out three types of constructions: those which depend on morphological syncretism, like the ones we have seen in Russian; those which involve źe complements; and those which involve źeby complements. These latter two types do not carry over into Russian, but provide interesting additional evidence against the NAC, of a type quite different from the morphological examples. It is not surprising that examples based on morphology turn up in Polish as well as in Russian, and they can also be found in Czech, which offers an even more fertile field for this purpose because of the extensive syncretism which has affected large classes of nouns and adjectives. I suspect that similar evidence could be found in many, if not all case languages; they should at least have examples of the type discussed in 16, and to the extent that they had case syncretism, I would expect them to have examples on the order of 18, 21 and 22.

In conclusion, then, it would seem that, whereas the NAC may work for some dialects of English, it does not appear to hold for all; and it certainly does not work for Russian. Thus, it cannot be a universal condition or even an unqualified English-specific condition as proposed. We have seen that the condition which restricts Gapping cannot rule out all applications which produce ambiguity, structural or otherwise. Further, the relevant condition cannot be stated simply in terms of
constituent structure, but must be able to take morphological or relational information into account; and even this information may not be sufficient for stating the constraint.\footnote{9}

FOOTNOTES

1. The numbers in parentheses are Hankamer's example numbers. Where grammaticality judgements are given to sentences with Hankamer's numbers, they represent his judgements, as they appear in his article. Judgements of sentences identified only with my numbers are my judgements. Roman numbers identify quotations from Hankamer's text.

2. I do not agree with Hankamer that the generic nature of the object is of relevance here, but this matter is far from the discussion at hand, and will not be pursued. What is relevant is that Hankamer agrees that the NAC cannot account for 3/(86).

3. A problem in assigning judgements to many of these sentences is that they belong to oral rather than written style. They are therefore more likely to seem "peculiar" when read and may be adjudged worse than the same sentences would be if they were heard. A complementary problem is that the interpretation is highly dependent on stress and intonation, and this information is absent when the sentence is presented on paper. Perhaps the most significant factor, though, is context, which also is usually lacking when sentences are tested. Appropriate context can often significantly increase the acceptability of a gapped sentence which violates the NAC, e.g.,

\begin{enumerate}
  \item All of the children in the crafts class thoroughly enjoyed themselves, and, to show their appreciation, Alice made the teacher a knitted scarf, and Eric a charm bracelet.
  \item The twins are very good at finding unusual birthday presents for their parents. Last year Bob gave their father a fur-lined nose warmer, and Allan an electric nutcracker.
\end{enumerate}

4. This set of examples was suggested by sentence 12, which is taken from D. Perlmutter and S. Soames, Problems and Arguments in Syntax, in preparation.

5. While I think there is nothing wrong with having to seek out such differences and motivation, since speech varies syntactically, has phonological properties and occurs in a discourse context, I do agree that patent examples which are unquestionably acceptable make a more convincing argument.

6. Hankamer says, "Note that it is identity of structure that is specified as crucial in this condition, and thus it is
structural ambiguity that is disallowed. Gapping is blocked in (52c), even though there is no grammatical sentence with a left-peripheral gap having the same sequence of morphemes:

(52)a. *Jack asked Mike to wash himself, and Sue to shave himself.
   b. *... and [Jack asked] Sue to shave himself.
   c. ... and Sue *[asked Mike] to shave himself.

This condition thus blocks Gapping in cases where there could be structural ambiguity as to the location of the 'gap'.'" (p.30)

7. This article was recently brought to my attention by Ivan Sag.

8. Fedorowicz-Bacz worries that the accidental nature of case syncretism is not "systematic" and that therefore these examples do not "constitute a real counterexample to the NAC." (p.47) The fact remains, however, that Gapping here is producing structurally ambiguous sentences, which the NAC says it cannot; and these syncretisms can be quite far-reaching in Russian, as is seen from examples like 18, 21 and 22. Furthermore, I see nothing wrong with seeking examples where they are to be found. A proposed condition should account for the facts, and the facts of Russian are that Gapping can apply to different inputs to produce the same output, thereby creating ambiguity.

9. After this paper had been completed, I had the opportunity to see a recent unpublished study by Kuno (1974), in which he examines Gapping from the functional point of view. The basis of Kuno's analysis is a functional pairing or matching procedure to relate the surviving constituents in the gapped conjunct to corresponding constituents in the ungapped conjunct. This matching procedure is affected by many factors, including anaphoricity, remoteness in processing, and interpretive tendencies.

Kuno's proposal strikes me as being very much on the right track, and it can account very nicely for most of the problems and observations noted here. In particular, it offers an obvious way of incorporating the morphological and relational information which we have seen is necessary for Russian into the analysis of Gapping: the pairing procedure, among other things, can look for a match in case. Furthermore, this proposal offers a natural reason for why word order, intonation and context have such a dramatic effect on the acceptability judgements of gapped sentences (see footnote 3 above and Kuno, pages 10-13). Although the matter bears further investigation, it seems to a first approximation that Kuno's analysis accounts very well for Gapping in Russian.
REFERENCES


1. Introduction

Traditionally, three procedures have been used to establish the relative chronology of linguistic changes with special reference to phonology: (a) the dating of phonological innovations on the basis of philological evidence. Thus Richter 1934 segmented the history of French from Classical Latin to Gallo-Romance (late 8th c.) into 170 successive shifts. (b) a logical inference of the historical sequence of phonetic laws required to derive modern reflexes from proto-forms. This time-honored technique has been refined by the concept of the linear ordering of phonological rules developed in the generative school. Halle (1962:347) made explicit the theoretical foundation underlying the method of projecting synchronically motivated rule order onto the dimension of relative chronology. Chafe 1967 best exemplifies such an approach: from the morphophonemic alternations still prevailing in Caddo, he inferred at least 11 time depths among the 67 rules needed. Finally, (c) the method referred to as 'latitudinal reconstruction' in Chen 1973, which consists in a projection from the horizontal plane of geographical distribution onto the vertical axis of time depth. The rationale can best be made clear by an example. Consider, for instance, three major phonological changes -- devoicing (DEV), final stop deletion (DEL), and nasalization (NAS) -- which gave rise to the diversification of three Mandarin subgroups, namely Northwestern (NW), Northeastern (NE), and Southeastern (SE). The hypothetical situation can be schematized roughly in the following way ('+' = application of the relevant change):

<table>
<thead>
<tr>
<th>(1)</th>
<th>NAS DEL DEV</th>
<th>Mandarin</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>+ + +</td>
<td>NW</td>
</tr>
<tr>
<td></td>
<td>+ +</td>
<td>NE</td>
</tr>
<tr>
<td></td>
<td>+</td>
<td>SE</td>
</tr>
</tbody>
</table>

Given these facts, one could hypothesize two (among others) genetic trees (A) and (B):
In terms of branches, nodes and pluses/minuses, (A) is obviously simpler than (B). The formal simplicity reflects the fact that the monogenetic hypothesis (A) makes the least number of (fortuitous) coincidences and independent innovations by positing that, e.g. DEV happened only once, namely early in Common Mandarin, rather than assuming that it was duplicated in each of the dialects. The rationale behind the latitudinal reconstruction of relative chronology is basically the same as the hierarchy of distinctive features that yields the simplest feature tree. For an early example of the application of 'latitudinal reconstruction', the reader is referred to Bremer 1894; and for further discussion, cf. Chen 1973.

In the following sections I will compare these methods of reconstructing relative chronology, using as a case study the aperiodization process that is observable in a large number of contemporary dialects of China. It will become obvious in the ensuing discussion that each of the approaches has inherent limitations and that all three are complementary to each other.

Before we proceed further, it would be well to clarify a few concepts and symbols. An apical vowel /i/ typically has its origin in a high, especially front, vowel, and can be regarded as an homorganic vocalic release of the syllable-initial sibilant. A sibilant, as the term is used here, is defined as a dental, alveolar, retroflex, alveopalatal or (pre)palatal affricate or fricative; in short, sibilant denotes the class of sounds specified as [+strident, -grave] in Jakobsonian terms. The phoneme /i/ has two contextually determined allophones: a dental [i] after a dental sibilant, and a retroflex [ɻ] after a retroflex sibilant. In the discussion that follows I will ignore this distributional fact.

Here are some of the symbols commonly used in the Chinese linguistic literature:
/č, č’, ç; ć, ć’, ć/ = (pre)palatals
/tš, tš’, dž; š, ž/ = alveopalatals
/ʃ, ŝ’, ʒ; tš, tš’, dž, š, ž/ = retroflexes (supradentals)

Aspiration is marked by [’]; and all digraphs (ts, dz...) represent unitary symbols. The starred forms represent Middle Chinese (hereafter MC) reconstructions.

2. Internal reconstruction à la generative grammar

Let us begin with Wen-zhou, a Wu dialect spoken in Zhè-jiang (an eastern coastal province of China). Consider the following correspondences:

<table>
<thead>
<tr>
<th>(2)</th>
<th>MC</th>
<th>Wen-zhou</th>
<th>Gloss</th>
</tr>
</thead>
<tbody>
<tr>
<td>*tsi</td>
<td>ts</td>
<td>$</td>
<td>'to nourish'</td>
</tr>
<tr>
<td>*ši</td>
<td>s</td>
<td>$</td>
<td>'teacher'</td>
</tr>
<tr>
<td>*tši</td>
<td>ts</td>
<td>$</td>
<td>'branch'</td>
</tr>
<tr>
<td>*či</td>
<td>ts</td>
<td>$</td>
<td>'to know'</td>
</tr>
<tr>
<td>*ki</td>
<td>ts</td>
<td>$</td>
<td>'self'</td>
</tr>
<tr>
<td>*kiai</td>
<td>ts</td>
<td>$</td>
<td>'chicken'</td>
</tr>
<tr>
<td>*xi</td>
<td>s</td>
<td>$</td>
<td>'hope'</td>
</tr>
</tbody>
</table>

Oversimplifying the matter somewhat, one may capture the historical changes by positing simple rules such as:

<table>
<thead>
<tr>
<th>(3)</th>
<th>I-UMLAUT:</th>
<th>iai &gt; iei &gt; i</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>SOFT:</td>
<td>č, č’, ç &gt; tš, tš’</td>
</tr>
<tr>
<td></td>
<td>PAL:</td>
<td>k, k’, g &gt; tš, tš’</td>
</tr>
<tr>
<td></td>
<td>APIC:</td>
<td>x, ŝ &gt; š</td>
</tr>
<tr>
<td></td>
<td>RET:</td>
<td>palatals &gt; retroflexes except before i, y</td>
</tr>
<tr>
<td></td>
<td>DENT:</td>
<td>retroflexes &gt; dentals</td>
</tr>
</tbody>
</table>

(SOFT = softening; PAL = palatalization; APIC = apicalization; RET = retroflexion; DENT = dentalization). Both I-UMLAUT and SOFT stand in a feeding order to APIC by either creating more inputs (i's), or more suitable environments (sibilants). Thus:

<table>
<thead>
<tr>
<th>(4)</th>
<th>*tši</th>
<th>*či</th>
<th>*ki</th>
<th>*kiai</th>
<th>MC</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>ki</td>
<td>I-UMLAUT</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>tši</td>
<td>-</td>
<td>-</td>
<td>SOFT</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>-</td>
<td>tši</td>
<td>tši</td>
<td>PAL</td>
</tr>
<tr>
<td></td>
<td>tši</td>
<td>tši</td>
<td>tši</td>
<td>tši</td>
<td>APIC</td>
</tr>
<tr>
<td></td>
<td>tši</td>
<td>tši</td>
<td>tši</td>
<td>tši</td>
<td>RET</td>
</tr>
<tr>
<td></td>
<td>tši</td>
<td>tši</td>
<td>tši</td>
<td>tši</td>
<td>DENT</td>
</tr>
</tbody>
</table>

The ordering relations postulated in a 'standard' account such as that of (4) suggest that, firstly, apicalization happened only once and, secondly, at a fairly recent date in Wen-zhou. Both of these conclusions -- based solely on internal reconstruction à la generative grammar -- will be shown to be implausible in the light of other methods of reconstruction.

One of the notable facts about Wen-zhou is the absence of the contrast between dental and retroflex sibilants, a shibboleth of 'correct' speech prevailing in the Northern Mandarin provinces. This neutralization of the dental/retroflex contrast (of a fairly
recent origin) has obscured an important phase in the development of Wen-zhou. To illustrate this point, let us look at Lán-zhou, a NW Mandarin dialect. Lán-zhou has maintained a curious contrast: whereas the original MC palatal sibilants have become dentalized, the secondary palatal sibilants derived from MC palatal stops (via SOFT, see above) remained retroflex. Thus MC *tší > Lán-zhou /tśi/; but MC *tǐ > /tśi/ > /tşɪ/. The Lán-zhou correspondences require the following derivation:

<table>
<thead>
<tr>
<th>(5)</th>
<th>'branch'</th>
<th>'to know'</th>
<th>Gloss</th>
</tr>
</thead>
<tbody>
<tr>
<td>*tší</td>
<td>*tǐ</td>
<td>-</td>
<td>MC</td>
</tr>
<tr>
<td>tśi</td>
<td>-</td>
<td>-</td>
<td>APIC-1</td>
</tr>
<tr>
<td>tsī</td>
<td>-</td>
<td>-</td>
<td>RET-1</td>
</tr>
<tr>
<td>-</td>
<td>tśi</td>
<td>-</td>
<td>DENT-1</td>
</tr>
<tr>
<td>-</td>
<td>tsī</td>
<td>-</td>
<td>SOFT</td>
</tr>
<tr>
<td>-</td>
<td>-</td>
<td>-</td>
<td>APIC-2</td>
</tr>
<tr>
<td>tśi</td>
<td>tşī</td>
<td>-</td>
<td>RET-2</td>
</tr>
</tbody>
</table>

The only difference between Lán-zhou and Wen-zhou is that the latter further underwent DENT-2, thereby obliterating the /tśi/ vs. /tşī/ contrast still maintained in the former. A full justification of derivation (5) can be found elsewhere (Chen 1975a,b). The important thing to notice is the recurrence of APIC at least at two different points in the history of Lán-zhou.

But, in the generative tradition, according to which each language (or dialect) is considered as a self-contained, homogeneous system in isolation, the evidence from Lán-zhou is irrelevant to Wen-zhou. One of the basic assumptions underlying latitudinal reconstruction, on the other hand, is precisely the concept of 'maximal dialect coverage': phonological rules and their ordering postulated should account for the largest number of related linguistic systems possible. According to this view, Wen-zhou is but a further step ahead of Lán-zhou in the relevant aspects.

Apicalization in Wen-zhou illustrates one crucial problem in the current practice of equating relative chronology with the order of application of the rules which successively convert protoforms into modern reflexes ad instar of synchronic description (cf. Halle 1962). Chafe (1967:129) already anticipated this fundamental deficiency, as he observed: "the synchronic ordering minimizes the degrees of depth, maintaining only as much depth as is necessary, while historically the number of intervening stages was at a maximum." The internal evidence as provided by Wen-zhou points to one single instance of APIC occurring at the end of the derivation; cross-dialectal evidence, on the other hand, suggests a very early instance of APIC and its recurrence at the end.
3. Latitudinal reconstruction

The validity of latitudinal reconstruction is predicated on the assumption that comparative data are relevant to the reconstruction of the history of one particular dialect. With reservations to be noted below, one can hardly doubt the soundness of this assumption. An areal study of apicalization among the contemporary Chinese dialects offers a striking example of the usefulness and the well-foundedness of this approach.

For the vast majority of cases, the rule of APIC is exactly as formulated in (3): a syllable final /i/ becomes /yi/ when preceded by a sibilant. Now, there are three sources of /i/: /I₁/ from MC *-i; /I₂/ from MC *-iai; and /I₃/ from MC *-i̯C (where -C stands for a final stop). Likewise, the sibilant (S) has three origins: /S₁/ is the original MC sibilant; /S₂/ comes from MC palatal stops; and /S₃/ is derived from MC velar stops and fricatives via PAL. The scope of APIC varies from dialect to dialect, and can be defined in terms of the historical sources of /i/ (input) and /S/ (environment). Figure 1 is a synoptic view of four chosen dialects.

Figure 1 is interpreted as follows: the most restricted (and therefore, most primitive) form of APIC, confined to syllable types consisting of /S₁+I₁/, is still observable in Huáng-xiàn, a Northern Mandarin dialect spoken in the Shan-dong province. Thus whereas MC *tśi 'branch' > Huáng-xiàn /tʃi/, both MC *t$i$ 'to know' and *ki 'foundation' retain a palatal vowel, becoming both /tʃi/.

Jin-huá, a Wu dialect, also spoken in the Zhé-jiang province, shows a slightly more advanced stage of APIC. Here the scope of APIC may be defined as /S₁₂+I₁₂/. Thus not only MC *tśi 'branch' and *t$i$ 'to know' > Jin-huá /tʃi/, but also *tśiai 'regulation' and *diāi 'stoppage' > /tʃi/.

Peking and Wen-zhou both show pervasive influence of APIC. In Peking, any pre-modern i from any of the three aforementioned historical sources (i.e. /I₁₂₃/) constitute an appropriate input to APIC; on the other hand, only /S₁₂/ (but not /S₂/) serve as appropriate environment for the rule. Thus *t$s$i$, *tśiai and *tśia̯k 'to weave' all became /tʃi/; but whereas *t$i$ > Peking /tʃi/, *ki 'foundation' > /tʃi/.

The Peking situation is reversed in Wen-zhou: all /S₁₂₃/ are appropriate environments for APIC, but only /I₁₂/ are the right input. Thus *t$s$i$, *t$i$, *ki all became /tʃi/; but while *ki, *kiāi 'chicken' > Wen-zhou /tʃi/, *kiək 'attack' > /tʃia/. Figure 1 simply superimposes the varying scopes of APIC operative in these dialects; by projecting the individual frames in succession, one gets a motion picture of the gradual expansions of the domain of APIC from Huáng-xiàn to Jin-huá, then on either in the direction of Peking or of Wen-zhou. It is clear that APIC must have grown by successive stages if only because the other processes fed new inputs into APIC on instalments, so to speak. This dynamic picture obtained from latitudinal reconstruction allows us to recover certain intermediate phases of Wen-zhou not otherwise reconstructible purely from the internal evidence available in this
dialect. A more realistic diachronic picture of APIC in Wen-zhou can be schematized as follows:

\[(6) \quad *t̂i \quad *fi \quad *ki \quad *kiai \quad \text{MC} \]
\[
\begin{array}{cccc}
\text{t̂i} & \text{-} & \text{-} & \text{-} & \text{APIC-1} \\
\text{-} & \text{t̂i} & \text{-} & \text{-} & \text{SOFT} \\
\text{-} & \text{t̂i} & \text{-} & \text{-} & \text{APIC-2} \\
\text{-} & \text{-} & \text{ki} & \text{-} & \text{I-UMLAUT} \\
\text{-} & \text{-} & \text{t̂i} & \text{t̂i} & \text{PAL} \\
\text{tŝi} & \text{tŝi} & \text{tŝi} & \text{tŝi} & \text{other rules} \\
\end{array}
\]

\{ as in Huáng-xiàn

\{ as in Jin-huá

\{ as in Peking

\{ as in Wen-zhou

Notice that we can infer from the comparative dialectological data a sequence of events as (6) for the same reason that we can deduce tree (A) from the comparative evidence given in (1). To make the rationale more explicit, let us define the three occurrences of APIC:

APIC-1 applies to /S_1+I_1/
APIC-2 applies to /S_2+I_2/
APIC-3 (a) applies to /S_3+I_{1,2}/
APIC-3 (b) applies to /S_{1,2}+I_3/

Having defined APIC-1,2,3a/b, we can construct a matrix (7) like (1):

\[
\begin{array}{cccc}
\text{APIC-3b} & \text{APIC-3a} & \text{APIC-2} & \text{APIC-1} \\
+ & + & + & + \\
+ & + & + & + \\
+ & + & + & + \\
\end{array}
\]

Wen-zhou

Peking

Jin-huá

Huáng-xiàn

By the same token, one can construct tree (C):

(C)

\[
\begin{array}{c}
\text{APIC-1} \\
\uparrow \\
\text{APIC-2} \\
\downarrow \\
\text{APIC-3} \\
\uparrow \\
\text{a/b} \\
\end{array}
\]

Huáng-xiàn  Jin-huá  Peking  Wen-zhou

According to (C), Wen-zhou and Peking represent more advanced cases of Jin-Huá, which in turn developed from the Huáng-xiàn prototype. (C) is the simplest tree, covering the largest number of dialects; but it is by no means the only possible tree. One could, for instance, hypothesize that APIC developed independently in Huáng-xiàn, after this dialect split off from the Wú subgroup (represented here by Jin-huá and Wen-zhou). This complicates the tree somewhat; but is a perfectly conceivable turn of events.

This observation brings out the fundamental weakness of latitudinal reconstruction: it is based on an exclusively 'tree' model of linguistic diversification, and ignores other modes of linguistic evolution, such as areal convergence (owing, e.g. to continued contact among speech communities after the initial split)
and independent innovations (motivated, e.g., by universal tendencies). The simplicity argument underlying latitudinal reconstruction is weakened by the possibility not only of 'wave'-like propagation of linguistic changes (areal convergence) but also of the 'mushrooming' effect of similar but independent developments in non-contiguous speech communities.

4. Philological 'Einfühlung'

Derivation (6) is, then, a plausible schematization of the evolution of apicalization in Wen-zhou, but not a compelling conclusion based on comparative evidence alone. Fortunately, philological records, which abound in a cultural tradition like the Chinese, provide independent validation of the inference expressed as (6) or, equivalently, tree (C). The first unambiguous evidence of APIC is given in Zhong-yuan Yin-yün (hereafter ZYYY, dated at 1324). This rime book set up a separate category labeled 'zhi-si' (/i/-/) in contrast to 'qi-wei' (/i/-/). The striking thing is that it classed /S₁+I₁/ words under /-i/, but /S₂+I₂/ as well as /S₃+I₃/ words under /-i/ category. Thus MC *tši 'branch' > ZYYY /tši/, but *ti 'to know' > ZYYY /tši/. I₂ (from *iai) and I₃ (from *iaC) were grouped under /-i/, regardless of the initial (S₁₂₃). ZYYY, then, represents the most primitive form of APIC as found in Huang-xian. The stage reached by ZYYY is as follows:

(8)  
<table>
<thead>
<tr>
<th>I₃</th>
<th>I₂</th>
<th>I₁</th>
</tr>
</thead>
<tbody>
<tr>
<td>-</td>
<td>-</td>
<td>+</td>
</tr>
<tr>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

(where '+' = application of APIC; '-' = non-application)

The next document giving clues to APIC is the earliest transcriptions of Chinese by means of a Western alphabet. The first romanization of Chinese was devised by the late 16th c. Jesuit missionaries Matteo Ricci (in China from 1583 to 1610) and others, and improved upon by Nicolas Trigault in Xi-rú Ėr-mù-zì, a systematic treatise of Chinese phonology published in 1626. In contrast to ZYYY, both *tši 'branch' and *ti 'to know' were now transcribed as ċhi /tši/, and both *tš'i 'owl' and *t'i 'foolish' were recorded as ċhi /tš'i/. This means that APIC now extended to both S₁ and S₂ when followed by I₁. Furthermore, APIC affected I₂ as well as I₁. Thus *ţiai 'oath' and *şiai 'earth' were transcribed as xi /şi/. Trigault employed a special symbol ê for *iaC. The phonetic value of this symbol is uncertain; at any rate, it had not yet merged with the apical vowel, written i (or ü after dental sibilants). The situation as attested by Xi-rú Ėr-mù-zì can be summed up in the following chart:
It becomes obvious immediately that Trigault’s romanization resembles the Jīn-huá situation, and represents an intermediate phase between ZYYY and modern Peking. With regard to apicalization, the modern pronunciation of Peking differs from the 17th c. Northern Mandarin reflected in Trigault’s romanization only in one aspect, namely the further expansion of APIC to $I_3$ preceded by nondental sibilants. If we compare Trigault’s transcriptions with Wen-zhou, on the other hand, the difference consists in the extension of APIC not to $I_3$ but to $S_3$. Simplifying somewhat the matter (by ignoring the distinction between dental and nondental sibilants—which behave differently in the matter of apicalization), the diachronic profile of APIC can be constructed by superimposing the three successive ‘frames’ defining the scope of APIC at three different points in time: 1324 (ZYYY), 1626 (romanization) and the 1962 survey of Peking and Wen-zhou undertaken by the University of Peking. The resulting composite picture is Figure 2.

The point by point match between Figure 1 and Figure 2 demonstrates in a rather dramatic way the basic well-foundedness of the assumptions underlying latitudinal reconstruction.

However, the method of philological ’Einfühlung’ (cf. Malkiel 1968:102) presents certain heuristic problems of its own. The not uncommon uncertainties surrounding the dates, provenience, and the historical background in general of ancient documents are often compounded by the difficulties of interpretation. The process of apicalization itself provides an excellent example in this regard. As I mentioned above, ZYYY offers the first certain proof of APIC-1; we can safely conclude that APIC-1 must have taken place at least before 1324. More specifically, some would venture to place APIC-1 at late 12th or early 13th c., based on the formal rearrangements of the rime dictionary Qiè-yùn Zhi-zhăng-tu (dated at 1176-1023). APIC-2 and APIC-3 came even later, between 1324-1626 and after 1626 respectively. SOFT, on the other hand, apparently had a much earlier beginning. Throughout the Sino-Tibetan transcriptions of the 8-9th centuries, which represent the earliest phonetic records of Chinese in any alphabetic script, both palatal stops /ʃ, ŋ/, /ʃ/ and palatal affricates /tʃ, tʂ, dʐ/ were transcribed without distinction as ʃ, ʂ, ʒ, interpreted as /tʃ, tʂ, dʐ/. In summary, the existing philological records suggest the following chronology:

(10) SOFT: 8-9th c.
APIC-1: late 12th c. or, conservatively, 14th c.
APIC-2,3: after 1324

The internal evidence from Lán-zhou, on the other hand, requires that APIC-1 preceded SOFT (see derivation (6)); for if SOFT were applied before APIC-1 (and RET-1 and DENT-1), it would oblige-
rate the contrast between *tɕi and *tj, and the derivation would result in erroneous output, as illustrated in (11):

(11) 'branch' 'to know' Gloss
  *tɕi               *tj          MC
  tɕi               tɕi          SOFT
  tʃi               tʃi          APIC-1
  tʃi               tʃi          RET-1
  tʃi               tʃi*         DENT

/tʃi*/ for 'to know' would be indistinguishable from /tʃi/ for 'branch', whereas in fact Lán-zhou maintains the retroflex vs. dental contrast: /tʃi/ 'branch' vs. /tʃi/ 'to know'.

Faced with the paradox of conflicting chronologies, one established on documentary evidence, the other inferred from internal systematic correspondences, I have attempted to re-interpret the documentary evidence. The Sino-Tibetan transcriptions referred to in the preceding paragraph still consistently represented MC *-i with i. This fact has led linguists to regard the 9th c. as the terminus post quem of APIC-1. I will argue, instead, that this fact need not be construed as an argumentum ex silentio for the absence of an apical vowel. Notice that APIC began as a transparently context-sensitive rule (i.e. with the context that determined the split still observable in the phonetic output -- cf. Kiparsky 1972): MC *-i > 'i /sibilant #. The emergence of the apical vowel /i/ merely created a complementary distribution in the sound pattern of the 8th and 9th c. Chinese, since the new crop of secondary /I/ (= I₂ 3) had not yet evolved from MC *-iV and *-iC, but were still symbolized distinctively as -ei and 1C respectively. The co-occurrence relations (indicated by 'i' +') during this phase of Late MC may be represented as follows:

(12)           -l  -i  -ei  -1C  -V
  sibilant         +  -  +  +  +
  other init.      -  +  +  +  +

Given this distributional pattern, a new symbol for the apical vowel was not necessary. All that was needed was a simple interpretive rule: syllable-final /I/ = [i] after sibilants; = [i] elsewhere. The Sino-Tibetan manuscripts merely reflected this (morpho-)phonemic principle of parsimony, according to which new symbols are used only where ambiguity or unpredictability may arise. The absence, therefore, of a specific symbol for the apical vowel in the Sino-Tibetan transcriptions of the 8–9th c. does not necessarily mean the absence of an apical vowel in the phonological system of the time.

This negative argument presented above is strengthened by a positive one. Traditional Chinese pronouncing dictionaries employ a peculiar system of 'spelling' called 'fān-qìè'. 'Fān-qìè' is an analytic phonetic notation by breaking the syllable into the 'initial' (i.e. the initial consonant) and the 'final' (all the rest); the two components of the syllable are then 'spelled' out separately by two characters, the first alliterating with the initial, the second rimiting with the final of the lexical item in question. E.g.
/fan/ would be phonetically specified by means of /fu/ + /lan/ or similar characters; that is, /fan/ = /f(-u) + (l-)an/, where the elements that do not enter into the phonetic definition enclosed in parentheses. Suppose we let S stand for sibilants, as before, and K for all other consonant types. According to this formula, a syllable like /Si/ can, in principle, be defined either as (a) /S(-X) + (S-)i/ or (b) /S(-X) + (K-)i/, with either S or K in the second 'spelling' character, since neither S nor K in the second character enters into the definition. Curiously enough, however, /Si/-type syllables were uniformly defined as (a) rather than (b) in Huī-lin's Yi qie Jing Yin-yi (810 A.D.), a glossary of Buddhist texts. This self-imposed restriction in the choice of characters serving as phonetic 'spellers' can be explained if we posit two allophones: /i₁/ (= [i]) after sibilants, and /i₂/ (= [i]) elsewhere (i.e. = APIC-1). Given this contextually determined value of /i₁/ vs. /i₂/, it becomes obvious why (b) is not a viable definition of the intended /Si/-type syllables, since the definitens would be phonetically /Si₂/ (= [Si]), while the intended definitum is /Si₁/ (= [Si]). This fact, first noticed by Chou (1948[1968]:164), lends considerable support to my interpretation of the 'ambi-valent' nature of the symbol i in the Sino-Tibetan manuscripts.

If my interpretation of the relevant philological documents is substantially correct, one must revise the date of APIC-1 upwards, at least to 810 A.D., probably even earlier. The resultant revised chronology (13) is now compatible with the internally motivated diachronic order of rules reflected in the derivation of (6):

(13) APIC-1: 8-9th c., at least before 810
SOFT: also 8-9th c.
APIC-2,3: after 1324

5. Conclusion

It has become patently clear that none of the three methods examined here is self-sufficient for the purpose of reconstructing relative chronology — and hence for the purpose of recovering linguistic histories in general. Each has its own inherent inadequacies. This fact calls for a healthy dosage of skepticism regarding the conclusions arrived at by any one method. On the other hand, the evidence derived from each procedure can serve as an independent validation (or disconfirmation) of each other. In view of their individual limitations and mutual complementarity, historical linguistics can only benefit from a closer interdisciplinary approach than has been customary, an approach that combines the strengths of the internal reconstruction à la generative grammar, philology, and dialectology.
Footnotes

1 The present article is based on two substantially longer manuscripts (Chen 1975a,b), in which the issues raised here are more fully developed. The gist of this paper was presented at the First Berkeley Linguistic Society on February 15, 1975.

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2 Stolz 1894, for instance, argued on the basis of such attested Latin forms as *orus < *osinus, that rhotacism must have preceded syncope. The reason is obvious, for if the chronology of events were reversed, *osinus would have become osnus (by syncope), to which rhotacism could no longer apply, as s no longer occurred intervocalically.

3 For a broader survey of the phenomenon of apicalization, the reader is referred to Baron 1974, where some even more advanced cases of apicalization are discussed.

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Interactions of the Expression 'Let's Just Say' with the Gricean Maxims of Conversation*

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Grice (1967) has shown how implicatures arise from violations of the rules of conversation. In many cases, these violations are intentional and serve specific purposes. We have found that the expression 'let's just say' serves as an overt marker on sentences which constitute an opting out from the normal flow of a discourse. It is used as a cue to the addressee that a violation follows, and that the violation is significant.

The expression 'let's just say' can operate in ways similar to 'well', as Lakoff (1973) described it. That is, both 'well' and 'let's just say' can be used in replies to questions in case the respondent is not giving directly the information the questioner sought, and both can operate as a signal that what follows is not to be taken as a complete reply which gives all the necessary information; the addressee will have to deduce the details that have been left out. However, it is obvious from a comparison of (1) and (2) that 'let's just say' has properties that 'well' does not share.

1)P: Did you have beef stroganoff last night?
Y: Let's just say we had a combination of beef, noodles, and sour cream.
2)P: Did you have beef stroganoff last night?
Y: Well, we had a combination of beef, noodles, and sour cream.

While (1) gives an assessment of the quality of the stroganoff, namely, "yech", (2) does not necessarily do that.

We see from (3) that 'let's just say' also functions differently from 'let's say', as James (1973) described it.

3)M: What does Alfred look like?
Q: He's, let's just say, beautiful.
Z: *He's, let's say, beautiful.

James seems to be correct in saying that 'let's say' would not be used with such a strong expression of emotion. She claims that 'let's say' indicates that a description is not very far from the truth, but is not quite the truth, and it would be difficult in a sentence like (3) to imagine a state of feeling which would be close, but not quite the same as that expressed by (3). 'Let's just say', on the other hand, seems to indicate that the actual description is beyond words; that is, to say Alfred is beautiful is putting it mildly.

We mentioned above that 'let's just say' functions as an overt marker on a statement which constitutes an opting out from the normal flow of a discourse. These options out are often accomplished
by failure to adhere strictly to Grice's maxims of conversation. For example, in (4), 'let's just say' is used with a violation of the maxim of quantity, "make your contribution as informative as required."

4) Z: Do you want to hear a funny story? The most embarrassing thing happened to me. I was in the car with my brother-in-law and I asked him if he wanted to hear a funny story. I told it to him and he didn't even laugh. Then about five minutes later he says, "Hey, weren't you going to tell me a funny story?"

O: Huh? Say it one more time.

Z: Let's just say it was a really embarrassing situation.

O: (later) Hey, weren't you going to tell me a funny story?

In (4), Z violates quantity because the point of telling her story, to be funny, would be lost if she explained it again. Thus, she uses 'let's just say' to signal that a violation of quantity is about to take place.

Speakers commonly use 'let's just say' as a means of hedging their responses. That is, it allows a speaker to "drop" information in explicitly, or it allows a speaker a means of responding to a question, without really answering it. When the actual answer to a question is made available to the addressee, it is deductible by implicature. In either case, 'let's just say' signals to the addressee that the speaker is opting out from one of the maxims of conversation.

5) Dick: Where's your cute girlfriend live?
   Pat: Let's just say she lives on Northside, creep.

In (5), Pat uses 'let's just say' to signal not only that she is not giving Dick complete information, but also that she does not want him to have that information. Thus, her response did not implicate an answer to Dick's question. In (6), (7), and (8), however, the responses do implicate at least a general answer to the questions asked.

6) Antony: Who was that man I saw you with last night?
   Cleopatra: Let's just say he's a friend.

7) Sister: Why did Joseph and Mary decide to get married all of a sudden?
   Father: Let's just say they thought it would be a good idea given the circumstances.

8) Otto: What's your average in your physics class?
   Albert: Let's just say I'm no Isaac Newton.

Each of the responses in (6), (7), and (8) opted out from the first maxim of quantity by not being as informative as required. However, answers to the questions are deductible from the responses given. In each case, the use of 'let's just say' in a sense "presupposes" that the respondent, for one reason or another, refuses to be explicit.
The sentences following 'let's just say' in each of examples (6), (7), and (8) violates or in some way fails to adhere to a Gricean maxim of conversation. In (6), the response would have violated quality had it not been prefaced with 'let's just say.' The addition of 'let's just say' communicates the intentionality of the violation, and prompts Antony to deduce that Cleo's relationship with the man is more than friendship, that is, more than she explicitly stated in her response.

In (7), the respondent refuses to explicitly state the reason for Joseph and Mary's hasty marriage (but you know why they got married). The respondent relies on 'let's just say' to point out that a violation of Grice's maxim of manner is about to occur. The questioner, rather than assuming that the respondent is being uncooperative, is able to deduce the actual reason from what is said.

In (8), the statement following 'let's just say' both states the obvious, a violation of Grice's second maxim of quantity, and in itself, without its implicature, is irrelevant to the issue at hand. In order to see the relevance of Albert's response, Otto has to make a connection between Isaac Newton and doing well in physics. 'Let's just say' signals that Albert is intentionally violating a maxim of conversation, thus leading Otto to look for an implicature instead of assuming his response is irrelevant. In each of the examples just discussed, the statement following 'let's just say' involves a violation of quantity in the negative direction. That is, the speaker gives less explicit information than was asked for.

Returning now to sentence (1), repeated here for convenience,

1) P: Did you have beef stroganoff last night?
   Y: Let's just say we had a combination of beef, noodles, and sour cream.

we see that 'let's just say' can also be used with responses that give even more information than the questioner asks for. The response in (1) through a violation of Grice's maxim of quality, "be succinct", not only gives an affirmative answer to the question asked, but also conveys the quality of the stroganoff. The same statement prefaced by 'well', as in (2), does not necessarily have this implicature. We feel that 'well' would more likely be used in case the speaker wasn't sure whether he/she actually had beef stroganoff. Thus we have, "Well, we had a combination of beef, noodles and sour cream; is that stroganoff?", versus, "Let's just say we had a combination of beef, noodles, and sour cream, if you can call that stroganoff!".

The response in (9a) also gives more information than asked for.

(9) Nurse: Did he kiss you goodnight?
   Juliet: a) Let's just say I'm not a virgin anymore.
   b) *Let's just say we slept together.
   c) *Let's just say we finally got it on.
Notice, however, that while (9a) is acceptable, (9b) and (9c) are not. The use of 'anymore' in (9a) implicates that the speaker was previously a virgin, and that she no longer is. The same response without 'anymore', namely, "Let's just say I'm not a virgin", does not implicate information relevant to the question asked. While the fact that Juliet slept with the man is deducible from her response in (9a), it is overtly expressed in (9b) and (9c). Thus, it seems that in order to give more information than a question asked for, that information must be deduced from the response. In other words, if you're going to "drop" information you'll do it as inexplicitly as possible. Notice, also, that the response in (9a) does not necessarily convey an answer to the question asked, but it does seem to satisfy the ostensible purpose of the question, namely, to find out how the relationship is progressing.

Thus far, we have looked at the conversational functions which 'let's just say' can serve. These are listed in (10).

10) a. summing up a description
   b. indicating that the true state of affairs is beyond words.
   c. giving an indirect response which implicates additional information
   d. refusing to give adequate information to answer a question (no implicature)
   e. communicating information by using sentences which give more than asked for

We have shown how each occurrence of 'let's just say' is accompanied by a violation of Grice's maxims of cooperative discourse.

We have also found that 'let's just say' obeys Ross Constraints. For example, 'let's just say' cannot refer into part of a Complex NP. Compare (11) and (12).

11) U: Let's just say I met a man who was beautiful.
12) W: I met a man who was, let's just say, beautiful.

(11) would most likely be used to mean "all I want to tell you is that I met a beautiful man." In other words it fits into category (10d). (12), however, fits into category (10b), indicating that the true state of affairs is beyond words. We see then, that 'let's just say' in (11) is not referring into the Complex NP; that is, it does not refer to 'beautiful' as it does in (12), but rather, it refers to the entire statement that follows it.

'Let's just say' cannot refer into part of a sentential subject, as we see from (13).

13) E: How did Harry feel after he flunked his algebra test?
   O: Let's just say, that he was pretty upset, was obvious.

Most speakers seem to feel that 'let's just say' in O's response
refers to the whole sentence or just to 'obvious'. We find that 'let's just say' can only refer to part of a sentential subject when it occurs within that sentential subject, as in (14) where 'let's just say' refers to 'pretty upset'.

(14) I: That he was, let's just say, pretty upset, was obvious.

In coordinate structures, a pause between the first and second conjuncts can indicate that 'let's just say' only refers to the first. Take for example (15) and (16).

(15) Y: What happened when Jack Spratt and his wife came over last night?
   S: Let's just say Jack's weird, and his wife's even weirder.
(16) S: Let's just say Jack's weird....and his wife's even weirder.

In (15), S indicated that she was opting out on quantity in her response. In (16), however, she seemed to limit the scope of 'let's just say' to the information contained in the first conjunct. In (15), S indicated that she did not want to discuss the situation any further--in (16), that she did not want to go into the details of Jack's weirdness. No speaker we have come across can use S-initial 'let's just say' to refer only to the content of the second conjunct.

In (17), it seems that 'let's just say' can only refer to the entire coordinate structure.

(17) J: Was that class hard?
   K: Let's just say Mary flunked and Sandy got an A.

K's response in (17) might be paraphrased as "Mary flunked and Sandy got an A, so draw your own conclusions". It is easy to see why 'let's just say' in this case must refer to the whole sentence. By itself, "Let's just say Mary flunked" would communicate that the course could be considered difficult, since Mary flunked. To then add, "...and Sandy got an A", would seem contradictory. The reading where 'let's just say' refers to both conjuncts is more consistent within itself.

Notice that in (18), where 'let's just say' occurs within the first conjunct and is used solely to modify 'flunk', it serves a different function from that in (17).

(18) J: Was that class hard?
   K: ?Mary, let's just say, flunked, and Sandy got an A.

In (18), 'let's just say' is used to indicate that a lot more could be said about Mary's performance in the class. J, then, would deduce this implicature, which is not relevant to the question asked. Thus, K's response seems inappropriate. We see, then, that with coordinate structures, S-initial and S-internal 'let's
just say' often function quite differently.

The fact that sentence-initial and sentence-internal 'let's just say' serve different functions in each of the cases we have been discussing, perhaps explains why 'let's just say' cannot refer into an island construction from the outside. It is also evidence that the two types of constructions are not related by a simple movement rule.

Let us now return to the sentence pair of (11) and (12), in which 'let's just say' prefaces the answer in the first case and occurs inside the Complex NP in the second. We repeat these examples here, for convenience.

11)\text{T: What did you end up doing last night?}  
M: Let's just say I met a man who was beautiful.

12)\text{T: What did you end up doing last night?}  
M: I met a man who was, let's just say, beautiful.

We have already shown that 'let's just say' refers to different things in each case. Moreover, we find that 'let's just say' performs a different conversational function in each. While (11) acts as an opting out from the flow of the conversation, (12) leaves open the possibility for further discussion. That is, in (11), the speaker indicates that she does not want to give more information, in effect, stopping the flow of the conversation in the direction it was previously headed. In (12), the speaker is not withholding information, and thus does not stop the flow of the conversation. In fact, (12) might be used to promote further discussion. We find that sentences like (5), which have no implicatures, are more effective conversation stoppers than sentences which do implicate the answer to the question. Responses having no implicature may be considered rude. They deny the questioner access to the information he/she sought.

The interaction of 'let's just say' with indefinites is interesting, in that the respondent is able to avoid indentifying a particular person or thing, while indicating, at the same time, that he/she does, in fact, have the information that the questioner seeks. Consider, for example, (19).

19)\text{V: What's Mom getting at the store?}  
W: Let's just say she's getting something.

W's reply in (19) seems to have the following components:

20) a. W knows what Mom is getting at the store.
   b. W is not telling V all he knows about what V asked.
   c. W is telling V \underline{that} Mom is getting a non-specified item.

The (c) component seems to be what is asserted after 'let's just say'. The (b) component is what 'let's just say' itself conveys, and the (a) component is implicated by the combination of (b) and (c). The process for deduction of the implicature in (19) might be something like (21).
21) (c--W is saying that Mom is getting a non-specified item--c) and that (b--he is not telling all that he knows in response to V's question--b). W asked what Mom is getting. The only information W could be withholding is what Mom is getting, since he admits that Mom is in fact getting something. Therefore, (a--W must know what Mom is getting--a).

A similar analysis explains why (22) works as it does.

22)L: Who did Bill hit?
   B: Let's just say he hit someone.
   L: Who did he hit? Come on, tell me.
   $L_2$ "Oh, then you don't know who he hit.

The final comment on L's part is unacceptable because it ignores the implicature of B's response. If B communicated only the fact that Bill hit some non-specified person, $L_2$ would have been a possible rejoinder. But, since B's response implicates that B knows who Bill hit, $L_2$ is not acceptable.

Notice further that in (19), the statement that follows 'let's just say' gives no new information to the addressee. In fact, the response is merely a restatement of the presupposition of the question asked.

23) What's Mom getting at the store?
   Presupposes: Mom is getting something (at the store).

In (24), then, L's response is inappropriate.

24)C: What's Mom getting at the store?
   L: ?She's getting something.

The implicature which is added by 'let's just say', namely, that the speaker knows what the indefinite refers to, is the additional information which makes the response in (19) acceptable.

Sentences with 'let's just say' followed by an indefinite description are only acceptable when the context or situation enables the addressee to postulate a reason for the speaker to withhold information. Thus, for many speakers, G's response in (25) seems odd until it is pointed out that there are reasons why G might not want H to know what her friend is writing about.

25)H: What's your friend writing her paper on?
   G: ?Let's just say on something.

Few speakers have trouble with (19), however, because it is easy to understand why W might not want V to know what Mom is getting.

Extending this analysis, we can better understand how some 'let's just say' sentences result in implicatures. For example, consider (6), which we can break down as shown in (26).

6) Antony: Who was that man I saw you with last night?
   Cleopatra: Let's just say he's a friend.
26) a. Cleo is involved in a deep relationship with this man.
   b. She is not telling Antony all she knows about what he asked.
   c. She is telling Antony that the man is a friend.

The implicature, (a), is deduced by the addressee through a process which involves attempting to guess what reason the speaker had for "not telling all". Guessing at the possible reasons for withholding information leads the addressee to the information itself. For this to work, however, the reasons for withholding information must be accessible to the addressee and must be related to the question asked. In (6), then, the reason for not answering explicitly is, in essence, the answer to the question. In (5), however, there are no reasons for withholding information which are related to the question, and therefore, an answer is not implicated.1

We have seen that 'let's just say' is used to signal violations of the maxims of conversation, and that it thereby serves specific conversational functions. It seems as if Grice's rules of conversation were made to be broken. Speakers are often aware that they are violating the rules for normal serious discourse. In fact, speakers use violations for specific effects in conversation, for example, sarcasm, implicatures, and the uses associated with 'let's just say'. 'Let's just say' explicitly indicates that a violation is about to occur, that it is intentional, and that it serves a specific purpose. Its use insures that the addressee will notice the violation, and will realize that it has occurred for a reason.

FOOTNOTES

*This paper was originally entitled "But it, let's just say, this way".

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1. It is possible that (5) could have an implicature if there were reasons for the respondent to withhold more specific information. For example, suppose that the "cute girlfriend" lived in a certain well-known house on Northside, which people preferred not to mention. It is possible that the response in (5) could, in such a case, have an implicature, namely, that she lives in that house.

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On the origin of several medio-passive personal endings
in Indo-European

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I. AN OLD PROBLEM RECONSIDERED

A close look at the personal endings of IE verbs reveals a surprising degree of uncertainty concerning their origin and development. In spite of an extensive literature on these endings by the beginning of this century\(^1\), Brugmann (1967: 589) was led to comment that 'the origin of the PIE personal endings...is just as difficult ("schwierig") as the PIE case endings'. Indo-Europeanists have continued to be puzzled by this subject\(^2\), and examples of uncertainty can be readily cited. A few of them are:

1. Brugmann (1967: 650), commenting on *-madhi in I-I (1 pl. m.) vs. -metha in Greek, says: 'because Greek differs (from I-I), the final sound in PIE cannot be determined...Even less clear is the etymological relationship to the probably related active endings *-mes, *-mesi, *-me'.

2. Brugmann (1970: 596) says about 2, 3 du. m.: 'The relationship of the I-I forms to the Greek ones is unclear'. E.g. Skt. 2 du.: -\(\text{\textcircled{\textit{\text{\text{"}}}}}\)eth\(\text{\textcircled{\textit{\text{\text{"}}}}}\) (PE) vs. Greek 2, 3 du. -\(\text{\textcircled{\textit{\text{\text{"}}}}}\)thon (PE and SE).

3. Buck (1963: 245), commenting on 1 pl. act., says: 'The -men of Attic-Ionic, etc. agrees with Skt. -ma except for the final -n (of uncertain source)...'

4. Hirt (1912: 492), referring to the origin of the pl. mid. endings, says: 'The plural endings of PIE are difficult to determine'.

The inability of the leading Indo-Europeanists to solve these problems over the past century and a half has apparently led to the widespread belief that these problems are truly unsolvable, at least given the data we currently possess. I was therefore surprised to notice a possible solution to the origin of 1 pl. m. (e.g. Greek: -metha), and I now believe that an understanding of this ending may help shed light on the origin of several mid. pers. endings.

The starting point for my work on this subject was the realization that constructions of the type pojd\(\text{\textcircled{\textit{\text{\text{"}}}}}\)me (= let's go; Russian) and -mente (1 pl. ending; Cypriot Greek)\(^3\) may furnish parallels to developments in I-I and early Greek. Pojd\(\text{\textcircled{\textit{\text{\text{"}}}}}\)me is known\(^4\) to be derived from pojd\(\text{\textcircled{\textit{\text{\text{"}}}}}\)m (= let's go) and -te (2 pl. ending, e.g. pojd\(\text{\textcircled{\textit{\text{\text{"}}}}}\)ke = go; imperative pl.). The 2 pl. ending (-te) was therefore tacked onto the 1 pl. ending (-\(\text{\textcircled{\textit{\text{\text{"}}}}}\)m), and the purpose of this development was apparently to strengthen the inclusion of 'you' in the concept of 'we'. Cypriot Greek -mente (< 1 pl. -men + 2 pl. -te) is also clearly such a blend; cf. Schwyzser (1939: 670), who speaks of a 'mixing of endings' here.
When Greek and I-I are approached with pojönte and -mente in mind, the following picture emerges: the mid. pers. verbal endings of Greek and I-I underwent a series of shifts whose starting point was 2 pl., and the initial shift was the addition of a 2 pl. ending onto the 1 pl. ending. The importance of this shift should be doubly underscored, because once it is grasped, most of the succeeding changes can be readily discovered.

A. INDO‐IRANIAN

In the primary endings I-I presents 1 pl. m. *‐madhai and 2 pl. m. *‐dhwai. Hirt (1905: 82) has already suggested that the *‐dh‐ and *‐ai‐ of these two forms may be related, and although my treatment will follow Hirt on this point, the incompleteness of his treatment should be noted. He devotes only one paragraph to this discussion and does not explain how these forms came to be related. He is also unclear as to why *‐w‐ is not present in *‐madhai. He says only (ibid.): 'Since we have seen how often -w- has disappeared after a consonant in IE, we may venture to suggest that...', and he then connects the above-cited forms.

I would therefore like to state specifically that 2 pl. *‐dhwai was tacked onto 1 pl. *‐ma, thereby yielding *‐madhwa, which later became *‐madhai. The only lack of correspondence here is -w- in *‐dhwai vs. no -w- in *‐madhai (e.g. Skt.: -mah@). Hirt, as mentioned above, tries to explain away the absence of -w- here by reference to the frequent disappearance of -w- after a consonant in IE, but there are sufficient attestations of -w- in this position to call for a specific reason for the loss of -w- in *‐madhai.

An answer can be found by bringing 2 du. m. into the picture (*‐wadhai), and the following hypothesis may be set forth: after 2 pl. *‐dhwai was tacked onto 1 pl. *‐ma producing *‐madhwai, the ending *‐dhwai spread from 1 pl. m. to 1 du. m. resulting in *‐wadhwi. With two -w-'s present in close proximity in *‐wadhwa, dissimilation occurred, and one of the -w-'s was lost. Since the first -w- was morphologically more important than the second -w- (the first -w- was a clear marker of the dual), this first -w- was maintained, and it was the second -w- that was lost. For another example of the loss of a -w- by dissimilation, cf. French Gautier, where a /‐w‐/ has also been lost: Walter > Gualt→ Guaut← Gaut-. Cf. by way of contrast Guillaume < William, where /‐w‐/ was preserved (until the later change of French /gw/ > /g/), since the two -w-'s in Guillaume (spelled -u-) were not in close proximity.

B. GREEK

In analyzing the Greek endings I will first adopt a suggestion
found in Brugmann⁶ that 2 du. -sthon was originally a 2 pl. ending and as such corresponds formally to Skt. 2 pl. (SE) *-dhwm (except for s-, which is widely agreed to be a secondary addition). If *-dhwm is therefore accepted for Greek, and if we accept the possibility of a 2 pl. ending being tacked onto 1 pl., the following development can be discerned in Greek: 2 pl. *-dhwm was tacked onto 1 pl. *-me, resulting in 1 pl. m. *-medhwom. This form *-medhwom later appeared in the zero grade (*-medhwm), which then developed to -metha according to Greek phonological laws. Also, if *-dhwm were originally an SE, as indicated by I-I, we would deal with a levelling of the PE and SE in Greek; Greek makes no distinction between PE and SE endings for 1 pl. m.

The above discussion on I-I and Greek, if correct, would seem to solve two thorny problems connected with the ending of 1 pl. m. First, what is the origin of the final vowel (–a in Greek -metha vs. –ai/-i of I-I? Brugmann mentions (1967: 650) that the final sound cannot be reconstructed because of the difference between the Greek and I-I forms. Buck (1963: 250) says: 'Greek -metha agrees with the Skt. secondary ending –mah… on the basis of IE *-medha'. Other scholars do not accept Buck's view, however. The schwa that he reconstructs would now be interpreted within the laryngeal theory, but no mention is made of it by Cowgill 1965 or Burrow 1965. Burrow attempts to take a strictly noncommittal position here; after mentioning I-I *-madhai/ *-madhi he says merely (p. 311): 'Closely related to these forms but different in the matter of the final vowel is Greek -metha < *-medha…'

The solution to this problem can be summarized as follows:

Greek 1 pl. m. -metha is traced to *-medhwm, with *-dhwm coming from *-dhwm of 2 pl. m. (possibly SE). The I-I 1 pl. m. *-madhai is traced to *-madhraw, with *-dhraw spreading to 1 pl. m. from 2 pl. m. (PE, Perf. E.). Then *-dhraw spread to 1 du., where dissimilation occurred (the second -w- was lost), and this dissimilated form was then levelled back to 1 pl.

A second problem is finding the origin of *-dh- in 1 pl. m. Brugmann 1967, Burrow 1965, Buck 1963 et. al. pass over this point in silence. An answer to this problem can be found in the *-dh- of 2 pl.: *-dhwom, *-dhraw.

II. FURTHER DEVELOPMENTS

At this point in the analysis there are still several unanswered questions: Why did the zero grade appear in Greek *-medhwm? Why was 2 pl. Greek *-dhwom replaced by -sth? What is the origin of Greek 1 pl. m. -metha?⁷

The answers to these questions can be found in two further major developments in Greek: the restoration of morphological clarity
to the endings of 1 pl. (*-medhwom) and 2 pl. m. (*-dhwom), both of which now possessed a very similar ending (the last four phonemes were the same), and (2) the shift of -methon and -sthon from the pl. to the du.

A. RESTORATION OF MORPHOLOGICAL CLARITY

When 2 pl. *-dhwom spread to 1 pl., the resulting form was *-medhwom. At this point an unacceptable confusion was present in Greek: 1 pl. *-medhwom, 2 pl. *-dhwom; both forms end in *-dhwom. As mentioned above, we do not deal with just a final vowel being the same (e.g. 1 sg. -omai, 3 pl. -ontai) but with a similarity involving the last 4 phonemes (3 consonants and a vowel). The ensuing developments of analogy may be viewed as an attempt to restore morphological clarity to 1 pl. m. and 2 pl. m., viz:

(1) In 1 pl. m. *-medhwom the final *-om was modified to its zero grade *-m, resulting (for some time at least) in two variations of 1 pl. m.: *-medhwom, *-medhwm.

(2) 2 pl. m. was also undergoing changes aimed at reestablishing its morphological clarity:

(a) The *-om in *-dhwom was replaced by -e, yielding (s)the, with -e apparently being imported from 2 pl. act. 8

(b) -s- was also imported, resulting in -sthe. If one assumes that this -s- was added before *-om in 2 pl. was completely crowded out by -e, an attested form -stha would be expected. Such a form does appear viz. in 2 du., but as mentioned earlier, Brugmann and several earlier scholars believed that this 2 du. form might derive from an original 2 pl. ending. The main question to be answered now is where the -s- in -sthe came from.

1. -S- IN STHE (2 PL. M.) AND -MESTHA (1 PL. M.)

The origin of -s- in Greek 2 pl. m. -sthe has long troubled Indo-Europeanists and is still an unsolved problem. 9 I would like now to advance the following hypothesis on this subject: -s- in 2 pl. m. -sthe spread there from 1 pl. m. -mestha, a form which is attested only in poetic language but whose appearance in several main dialects (Attic, Doric, Epic Greek) indicates that its use was once widespread.

The following comments may be made here:

(1) The -s- in -mestha is currently considered to derive from the -s- in -sthe (2 pl. m.), -sthan (2 du. m.; 3 du. m. PE), and -sthan (3 du. m. SE). 10 However, the origin of -s- in these latter forms is unknown, and hence one cannot be sure that -s- in fact spread from -sthe et. al. to -mestha.

(2) The active endings are *-me (SE) and *-mes (PE, Perf. E.), but it
is possible that they may have originally been used for the middle too; cf. Brugmann (1967: 590), who underscores the possibility that the distinction of active vs. middle endings was not the original situation in PIE. Such an assumption for 1 pl. act. jibes well with my suggestion on the origin of *-metha; in 1 pl. m. we see *-dhwom being tacked onto *-me yielding *-medhwom and ultimately *-metha. If we assume that 1 pl. act. *-mes could also have been used in the middle, the following pattern would emerge for Proto-Greek:

1 pl. *-mes → *-me  
2 pl. *-dhwom → *-dhwom  

result: *-mesdhwom, *-medhwom

Greek then eliminated one of these forms, viz. *-metha, as being superfluous. The choice of *-metha over *-mestha is explainable by the move to restore the morphological clarity of 1 pl. m. and 2 pl. m. By choosing *-medhwom for 1 pl. an additional distinction was introduced between the 2 forms: *-s- in 2 pl. *-sdhwom (later: *-sth) vs. no *-s- in *-medhwom.

B. SHIFT OF GREEK *-METHON AND *-STHON TO THE DUAL

The developments in the Greek dual are still partially unclear to me, but the following can be said about them: *-sthon (as 2 du. m.) and the slightly attested *methon (1 du. m.) were originally plural forms. Their shift from plural to dual was due to the following factors:

1. After the appearance of 1 pl. m. *-medhwom and 2 pl. m. *-sth the forms *-medhwom (1 pl. m.) and *-sdhwom (2 pl. m.) were superfluous. When fluctuation appears in language, one of two developments will eventually occur: one of the two forms will be eliminated (e.g. Russian short adjective, neuter: nòvo/novó > nóvo), or a new function will be assigned to one of the forms (e.g. Russian short adj., pl. mály/malý > mály (= small), malý (= too small). The loss of *-w- in Greek rendered 1 du. *-we-/*-wo- morphologically unclear.

2. *-om was already a sign of the dual (2 du. act.): *-tom (attested: *-ton).

3. Since *-sthon as 2 pl. m. was superfluous, and since it resembled the dual form *-ton (2 du. act.), *-sthon (2 pl. m.) was reinterpreted as a dual and is attested as 2 du. m.

4. *methon also shifted from 1 pl. m. to 1 du. m. to help alleviate the loss of morphological clarity that occurred when Greek *-w- was lost. The striking thing about *methon as 1 du. m. is that this solution to the loss of morphological clarity in 1 du. m. was not accepted by Greek, and 1 du. m. was eliminated. Two factors may be discerned here to explain the failure of *methon to take root as 1 du. m.
a) *-w- was lost in all 1 du. forms, but the use of *methon solved the problem of this loss only in the middle forms. The active forms were still left without a representative in 1 du.

b) It must be remembered that the problem that triggered a series of developments in the plural was the similarity at the end of the forms of 1 pl. m. *-_medhwom and 2 pl. *-_dhwom. In the dual we now encounter a similar situation: *methon (1 du. m.) and *-_ston (2 du. m.) Just as the plural moved to eliminate this lack of morphological clarity, the dual moved too; *methon was not permitted to become firmly rooted as a 1 du. m. ending, which contributed to the category of 1 du. being eliminated.

6) The use of *-_ston as 3 du. as well as 2 du. can be traced to the use of *-_on as a designation of the 3rd person (pl. act.) after the loss of *-_t: *-_out > *-_on, e.g. epheron = they carried. After *-_ston (as 3 du.) arose, it influenced 3 du. act. to remodel as *-_ton. For an opposing view see e.g. Buck (1963: 250) who considers *-_ton (2, 3 du.) as the model on which *-_ston (2, 3 du.) was formed.

C. *-_DH- IN 2 PL. MID.

All works reconstructing the endings of I-I 2 pl. m. go back only to *-_dhwom, *-_dhwal, but it seems possible to go further than these standard treatments; the *-_dh- in these forms can be reconstructed as coming from *-_t-, the same consonant as in other IE 2 pl. endings (e.g. Latin: _ama-tis). The change of *-_t- > *-_dh- could have occurred in accordance with Bartholomae's Law, which states:14

If an IE aspirated voiced stop was followed directly by an unaspirated voiceless stop, aspiration was transferred from the beginning of the cluster to the end, and the entire cluster became voiced, e.g. IE *lubh-tos > *lub-dhos > Skt. lub-dhas (= covetous) beside lubh-yāmi (= I yearn).

According to this law, 2 pl. m. *-_t- would be pronounced *-_dh- when added to an athematic verbal root ending in an aspirated stop. It is possible that *-_dh- was then generalized at the expense of its allophone *-_t-.

III. SUMMARY

In summary, here is a chronological listing of the developments I have suggested for I-I and Greek:

1. *-_t- > *-_dh- in 2 pl. m. (I-I) according to Bartholomae's Law,
2. addition of a 2 pl. m. ending to a 1 pl. m. ending in Greek and I-I,
3. a series of changes in Greek aimed at restoring the morphological clarity of 1 pl. m. as distinct from 2 pl. m.,
4. shift of two superfluous endings in Greek from pl. to du.
Footnotes

1 For the literature on this subject in the 19th and early 20th centuries see Brugmann (1967: 583–589) and additional references under the specific endings he discusses.

2 e.g. Burrow 1965, Kuryłowicz 1964, Meillet 1966, Watkins 1969. Although Indo-Europeanists do not always state directly that a given ending is of uncertain origin, it is clear that many of the problems concerning the personal endings that troubled Brugmann have not yet been solved.

3 Menardos (1925: 38, 39).

4 e.g. Vasmer (1958: under –te).

5 He also relates the *–dh– of *–madhai and *–dhwaɪ to the *–dh– of the imperative for *'go': Greek ἰθ, Skt. ḷ.

6 Brugmann (1967: 657) makes this suggestion tentatively ('perhaps'). Also, although Brugmann was the first to clearly state that Greek 2 du. –sthon might have originally been a 2 pl. ending, linguists had earlier noticed that the two forms may correspond. Misteli (1866: 331) was apparently the first to make this connection, and Hildebrandt (1892: 281) speaks of die alte Gleichsetzung of Skt. *–dhvan and Gr. –sthon. Cf. also Buttmann, who as early as 1819 (p. 348) had pointed out two instances of –sthon being used as 2 pl. and regarded this use of –sthon as being an archaic rather than an innovative feature of Greek.

7 The ending –mestha appears beside –metha in Epic Greek and in the poetic language of Attic and Doric; see Hirt (1912: 492, 493).

8 Wackernagel (1895: 59).


10 See e.g. Brugmann (1967: 651).

11 This hypothesis is based in large measure on Henry 1889. Henry speaks of –tha being added to –me, –mes, although he is unable to determine the origin of this –tha.

12 The possibility that 2 du. –sthon was originally a 2 pl. ending was well recognized in the 19th century; cf. above, footnote 6. However, 1 du. –methon has not previously been recognized as possibly having once been a plural ending; it has been regarded as a reformation of 1 pl. –mētha, with –on coming from 2 and (PE) 3 du. –sthon; cf. Brugmann (1967: 655).
Examples of semantic differentiation according to stress in Russian short adjectives are given in Cohen (1971: 164, 165).

Bartholomae (1885: 206).

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The Case of the Vanishing Presupposition

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One of the few things in linguistics which we are generally allowed to assume to be a constant is the presupposition of a given word at a given time; either a presupposition is there or it isn't. It appears, however, that even this "constant" must bite the dust. Take, for example, manage:

(1)a John managed to cash the check.
   b John didn't manage to cash the check.

(2) John tried to cash the check.

(2) follows from both (1)a and its negation, (1)b--therefore manage presupposes try. Unfortunately, there are a number of counterexamples, where the subject "manages" something which he is clearly not "trying" to do, as in (3) - (8).

(3) My dog manages to get clawed by every cat that comes along.
(4) Fred managed to get himself killed.
(5) Pearl accidentally managed to get caught in the crossfire.
(6) Harry managed to insult Ursula, and he hasn't the foggiest idea how he did it.
(7) I seem to have managed to lose my wallet.
(8) I wonder how I managed that?

A reading of try is blocked by overt semantic information in (5) - (8), e.g., "accidentally," "seem to," etc. While this presupposition is not overtly blocked in (3) and (4), it requires so much contextual chess-playing that I think it can be ruled out. Furthermore, manage is still possible even if it is specified that the subject tried not to accomplish the action in question, as in (9).

(9) Harry spent all evening trying very hard not to insult Ursula, but he managed to insult her, all the same.

And, as if that were not enough, we can find quite a few examples in which the subject is inanimate, and so cannot try to do anything:

(10) Sad movies always manage to make me cry.
(11) It always manages to rain on my day off.
(12) That old house has managed to remain unoccupied for years.
(13) Writing this paper has managed to become quite a problem.

It seems, then, that manage sometimes presupposes try and sometimes does not. Interestingly, (4) is slightly worse without the re-
flexive, and (3) and (5) sound slightly more natural with it. Furthermore, all three of these must take the get-passive or the result is disastrous:

(3)a ??My dog manages to be clawed by every cat that comes along.
(4)a *Fred managed to be killed.
(5)a *Pearl accidentally managed to be caught in the crossfire.

As Robin Lakoff has noted (1971), the use of the get-passive frequently implies some responsibility on the part of the subject. Although clearly none of the subjects in (3) – (5) were trying to accomplish the action, it is implied that the result is not necessarily what would naturally have happened without any interference from the subject, which implies that the action was difficult (had the subject been trying to do it) or unlikely. In a similar sentence, for example:

(14) It's pretty difficult to get yourself killed while sweeping the streets, but Fred managed it.

the implication, unless otherwise specified, is that Fred was not by any means trying to be killed, but rather that he did something unintentionally which resulted in his death. This analysis is supported by sentences like (15) – (18).

(15)a *Harry managed to insult Ursula, and anyone could have done it, really.
b Harry managed to insult Ursula, but anyone could have done it, really.
(16)a *Queen Elizabeth managed to live in Buckingham Palace.
b Annie the Charwoman managed to live in Buckingham Palace
(17)a *Zeus managed to make it thunder.
b Agamemnon managed to make it thunder.
(18) The baby managed to wake me up just after I had gotten to sleep.

(15) is not acceptable unless the conjunction but is used, which, as Lakoff has noted (1970), indicates that the material in the second clause conflicts with expectations raised in the first clause. The clause "Harry managed to insult Ursula," therefore, leads us to expect that it is not true that "anyone could have done it," i.e., there is something difficult about insulting Ursula. In (16) and (17), again, the 'a' sentences are bad because it is pragmatically presupposed to be no difficulty for Queen Elizabeth to live in Buckingham Palace or for Zeus to make it thunder. It is assumed to be difficult for Annie the Charwoman or Agamemnon to accomplish the respective actions. Finally, in (18), there are three possible interpretations: (a) the baby was
intentionally trying to wake the speaker up; more likely are (b) it is difficult to wake the speaker up, and (c) it is unlikely that the speaker should be awakened just exactly after he had gotten to sleep. Such multiple possibilities will be discussed in more detail below.

Manage, then, if it does not presuppose try, presupposes difficulty, as can be seen by negating a sentence like the first clause of (6), which will still commit us to the assumption that it is difficult to insult Ursula. As it happens, difficulty is a presupposition of try, since both (19)a and b commit us to (20):

(19)a Poppy tried to write a 25-word squib.

b Poppy didn't try to write a 25-word squib.

(20) It is difficult to write a 25-word squib.

Since, if P presupposes Q and Q presupposes R, it follows that P presupposes R, manage would, by virtue of its presupposition of try, presuppose difficulty in any case. What is peculiar is that manage can presuppose difficulty when such presupposition clearly does not come via try.

However, a presupposition of difficulty will not necessarily account for examples like (10) - (13), as well as sentences like (21) and (22).

(21) Our dog always manages to get diarrhea whenever the vet is on vacation.

(22) Our neighbors managed to schedule their one wild party of the year the night before my German exam.

I don't think we want to say that it is difficult to make the speaker of (10) cry, or for a house to remain unoccupied, or for a dog to get diarrhea. There is, however, an implication of unlikelihood in all these sentences--i.e., it is unlikely that the dog should always get diarrhea at precisely the time when it cannot be treated, that the neighbors would schedule their rare noisy party for a time when the speaker of (22) needs quiet, and there is in (13) an implication that the speaker did not expect that the paper would be a problem to write--that he thought it unlikely. Note that if the frequentative adverb in (10) and (11) is removed, the sentences are much worse, unless of course we do some contextual chess-playing:

(10)a ??That movie managed to make me cry.

(11)a ??It managed to rain on my day off.

That is, rain on any given day off is hardly odd, nor is the idea of crying at one movie, at some point in one's life, while rain every time one takes a day off and crying at all sad movies are much more unlikely. Another possible interpretation of (10), (12) and (13) has it that the speaker of (10) was trying not to cry, that efforts were being made to
get occupants for the house, and that the speaker in (13) was trying not to allow the paper to become a problem. In fact, if one makes it explicit that no one is making any effort to accomplish the opposite of the action, the sentences become much worse:

(23) *I’ve heard that crying is good for the eyes, so I always try to cry as often as possible, and sad movies always manage to make me cry.

(24) *The citizens of this town are trying to keep that old house empty, and so it has managed to remain unoccupied for years.

(25) *I’ve tried to make writing this paper as hard for myself as possible, and so writing it has managed to become quite a problem.

Actually, however, this is merely unlikelihood in different guise. Like difficulty, unlikelihood can be based on inherent properties, or on the fact that someone with some power in the matter is trying to keep the event from occurring. Thus, if the citizens of the town want the house unoccupied, it is hardly likely, in the natural course of things and assuming they have some power, that someone will occupy it.

(26) That house didn’t manage to remain unoccupied for very long.

Both (12) and (26) commit us to the unlikelihood of the house's remaining empty; therefore, manage presupposes unlikelihood. There is, in fact, a relationship between difficulty and unlikelihood, although I am not at all sure how it should be formally regarded. In a totally natural situation dependent upon abilities, if a thing is difficult—like running a 4-minute mile—then it is probably true that, for any random person, it is unlikely that he will do it. On the other hand, if a thing is not unlikely in a similar situation, it would be assumed to be also not difficult. We could call this relationship either entailment or implication. (Unlikelihood is, of course, in the real world, generally dependent upon expectations, either generally held or of the speaker alone.)

In any case, an analysis of expectation of unlikelihood will allow us to handle minimal pairs like (27):

(27)a  *Jacques managed to learn French as a child.

b  Irving managed to learn French as a child.

where try and difficulty really do not matter—language learning as a child is not something you try for, but rather something which is thrust upon you; furthermore, it can be considered either difficult in either case, or not difficult in either case. That is, it is either difficult to learn a language at that age, or else young children pick up language so quickly that it is not difficult for them to learn,
and in neither case does it matter whether it is their first language. (27)a, then, can only be explained by the assumption that, given that one of the defining characteristics of a Frenchman is that he speaks French natively, there is nothing at all unlikely about "Jacques" having learned the language early, while it is generally assumed to be unlikely that someone with a name like "Irving" will do so. The inability to analyze this pair with a presupposition of volition is due to the nature of learning a language early—no volition is involved. We can assume that neither Jacques nor Irving was consulted about whether they were to learn French or not.

A particularly interesting result occurred when I showed (12) to a number of naive speakers. All of them immediately assumed that the house was haunted—i.e., that something inherent in the house itself was trying to keep it unoccupied. After this initial reaction, they all changed their interpretation to one of unlikelihood that the house would remain unoccupied. Thus, they immediately tried to analyze the sentence with the basic presupposition of manage, i.e., try; when they realized that this interpretation would not fit the real world, they went down the line of presuppositions until they found one that would.

Their behavior supports the theory which Dr. Fillmore advances in his paper in this volume. In fact, I think this sort of presupposition vanishing is a special case of frame-extension. Thus, as the tree in (28) shows,

(28)

```
MANAGE
  /   \\
success TRY
   /   \\
intention DIFFICULTY
       /   \\
volition UNLIKELY
```

we have a continuing line of presuppositions, and at each step, something is filtered out of the meaning. Let us assume, for example, that manage is decomposed into try + success, i.e., we have already shown that manage presupposes try, and if you say (1)a, you have committed yourself to the truth of "John cashed the check." Try, however, requires intentional action, and in order to account for sentences which block this interpretation, such as (3)–(8), we must filter out the notion of intention, which leaves us with difficulty; difficulty, again, requires volition—"It would not be difficult to fall down the stairs" is a possible sentence, but not "*It would not be difficult for that tree to fall over." By moving down to unlikelihood, we have eliminated volition, which will allow us to handle sentences like (10)–(13), (21) and (22).

This, I believe, is what a native speaker does when he
hears a sentence with manage. For example, when I showed

(29) Harry's dog manages to wake him up
whenever he dozes off on the couch.

to a number of naive speakers, I got three interpretations,
each of which is transparently dependent upon the individu-
al's view of the world. As with sentence (18), the inter-
pretations were: (a) the dog was trying intentionally to
wake Harry up; (b) the dog was not trying to wake Harry up--
possibly he was just howling at a siren or thumping his tail
on the floor--however, it is difficult to wake Harry up; and
(c) it is unlikely that the dog should awaken Harry every
time he falls asleep on the couch. This sentence was par-
ticularly useful, as there is a way to test the speaker's
view of the world. Dog owners, who tend to ascribe all
sorts of qualities to their pets, including intention,
thought, and the ability to solve quadratic equations, took
the first interpretation, which granted the dog intention.
Those who could not quite see this point of view presumably
went on to the second and third interpretations—in fact,
one of those who chose the third interpretation said that
she had first considered the second interpretation, but had
decided that it cannot be difficult to wake up someone who
has just "dozed off," as opposed to really falling asleep.
Thus, in each case, the speaker starts off considering the
basic interpretation, and, if that is blocked, proceeds down
to the next, and so on down the line.

The hierarchical analysis, then, allows us to explain
the various occurrences of manage without recourse to multi-
ple homonymous verbs or presuppositions that blink in and
out of existence at random, as well as furnishing some pre-
dictive possibilities for other verbs with some of the same
peculiarities.

While I have not yet made a very thorough study of the
predicate happen, it appears that this verb also may benefit
from the type of analysis I have suggested for manage. I am
not at all sure precisely what is going on with happen, but
tentatively I think we can assume that happen presupposes
something like "unplanned action." That is, both (30)a and
its negation, (30)b commit us to (31).

(30)a I happened to be in D.C. when Nixon
resigned.

b I didn't happen to be in D.C. when
Nixon resigned.

(31) I didn't plan to be/not to be in D.C.
when Nixon resigned.

i.e., although the speaker may have planned to be in D.C. at
a specific time, the fact that Nixon's resignation coincided
with the speaker's presence in or absence from the city was
unplanned. This will account very nicely for sentences like:
I happened to find a penny in the gutter.
If it happens to rain, Mary will be glad she took her umbrella.
If the dog happens to want out, make sure you dry his paws before letting him in again.

However, I don't think that with a sentence like (35) we want to say that it was totally by chance that the speaker bought the book--buying a book is something one has to intend to do.

I happened to pick up a book today that I think you might be interested in.

Furthermore, happen cannot occur when the speaker has a strong interest in the action, as shown by the unacceptability of (36), even when pure chance is involved, as in (37).

*I really want to know! Did John happen to cash the check?
*I happened to find the money to save my Aunt Bertha from starvation.

Thus, it seems that happen also presupposes that the action is unimportant to the speaker, since (39) follows from both (38)a and its negative (38)b:

John happened to cash the check.
John didn't happen to cash the check.
It is unimportant (to me) whether or not John cashed the check.

This presupposition of unimportance or disinterest also accounts for the polite uses of happen, e.g., in (40):

Did you happen to pick up those things I asked you to get?

which gives the addressee an out, in the event they failed to do what they were asked, by implying that the matter is of little importance anyway. We can, then hypothesize some sort of hierarchy, in which happen immediately presupposes unimportance or disinterest on the part of the speaker. This in turn entails that there will be a lack of planned action—with the assumption that the matter is unimportant enough that chance may be allowed to take its course. We would, then, expect that the next step would be the assumption that, given that there is no planned action, there will be no expectations as to possible results. To support this, then, we find sentences like (41) - (43):

I don't expect anyone to call, but if someone happens to, will you take a message?
*Queen Elizabeth happened to get a suite of rooms in Buckingham Palace.
Annie the Charwoman happened to get a suite
of rooms in Buckingham Palace.

(43)a *Jacques happened to learn French as a child.
b Irving happened to learn French as a child.

(42)a and (43)a are, as we noted with examples (16) and (27), rendered unacceptable by the conflict between the lack of expectations required by the predicate happen and the very definite expectations of likelihood pragmatically presupposed for Queen Elizabeth, Annie the Charwoman, Jacques and Irving.

I'm not very happy with this tentative analysis of happen, but I suspect that there is a possible hierarchical analysis similar to that proposed for manage, which will allow us to account for this predicate as well.

As another example, let us take manage's opposite number, fail. The one thing basic to all uses of fail is the expectation of likelihood, as in (44) - (48):

(44) Rain failed to materialize.
(45) It never fails! Harry always shows up just as we're sitting down to dinner.
(46) My car failed to start this morning.
(47)a Queen Elizabeth failed to get a suite of rooms in Buckingham Palace.
b *Annie the Charwoman failed to get a suite of rooms in Buckingham Palace.
(48)a Jacques failed to learn French as a child.
b *Irving failed to learn French as a child.

(44) - (46) are unacceptable unless it is presupposed that rain was likely, that the speaker has by now learned to expect that Harry will show up at dinner-time, and that the car is expected to start. The distribution of stars in (47) and (48) is reversed from that in (42) and (43), and in (16) and (27). This is due to the fact that while the lowest level of both the manage and happen hierarchies is the presupposition of unlikelihood, that of fail is the presupposition of likelihood. Thus, the 'a' sentences, which were bad in (16), (27), (42) and (43) because it is likely that the subject will perform the action anyway, are good in (47) and (48) for precisely the same reason. Mere expectation of likelihood, however, will not account for sentences like (49) - (51).

(49) John failed to catch the 8:30 train, so he was late to work.
(50) Dick failed to understand why Pat was sewing pieces of recording tape into her hems.
(51) Simon failed to realize that all flights had been cancelled.

Rather, I think there is some sort of notion of conditional necessity---i.e., if John is to arrive at work on time, he must catch the 8:30 train. Similarly, it is implied that had
Dick understood the reasons for Pat's actions, he would have done something which could not have occurred without this understanding—for instance, he would have stopped her, he would have helped her thread the needles, etc. Finally, had Simon realized that the flights were cancelled, he would not have wasted time getting to the airport or whatever. There seems to be a relationship between this conditional necessity (if that's what it is) and likelihood—i.e., if X is necessary in order for Y to take place, and if Y is the outcome desired, then I think we can assume that X is likely to take place, given human volition. Since we're fudging anyway, let's call it implication. However, there is another element in fail.

(52) The prosecution failed to call the one witness who could have shed some light on the matter.

(53) It makes no difference that Algernon failed, as usual, to do his assigned work.

(54) The one morning Merkel fails to arrive late, there's nothing for him to do until noon, anyway.

(53) eliminates any idea that mere unlikelihood or conditional necessity is all that is involved. The unacceptability of (54) shows that what we have in this set of examples is something like obligation. That is, in a context in which it is pragmatically presupposed that there is an obligation which is met, as in (54), fail cannot occur, even though the sentence makes it clear that arriving late was likely, and that there was no conditional necessity on his arriving late.

(43) is like (18) and (29) in that it can take any of the three readings, i.e., it is the obligation of the prosecution to call the witness; it is a conditional necessity if conviction is desired, and it is expected that the prosecution will call such a witness. Thus, I think we can construct a tree (albeit highly fudged) for fail, similar to that for manage.

(55) 

```
       FAIL
         /\  
        /   /
       OBLIGATION
         /\  
        /   /
       success
         /\  
        /   /
       CONDITIONAL NECESSITY
         /\  
        /   /
       LIKELIHOOD
```

Again, the hearer of a sentence involving fail will try to analyze its meaning with the presupposition of obligation, and if that is blocked, he will go on to conditional necessity, and if that is not possible, on to likelihood.

I am not at all sure what is being filtered out of obligation to produce necessity, or out of necessity to produce expectation of likelihood, although I suspect that the latter
may be something like volition. The 'X' may, of course, be intention, but I am not entirely happy with that answer.

As I mentioned before, I believe these vanishing presuppositions are special cases of Fillmore's progressively extended frames, discussed in this volume. The scenario is, after all, merely the background information which we bring to the understanding of any given word or utterance. Presuppositions are supposed to account for much of this background material.

By filtering out the various restrictions on the left hand side of the trees in (28) and (55) and proceeding down the line of presuppositions, then, we are progressively broadening the field of possible scenarios for occurrences of that predicate, and thus allowing more and more leeway for its usages and meaning. Dr. Fillmore has already demonstrated that possible contexts expand from a basic, central frame. This exercise in presuppositions of implicative verbs indicates, I think, that they expand in logical directions and in predictable order.

Footnotes
Those of you who have read the paper by Thompson and Wright in this volume will understand that I must be happy to absolve all those who helped me with this paper of any responsibility for the errors occurring therein. Among these kind people are Dr. George Lakoff, who, when I first noticed some of the peculiarities of manage, pointed out that my analysis seemed to be a special case of Dr. Fillmore's theory, and who spent a good deal of time discussing various aspects of implicative verbs with me, as well as suggesting the tree structure which I have used to analyze the sequence of presuppositions. Dr. Charles Fillmore discussed both his theory and my analysis with me before the conference and provided many helpful examples and insights. Finally, my deep thanks to Marlene Abrams, who listened to my ravings for some weeks, contributing a great deal of helpful discussion, while managing to happen to fail to allow me to lose my optimism about the project.

1 It is possible, of course, that such uses of manage originated as playful misuses of language, but the fact is that they are no longer playful, but rather are accepted as serious sentences, and must be dealt with on that level. Note, for example, the difference in tone between an obvious playful misuse of a predicate, as in (i), and a similar occurrence of manage:

(i) It wants to rain whenever I have a day off.
(ii) It manages to rain whenever I have a day off.

2 Another possible interpretation of (15)b is that manage
can be used because Harry was trying not to insult Ursula. However, this is just another form of difficulty. Something can be difficult either through its own inherent properties, as, for instance, if Ursula were a very easy-going person who never takes offense, or because someone (possibly yourself) is trying to keep you from doing it, as might be the case in (9).

3Of course, once human intention gets into it, all sorts of things happen. For example, it is difficult to do an all-nighter, although by no means unlikely. On the other hand, we might make a distinction between lexical "difficulty" and presuppositional "difficulty." The lexical item is used frequently when all we mean is "discomfort."

(iii) It's difficult to get up at 6:00 o'clock every morning.

We generally use sentences like (iii) when actually "getting up" at any hour isn't difficult at all--it's just very uncomfortable. Aside from the syrupy philosophical point that we create our own difficulties, I think there is a valid linguistic argument here.

(iv) It's difficult to learn a foreign language.

could be answered by something like "Yes, but we all have to because most schools have a language requirement." However, the amount of language that we learn for a language requirement isn't really difficult to learn--we just don't enjoy it. However, it is difficult to learn a foreign language thoroughly--and it's also very unlikely that any given person will do it.

Another argument that could be raised is that difficulty and unlikelihood can both apply either because of inherent properties or because someone is trying to keep the event from occurring. The difference between the two, then, is that difficulty involves volition, while unlikelihood need not. Thus, if something has no inherent difficulty, and no one is trying to keep you from doing it (including yourself), then it is not unlikely that you will do it. Examples like:

(v) It isn't difficult to fall down the stairs.

then, don't mean that it's likely that you will do so. If you are normal, you will try to keep yourself from falling downstairs.

4This sentence is still good even if we specify all sorts of things which eliminate simple likelihood of John's catching that train--e.g., it is his first day at work; he usually drives or takes a bus; the 8:30 train usually doesn't run, and so on.

5Karttunen, in his paper in this volume, takes fail as presupposing try. While the issue is not central to his
discussion, it might be best to avoid confusion by pointing out that this is not necessarily the case. For example, in (51), I do not think that we have to assume that Simon was expected to realize that the flights were cancelled, and he certainly did not try (and fail) to realize it.

References


Basques, Particles, and Babytalk: A Case for Pragmatics

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Pragmatics is catching on with the speed of a methadrine bat. And, rightly so. For several years we have concerned ourselves with the formalizing of rules, not taking into account the important facts of context that motivate and specify the conditions under which the rule in question may operate at all.

Within the domains of syntax and semantics we have a theory (or theories if you hold that there are real empirical differences between Generative Semantics and Interpretive Semantics) which gives us a basic framework in which to formulate and test data. It is doubtful, however, that such a theory exists in the area of pragmatics. There is Performative Theory (as best discussed in Sadock 1974) which attempts to account for the various forces underlying speech acts, and is thus able to account for a large part of the intent of the speaker. Performative Theory, however, comes dangerously close to bursting its seams if made to account for the more subtle factors of language use, such as deliberate ambiguity (Weiser 1974).

Much of the recent work of G. Lakoff (1971, 1974) is devoted to incorporating facts of context into existing theory, mainly by means of transderivational constraints. It is not clear, however, that facts of context should be included somehow in the derivation of sentences. Lakoff leaves the question open, but shows that such facts can be formalized using the transderivational constraints with a set of conditions on their operation.

What follows is not meant to contribute to the development of a theory of pragmatics, but rather offers data from Basque that is shown to be contextually dependent. Whatever develops as a theory of pragmatics will have to account for data like this. Despite the lack of a cogent theory much can be done to uncover and describe facts of context.

There are two independent problems at hand. One is the particle system in Basque, and the other is the use of palatalization in expressive speech. Of the recent work done in the area of pragmatics, most has been concerned with the interaction of syntactic rules and facts of context. The rule which moves Basque particles to their correct surface location (always the position of focus) is probably the same rule that puts the topic into the position of focus. The problem of focus in Basque is a difficult one, but is as interwoven with facts of context as it is in other languages. Thus, the Basque particles are subject to a syntactic rule that is motivated by pragmatic facts. The second problem, that of expressive palatalization in Basque, shows rules of phonology to be quite dependent on context.

I. The Basque Particles

Basque is an ergative language, exhibiting a complex system of inflection. Being a strong SOV language, Basque is only happy when suffixing. The particles, however, are perversely prefixing, proclitics to the verb. There are two conjugations: one, synthetic, is limited to only a few verbs; the other, periphrastic, consists of the infinitive form of the verb (sometimes marked for habitual or future) and an auxiliary
that carries all the agreement information. This auxiliary form is made up of pronominal markers referring to the speaker, the subject, the object, indirect object, and to the addressee of the utterance. In the second person familiar Basque makes its only gender distinction, using one marker for second person masculine singulars and another for feminine addressees.

The position immediately preceding the verb is the position of focus. Word order aside from this position is relatively free. The particles (with the exception of ba-/ba, bai which will be discussed below) always occupy the position of focus. Their function is similar to that of the modal adverbs in English, but they are not transportable. The particles are ba-/ba, bai 'affirmative', ez 'negative', omen (bide) 'reportedly', ote (oteh) 'perhaps', al 'interrogative', and edo 'probably'. The variants in parentheses represent forms present in dialects other than Gipuzkoan, the dialect exemplified here. There are several other particles occurring in the various dialects, but I have listed only those which occur in all the dialects. The particles, depending on their individual meanings, serve to alter the force of the proposition. Omen, ote, and edo are hedges, weakening or indirectly questioning the proposition, ez negates the statement, al (and oteh in the Northern dialects) changes a statement to a question, and finally, ba-/ba, bai serves to strengthen the force of the proposition. This latter particle is by far the most interesting and will be the focus of the discussion. Some examples of the other particles:

1) a. Patxi'i k lerkairekin Franco'ri etorriko da.
   Patxi - a bomb-with Franco-for- will come.
   b. Patxi'i k lerkairekin Franco'ri etorriko omen da.
      Patxi - a bomb-with Franco-for reportedly will come.
   2) a. Zure aitona joan da; Your grandfather went.
      b. Zure aitona joan ote da. Your grandfather perhaps went.
   3) a. Lerkaia undi bat dezu. You have a big bomb.
      b. Lerkaia undi bat al dezu? Do you have a big bomb?
   4) a. Sorgina menditik etorri da. The witch came from the mt.
      b. Sorgina menditik etorri edo da.
      The witch mt.-from probably edo.
   5) a. Sorgina LSA'ra Florida'n joango da.
      The witch LSA-to Florida-in will go.
      b. Sorgina LSA'ra Florida'n ez da joango.
      The witch LSA-to Florida-in not will go.

In the case of omen, ote, and edo, the particle indicates the speaker's evaluation of the proposition in which it appears. Omen, removes from the speaker the responsibility of the content of the proposition. It is similar semantically to 'they say' in English. Ote lends a sense of doubt as to the likelihood of truth of the proposition, and tends to indirectly question the proposition. The nearest equi-
valent to the function of *ote* in English is that of the rhetorical tag questions, with falling intonation. (R. Lakoff 1972) *Edo* assigns a degree of likelihood to the truth value of the proposition. *Al* is clearly interrogative and *ez* negates.

It could be claimed that *omen*, *ote*, and *edo* act like sentential adverbs in English and could therefore be accounted for in a Performative Theory similar to that discussed in Corum, 1974. 3 There are problems with such an analysis.

*Omen*, for instance, cannot really be accounted for in such a framework. *Omen* is translated as 'reportedly' or 'they say'. In order to use *omen*, both the speaker and the hearer must share a common understanding, sometimes called pragmatic presupposition (Keenan 1971), of who is responsible for the reporting, who the *they* of 'they say' refers to. In most cases the *they* of 'they say' does not have a specific referent, but the speaker is expected to be able to come up with a source should he be asked to do so. This notion of pragmatic presupposition cannot be accounted for in any existing theory of semantics. While the truth values of both 1) a. and b. are the same, one of the sentences may be appropriate under certain contextual conditions, whereas the other may be inappropriate, so the use differs, even though the two sentences have the same truth values.

*Ba/-ba, bai* is the most interesting of the Basque particles. It is also the most difficult to translate. It has no direct translation into English, and has many, many different uses and associated meanings in Basque. It also combines with other particles and other parts of speech to form idiomatic expressions that are likewise difficult to translate. Of the variants I have listed, *ba-* is proclitic, attached to synthetic verbs. As I mentioned above, there are two conjugations in Basque; the synthetic, or strong, conjugation is a single verb form, carrying tense and person information; the periphrastic conjugation contains a relatively stable verb form and an auxiliary form. Synthetic verbs cannot stand alone, or begin a sentence without the *ba-* proclitic.

6) *Badator mere maitea.*
   Is coming my sweetie-pie.

7) *Badakizu sorgiñak etorriko direla.*
   You know witches will come they-that (complementizer)

The synthetic verb forms are greatly limited in the language today, and for most of them there is a semantically equivalent periphrastic equivalent. When both a synthetic and a periphrastic variant do occur, the synthetic variant is felt to be the stronger. The frequency of the *ba-* proclitic with the synthetic verbs contributes to this stronger reading.

The other variants, *ba*, and *bai*, are free forms. They are semantically equivalent and are merely dialectal variants. In most dialects *bai* means 'yes', while in some dialects *ba* means 'yes'. I have not been able to determine if dialects using *bai* 'yes' use the *bai* form for the affirmative particle, and those with *ba* 'yes' use the *ba* form for the
affirmative particle, though I suspect this to be the case. *Ba* or *bai*, I will use *ba* in examples here unless they are examples quoted directly from a text written in a dialect that uses *bai*.

While *ba*- is a proclitic and thereby always in the position of focus, the free form *ba* occurs independently and is not restricted to the position of focus as the other particles discussed above. It often follows the proposition and serves to reinforce the proposition, to strengthen the force of the assertion.

8) a. Nekane'k lerkaia ekarrí du.  
   Nekane bomb-the brought it.
b. Nekane'k lerkaia ekarrí du *ba*!  
   Nekane bomb-the brought it, for sure!
9) Utzi ontzia, nik jasoko det *bai*! (from Alzo, pg. 177)  
   (You) left vase, I take care for sure!

You've left the vase, and I'll be sure to take care of it.

In the examples above I have translated the *ba* or *bai* as 'for sure', but it could mean 'certainly' or 'really' in the intensifier sense.

While *ba* is commonly attached to the end of a proposition it can appear near most any constituent in the sentence and serves to emphasize that constituent. Here again, *ba* shows a function similar to that of certain sentential adverbs, and some particles in English. (see Corum, to appear, and James 1973). Consider the following examples:

10) Elurra asi zan, *bai* laster etorri ziran aurrak. (Alzo)  
    Snow began to fall, very quickly came they the children.
11) Ciek *bai* direla gauze makurrek. (N'Diaye)  
    These certainly are things unlucky.

Unlike most sentential adverbs in English, *ba* can appear in questions. As was mentioned above, the position of focus is that position directly preceding the verb, and in a question the question particle *al* appears in the position of focus, as in 3) b. above. When *ba* occurs in a question, however, it takes the position of focus, and the *al* moves up one slot. The presence of *ba* serves to question the truth value of the whole proposition, not just question a particular constituent.

    Patxi ? is coming the witch with Yes, it is Patxi who is coming with the witch.
b. Patxi *ba* *al* dator sorgiña'rekin? *Bai*, badator.  
    Patxi ? is coming the witch with

'Is it the case that Patxi is coming with the witch?' 
'Yes, it is' (Lit. He is coming)

Note that if *ba* occurs in the question that it will appear in the answer.
A semantic note about the meaning of ba....the verb 'to
know' in Basque is a synthetic verb and for this reason takes
ba more frequently to begin with, but we note that 'know' al-
ways occurs with ba. Given a moment's thought it is noted that
'knowing' is not something that can be hedged, you either know
something or you don't, so it is not so unexpected that the affirm-
ative particle came to be associated with the lexical entry of this
verb. Ba can be used to strengthen a negation as in c. below:

13) a. Badakit. I know it.
   b. Ez dakit untsa. I don't know for certain.
   c. Bada nik ezdakit. It is for sure that I don't know it!

In the c. example above the ba lends a sense of absolutivity to the
negation. 4

There exists a strategic function of certain sentential adverbs
in English, like 'obviously', 'surely', 'certainly', and some adverb-
ial phrases like 'no doubt', 'needless to say', and 'of course'. In
some contexts they function to coax the hearer into believing the
proposition to which they are adjoined.

14) Obviously,
    No doubt,
    Surely,
    Needless to say
   } Patty Hearst is in Guatemala.

The speaker may not be certain, or even believe, that Patty Hearst is
in Guatemala, but for whatever reasons he may have, he wants the hear-
er to believe this, and uses the sentential adverb or adverbial phrase
as a strategic device to lend strength to the force of the assertion.
(See Corum, to appear, for a more detailed discussion of this function)

Ba has many different functions and associated meanings. One of
these functions appears to parallel that of the sneaky use of senten-
tial adverbs and adverbial phrases in English. The subtle differences
in the meaning of ba are very dependent on the context in which it
is used. Because ba is so context-dependent, it is often difficult
to elicit examples without setting some sort of context where the
ba might naturally occur. I have found Basque poker games to be one
of the best "contexts" for gathering examples. 5 Recently I tried
using ba in the following situation.....the betting in Basque poker
goes from player to player, first betting on the high card, then the
low card, then on pairs. In order to bet on a pair you must first
declare that you do have a pair. The dealer asks: "Pareja al dezu?"
'Do you have a pair?' to which you either reply: "Bai, pareja det."
'Yes, I have a pair.' or "Ez, ez det pareja." 'I don't have a pair.'
When it came my turn to bet on a pair I was asked if I had a pair, to
which I hasamzed the reply: "Bai, pareja bedet!" Following this, my
partner proceeded to bet a great deal on our hands, somewhat to my
horror, for while I did indeed have a pair, it was only a pair of
threes. In the rage that ensued following my exposure I was accused
of cheating, of declaring that I had had a "good" pair. When I argued
that I had only said that I had a pair, my partner yelled that I had
said "Pareja bedet!" with such strong stress on the ba that there was
little doubt that he had understood the *ba* to have a very strong intensifying function.

The point of this is the indication that *ba* can be used similarly to the sentential adverbs in English in their sneaky use, that of coaxing the hearer into accepting or believing the content of the proposition to which they are adjoined.

Finally, *ba* can occur with other particles. In the dialect of the Northern provinces, *ahal* is a modal meaning 'can, possibility' and is not the same as the *al* I discussed above, which had interrogative force. The particle with interrogative force in these dialects is *othe*, as was mentioned above:

15) *Ba ahal da bertze holako joiarik.* (Lafitte)
   'There are other such pearls.
   'There are other pearls, I think, of this type'

16) *Ba othe da bertze holako joiarik.* (Lafitte)
   'Are there other such pearls.
   'Do there really exist other pearls of this type?'

The translations given above do not clearly indicate the subtle ways that *ba* interacts with the other two particles. In 15) *ba* affirms the modal 'can'. Whereas 'can' is somewhat of a hedge, more clearly understood if the sentence had been translated 'There can be, etc.'; the *ba* strengthens, so the hedge and *ba* are contradictory in the ways that they affect the proposition. The translation, given by Lafitte expresses this combined force as a parenthetical.

The example in 16) is similar to the example 12) above where *ba* occurs in a question. In 16), however, the *ba* does not occur in the position of focus, i.e. immediately before the verb *da*, in which case it would serve to question the truth of the whole proposition, but rather it occurs in the position of secondary focus, in front of *othe*. Here it seems to indicate that the speaker expects a positive answer, the 'really' of the Lafitte translation is a request to confirm what the speaker already suspects to be the case. That the use of *ba* with *othe* indicates the speaker expects a positive answer is further confirmed by the following:

17) *Ba othe da nehor?* (Lafitte)
   'Is there really someone (there)?

18) *Ez othe da nehor?* (Lafitte)
   'Isn't there anyone (there)?

In the above examples the 'someone' and 'anyone' interpretations of *nehor* are determined by the presence of the affirmative *ba* or the negative *ez*. In 17) the speaker is assuming that there is indeed someone there, and is asking to confirm his belief. In 18) the speaker is assuming that there isn't anyone there, and expects a negative answer. A neutral way of asking if there was anyone there would not employ either the *ba* or the *ez*.
19) Nehor othe da?
Is anyone there? (No expectations)

In the discussion of ba-/ba, bai I have shown that this particle can be used to affirm or strengthen the force of an assertion, it can intensify a constituent within a proposition, can change the scope of a question from a constituent to the whole proposition, can be used in a way such as to coax the hearer to accept or believe the content of the proposition, can be used in combination with other particles, and finally, can be used in a question when the speaker expects a positive answer. Some of these various uses are dependent on context and on the reasons the speaker has for using them. Such facts cannot be wired into an analysis using a Performative Theory model. They could be accounted for using transderivational constraints that would hold over both the derivation underlying the surface structure and the derivation that is implied by the meaning of that derivation. Such an account implicitly claims that pragmatic information must be included in the derivation of a sentence, and given what little work has been done in the area of pragmatics, this is a premature claim.

As was previously mentioned, the other particles depend on facts of context as well. Omen requires that the speaker and the hearer share a common understanding of 'who' the 'they' of 'they say' can refer to. There are various reasons why a speaker may wish to remove from himself the responsibility of the content of the proposition. With ote, the force of the assertion is so weakened as to turn it into an indirect question. This is a case of an utterance having a double illocutionary force. It was also noted that despite the weakening effect of ote, the speaker still expects a positive answer or response.

The truth values for the sentences with or without the particles will be the same, but the conditions determining when they can be used appropriately differ. Although they have the same truth values there will be certain contexts where one will be appropriate and the other will not, e.g. in a poker game. While no widely-accepted theory of pragmatics exists to date, the kind of data discussed here is the sort of thing such a theory will have to account for.

II. Expressive Palatalization in Basque

Other than altering sentence stress or using nasalization for purposes of irony, I am not aware of the existence of any phonological rules that are completely dependent on context. In Basque, however, such an example exists. When speaking to a good friend, a child, or anyone with whom you share a certain amount of solidarity, Basques will palatalize several consonants which are not otherwise palatalized. For I should mention that palatalization does occur automatically given a strictly specified phonetic environment. These rules are given in the generalized schemata below:

\[
\begin{align*}
20) & \quad k, g, x, \quad \rightarrow \quad \text{front/} \quad _{i} \\
& \quad t, n, \quad \rightarrow \quad t' \quad n' / \quad _{i} \quad V \\
& \quad l, r, \quad \rightarrow \quad l' \quad \quad V
\end{align*}
\]

Automatic Palatalization
An example of this rule can be found above in the word sorgiña 'witch'.

In the case of expressive palatalization, however, there is no specific environment for the change. It is simply triggered by the feelings of the speaker. The following consonants undergo expressive palatalization:

21) \[l \rightarrow l'\] Example: polit pollut 'pretty'
\[n \rightarrow n'\] onon oñon 'bonbon'
\[s \rightarrow ŝ\] neska neška 'girl'
\[t \rightarrow ť\] tipi ţtipi 'little'
\[tz [c] \rightarrow č\] tsar čar 'naughty'
\[z [s] \rightarrow š\] zakur šakur 'dog'
\[d \rightarrow j\] (No. dialects) eder ejer 'beautiful'

Unless we are to say that phonological rules can 'look back' to semantic structure to find out how the speaker 'feels', there is no way to account for the facts of expressive palatalization in Basque. Even if the rule were able to look back to another level of the derivation, it isn't very clear how such information as to the speaker's feelings would be included in any level of derivation. We are faced with a case of a phonological rule completely dependent on the context of the utterance for its environment.

I mentioned early in this paper that the rule that moves NPs to the position of focus, immediately before the verb, would depend on pragmatic factors. The different meanings associated with ba when it is in final position, in the position of focus in a question, in the position of secondary focus in a question, or in the immediate environment of a particular constituent, indicate that the rule that delivers ba to its surface address is indeed dependent on facts of the context in which the utterance is made.

In conclusion then, we have found a syntactic rule and a phonological rule in Basque which both interact with pragmatic factors.

FOOTNOTES

* I am grateful to Maria Etchemendy of Gardnerville, Nevada and Eugene Aspetia of San Francisco, California, for their patient help. My poker partners prefer to remain anonymous, but their help in providing the proper "contexts" necessary for a study of the particles was invaluable, and costly.

1. A recent paper by Frances Karttunen shows the system of Finnish clitics to have several functional properties similar to those of the Basque particles.

2. The variant othe occurs in the Northern dialects and has a fully interrogative force.

3. This account derives sentential adverbs from a conjoined structure where the performative verb of the proposition is ASSERT. The second conjunct consists of the semantic structure of the adverb with its own illocutionary force based on the speaker's evaluation. I no longer consider this to be a viable analysis for all sentential adverbs. The use of evaluative adverbs is pragmatically more complex than
just saying they represent the speaker's evaluation of the proposition. This type of analysis does not capture the more subtle facts of use. The transportability of such adverbs is quite dependent on facts of context, such as speaker assumptions, previous discourse, etc. (See Chapter Two of Corum, to appear, for a detailed revision of the treatment of evaluative adverbs.)

4. Ba can be used to add strength to either an affirmative or negative reply:

i) Gezurra esan al dek?  
Bai, ba!

Lie-ä say ? you(masc. sg.)  
'Did you tell a lie?'

ii) Gezurra esan al dek?  
Ez, ba!

'Did you tell a lie?'

'Absolutely not!'

In ii) the reply 'Ez, ba!' gives the answer, no, and then the ba serves to affirm, or strengthen the answer.

5. Basque poker is called Muz, meaning 'face'. The game is played by four, forming two pairs of partners. When the hand, four cards, is first dealt, the partners can exchange information as to which cards they hold by a quick series of facial signals, such as a wink, raising an eyebrow, a quick smirk, etc. The signals are fixed and you are not allowed to signal something that you do not have. In a split second you must signal to your partner the contents of your hand, receive his signals, and at the same time try to catch the signals being exchanged by the other two partners.

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HOMONYMIC AMBIGUITY, PRONOMINAL SYSTEMS
AND THE CLITIC-LEM IN MODULAR LINGUISTICS*

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0

Introduction

0.1 My purpose in this paper will be to contest the notion of 'Surface Structure Constraints' as to its necessity for the solution of the clitic problem; a problem brought to our attention by the work of Perlmutter (1971), (ftn.1).

0.2 I shall not attempt, however, to prove that surface structure constraints are not necessary within the context of a transformational theory of grammar premising the ordered rule hypothesis. As a matter of fact I tend to agree with Perlmutter's conclusion that within the context of such a theory surface structure constraints are indeed necessary. His lucid and logical argumentative treatment of the clitic problem in Walbiri, French and Spanish bears ample proof to such a conclusion. I happen though to dissent from accepting the notion of a system of grammar such as Perlmutter premises in his work.

I shall therefore not attempt to prove my thesis by pointing out contradictions in Perlmutter's logical approach; for contradictions are to be considered as such only within the system in which they develop, and when the premises of that very system are a-priori refuted, as is the case here, the argumentation dialogue is necessarily somewhat blocked.

Nor shall I attempt to prove my thesis by pointing out inconsistencies in Perlmutter's data; for I tend to believe that his data are quite reliable, although in a few instances our consideration of their semantic evaluation differs basically.

0.3 This leaves me with one last option: to develop that portion of a new system of grammar, which I shall tentatively call here 'Modular Linguistics', in which all of Perlmutter's clitic data, together with other clitic data as well, are satisfactorily explained.

Modular Linguistics as a self-contained theory of language will not be presented here. I do not expect this, however, to affect in any negative way the conclusions of this study or the comprehension of its new approach.

The languages which were researched for this study are: English, Arabic, French, Spanish, Walbiri, Albanian, Italian, German, Russian, Persian, Czech, Japanese, Indonesian and Thai, of which the latter three do not exhibit clitics of the kind under consideration in this paper.

I shall prove that a specific clitic surface-feature, which I term 'Homonymic Ambiguity' and symbolize as $C^k$, is primarily the only feature controlling the final surface clitic order in the above-mentioned languages. The data for English, Arabic, Walbiri, French and Spanish will be presented in detail. Due to space considerations, the remaining languages will only be sparingly commented upon.

In order to prove my thesis I shall proceed to:

1. define $C^k$,
2. develop the new notion of a 'Clitic Linguistic Module', henceforth C-lem and define the mechanism of its functioning
in terms of $C_k$, and indicate its position in the derivation process of the surface clitic order.

3. finally embark on the consideration of clitic data in different languages and the solution to our problem.

4. I shall then close with some considerations of a general nature.

1. **$C_k$-definitions**

   1.1 The 'Pronominal and Clitic Systems' of English, Arabic, French and Spanish, henceforth the PCS of the specific language, as well as their respective $C$-lemms are given in the Appendix at the end of this paper.

   Depicted as PCS-rows are the NPG clitic subsets, where

   in general,

   $N =$ Number = (Singular, Dual, Plural).................. = (S, D, P)

   $P =$ Person = (First, Second, Third).................. = (1, 2, 3)

   $G =$ Gender = (Masculine, Feminine, Neutral, Impersonal) = (M, F, N, I)

   and where R stands for 'Reflexive'.

   For instance, in French, P3M is the Plural 3rd person

   Masculine pronoun and clitic subset.

   Depicted as PCS-columns are the semantic constituent subsets $C_k$ where $(k = 0, 1, 2, 3)$, and which, in order to keep

   with current terminology, are rendered throughout this paper

   respectively as

   $O^k = (0^0, 0^1, 0^2, 0^3)$

   $= (\text{Agent}, \text{Patient}^1, \text{Patient}^2, \text{Patient}^3)$

   $= (\text{Nominative}, \text{Accusative}, \text{Dative}^2, \text{Dative}^3)$

   $= (\text{Subject}, \text{Direct Object}, \text{Indirect Object}, \text{Beneficial Object})$

   $= (\text{Sbj}, \text{DO}, \text{IO}, \text{BO})$

   i.e. the superscript $(k = 0, 1, 2, 3)$ respectively denotes the

   semantic constituents ultimately giving rise to $(\text{Sbj}, \text{DO}, \text{IO}, \text{BO})$.

   Each $O^k$-column heads two subcolumns; the pronoun set

   $P^{O^k}$ and the corresponding clitic set $C_k$.

   The Agent/Subject and Patient/Object identification

   in the above will be dwelled upon under 2.1.

   The subscript 'i' in $C_i$ indicates the 'Homonymic Amb-

  iguity Value' of the clitic under consideration; a value

   presently to be defined under 1.2.

   We can see from the PCS that for French P3M we have:

   for $(k = 0, 1, 2, 3)$, $P^{O^k} = (\text{ils}, \text{eux}, \text{eux}, \text{eux})$

   and

   $C_1 = (\text{ils}, \text{les}, \text{leurs}, \text{leurs}, \text{leurs})$

1.2 The following $C_k$-clitic-subset definitions rest on the

consideration of clitic homonymity within each single NPG-row:

C$^0$ is the subset of Sbj clitics not homonymous with any of

BO, IO or DO clitics, e.g. Spanish S2 'tu' and French

P3M 'ils'.

C$^1$ is the subset of DO clitics not homonymous with any of

BO, IO or Sbj clitics, e.g. Spanish S3F 'la'.

C$^2$ is the subset of IO clitics not homonymous with any of

BO, DO or Sbj clitics.

C$^3$ is the subset of BO clitics not homonymous with any of

IO, DO or Sbj clitics.

C$^0$ is the subset of homonymous BO and IO clitics not

homonymous with any of DO or Sbj clitics, e.g. French

S3M 'lui' and P3M 'leur'.

C$^2$ is the subset of homonymous BO and DO clitics not

homonymous with any of IO or Sbj clitics.
is the subset of homonymous IO and DO clitics not homonymous with any of BO or Sbj clitics.

is the subset of homonymous BO, IO and DO clitics not homonymous with Sbj clitics, e.g. Spanish P1 'nos', Spanish dialect II (DII) S2 'te', French S1 'me' and all of the English and Arabic clitics.

is the subset of homonymous BO, IO, DO and Sbj clitics, e.g. Spanish S3I 'se' and Spanish dialect I (DI) S2 'te'. In the above definitions I have not considered any other logically possible C₁'s, for such did not occur in any of the languages researched for this paper and I suspect that they do not occur at all in any other natural language.

2.1 The C-lem

The cliticization process characterized by the C-lem accepts for input pronominalized (ftn.2) declarative (ftn.3) active grammatical (ftn.4) structures such as (i):

(i) (Sbj) (V) (DO) to (IO) for (BO).

(ii) (DO) (beVen) by (Sbj) to (IO) for (BO).

(IO) (beVen) (DO) by (Sbj) for (BO).

The order of the constituent Prok's in the above structures is not functional in Modular Linguistics. What is of importance here is whether the verb in the structure is (V) or (beVen); for as observation of language data tends to indicate, passivized structures such as (ii) do not qualify as C-lem input and thus cannot undergo cliticization. Given this observation and to keep with current terminology, I have identified above under 1.1, as one and the same the semantic notion of Agent and the syntactic one of Subject; for in a declarative active structure both Agent and Subject refer to the same semantic entity. The same is also true of the general identification Patient/Object. In Modular Linguistics, however, C₁'s will denote semantic, while (Sbj, DO, IO, BO) will denote syntactic notions.

2.2 The C-lem proper is formed for each specific language according to the characteristics of the C₁-set of that language. These characteristics are different for different languages (cf. PCS-tables in Appendix); it follows then that the C-lem's are different for different languages. However, though different, they are so only in degree, not in substance; for they all can be shown to derive from a universal C-lem which is defined in terms both of its own operation and mechanism, and of its sublem architecture.

2.3 The universal C-lem requires the following assumptions of the language to which it belongs:

(a) that it be that of an adult speaker of the language (ftn.5).

(b) that it allow for the cliticization of all of BO, IO and DO pronouns in at least one and the same sentence. For the purposes of this paper 'Cliticization' is not to be confounded with 'Pronominalization'. 'Cliticization' involves here the movement of pronominalized objects i.e. of pronouns, and their transformation thus into clitics.

(c) that it optionally allow for subject pronoun deletion and that it contain a C₁-clitic, e.g. Spanish, Albanian and Italian. Such languages will undergo what will be referred to as the operation of C₁-displacement (cf. KATHY below under 2.6 and in the Appendix).
2.4 The universal C-lem is a module whose building blocks are submodules \( C_0, C_2, C_1, C_4, C_2, C_3, C_4 \) and \( C_5 \) as defined above under 1.2 and arranged as in the following figure:

\[
\begin{array}{cccccccc}
C_0 & C_2 & C_4 & C_3 & C_2 & C_1 & C_4 & C_3 \\
\hline
C_0 & C_2 & C_4 & C_3 & C_2 & C_1 & C_4 & C_3 \\
\end{array}
\]

\( C_0 \) is not conceived as a C-lem sublem, and the symbol (1) denotes a Pointer or Scanner whose movement within the C-lem is well-defined (cf. footnotes to SALWA and KATHY in the Appendix).

2.5 The universal C-lem simplifies to the language specific C-lem because not all of the \( C_T \)'s which form the universal C-lem are usually found to exist in the specific language under consideration. The language specific C-lem's for English, Arabic, French and Spanish are shown in the Appendix, that for Walbiri is identical to that for English and Arabic. Notice the C-lem position relative to \( V \)-lem. The C-lem is inserted into that position after 'Pronominalization' but prior to 'Cliticization'.

2.6 The universal C-lem is depicted in blockdiagram in the Appendix. TURING is the program that describes the C-lem operation. It consists of an input block INTENT and of two subprogram blocks SALWA and KATHY. Due to space limitations I cannot undertake here the detailed description of the inner operations of these subprograms; the step by step derivations which are included in the Appendix will help the reader to meticulously follow these operations.

INTENT primarily registers which pronouns are to be cliticized and which are not. SALWA describes the cliticization operation proper and KATHY describes \( C_1 \)-displacement.

Whereas SALWA is language universal, KATHY is, in terms of changes it effects on the form of \( C_1 \)-clitics (cf. footnotes to KATHY in the Appendix) during and because of \( C_1 \)-displacement, language specific. In terms of the operations underlying \( C_1 \)-displacement proper, but not of the \( C_1 \)-clitic surface form change occurring, KATHY is also language universal.

2.7 The language that best demonstrates the operation of the universal C-lem is Spanish. It meets assumptions (a), (b) and (c) noted above under 2.3 and it contains a \( C_5 \)-clitic, namely 'se'. French does not meet assumption (c) and would make the demonstration easier. English, Arabic and Walbiri are in reference to their respective C-lem's identical and three of the simpler languages. Walbiri differs though from English and Arabic in that it contains \( C_0 \)-clitics while English and Arabic do not.

In the next section I shall discuss some derivations in English, Arabic, Walbiri, French and Spanish and I shall only advance a few comments on the remaining languages listed above under 0.3.

2  Derivations
2.1 General
2.1.1 In this section my main concern will be to acquaint the reader with the kind of derivations the modular C-lem approach provides us with.

The C-lem whose function is meticulously described by TURING is the only 'rule' controlling the cliticization process in all of the different languages considered here. It is thus a 'Universal of Language'. It is conceived of as being an
'obligatory rule' i.e. it must be gone through in any and all sentence derivations. The optionality of cliticizing one or more pronouns or none at all is a built-in TURING function provided for in INTENT.

3.1.2 I speak of an INTENT-input to denote the input sentence which is to undergo cliticization; of an INTENT-output feeding into a SALWA-input to denote the original sentence deemed by INTENT grammatical and candidate for cliticization, together with information as to which pronouns in it are to be cliticized; and of a TURING-output to denote the result of the overall cliticization process. The original sentence to undergo cliticization is thus deemed by TURING to be either cliticizable i.e. grammatical, in which case an output sentence O/GR is generated and printed out, or uncliticizable i.e. ungrammatical in which case the cliticization process has blocked somewhere and the information O/UNG is printed out.

3.1.3 The SALWA-input for each derivation is given as a parenthesized clitic quadruple $({C}^1, {C}^1, {C}^1, {C}^1)$ in which a symbol '#' substituting for a 'C' will denote a disregard instruction DRG.

Thus a SALWA-input candidate in French is $({#, le, lui, me})$. Its TURING-output is the ordered quadruple 'me le lui' in which the symbol '#' has been disregarded. To indicate this input-output quadruple pair I shall write: $({#, le, lui, me})/me le lui$.

3.1.4 The French and Spanish derivation lists of all of Perlmutter's clitic sentences, only partially included in the Appendix to this paper, use the above notation. The tables entitled 'Clitic Order Matrix', CfOM and COM, in the Appendix, list quadruple pair information, complete for English, Arabic, French and Spanish. Due to space limitations, tables of the remaining languages have not been included. For the assiduous scrutinizer, some actual step by step derivations have been provided. These show in complete detail the actual inner workings of the C-lem. Whereas the CfOM's and COM's only list the ordered quadruple pair (SALWA-input)/TURING-output, these step by step derivations show in detail what happens inside the C-lem from the moment SALWA accepts input to the moment TURING delivers output. The code numbers listed under 'DERIVATION STEPS' in these step by step derivations refer successively to the 'program step addresses' gone through in the derivation process. Such 'addresses' are marked next to each program step in the flowcharts of INTENT, SALWA and KATHY to be found in the Appendix. An example of one such derivation step is (S 18-19-20-25-GO TO KATHY-K 1-2.). It indicates that the program has successively gone through SALWA steps 18-19-20-25, then switched over to KATHY and then continued through KATHY steps 1-2, where it momentarily stopped to perform a change in the C-lem content. The C-lem content change resulting from the derivation step is listed under 'SENTENCE' to the right of the derivation step line.

3.1.5 Throughout the remainder of this paper, each sentence will have its own identifying code number. An example of such a code number is (...)Fr(1)P(49/91) where:

$$(...)=$$ Sentence identifying code in the present paper,
Fr = Language of the sentence under consideration, here French,
$$(1)=$$ Sentence identifying number in the unpublished version of this paper.
P = (Perlmutter, 1971); name of work where sentence originally appeared.
All the text and tables reduced in print in the Appendix to this paper are duplicate copies taken as is from the unpublished version of this paper, and therefore do not show the first (...) number of the code.

And now.........................ON WITH THE SHOW.

3.2 English and Arabic

3.2.1 English is an obligatory-subject language and Arabic, though an optional-subject language, does not contain any C₁-clitics. Both languages, therefore, do not undergo C₁-displacement and KATHY is always bypassed.

With respect to their C-lems, both languages are identical and two of the simpler ones. Because all of their clitics are without exception C₃'s, their C-lems simplify as shown in the Appendix. As a result, where neither 'emphasis' nor 'contrast' are intended, both languages do not permit, in one and the same sentence, cliticization of more than one pronoun, namely either Pro¹ or Pro² but never both together.

3.2.2 In the following examples of derivations '(1)' will denote the underlying structure US, '(2)' the pronominalized sentence, and '(3)' the cliticized sentence.

3.2.3

(E1 -1 ) I gave the book to Jane.
(E1 -2a ) I gave itp to Jane.
( b ) I gave the book to herp.
( c ) I gave itp to herp.
(E1 -3a ) (#,it,#,#)/it... I gave itc to Jane.
( b ) (##,her,#)/her... I gave herc the book.
( c ) (##,it,her,#)/*.....*I gave itc herc.
( c' ) same.................*I gave herc itc.

Notice that when emphasis or contrast are intended, Pro¹ which otherwise must cliticize, does not; and, given the right intonation, sentences paralleling (E1-3c*) in their surface structure are acceptable. Notice however that in sentences such as these the DO though pronominalized is not and cannot be cliticized. Thus we have

(E1 -3c* ) (##,her,#)/her..... I gave herc ITp (for emphasis).
(E1-3 ) (##,him,#)/him..... I give himc youp in marriage, not herp
( for contrast).

3.2.4

(E2 -1a ) He brings the book in.
( a' ) ?He brings in the book.
( 2a ) He brings itp in.
( a' ) He brings in itp.
( 3aa' ) (##,it,#,#)/it... He brings itc in.
( 3aa'* ) same.........................*He brings in itc.

The derivation of (E2-3aa'* ) is impossible simply because the English C-lem is positioned directly to the right of the English V-lem, thus allowing no element whatsoever, e.g. 'in', to interfere inbetween the both of them. Sentence (E2-2a') is ungrammatical as a final surface structure because DO, once pronominalized to Pro¹, must necessarily cliticize.

3.2.5

I gave the book to Jane.
Substitution of arabic 'ʔiyyaːhu' for english Pro¹ 'it',
of 'hu' for C¹ 'it' and of 'haː' for Pro²/C² 'her' yields:

 b (#,##/haː)/haː......... ?aʔaytu-haːc ?al kitaːba.
 c (#,h,haː,)/*............. *?aʔaytu-huʔ-haːc.
 c* same....................*?aʔaytu-haːc-huʔ.
 c** (#,##/haː)/haː......... ?aʔaytu-haːc ?ʔiyyaʔu.
 (E1'-3 ) (#,##/hu,)/hu,hi.... ?uʔti-i hi. ?ʔiyyaʔiʔa/kəʔawjat, 
 wa ʔaysa ?ʔiyyaʔa p, 

I give him you as wife and not her .

2.2.6 In the above examples I have considered only the verb 'to give/?aʔaːt'. Other verbs could have served as well; except that not all verbs that allow Pro²-clitization like 'to give/
?aʔaːt' match ir English and Arabic. Thus while 'to write' in
English allows Pro²-clitization, its counterpart 'kataba' in
Arabic does not. I cannot, however, consider here the reasons
for this behaviour.

3 Walbiri
3.3.1 Given Perlmutter's data, Walbiri seems to be an obliga-
tory-subject language that however always simultaneously cliti-
izes its sentence subject. It therefore does not undergo C₁-
displacement. I have noted above under 2.7 that the walbiri
C-lem is identical to that of English and Arabic. Strictly speak-
ing this is not completely true, although given the nature of
the walbiri C-set that Perlmutter provides us with, the walbiri
C-lem can simplify to the equivalent of the english and arabic
one. The walbiri C-lem is theotetically identical to the french
C-lem and like it, it is positioned before the V-lem. But con-
sider now the walbiri C₁'s and C₂'s. Unlike French which con-
tains in C₁ and C₂ clitics whose i= (0,1,4), Walbiri contains
only clitics whose i= (1,4); here I am discarding clitics whose
surface form is 'Ø' and whose 'i', by necessity, equals 'o'. If
I now set the only C₁-clitic 'laː1' to equal 'laː0' instead, the
walbiri C-lem will logically simplify to that of English and
Arabic and at the same time remain in its function logically
equivalent to the french C-lem when applied to the walbi-
ri C-set. In both the simplified and the unsimplified versions
of the C-lem, Pro³ does not cliticize (cf.3.3.4 below).

3.3.2 In the following, the change 'laː1' to 'laː' has been no-
ted and the resulting Perlmutter's (S¹, S², S³, P³) walbiri cli-
tics are respectively: C₀= (na, npa, 0, ?), C¹= (tyu, nku, ,
Ø, (Ø, tyanay) ) and C²= (tyu, nku, la, tyanay).

3.3.3 The following walbiri C[OM] predict the surface clitic
order in all of Perlmutter's walbiri sentence P(89, 93, 10, .31):
(C₀, Ø, C₂, #)/C₀C² (C₀, C¹, #)/C₀C¹, (C₀, C₄, C₅, #)/C₀C₅C₀ and
all others (C₀, C¹, C₂, #)/#.

3.3.4 Kenneth Hale (1973) provides more complete data to allow
for an in-depth study of the walbiri cliticization process. A
first reading of Hale's paper tends to indicate that his data are
C-lem predictable.

3.4 French
3.4.1 French is an obligatory-subject language and therefore
does not undergo C₁-displacement. Its C-lem is positioned di-
rectly before its V-lem. In a sense, it is a straightforward
case for the demonstration of the C-lem action. It is neither
as simple as English or Arabic nor nearly as complex as Spanish.
In the following examples of derivations I have deleted the
pronominalization step '2'.

106
2.4.2
(F1-1)

( 3a) (#,le0,#,#)/le
Tu donnes le livre aux étudiants.
You give the book to the students.

( 3b) (#,#,leur1,#)/leur
Tu le donnes aux étudiants.
Tu le donnes à eux.

( 3c) (#,le,leur,#)/le leur
Tu leur donnes le livre.
Tu le leur donnes.

2.4.3
(F2-1)

( 3a) (#,les0,#,#)/les
Tu donnes les livres à moi.
You give the books to me.

( 3b) (#,#,me4,#)/me
Tu les donnes à moi.

( 3c) (#,les,me,#)/me les
Tu me donnes les livres.
Tu me les donnes.

2.4.4
(F3-1)

Je donne toi à lui en mariage.
I give you to him in marriage.

Je te donne à lui en mariage.

*Je lui donne toi en mariage.
/lui; with emphasis or contrast intended.
Je lui donne TOI en mariage.

......., et non pas Jane.

*Je te lui donne en mariage.

2.4.5
(F4-1)

Je dirai cela à John pour toi.
I will say this to John for you.

Je te le dirai.

*Je te lui dirai.

2.4.6
Comparison of (F3-3c) and (F4-3b) indicates that the clitic sequence 'te lui' is in and by itself neither grammatical nor ungrammatical.

2.4.7
Notice that if DO 'cela' is deleted in (F4-1) the sentence will be deemed ungrammatical by INTENT first constraint
(/or#,NE,v/)/.*. Thus

(P4-3c) (#,NE,lui,te)/

*Je te lui dirai.

Notice however that in this case the original US sentence is ungrammatical prior to C-lem entry. This is why I have noted in INTENT that the first constraints might very well be superfluous.

2.4.8 The derivations of eight of Perlmutter's French sentences are given in the Appendix. Also, as indicated in the Appendix, notice that the set of clitics (me;te;nous;vous;se) which appears as a constituent in Perlmutter's surface structure constraint on French clitics P(57/121) exactly matches the French C4-set according to the C-lem approach.

2.4.9 I have not considered here sentences in which the French clitics 'y' and 'en' appear P(59/124..130), nor sentences whose verb is a compound of two or more verbs, e.g. 'laisser voir/to let see' P(55/111..114), but it can be shown that these can be easily incorporated in the C-lem theory. All others of Perlmutter's French sentences are found to be C-lem predictable.

3.5 Spanish

Spanish is an optional-subject language and it contains a C1-clitic. It therefore undergoes C1-displacement i.e. KATHY comes into action here.

C1-displacement achieves in Spanish what Perlmutter's 'spurious se rule' P(22/10) was set up to accomplish. Like the 'spurious se rule' C1-displacement requires a C1-clitic surface
form change. In Spanish it is the following: all $C_1$-clitics will change into 'se'.

3.5.2 $C_1$-displacement is, however, not to be equated with the 'spurious se rule'. For KATHY, which describes the function of $C_1$-displacement, is a universal process that applies not only in Spanish but also as well in Albanian and Italian, and in all other optional-subject languages containing a $C_1$-clitic, while the 'spurious se rule' does not make such a universal claim.

3.5.3 Unlike English, Arabic, Walbiri and French, Spanish raises the possibility that in one and the same sentence all of the four clitics in the SALWA-input quadruple might be marked by INTENT for cliticization. But as we can observe in Spanish COM-13, (C0C1C2C3)/*, this cliticization is always ruled out as ungrammatical. This conclusion can be easily proven to follow logically from the TURING operation itself. Given my observations to this date, I can safely predict that in any and all languages the maximum number of clitics, of the kind that we are dealing with in this paper, that can appear together in one and the same sentence is 'THREE', and that this fact is a Universal of Language.

3.5.4 Observation of Spanish data P(51/94-95-96) reported in the Appendix, leads to the necessary conclusion that the Spanish two dialects reported by Perlmutter differ only in that in one dialect DI the clitic 'te' is $C_5$, while in the other DII it is $C_4$. In DI but not in DII 'te' is conceived of also as Sbj-clitic.

3.5.5 A derivation list of all of Perlmutter's Spanish sentences is available; it cannot, however, be included here. Relying upon this list, I can safely state that all of Perlmutter's Spanish sentences are C-lem predictable.

3.5.6 To state only a few observations: we now have an explanation for:

(a) the ambiguity of P(62/141) 'Te me recomendaron/(i) they recommended me to you, and (ii) they recommended you to me'. P(62/141) can derive from both (#,me,#,te) and (#,te,#,me).

(b) the ungrammaticality of sentences such as P(70/161b)
'*Ramon me le complico la vida a mi hija, pero a mi hijo no se me la complico'. It is not ungrammatical due to clitic order considerations but due to what I have termed 'Semantic Clash', a notion not defined in this paper.

(c) the ungrammaticality of sentences such as P(76/174ab)
'*Me lo queria seguir gritandome, and *Me lo queria seguirme gritandome'. They are both ungrammatical due to the occurrence of the grammatical clitic sequence 'me lo' more than once in the sentence, with no apparent justification.

3.5.7 In the Appendix I have included derivations of and comments upon Perlmutter's sentences P(51/94-95-96). These, together with others not reported on here, prove beyond any doubt my contention that in Spanish DI 'te' is a $C_5$-subject clitic.

Here, a note is in order on the verb 'echar' which appears in P(51/95): In his 'Dictionary of the Spanish and English Languages', Velazquez (1967:257-258) reports that 'the verb 'echar' is well described by a Spanish lexicographer as a verb of general utility; and that) it serves frequently to assist the meaning of another verb, (and that) it enters into many phrases'; whereupon he lists over a hundred different meanings for 'echar' as it appears in different phrases. Some of these meanings translate into English reflexives, some do not. Given that P(51/95) 'Te me le echaste encima' translates into French as 'Tu me lui as sauter/to jump', I translate
it into English as 'You jumped him over (on me)'. Notice that thus we have the lexical equivalence 'encima/dessus/over', and the syntactic quasi-equivalence of the sentence in the three languages. The Spanish sentence and its French translation are only semantically, but not syntactically, equivalent to Perlmutter's English translation 'You threw yourself on him on me'. Thus, I differ basically with Perlmutter in my interpretation of this sentence.

2.5.8 I have also included in the Appendix one of Emonds Spanish sentences as reported by Rivero (1973:1698/1) 'El hombre que quiere lavarse se fue/The man who wants to wash himself left'. It is apparent that its derivation is a straightforward case of cliticization dealing not with one but with two C-lem's. (In Appendix text please correct 'to elope' so as to read 'to flee').

2.5.9 Due to space limitations, it is quite impossible to give the clitic problem in Spanish its due consideration here. Much has necessarily been left out, and the many predictions, explanations and foundations of ungrammaticality that it affords us will have to wait for another forum.

3.6 Comments on the remaining languages

Albanian and Italian, like Spanish, are languages that undergo C₁-displacement. their respective C₁-clitic surface form changes are indicated in the footnotes to KATHY in the Appendix. The Albanian clitic order is completely C-lem predictable. That of Italian is C-lem predictable except for one category of instances involving the clitic 'si'. This category of seemingly unpredictable instances can however be explained by incorporating an otherwise well-motivated principle into our theory of Language. Roughly, this principle states that 'the linguistic system of a specific language is, like Man himself, an adaptive system; and that it therefore tends to reach its optimal state of development by successively striking a middle ground between its own hardware/software structure and an optimal state of understanding and comprehension of its output messages by other like-systems in its own family'.

This principle, which I metaphorically term 'L-SURVIVAL', motivates us to incorporate a new rule specific to Italian and based on an extended notion of 'Homonymic Ambiguity', in order to completely explain and predict the Italian surface clitic order. In German, L-SURVIVAL has the effect that the C-lem allows only for Pro₁-cliticization, never for Pro₂; the result is that in German we are always dealing with a surface clitic order which is C₁Pro² and which happens to be homonymic with Pro₁Pro², i.e. in German, due to specific reasons not stated here, L-SURVIVAL opts for the simplification of the C-lem action. In French and Spanish, it is also L-SURVIVAL (if not pre-C-lem semantic constraints) that rules out all, except for one, identical i.e. homonymic surface sequences of clitics generated by the C-lem. In Russian, the C-lem can optionally be positioned either before or after the V-lem, and the surface clitic order is completely C-lem predictable. Work is in progress on Persian and Czech; and Japanese, Indonesian and Thai do not exhibit clitics of the kind we have been discussing here.
Concluding remarks

My involvement with the clitic problem stemmed mainly from a deep-seated dislike of mine of the notion of 'Surface Structure Constraints'. At the very moment I became aware of the existence in Linguistics of such a notion, I found that it not only disturbed my sense of scientific symmetry but also that it ran as well counter my sense of scientific logicty. For what are 'surface structure constraints' if not an easy backstage comeback to a quasi-tagnemicist view of how language phenomena are to be handled? The argument that, in Perlmuter's approach, surface structures have been 'generated' should not escape our scrutiny. Theoretically, the generation by any means of an infinity of surface structures of which the ungrammatical ones are to be ruled out by surface structure constraints, is simply equivalent to no more than just a description of the remaining grammatical surface structures of the language. No matter what the generation means are, if but one surface structure constraint of the kind Perlmuter proposes is allowed into linguistics, the fact will necessarily remain that such a linguistics will basically always be of the descriptive kind. I wholeheartedly agree with Suffer (1974: 149-150) in her evaluation of surface structure constraints: '...it is pertinent to ask about the explanatory power of a device such as SSC. It has none. A SSC does not explain anything. It is merely an expression of despair, like throwing one's arms up in the air and confessing that in spite of all the powerful machinery we have at our disposal, we are not able to filter out ill-formed sentences, so we give up and add an extra piece of machinery at the end of the production line in order to discard the 'bad sentences'.

In order to solve the clitic problem, I have developed in this paper, rudiments of a novel approach to research, namely that of a new modular linguistics methodology. I have also introduced a number of new notions, namely those of the 'clitic linguistic module, C-lem', of the L-SURVIVAL principle and of the surface feature of 'homonymic ambiguity' which I have shown to play the central role in the syntactic derivation of the surface clitic order in many languages. I have presented complete solutions to the clitic problem in English, Arabic, Waalbiri, French and Spanish; and I have indicated that the problem is solved in Albanian, Italian, German and Russian. Much, however, had to be left out.

The preponderance of evidence presented here in favor of the new theory cannot be denied. But the strength of the argument for Modular Linguistics and the C-lem mechanisms lies not in that they explain clitic data, but in that they explain it not only across different languages, but also across different families of Language; and also in that the C-lem is a principled mechanism, only one in number, and not an ad-hoc pragmatic multitude of mechanisms. The strength of the argument points to a 'Universal of Language', and its challenge for the incorporation of this Universal, if at all possible, into the structure of current linguistic theories.

Generally, theories are always in a state of flux. At a certain point in time, a theory is perceived to be less than adequate if the unexplained data that it contains, the contradictions that it generates and the ad-hoc constraints that it requires greatly outweigh the explanations that it provides. Such is today the state of linguistic theory.
It is only when a new theory is introduced, that has the power to overwhelmingly explain the data, ease out the contradictions and do away with the ad-hoc constraints, that the old theory is proven wrong to the satisfaction of a new generation of researchers and in time discarded by them in favor of the newcomer. Such a new theory was at the basis of this presentation.

..................CURTAIN.

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Notes
* This paper heavily relies on data appearing in (Perlmutter, 1971). The reader is well-advised to consult Perlmutter's book in conjunction with this paper.
** 'The views of the author do not purport to reflect the position of the Department of the Army or the Department of Defense.'
(1) Dinnsen (1972) subsequently added to the notion of 'surface structure constraints' in Spanish, and to my knowledge Lakoff (1968) used it in at least one of his papers.
(2) Modular Linguistics will eventually allow cliticization to take place without pronominalization having first taken place. Some Spanish and some French sentences give good indication that the above might very well be the case.
(3) Although not covered here, imperative structures such as French 'Donne le lui/Give it to him' and 'Donne le moi/Give it to me' are easily explained within the C-lem approach.
(4) Although in the final analysis the C-lem input will have to be grammatical - in the sense that structures whose underlying structure is itself ungrammatical prior to C-lem entry will not be generated and thus will not possibly become candidates for cliticization - in this paper, I have built into the C-lem INTENT-block (cf. 2.6 below) such mechanisms as to rule out ungrammatical structures in the sense noted above. Such mechanism can however be completely omitted
from INTENT without in the least affecting the C-lem over-
all function and output. The inclusion of such a mechanism
here is due to the fact that the tables given in the Appen-
dix to this paper, are duplicate copies of tables appearing
in the unpublished preliminary version of this paper and
that they could not be rewritten at this time. The same
is also the reason why the 'current terminology' (Sbj,DO,
IO,BO) and not the modular OK-terminology is used through-
out. Such mechanism as noted above can be viewed as a dou-
ble check on the grammaticality of C-lem input-structures
without however carrying in the least any theoretical
weight or meaning.

(5) The realm of a study interested in the possible evolution
of the C-lem from child to adult speaker of the language
is that of psycholinguistics and might prove to be very
enlightening.

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The Appendix follows....
HOMONYMIC AMBIGUITY, PRONOMINAL SYSTEMS
AND THE CLITIC-LEM IN MODULAR LINGUISTICS

Post-meeting notes

1. A question was raised during the meeting concerning the notational semantic/syntactic identification Agent/Subject and Patient/Object discussed under 1.1, 2.1 and in ftn.4. So as to dispel any possible misunderstanding, the reader is kindly requested to read throughout \((0^0, 0^1, 0^2, 0^3)\) for \((\text{Sbj}, D_0, I_0, B_0)\) respectively, keeping in mind that we are dealing here exclusively with semantic not syntactic categories, i.e. in all of the following three structures:

(a) John gave the book to Jane (for Jack).
(b) The book was given by John to Jane (for Jack).
(c) Jane was given the book by John (for Jack).

0^0 is the semantic category relation that holds between 'John' and the verb 'to give'.
0^1 is the semantic category relation that holds between 'book' and the verb 'to give'.
0^2 is the semantic category relation that holds between 'Jane' and the verb 'to give'.
0^3 is the semantic category relation that holds between 'Jack' and the verb 'to give'.

2. Contrary to what is stated under 2.1 some French passive structures can undergo cliticization, e.g. 'Le livre lui a été donné/The book was given to him'. This paper deals however exclusively with active declarative structures and with this in mind its conclusions stand as stated.

3. The walbiri case discussed under 3.3 is not rigorously defined. Its description follows a permissible logical simplification that is however not explicitly stated. Its conclusions stand as stated.
INPUT

TURING

EDITENT

SALWA

KATHY

Q/UNG. (output *).

Q/GK. (output sentence).

FIRST CONSTRAINTS

<table>
<thead>
<tr>
<th>IF</th>
<th>SBj/C²</th>
<th>DO/C¹</th>
<th>IO/C²</th>
<th>BO/C³</th>
<th>THEN GOTO</th>
</tr>
</thead>
<tbody>
<tr>
<td>#</td>
<td>/</td>
<td>/</td>
<td>/</td>
<td>/</td>
<td>SALWA</td>
</tr>
<tr>
<td>/or#</td>
<td>/</td>
<td>NE</td>
<td>/</td>
<td>/</td>
<td>SALWA</td>
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<tr>
<td>/or#</td>
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<td>/or#</td>
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<td>NE</td>
<td>/</td>
<td>OR/UNG!!</td>
</tr>
<tr>
<td>/or#</td>
<td>NE</td>
<td>NE</td>
<td>NE</td>
<td>/</td>
<td>OR/UNG!!</td>
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<td>/or#</td>
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<td>NE</td>
<td>NE</td>
<td>NE</td>
<td>OR/UNG!!</td>
</tr>
<tr>
<td>NE</td>
<td>NE</td>
<td>NE</td>
<td>NE</td>
<td>NE</td>
<td>NO SENTENCE</td>
</tr>
</tbody>
</table>

(1) Unless BO can in some way be considered IO in the language, in which case GOTO SALWA.
(11) Unless the verb in the sentence is intransitive, in which case GOTO SALWA.

GENERALLY:
(a) DO-pronoun always cliticizes except when emphasis is intended.
(b) IO-pronoun cliticizes if and only if DO-pronoun cliticizes, or DO appears in the sentence.
(c) IO-pronoun cliticizes if and only if either IO or DO is pronounized and (d) holds true.
(d) BO-pronoun cliticizes if and only if either IO-pronoun and/or DO-pronoun cliticizes.

INPUT

((**))

1 READ AND STORE FROM SENTENCE ALL Cκ; TO BE CLITICIZED, CONSIDER ONLY CLITICS WHOSE κ SHOWS IN A Cκ-SUBLEM. FLAG / EACH Cκ INTENDED FOR CLITICIZATION.

2 SET ALL EXISTING OBJECTSκ AND/OR PRONOUNSκ NOT TO BE CLITICIZED = #, AND SET THEIR RESPECTIVE κ = DISREGARD. (SW).

3 OUT OF κ=(0,1,2,3), SET ALL NONEXISTING OBJECTSκ AND/OR PRONOUNS = NE.

(*)

4 CHECK FIRST CONSTRAINTS ACCORDING TO TABLE ENTITLED 'FIRST CONSTRAINTS'.

5 SET ALL NE = #.

Q/UNG.

SALWA.

(*) These first constraints may turn out to be universal and logically derivable from the C-len operation or otherwise they may turn out to be pre-C-len entry constraints. Also, as table indicates the underlying subject SBj is assumed always to exist.

(**) Given the C-set for a specific language, it can logically be predicted whether DO-pronoun can or cannot cliticize into C² in that language. As is the case in Arabic and English, this prediction allows for simplification in the construction of the C-len proper, where, as we shall see later, the C²-sublem completely drops out. In order to prove this in truly the case, one must assure the existence of C²-clitics in the language under consideration and prove that they cannot possibly cliticize for otherwise, without the stated assumption, one would have to beg the question. Thus, one finds that while in French, Spanish and Albanian DO-pronouns do cliticize, they cannot possibly do so in Arabic or English. (Note that for all languages considered except for Czech the C²-set is identical to the C²-set of clitics.)
(*) Movement of Scanner P (1):

1. P-start is at $O^2$, (logical simplification yields P-start at $O^2$)
   (in Spanish, $C^2$ in French and $O^2$ in both English and Arabic).
2. From $O^2$ P moves right into the least C-lem sublem.
3. P changes its direction of movement whenever it reaches a
   C-lem border sublem from within the C-lem itself.
4. P moves left whenever a 'block-lem sublem', (x), is introduced
   at P within the C-lem itself.
5. P cannot cross over a blocked sublem.

(**) Loop:- (This step might turn out to be superfluous.)

If P performs a loop, i.e., if P returns to one and the same
sublem after having changed its direction of movement without
an 'insert-clitic' operation having taken place in the mean-
while, then SALWA will output O/UNG.

(***1) Step 17 is optional in some languages, e.g., Spanish and French.
More specifically, when the pronoun is criticized, it is the
original object itself that usually appears in the sentence
rather than the pronoun.

P1: For purely logical considerations step 8 should precede rather
than follow step 1). This, however, does in no way affect
the linguistic derivations presented in this paper.

The diagram [ ] denotes an operation to be performed, thus
read 1 instructs the system to read 1 in $C^1$ e.g., if $C^1 = A^1$,
1 = 5. The diagram [ ] denotes a decision to be made by
the system, with two and only two possible output 'yes' or
'no'; thus [ ] asks whether $k = 1$; if $k = 1$ then the
output is 'yes', if $k \neq 1$ then the output is 'no'.
**KATHY**

1. **IS A CLITIC**

   1. **REPLACE**
   2. **CONSIDER**
   3. **READ AND STORE**
   4. **PERFORM LANGUAGE SPECIFIC CHANGE**

   **C**

   5. **SET R=0**

   6. **CAN MOVE LEFT ONE STEP**

   7. **MOVE P LEFT ONE STEP**

   8. **CLITIC AT P**

   9. **SET R=1**

   10. **AND CLITIC AT P CONTRACT**

   **C**

   11. **ENTER C**

   12. **ENTER C**

   13. **AT P**

   14. **ENTER C**

   15. **ENTER C**

---

**Fig.: KATHY**

---

**Notes:**

- **Language specific due to KATHY steps K4 and K10.**
- **Language specific changes:**
  - Clitic always $g'$ in Spanish $C_i$, in Italian $e = C_i$, in Albanian $C_i$ same but can undergo contraction.
- **R=0** means that P has not previously crossed over a non-vacant sublem.
- **R=1** means that P has previously crossed over a non-vacant sublem.
- **When P is at the leftmost sublem of the C-lem, e.g. C5 in Spanish and C6 in Italian, then P cannot move left anymore and the output is 'NO'.**
- **At this point some clitics in Albanian will undergo contraction.**
### English: PSG: Pronominal and Clitic Systems

**DERIVATION STEPS**

<table>
<thead>
<tr>
<th>S1</th>
<th>Fez</th>
<th>D0</th>
<th>D1</th>
<th>D2</th>
<th>D3</th>
<th>D4</th>
<th>D5</th>
<th>D6</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>S1</td>
<td>none</td>
<td>me</td>
<td>me</td>
<td>same</td>
<td>same</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>you</td>
<td>you</td>
<td>you</td>
<td>you</td>
<td>as DO</td>
<td>as DO</td>
<td></td>
<td></td>
</tr>
<tr>
<td>SW</td>
<td>me</td>
<td>him</td>
<td>her</td>
<td>him</td>
<td>her</td>
<td>him</td>
<td></td>
<td></td>
</tr>
<tr>
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<td>it</td>
<td>it</td>
<td>it</td>
<td>it</td>
<td>it</td>
<td>it</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td>we</td>
<td>us</td>
<td>us</td>
<td>us</td>
<td>us</td>
<td>us</td>
<td></td>
<td></td>
</tr>
<tr>
<td>F2</td>
<td>you</td>
<td>you</td>
<td>you</td>
<td>you</td>
<td>as DO</td>
<td>as DO</td>
<td></td>
<td></td>
</tr>
<tr>
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<td>them</td>
<td>them</td>
<td>them</td>
<td>them</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**SENTENCE**

Fredd gave it to Jane.

### Arabic: PSG: Pronominal and Clitic Systems

**DERIVATION STEPS**

<table>
<thead>
<tr>
<th>S1</th>
<th>Fez</th>
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<th>D1</th>
<th>D2</th>
<th>D3</th>
<th>D4</th>
<th>D5</th>
<th>D6</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>S1</td>
<td>none</td>
<td>tiya:ya</td>
<td>si:ya</td>
<td>ka</td>
<td>ka</td>
<td>same</td>
<td>same</td>
</tr>
<tr>
<td></td>
<td>you</td>
<td>tiya:ka</td>
<td>ka</td>
<td>ka</td>
<td>as D1</td>
<td>as D0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>SW</td>
<td>tiya:he</td>
<td>ki</td>
<td>ki</td>
<td>ki</td>
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<td></td>
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<tr>
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<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Pl</td>
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<td>ha:</td>
<td>ha:</td>
<td>ha:</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F2</td>
<td>you</td>
<td>tiya:kuna:</td>
<td>kuna:</td>
<td>kuna:</td>
<td>kuna:</td>
<td>kuna:</td>
<td>kuna:</td>
<td></td>
</tr>
</tbody>
</table>

**SENTENCE**

Fredd gave her the book.

---

**Note:** Because all clitics are C1, one and only one pronoun, namely either Pro1 or Pro2 but never both, can cliticize in one and the same sentence.
**ENG**

**Derivation Steps**

**US**

Promononalization
I 1-2-3-4-5, Go to Salwa: 1) sbj do i0 do

**PS:** The ungrammatical sentence 'Fred gave her, ite,' has the same blocked derivation as (EngDer, 1)

**PS:** The grammatical sentence 'Fred gave her, ite,' has the same derivation as (EngDer, 2) with prononationalization but not cliticization of DO for reasons of emphasis as noted in the text. To indicate emphasis we could write the sentence as 'Fred gave her, ITe,'. Notice that emphasis of DO-pronoun here is achieved by de-emphasizing IO-pronoun through cliticization.

**FRENCH**

<table>
<thead>
<tr>
<th>NPQ</th>
<th>sbj</th>
<th>do</th>
<th>io</th>
<th>bo</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pro0</td>
<td>C00</td>
<td>C1</td>
<td>C1</td>
<td>C1</td>
</tr>
<tr>
<td>Pro1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pro2</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>C2</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pro3</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>C3</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

I | S1 je | moi | meu | same as | meu |
| you | S2 tu | toi | teu | lui |
| he | S3N il | lui | le0 | lui |
| she | S3F elle | elle | leu | lui |
| we | S1 nous | nous | nous | nous |
| you | S2 vous | vous | vous | vous |
| they | S3M ils | eux | le0 | leur |
| they | S3F elles | elles | leu | leur |
| S3E on | moi-meme | meu | meu |
| S3N -il | Pro-meme | meu |

**Derivation**

**US**

Promononalization
I 1-2-3-4-5, Go to Salwa: 1) sbj do i0 do

**PS:** The derivation of the ungrammatical sentence 'He brings ite, ite,' is impossible simply because the C-lens in English is positioned directly after the verb thus allowing no element to interfere inbetween the both of them.

**PS:** The sentence 'He brings in ite,' or 'He brings in ITe,' is grammatical as the output of Pronononalization only if emphasis of DO-pronoun is intended. (see PS of (EngDer, 3))
<table>
<thead>
<tr>
<th>PrDmrA cont.</th>
<th>PrDmrA'</th>
<th>PrDmrA''</th>
</tr>
</thead>
<tbody>
<tr>
<td>Je te le lui dirai cela.</td>
<td>Je te le lui dirai cela.</td>
<td>Je te le lui dirai cela.</td>
</tr>
<tr>
<td>I you him will say this.</td>
<td>I will say this to him for you.</td>
<td>I will say this to him for you.</td>
</tr>
<tr>
<td>SENTENCE</td>
<td>SENTENCE</td>
<td>SENTENCE</td>
</tr>
</tbody>
</table>

**DERIVATION STEPS**

Same as (PrDmr.4) but without pronominalization of DO 'cela', See (PrDmr.1)'.

Note that the sequence 'te lui' is not ungrammatical here, while it is ungrammatical in (PrDmr.4).

**REFERENCE #**

<table>
<thead>
<tr>
<th>'SALVA'-INPUT</th>
<th>'TURING'</th>
<th>PERLMBUT'S FRENCH SENTENCES</th>
</tr>
</thead>
<tbody>
<tr>
<td>F(1)(P39/91)</td>
<td># le vous# vous le</td>
<td>Gertrude vous le donnera.</td>
</tr>
<tr>
<td>F(2)(P39/92)</td>
<td># le lui # le lui</td>
<td>Gertrude le lui donnera.</td>
</tr>
<tr>
<td>F(3)(P53/102)</td>
<td># le # le</td>
<td>Roger t'as avance une pente, pue.</td>
</tr>
<tr>
<td>F(4)(P53/103)</td>
<td># le te # le</td>
<td>Roger t'as avance une pente.</td>
</tr>
<tr>
<td>F(5)(P53/104)</td>
<td># nous# nous</td>
<td>Roger nous avance une pente.</td>
</tr>
<tr>
<td>F(6)(P54/505ab)</td>
<td># nous# nous</td>
<td>Roger nous avance une pente.</td>
</tr>
<tr>
<td>F(7)(P54/108)</td>
<td># vous# vous</td>
<td>Roger vous m'a avance une pente.</td>
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<tr>
<td>F(8)(P54/109ab)</td>
<td># vous# vous</td>
<td>Roger vous m'a avance une pente.</td>
</tr>
</tbody>
</table>

**COMMENT ON:**

Perlmutter's SSC on French clitics:-

's (me) III III & en 1 '

(tu) / (vous) / (nous)

Notice that in the above SSC the component (me,te,nous,vous,de) is exactly the French 

C1-set according to the C-lem theory. (See text)
null
To me he is like a son.

(Perlmutter's English translation reads: "You are like a son to me.")

This is an essential translation. The literal translation of the Spanish sentence under consideration, first, the verb under consideration is "to be" ("ser") not "to have" ("tener"), it cannot possibly be thought of as reflexive. This brings us to the literal translation: "You jumped into my arms" ("me abrazaste"), leaving "loving" as the undisputed S of the sentence.

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An Alternative to Checklist Theories of Meaning

Charles J. Fillmore
University of California, Berkeley

There seem to be in the air today two ideas whose times have come: the Prototype and the Frame. I'd like to consider here their relevance for semantic theory.

The Prototype idea is roughly this. Instead of the meaning of a linguistic form being represented in terms of a checklist of conditions that have to be satisfied in order for the form to be appropriately or truthfully used, it is held that the understanding of meaning requires, at least for a great many cases, an appeal to an exemplar or prototype--this prototype being possibly something which is innately available to the human mind, possibly something which, instead of being analyzed, needs to be presented or demonstrated or manipulated. The Frame idea is this. There are certain schemata or frameworks of concepts or terms which link together as a system, which impose structure or coherence on some aspect of human experience, and which may contain elements which are simultaneously parts of other such frameworks.

These two notions, used together, can offer us a new (possibly not altogether new) way of looking at a number of questions in linguistic semantics. One obvious way of linking them together is by claiming that in some cases the area of experience on which a linguistic frame imposes order is a prototype. For example, we know, without knowing how we know, the prototypic ways in which our bodies enable us to relate to our environment; this is knowledge we might speak of as part of our body image. Our language provides us with orienting and classifying linguistic frames--such as UP/DOWN, FRONT/BACK and LEFT/RIGHT--which we could not understand, or could not easily understand, if we lacked bodies or if we lacked a body image.

The prototype idea can be seen in the color term studies of B. Berlin and P. Kay (1969) and in the 'natural category' researches of E. Rosch (1973). I find it in the 'open texture' idea of the philosopher F. Waismann (1952); in the concepts of enactive and iconic memory representations of J. Bruner (1964); in R. Lindsay's discussion of the need for something akin
to 'mental pictures' in the design of language translation and problem-solving systems within artificial intelligence (1963); in H. Dreyfus's criticisms of artificial intelligence in which he speaks of the non-formalizable ability to perceive an individual case as being or not being an instance of a paradigm case (1972); in traditional studies of figurative language, in which any typical or believed-to-be-typical property of the 'vehicle' can contribute to the 'tenor'; and in the various recent works on vagueness in linguistic categorizations by such diversely motivated researchers as L. Zadeh (1971), G. Lakoff (1972) and W. Labov (1973).

The frame idea, under various names, goes back at least as far as the 'schema' idea of F. Bartlett (1932) and has many realizations in work on artificial intelligence, most elaborately in M. Minsky (1974). I also see it in the 'associative relations' idea of the psychologist G. Bower (1972) and in the work of the European semantic field theorists (see H. Geckeler, 1971).

Leaving explanations and justifications for another occasion, I will content myself here with showing some of the ways in which I would like to use these terms. I would like to say that people associate certain scenes with certain linguistic frames. I use the word scene in a maximally general sense, including not only visual scenes but also familiar kinds of interpersonal transactions, standard scenarios defined by the culture, institutional structures, enactive experiences, body image, and, in general, any kind of coherent segment of human beliefs, actions, experiences or imaginings. I use the word frame for any system of linguistic choices—the easiest cases being collections of words, but also including choices of grammatical rules or linguistic categories—that can get associated with prototypical instances of scenes.

Borrowing from the language of artificial intelligence and cognitive psychology, and recognizing that what I say may sound like extremely naive psychology, I would like to say that frames and scenes, in the mind of a person who has learned the associations between them, activate each other; and that furthermore frames are associated in memory with other frames by virtue of their shared linguistic material, and that scenes are associated with other scenes by virtue of sameness or similarity of the entities or relations or substances in them, or their contexts of occurrence.

I believe that this way of talking allows one to formulate an integrated view of many aspects of inquiry into the nature of language—the nature of meaning,
the acquisition of meaning, the nature of communication, the comprehension of texts, the developmental changes in meaning in the early life of an individual, and the changes of standard meanings in the history of a language. I would like to examine three issues in the study of meaning and comprehension, in the belief that sensible views of them can be formulated within the scenes-and-frames paradigm, and then to suggest ways in which the paradigm could be applied to other questions.

Take, first, the analysis of discourse. It seems to me that what is needed in discourse analysis is a way of discussing the development, on the part of the interpreter, of an image or scene or picture of the world as that gets built up and filled out between the beginning and the end of the text-interpretation experience. One way of talking about it is this: the first part of a text creates or 'activates' a kind of schematic or outline scene, with many positions left blank, so to speak; later parts of the text fill in the blanks (or some of them, anyway), introduce new scenes, combine scenes through links of history or causation or reasoning, and so on. In other words, a person, in interpreting a text, mentally creates a partially specified world; as he continues with the text, the details of this world get filled in; and in the process, expectations get set up which later on are fulfilled or thwarted, and so on. What is important is that the ultimate nature of this text-internal world will often depend on aspects of scenes that are never identified explicitly in the text.

One simple way to look at this text development is to consider text-coherence relations in a two-party conversation. The Japanese verb kaku and the English verb write are frequently acceptable translations of each other; but a frame-and-scene analysis of the two words would have to show them to be different. For both words there is an associated scene of somebody guiding a pointed trace-leaving implement across a surface. With the Japanese word, the nature of the resulting trace is left more or less unspecified. Thus, if somebody asks, "Nani o kakimashita ka?"—meaning, "What did you kaku?" —the answer can identify a word or sentence or character, or, just as well, a sketch or a doodle.

The frame linked to the English word write has that same scene associated with it, but it also has more. What it shares with the Japanese verb is a set of concepts including such entities as the writer, the implement, the surface on which the traces are left, and the product. Since I know at least that much about writing,
I know that if you tell me that you have been writing, I can, talking within the frame that you have introduced into our conversation, ask you such questions as "What did you write?", "What did you write on?", "What did you write with?". (If, instead, I were to say something like "What time is it?" or "I've got a toothache", I would not be talking within the frame you introduced: I would be changing the subject.)

The English verb write, unlike the Japanese verb kaku, has an additional scene associated with it, for which there is what we might call a language frame. It happens that the product of an act of writing cannot be a picture or a smear, but has to be something linguistic. Because of that fact, if you tell me that you have been writing, I can then ask, talking within one of the frames that your remark has introduced, such questions as "What language were you writing in?" or "What does what you wrote mean?". The word write, in other words, simultaneously activates both an action scene of a particular kind and, linked to it with the 'product' of the act of writing, a linguistic communication scene.

If your sentence gives some name to the product of your writing, then you will have introduced a new frame associated with that word or phrase. For instance, if you tell me that you have been writing a letter, you have introduced into our conversation what might be called a correspondence frame. Talking within that frame, I can ask questions like "Who are you sending it to?", "Do you think he will answer?", "How long do you think it will take him to get it?", and so on. Or, if what you say to me is that you have written another letter, then we have a kind of historical frame going, and it is now appropriate for me—assuming that I don't already know the historical setting for your remark—to ask such questions as, "How many earlier letters did you write?", "Who did you send those earlier letters to?", and so on.

So far I have treated these reports (about you having written something) as first contributions to a two-party conversation whose participants do not know very much about each other—an assumption which contributed to the unnaturalness of the responses I suggested. In most natural conversations, the participants have, already 'activated', a number of shared, presupposed, scenes that we can speak of as being in their consciousness as they speak. If, for example, I know that you are in the finishing stages of preparing an article on Latvian palatalized consonants, and in that context
you say to me, with a pleased look on your face, that you have been writing, I can then quite appropriately ask a question like, "Have you decided what journal you're going to send it to?". In this case, I have fitted what you said to me into some scenes that I have already activated, and I can quite legitimately talk within the frames associated with that larger complex scene.

My examples have been with simple two-party conversations. Single author texts have, of course, analogous coherence properties. In each case, a text is coherent to the extent that its successive parts contribute to the construction of a single (possibly quite complex) scene.

Let me take some questions of the acquisition of word meaning as a second example of the ways in which the scene-and-frame model can be put to use. Workers in child language like F. Antinucci (personal communication) have argued that a child first learns labels for whole situations, and only later learns names for individual objects. A child might first associate the word pencil, for example, with the experience of himself sitting in a particular room with his mother, drawing circles; later on he isolates out certain parts of such a scene (pencil, paper, drawing, circles, etc.); still later he acquires different names for the parts of different but similar scenes: drawing, writing, printing, sketching; pencil, pen, crayon, chalk; paper, blackboard, lavatory, walls, etc.; so that when he is finished he has a mature repertory of syntagmatic, paradigmatic and hierarchical frames for scenes of both greater degrees of abstractness and greater degrees of precision and boundedness than the original scene in which he first used the word pencil.

It appears, then—if this account is correct—that in meaning acquisition, first one has labels for whole scenes, then one has labels for parts of particular familiar scenes, and finally one has both a repertory of labels for schematic or abstract scenes and a repertory of labels for entities or actions perceived independently of the scenes in which they were first encountered.

Once in a while one comes across a nice piece of evidence about the middle stage of this development. Mary Erbaugh, of the Berkeley linguistics department, working in north Oakland last summer with some small children, brought a grapefruit for her lunch one day. She showed the grapefruit to the children, got an acknowledgment from them that the thing was a grape-
fruit. She then peeled it, separated it into its segments, and started eating it. She reports that the children around seven years old in this group were surprised that what at first had looked like a grapefruit turned out to be an orange! Guessing at their reasoning, it would seem that a grapefruit, after all, is something you cut in half with a knife and eat with a spoon. This thing was obviously an orange. The categorizing function of these two words had not yet been liberated from the scenes of people in their experience eating them.

For a third example of a scene-and-frame analysis of linguistic phenomena, let us consider the so-called boundary problem for linguistic categories. Given the checklist theory of meaning, the most typical kind of research in lexical semantics involves examining, by presenting native speakers with bizarre contexts for word uses, the boundaries of application of particular words. One instance of this sort of research is Labov's study of category boundaries for the semantic domain that includes cup, bowl, glass, etc. Examining such features as having one handle, being made of opaque vitreous material, being used for consumption of liquid food, being accompanied by a saucer, tapering, and being circular in cross-section, Labov ends up with a complicated function for cuphood that has a built-in range of variation for each of these dimensions. (See Labov, 1973, p. 366.) My way of talking about his results is to say, not that they provide us with the function that specifies the boundary conditions for a category, but that they amount to a kind of statistical summary of the strategies that his subjects used in projecting from a familiar repertory of categories onto situations and experiences that were not covered by their associated prototypic scenes.

Given a checklist theory of meaning, boundary research on words like bachelor and widow would take seriously such questions as these: How old does an unmarried man have to be before you can call him a bachelor? Is somebody who is professionally committed to the single life properly considered a bachelor? (Is it correct to say of Pope John XXIII that he died a bachelor?) If so, is bachelorhood a state one can enter? That is, if a man leaves the priesthood in middle life, can we say that he became a bachelor at age 47? When we say of a divorced man or a widower that he is a bachelor, are we speaking literally or metaphorically? How can we tell? Would you call a woman a widow who murdered her husband? Would you call a woman a widow whose divorce became final on the
day of her husband's death? Would you call a woman a widow if one of her three husbands died but she had two living ones left? If people give different answers to these questions, do they speak different dialects? Are these dialects stable? And so on.

These are all reasonable questions, given the checklist theory of meaning. According to a prototype theory of meaning, these concepts are defined in the context of a simple world in which men typically marry around a certain age, they marry once, they marry exclusively, and they stay married until one partner dies. Men who are unmarried at a time when they could be married are called bachelors. Women whose husbands have died are called widows. This prototype world simply does not cover the bizarre cases proposed by the category-boundary-researcher's questions.

When you submit subjects to category boundary research, you are actually asking them to make judgments about whether they are willing to extend the frame containing the word under question to a situation not covered by the prototype scene he associates with it, or you are asking him whether he is willing to create a new frame for the new situation borrowing a word from an already existing frame. You are probably not even exploring his strategies for making such extensions, since the contexts you present to him are not meaningful enough to him to let him depend on his own expressive or communicative needs for making the decision. In general, introspection about appropriate language use in bizarre contexts does not yield highly dependable data for semantic research.

Other areas in which scene-and-frame approaches could give sensible alternatives to traditional accounts are: selection restriction, synonymy, the recognition of polysemy versus the formulation of core meanings, metaphor, the nature of semantic fields. Briefly, the standard theory of selection restrictions recognizes as a relation between elements in a frame something that should be recognized as the relationship between a frame and a scene; the concept of synonymy can be given a prototypic definition (participation in identical frames, association with the same scene), with arbitrary decisions made on the use of the term in cases of partial synonymy; the search for core meanings can be criticized on the grounds that it requires the separation of words from their contexts; metaphorizing can be seen as the act of applying to one scene a frame which is known to be more basically associated with a different scene; and semantic field theories can often
be shown to presuppose prototypes, even though they rarely say anything about them.

I am convinced that something like the model I have been talking about will allow an integrated view of many subfields in the study of meaning and comprehension, though I will readily admit that my remarks here are little more than suggestive. Sometimes I think that what I am proposing is new, but sometimes I fear that it is exactly what everybody has been talking about all along. If it is new, it is probably too commonsensical to be impressive, and will have to undergo some careful reformulation. If, however, this is what semantic theorists have believed all along, then with this paper I am doing no more than announcing that I at last understand something about my field.

NOTE: In the literature I have examined, the word frame seems to have been introduced by quite various routes, though the sense of organizational coherence is present in all its uses. My own use of the word in linguistics began with the pre-transformationalist view of sentence structure as consisting of a frame and a substitution list (a syntagmatic frame and a paradigmatic set of mutually substitutable items); continued with my notion of case frame (a formula for indicating the valence or contextual requirements of a given predicador--C. Fillmore 1968); and finished with the concept I have tried to present here. In Minsky's case, the metaphor is that of a single frame in a film. In Goffman's case the word was borrowed from Gregory Bateson (see E. Goffman 1974, p. 7) to refer to analytical frameworks within which human experience can be made intelligible.

A word that I have at times preferred to frame is module, which, because of its association with, say, modular furniture, makes the process of assembling frames together to make larger frames easily visualizable. But since frame, unlike module, suggests more strongly the idea of being for something, I prefer to keep that word and to urge my readers to be cooperative interpreters.

NOTE: At a time when it was too late to re-type this paper, Mary Erbaugh told me that I had the details of the grapefruit story wrong. It's not true, I regret to have to report, that the children first acknowledged that the thing was a grapefruit. They thought from the start that it was an orange. What happened, if I've
got it right this time, is that Mary tried to teach them that this thing was a different kind of fruit, a grapefruit, but that by the time she peeled it and segmented it, they couldn't believe that it was anything but an orange. Part of the point I wanted to make is still there, but a big part of it is lost. (Maybe it could have happened the way I said it happened.)

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How to say "no" in Palauan
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In dealing with the syntax of a language other than one's native language, it is important to cast the grammar in terms of the processes and categories it clearly motivates. This becomes imperative if the final aim of our work is to develop a theory of universal grammar, rather than merely a descriptively adequate statement of the grammar of the language. The necessity to avoid imposing foreign grammatical categories on the grammar of a language has been stressed by many of the great minds in our field. The grammars of the world's languages must be motivated only in terms of their own rules and constructions, and these will provide a proper input to a theory of universal grammar. In this age of generative grammar we must avoid imposing the categories and rules of English grammar on other languages. If the sentences of the language motivate a rule similar to an English transformation, then we may, of course, posit it as a synchronic rule, but not otherwise. In this way we will develop a proper theory of universal grammar, one arrived at inductively from the independent data of many languages as witnesses. Any other approach will simply distort our view of universal grammar and not account in an equal manner for the rich diversity of the world's languages.

In the generative semantic view of language, which we shall use in this paper, there are three basic aspects to language, as schematized in (1):

\[
\text{semantic structure} \quad \Downarrow \quad \text{rules} \quad \Downarrow \quad \text{surface signal}
\]

Rules, syntactic, lexical, and phonological, convert the semantic structure to the surface signal. The surface signal is, of course, the given data in all languages. The grammars of languages consist of the rules, and, of course, in the rules of grammar languages may differ. However, there is no reason why the semantic structures of languages cannot differ, and the importance of this as a working assumption follows from what was said above. We should not simply assume that the semantic structure of all languages is identical. Rather we should formulate our rules on the basis of data of the language and set up semantic structures which will be converted by these general rules into the correct surface signal. We should not be concerned whether these semantic structures are the same as those of English or French etc. for synonymous sentences. We should only be concerned that these semantic structures will produce the surface signals by general rules. It is necessary to constrain our grammars to minimize idiosyncratic rules. This, of
course, decreases the possibility of imposing foreign rules on the grammar and causing semantic structures to appear more alike. Of course, these remarks apply to semantic structures represented as tree diagrams. Using logical formulae is a different matter, which I do not wish to go into here, except to say that the transition from a logical formula as a representation for semantic structure of a particular sentence in a natural language to a structure to which transformational rules could apply is not at all clear.

In this paper we will discuss the phenomenon of negation in Palauan, an Austronesian language spoken in the western Caroline Islands in Micronesia. I have constrained the grammar so that only rules needed outside the phenomenon of negation are posited. This is to prevent the development of an idiosyncratic analysis of Palauan negation, but rather to incorporate it into Palauan grammar as a whole. An interesting result of this study is that in some cases abstract structures posited by generative semantics for English are shown in fact to be motivated directly in Palauan, whereas in other cases the structures for Palauan are somewhat different.

The simple negative word in Palauan is diak. It is syntactically a stative verb and has a past tense form dimlak from *di+mle+ak. The mle is the past tense marker for stative verbs. The negative is a main verb, and that which is negated is its subject. There are two moods in Palauan: a realis and an irrealis. This difference is signalled by a different set of subject pronouns as in (2):

<table>
<thead>
<tr>
<th>Sg</th>
<th>1st</th>
<th>irrealis</th>
</tr>
</thead>
<tbody>
<tr>
<td>2nd</td>
<td>kɔ</td>
<td>k(u)-</td>
</tr>
<tr>
<td>3rd</td>
<td>ṭ̣</td>
<td>ʈ̣-</td>
</tr>
<tr>
<td>1in</td>
<td>kədə</td>
<td>d(ə)-</td>
</tr>
<tr>
<td>1ex</td>
<td>(a)ki</td>
<td>kim-</td>
</tr>
<tr>
<td>2nd</td>
<td>kɔ</td>
<td>ṭ̣om-</td>
</tr>
<tr>
<td>3rd</td>
<td>tɔ</td>
<td>ʈ̣-</td>
</tr>
</tbody>
</table>

The irrealis mood is used in various environments: conditional, certain temporal clauses and, what concerns us here, negated sentences. Thus, the negation of sentence (3) is (4):

A Juan a məŋə ra ŋikol. 
John is eating of fish
"John is eating some of the fish."

A Juan a diak lɔŋə ra ŋikol. 
John NEG 3rd irrealis-eat of fish
"John isn't eating any of the fish."
loña is the third singular irrealis form of məna derived by regular morphophonemic rules. (4) has the abbreviated semantic structure:

\[
\begin{array}{c}
\text{NP} \\
S_1 \\
S_2 \\
\text{a Juan məna ra ḋikọl}
\end{array}
\]

(4')

Structures like those proposed by Klima (1964) for English will not work for Palauan as will become apparent below. In (4') the verb in \( S_2 \) is in a sentence being negated. The sentence will be in the irrealis mood, as indicated by the inflection of the verb. The condition for marking as irrealis may be described formally in that the \( S \) affect must be commanded by the negative. Also, the scope of the negative may not cross another \( S \)-node within the sentence being negated. Thus, the verb in (4') will be marked \( [+\text{irrealis}] \) and by agreement with its subject \( [+3\text{rd}] \). This will generate (4''):

\[
\begin{array}{c}
\text{NP} \\
S_1 \\
S_2 \\
\text{a Juan loña ra ḋikọl}
\end{array}
\]

(4'')

Subject Raising applies to (4''). This formulation of Raising entails raising of the embedded subject and extraposition of the remaining sentence in one step. Finally, a-Insertion applies and places an \( a \) before the concatenation of verbal elements and yields (4) above.

Sentence (5) has the negation (6) which has the semantic structure (6'):

\[
\text{A bli-k a mle upil.} \\
\text{house-my past good} \\
\text{"My house was nice."}
\]

\[
\text{A bli-k a dimIak 1-upil.} \\
\text{house-my neg-past 3rd irr-good} \\
\text{"My house wasn't nice."}
\]

(5)

(6)

(6')
Of course, in (4') a node for present tense was needed, but it did not affect the derivation and was omitted. In (6') it is needed for the derivation. The rule of Subject Raising is cyclic as (6) will demonstrate, but Negative Scope is pre-cyclic and applies to (6') first. $S_3$ is commanded by the negative and the verb is put into the irrealis. Thus, we generate (6''):

$$
\begin{array}{c}
\text{NP} \\
\text{S}_1 \\
\text{V} \\
\text{past} \\
\text{NP} \\
\text{S}_2 \\
\text{V} \\
\text{diak} \\
\text{a blik lunjil} \\
\end{array}
$$

On the cycle of $S_3$ nothing happens. On $S_2$ Subject Raising and Node Pruning occurs to generate (6'''):

$$
\begin{array}{c}
\text{NP} \\
\text{S}_1 \\
\text{V} \\
\text{past} \\
\text{NP} \\
\text{S}_2 \\
\text{V} \\
\text{a blik diak lunjil} \\
\end{array}
$$

On $S_1$ Raising and Node Pruning apply again to yield:

$$
\text{A blik past diak lunjil. (6''''\text{)}}
$$

A-Insertion applies and past diak are read out as dimlak, generating (6).

There are several crucial points to be made about the derivation of (6). With this formulation of Raising the node of past must be above diak to prevent tense from being read out on lunjil, generating the ungrammatical (7):

$$
\text{A blik a diak mie lunjil. (7)}
$$

Also, this ordering of the nodes of diak and past is necessary for the proper operation of Negative Scope. If this order is reversed $S_3$ would be commanded by the negative, but the $S$-node of past would intervene, and the verb would not appear in the irrealis mood. This would be ungrammatical. Finally, instead of Subject Raising we could propose an alternate rule of Predicate Lowering to generate (6). This would also be cyclic and lower the higher predicate into the verb phrase of the lower sentence. The major advantage of this approach concerns the a which introduces the verbal elements and after which the negative and tense elements occur.
In an analysis with Predicate Lowering the \( a \) may be present in the underlying structure, and, consequently, there will be no need for a transformation of \( a \)-Insertion discussed above. However, there is good motivation for this transformation. The \( a \) will be introduced transformationally very late in the derivation. In some sentences with pronominal subjects there are clear cases of Raising, as in (8) from (8'):

\[
\text{Tə dimlak lərəel.} \\
\text{they neg-past 3rd irr-walk} \\
\text{S_1} \quad \text{V past} \\
\text{S_2} \quad \text{V diak} \\
\text{S_3} \quad \text{ta mərəel} \\
\]

Negative Scope and Raising on \( S_2 \) and \( S_1 \) produce (8). Note that there is no \( a \) introducing the VP of (8). A-Insertion apparently only applies in sentences of the order NP-VP (see (11) and (12) below), when the subject is a noun, not a pronoun. Negative Scope and Raising are clearly involved here because there is no other way to get Tə, the subject of the lower sentence, into its position in (8). Clear cases of Raising such as (8) only occur with sentences with second and third plural pronominal subjects and may be due to the fact that in these cases the irrealis prefixes do not distinguish singular from plural. In cases in which there is no ambiguity, the raised pronominal subject is obligatorily deleted, as in (9) from (9'):

\[
\text{Dimlak ku-luʃəs.} \\
\text{neg-past 1st sg irr-write} \\
\text{"I didn't write."} \\
\text{S_1} \quad \text{V past} \\
\text{S_2} \quad \text{V diak} \\
\text{S_3} \quad \text{ak luʃəs} \\
\]

After Negative Scope and Raising on \( S_2 \) and \( S_1 \) have applied we have the intermediate string:

\[
\text{Ak dimlak ku-luʃəs.} \\
\text{(9'')} \\
\]
The ak is obligatorily deleted because there is no ambiguity with ku-. The obligatory pronoun deletion will also be needed in an alternative Predicate Lowering analysis to account for the differing structures of (8) and (9). Also, a sentence (10) generated by Predicate Lowering from (8'):

*Dimlak lorael. (10)

was not accepted by my informant.

At this point we need to consider a rule of very general application in Palauan, Subject Extraposition. This takes a full subject NP(non-pronominal) and moves it to the right of the verb, leaving a pronoun in its place. Thus, Subject Extraposition generates (12) from (11):

A Juan a mọa ra ɲikəl. (11)
John eat of fish
"John is eating some of the fish"

ŋmọa a Juan ra ɲikəl. (12)
ŋ- is the third singular realis pronoun. Note that there is no a preceding the verb. This string does not meet the structural description for the a-Insertion transformation outlined above.

Subject Extraposition quite regularly applies to existential sentences or sentences containing the negative. Existential sentences are formed with the existential verb "be" arniy or with the copula which is realized as nothing. Thus, (13) and (14) have essentially the same meaning, although (13) emphasizes the existence of the five baskets.

ŋarniy eim əl suk. (13)
exist five linker basket

ŋeim əl suk. (14)
five linker basket

"There are five baskets."

(13) and (14) derive from underlying structures (13') and (14') by Relativization and Subject Extraposition:

In (14') Subject Extraposition can apply even though the
verb is null. The prefix simply attaches to the next word. The derivation for (14) is:

\[ \text{NP} \quad \text{S} \quad \text{V} \quad \text{Relativization} \quad \text{NP} \quad \text{S} \quad \text{V} \quad \emptyset \]

\[ \text{Relative} \quad \text{Preposing} \quad \text{NP} \quad \text{S} \quad \text{V} \quad \emptyset \quad \text{Subject} \quad \text{Extraposition} \]

\[ \eta \text{em} \quad \emptyset \quad \text{suk}. \]

(14)

With the negative this rule is optional except that when it is the main verb of the derived sentence, it is obligatory. Thus, (16) is the underlying source for both (17) and (18). In (17) Subject Extraposition does not apply, while in (18) it does:

\[ \text{NP} \quad \text{S}_1 \quad \text{V} \quad \text{past} \]

\[ \text{NP} \quad \text{S}_2 \quad \text{V} \quad \text{diak} \quad \text{negative} \]

\[ \text{ak lu\textsuperscript{\textbeta}s} \quad \text{I write} \]

\[ \text{Dim\textlak} \quad \text{klu\textsuperscript{\textbeta}s.} \]

\[ \text{neg-past 1st sg irr-write} \]

(17)

(18)

Subject Extraposition has applied in (19)-(24), but again is optional because the negative is not functioning as the main verb of the sentence. If Subject Extraposition had not applied there would be no initial \( \eta \) - in these sentences and the word order would have been strictly SVO.

\[ \eta \text{diak} \quad a \quad \text{Juan} \quad a \quad \text{lo\textkappa} \quad \text{ra} \quad \text{njik\textlak}. \]

\[ \text{neg} \quad \text{John} \quad 3\text{rd irr-eat of fish} \]

"John isn't eating any of the fish."

(19)

\[ \eta \text{diak} \quad \text{1\textlak\textkappa} \text{ek\textkappa} \text{rey}. \]

\[ \text{neg} \quad 3\text{rd irr-small} \]

"It isn't small."

(20)

\[ \eta \text{diak} \quad k\text{-bo} \quad \text{ku-lim}. \]

\[ \text{neg} \quad 1\text{st irr-go 1st irr-drink} \]

"I won't drink."

(21)
(22) ηdimlak k-kyey.
   neg-past 1st irr-stay
   "I didn't stay."

(23) ηdiak k-luwt əl rul-iy.
   neg 1st sg-again linker do-3rd sg obj
   "I'm not doing it again."

(24) ηdiak ə-bo ə-bes-kaw.
   neg 3rd irr-go 3rd irr-give-2nd sg obj
   "He won't give it to you."

In sentences (25)-(27) the negative is the main verb of the derived sentence, and Subject Extraposition is obligatory:

(25) ηdiak a ək-əl ra Belaw.
    neg name-3rd sg poss loc Palau
    "It has no Palauan name."

(26) ηdimlak a udu-ek.
    neg-past money-1st sg poss
    "I had no money."

(27) ηdimlak a əliwd er əiy.
    neg-past tobacco loc 3rd sg
    "It has no tobacco in it."

These sentences all derive from structures with the null copula as the main verb of the sentence embedded under the negative. (25'), an abbreviated underlying structure for (25), is typical:

(25')

(25) is derived by applying Raising on $S_2$ and $S_1$, and then Subject Extraposition.

This completes the sketch of the basic transformations to which the negative is subject. The fact that these rules apply to many other verbs in the language and are in no way idiosyncratic to the negative is strong evidence that it is a basic verb in the language.

We will now go on to investigate syntactic and semantic
constraints on Negative Scope and negation of other than main clauses. Negative Scope only affects the sentence which is immediately commanded by the negative. Negative Scope does not affect any other sentence embedded in this sentence, that is, does not cross another S-node. Thus, in (26) only the verb latk is in the scope of the negative:

(26)

Because latk is within the scope of the negative it will be in the irrealis mood. If we apply Subject Raising of nələk into object position on S₃ and Subject Raising on S₂ and S₁, we derive (27):

A maɾas a dimlak lələtəkiy a nələk əl mo məɾiwaywə. (27)
"The old woman didn't remember that the child went to sleep."

The verb form lələtəkiy is a perfective in the irrealis mood with a third singular object pronoun suffix.

It is also possible that the negative be embedded lower in the tree, that is, (28) is also a perfectly well formed underlying structure:

(28)
In (28) the negative commands only $S_5$ and, therefore, it is within its scope. Consequently, the verbs mo maśiwaywa will appear in the irrealis. The negative does not command $S_2$, and latk will remain in the realis. By a derivation of Subject Raising on $S_1$ and $S_3$, and then Raising of ṣalak to object position on $S_2$ and finally Raising on $S_1$, we get:

*A ma₂as a lilatkiy a ṣalak əl dimlak Iəbo ɪmaśiwaywa.  
"The old woman remembered the child didn't go to sleep."  

(29)

We now turn to the relations between quantifiers and negation. "Nothing/anything" in Palauan is expressed by negation of a sentence containing ḳara(ŋ) "what." This is similar to Mandarin Chinese. ḳara(ŋ) may be perhaps be best looked upon as a general indefinite, which in a question frame functions as "what," but in a negative frame functions as "nothing/anything." 

(30) and (31) exemplify:

\[\text{ŋdimlak } \text{k-du } \text{ŋaraŋ.} \]
\[\text{neg-past 1st sg irr-say indef } \text{I said nothing."} \]

\[\text{ŋaraŋ } kə \text{ rulįy?} \]
\[\text{indef you make-3rd sg obj } \text{What did you make?"} \]

(30)

The relations between the negative and other quantifiers in cases of both sentential and phrasal negation (Klima, 1964) are more complex. Let me first make the claim, to be supported below, that there is no distinction in Palauan between phrasal and sentential negation, but that all cases of negation are ultimately sentential. Apparent examples of phrasal negation are in fact sentences in which the negative functions as a verb in an embedded sentence. A corollary of this hypothesis is that in cases of apparent phrasal negation, the verb will appear in the realis mood, not the irrealis, because the negative will not command the main sentence. This is, in fact, exactly what is found. In the remainder of this paper I will use the term sentential negation versus phrasal negation in the sense of negation of the main clause versus negation of an embedded clause within the matrix sentence. They actually have more reference to the English translations, but they are useful cover terms. But, as I have said, in Palauan all negation is ultimately sentential, in that the negative functions as a predicate of a sentence with a subject which is
itself an embedded sentence.
To make this clearer compare sentences (32) and (33):

A bebi 'l ra ḳalak a dimlak ləbo (32)
some linker pl child neg-past 3rd irr-go
loodney əraŋ-
3rd irr-know indef
"Some of the children didn't know anything."

Dimlak ḳl ra ḳalak a mədəney əraŋ. (33)
neg-past linker pl child know indef
"None of the children knew anything."

(32) is a clear case of negation of the main sentence, sentential negation, as exhibited by the presence of dimlak in the verb phrase and the irrealis inflection of the main verb ləbo lodney. (33), however, is an example of phrasal negation, negation of an embedded sentence. Note that the main verb mədəney is in the realis mood. (33) has the underlying structure (33'):

```
            S₁
               /\  
             /   \  
            S₂   S₃  
               /\  
             /   \  
            NP   NP  
               /\  
             /   \  
            S₄   V   past
               /\  
             /   \  
            NP   S₃     mədəney əraŋ
               /\  
             /   \  
            NP   NP     ləbo lodney
               /\  
             /   \  
            NP   V     əraŋ
               /\  
             /   \  
            ra ḳalak
```

Note that the negative is in the sentence embedded in the subject of the main sentence. (33') may be paraphrased as "there does not exist any children (of a group defined in the discourse) who know anything."
Various Raising as discussed, Relativization on S₂ and Relative Preposing will derive (33) from (33').
The linker ḳl in (33) surfacely signals the subordination of the negative. Various formal syntactic tests can be performed to (32) and (33) to illustrate the formal differences. Subject Extraposition, as discussed above, can only apply to elements of verb phrases in main sentences. Thus, if we apply it to (32) and (33), we derive (34), which is grammatical, and (35), which is not:

CONDIMLAK a bebi 'l ra ḳalak ləbo lodney əraŋ. (34)
CONDIMLAK ḳl ra ḳalak a mədəney əraŋ. (35)
Negative Scope can only affect verbs embedded under the negative. Because (33) is an example of phrasal negation, the negative is in an embedded sentence. Consequently, in (33) the main verb cannot be under the negative and cannot be in the irrealis. Therefore, (36), a form of (32) in which the verb is in the irrealis, is ungrammatical:

*Dimlak əl ʁə ʃalək a lədəŋey ʃaraŋ.  (36)

Even though dimlak in (32) is not the main verb it can still take the tense of the sentence. It appears that the negative is a strong tense attracting morpheme. However, in (32) the main verb can take tense instead of the negative, as in (37):

Diak əl ʁə ʃalək a mlədəŋey ʃaraŋ.  (37)

The infix -l- in mlədəŋey marks past tense. Note that the negative appears in the non-past form diak. A sentence in which tense appears in both morphemes is ungrammatical. However, in cases of sentential negation, in which the negative is a main verb, only the negative can take tense. Thus, (38) as a variant of (32), in which the main verb and its auxiliary take the past tense, is ungrammatical:

*A bebi 'l ʁə ʃalək a diak ʃəbəlo ʃəmlədəŋey ʃaraŋ.  (38)

More examples of phrasal negation parallel to (33) and also surfacely signalled by the subordinator əl are (39)-(40):

Dimlak əl bətok əl ʃalək a mədəŋey əl mlədəŋey ʃaraŋ  indef  (39)

"Not many children knew anything."

Diak əl ʁə səʔəlik a mləʔiʔitək.  (40)

"None of his friends sang."

These are cases of phrasal negation because variants with the main verb in the irrealis mood are ungrammatical:

*Dimlak əl bətok əl ʃalək a lədəŋey ʃaraŋ.  (41)

*Diak əl ʁə səʔəlik a ləʔiʔitək.  (42)
A sentence similar to (39), but a case of sentential negation, is (43):

\[
\text{Bötok el ḳalēk a dimlak lodāney} \quad (43) \\
\text{many linker child neg-past 3rd irr-know} \\
\text{ịnđef} \\
\text{"Many children didn't know anything."}
\]

The sentence is negative and is embedded in the NP which functions as the subject of the negative, as was discussed above. The verb in the sentence is within the scope of the negative and must appear in the irrealis mood. Thus, (44) is ungrammatical:

\[
\text{"Betok el ḳalēk a dimlak mēndějey ịnđef.} \quad (44)
\]

It might be objected that it is the presence of an immediately adjacent negative which triggers the irrealis mood, and, consequently, that there is no need for a recourse to the complex notion of a pre-cyclic application of Negative Scope. However, sentences like (19) and (34), in which Subject Extraposition has separated the negative from the rest of the verb phrase, but the verb phrase is still in the irrealis mood, contradicts this. (45) provides still another example:

\[
\text{dimlak a ta '1 ḳikọl Ionu\w} \quad (45) \\
\text{neg-past one linker fish 3rd Irr-take-middle} \\
\text{er ti\an̄} \\
\text{loc here} \\
\text{"No fish was caught here."}
\]

In (45) Subject Extraposition has separated the negative from the main verb, but it is still in the irrealis. In (45) also brings up another point. Many sentences with phrasal negation of this type have paraphrases with sentential negation by using ta "one" as an indefinite. Thus, (46) with phrasal negation has the paraphrase (47) with sentential negation:

\[
\text{Dimlak əl ra səgə\lil a} \quad (46) \\
\text{neg-past linker pl friends-3rd sg poss} \\
\text{mọhitakl. sing} \\
\text{"None of his friends sang."}
\]

\[
\text{A ta '1 ra səgə\lil a dimlak} \quad (47) \\
\text{one linker pl friend-3rd sg poss neg-past} \\
\text{Ionitakl. 3rd irr-sing} \\
\text{"None of his friends sang."}
\]
(47) cannot mean "one of his friends didn't sing."
Syntactically (47) is a sentence with sentential negation
Not only is the verb lohitakl in the irrealis mood, but also (47) undergoes Subject Extraposition with the negative as the main verb, as in (48)

ŋdimlak a ta '1 rə savəlil a lohitakl. (48)

Clearly, (47) is a case of sentential negation in my definition above, that is, the negative functions surfacely as part of the verb phrase of the main sentence. In almost all cases this corresponds semantically to a negation of the main sentence: "it is not that S." However, the semantic reading of (47) is negation of the quantifier "not one of his friends sang," not negation of the sentence: "one of his friends didn't sing."

There is a related phenomenon in English. Carden(1970) and others noticed that speakers of English have different readings for certain sentences with negatives and quantifiers. For sentence (49)

All the children didn't leave. (49)

some speakers of English have readings which involve negation of the quantifier, that is, (50):

NEG-Q Not all the children left. (50)

For other speakers (49) has a reading of negation of the verb (in the terms used above, negation of the sentence), that is, (51):

NEG-V All the children stayed(NEG-leave). (51)

And for still other speakers, myself included, (49) is ambiguous between the two meanings of (50) and (51). I get the same ambiguity in (52), which is parallel to the Palauan sentence (47):

One of his friends didn't sing. (52)

For me (52) can mean either (53) or (54)

NEG-Q None of his friends sang. (53)
NEG-V Of his friends, only one didn't sing. (54)

However, for my Palauan informant, (47) (sentence (52) is its literal translation) can only have the NEG-Q reading. In other words the Palauan sentence has only the meaning (53). There is a clear conflict in Palauan
between the syntactic structure of (47) and its meaning. 
(47) has the structure of a sentence with sentential 
negation, with the negative inside the verb phrase(NEG-V), 
as established above. But in its meaning the negative 
is construed with the quantifier(NEG-Q), as is evident 
from the identical meaning between (46) and (47). Pres-
sumably, there is a similar conflict between structure 
and meaning for English speakers who only get the NEG-Q 
reading for (49).

Palauan exhibits the same phenomenon with beki "all."
Thus, (55) can only have the NEG-Q reading:

A rə beki 'l səʔəlil
pl all linker friends-3rd sg poss
dimlak loŋitəkl, 
neg-past 3rd irr-sing
"All of his friends didn't sing."

(55) is structurally a clear example of sentential nega-
tion. Note that the verb loŋitəkl is in the irrealis 
mood. Yet the meaning of the sentence is negation of 
the quantifier. In fact, when I asked my informant for 
"not all of his friends sang," which overtly negates the 
quantifier, she gave me:

ŋdimlak loŋitəkl a rə beki 'l səʔəlil.

which is simply a variant of (55) to which Subject 
Extraposition has applied.

It is evident that if two such widely structurally 
divergent languages such as Palauan and English exhibit 
these similarities in (47)-(56), there is a more gene-
ral principle to capture here. What this principle would 
be is not apparent, but research in this area should prov-
to be very useful.

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PHONOLOGICAL PROCESSES IN THE AMERICAN SIGN LANGUAGE
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This paper presents a discussion of types of phonological processes—synchronic and historical—that occur in the American Sign Language (ASL) as compared with those of oral language. Presented below are types of processes that occur in both oral and visual language, those which cannot occur in visual language, and those which cannot occur in oral language, due to the nature of the physical signals.

The American Sign Language is a manual/visual language used by over a half a million deaf people (and numerous hearing people) in the United States today. It is one of many visual languages. There is no one universal Sign Language. Various visual languages such as ASL, Israeli Sign Language, Chinese Sign Language and Iranian Sign Language are totally unrelated historically. Others are related—such as the American and French Sign Languages—but their historical relationship does not in any way correspond to the relation of the oral languages spoken in the same areas. It is true of all sign languages that they are not derivative of any oral language. Their structures "phonological", syntactic and semantic are unique.

How can the term "phonology"—the study of sound and sound systems—possibly be applied to a language without sound? For some the term is anomalous. Others can intuitively grasp the correspondence of the study and analysis of the structural organization (or formal properties) of language using a manual/visual signal to the structure of oral/auditory language. The term "phonology" is used throughout this paper in this abstract sense—to cover the description and analysis of the formational structure of any language, be it in the oral/auditory or in the manual/visual modality.

There are four basic articulatory parameters in Sign Language: 1. hand configuration—the shape of the articulators; 2. place of articulation—the area or point on the signer's body, or in the articulation space defined by his body, at which or near which the articulation is made; 3. movement—the movement of the articulator(s) from one point to another within the articulation space; 4. orientation—the orientation of the hand(s) in relation to the signer's body.

A simple sign involves one hand configuration, made with one or both hands, combined with one place of articulation, a specification for orientation and a movement of the articulator(s).
The process of phonological assimilation occurs as readily in visual language as it does in oral language. There are countless examples of assimilation in oral language—historical and synchronic: place of assimilation—e.g. palatalization, labialization, vowel harmony, nasal assimilation (to following stop), etc.; manner assimilation—e.g. nasalization of vowel (preceding nasal), consonant gradation (e.g. Finnish voiceless stop becomes voiced intervocalically), devoicing, fricativization (medially), etc. The types are numerous and the number of occurrences endless. In visual language comparable types of assimilation occur. The following are examples of types of phonological assimilation which occur in ASL.

Frishberg (1974) discusses what she has called "fluidity" as a historical phonological change in ASL. Primarily, the term fluidity refers to processes of assimilation. However the tendency toward fluidity is possibly broader in scope than assimilation. Frishberg states that fluidity
"or the smoothness tendency of signs says both that the movement involved in a sign should be smooth and that the transitions between the two parts of a compound sign should be minimized". (p.4)

In regard to the first principle given—that the movement of a sign should be smooth—she offers as an example of the change toward smoothness the sign ANY. This sign 60 years ago was articulated with an "ague-like shaking motion". Today the sign is made with the fist, thumb extended palm toward signer with a quick movement in which the hand is turned so that the palm is away from the signer, while the hand moves rightward. The Old French sign FOR (Fr. 'pour') consisted in two parts—first touching the extended index finger to the forehead and then pointing outward to the object. Today FOR entails a single movement from the forehead outward. If we examined the possible cause for such changes, we might refer to this aspect of "fluidity" as a change for the purpose of ease of articulation. This type of change in ASL most closely corresponds to manner assimilation in oral language in which a segment is altered in its manner of articulation for the primary purpose of ease of articulation—e.g. the voicing of voiceless stops in intervocalic position. The change in movement described here represents a change from a more complex to a simple movement, caused presumably by the signer's desire for simplification of effort.

The second type of "fluidity" described by Frishberg very clearly amounts to assimilatory processes. The sign INFORM was originally composed of a compound of KNOW plus BRING. Today the sign is monomorphemic and
"monosyllabic". The sign is made by an opening motion of the two hands, one at the forehead and one in neutral space. That part of the sign in which the hand is at the forehead is derived from the original KNOW (bent palm toward signer, fingers touching forehead), but the ending hand configuration, orientation (palm away from signer in INFORM) and movement is derived by means of anticipatory assimilation to the sign BRING (two hands in neutral space, moving outward, palms upward). As another example consider the sign GOLD (or CALIFORNIA), earlier a compound of EARRING (index and thumb grasp ear lobe) and YELLOW (twisting or shaking movement of Y hand—pinky and thumb extended—in neutral space). Currently the sign GOLD entails a twisting movement of the Y hand shape moving from the ear downward. Again, the change in GOLD represents an anticipatory assimilation of both hand configuration and movement. Another example of assimilation can be seen in the sign TOMATO, derived from the compound RED (index finger extended, palm inward, drawn downward along mouth) and a form of SLICE (side of dominant hand—open palm—drawn downward along side of non-dominant hand—in fist shape). The current sign TOMATO entails an anticipatory assimilation of the orientation of the hand in the first part from the orientation in SLICE and a perseveration of the hand configuration of RED (index finger extended) which remains constant throughout the sign.

Synchronously, we find numerous examples of unintended assimilation, particularly hand shape assimilation. As an example consider the phrase TEMPT STEAL 'tempted to steal'. In citation form TEMPT is made with the bent extended index finger (X) tapping the elbow of the crooked non-dominant arm. STEAL uses the V hand shape (index and middle fingers extended, separated), bent sharply as the hand is moved along the underside of the crooked (non-dominant) forearm toward the hand. I have witnessed the unintended anticipatory assimilation of the V hand shape from STEAL to the sign TEMPT—so that TEMPT is made with the V shape rather than the X shape.

There are countless examples of this type of unintended anticipatory assimilation of hand shape. This is not surprising in the light of the following evidence in regard to hand configuration and place of articulation in signs in which the non-dominant hand acts as the place of articulation. It would appear that, in general, the onset of appearance of hand configuration and place of articulation (where location = non-dominant hand) are not simultaneous as previously supposed. From evidence gathered from video taped portions of normal signing, we find that hand configuration of the articulator of a
given sign is formed first, and only then is the hand shape of the place of articulation hand shaped or even brought into the signing space. In addition, we find that in connected discourse, the hand shape of the articulator anticipates the following sign, while the shape of the place of articulation hand lags or perseverates the shape of the previous sign. In most cases, this does not cause assimilation—that is to say that the anticipation comes after the previous sign has already been formed correctly and the perseveration does not remain throughout the following sign. However this tendency for hand shape to anticipate the following sign's hand shape and for shape of place of articulation hand to perseverate shape of the previous sign would tend to cause unintentional assimilation in rapid signing and would thereby be a viable explanation for the type of assimilation discussed here. For example, consider the phrase SCHOOL SAVE 'school saves'. The sign SCHOOL is made with two B hand shapes (open flat palm). The dominant hand taps the palm of the non-dominant hand twice. SAVE is made by touching the palm-up side of the extended index and middle fingers (V) to the underside of the closed fist (A), thumb upward. In connected discourse, rather than signing each sign as in citation form, the signer first makes the sign for SCHOOL, then as he moves the dominant hand around to the underside of the non-dominant hand (for SAVE) he changes the B shape to the V shape of SAVE while maintaining the B shape of the non-dominant hand from SCHOOL. Only after the dominant hand has come into position for the articulation of SAVE does the shape of the non-dominant place of articulation hand change to the A shape required for SAVE. The fact that this type of anticipation-lag process occurs consistently in the language can be unambiguously supported by evidence from video tapes, when viewed frame by frame.

This type of process is hardly surprising. The anticipation of following and perseveration of preceding segments occurs constantly in oral language and is the basis for the explanation of assimilatory change in speech production. As an example consider the evidence of nasalization of vowels in English caused by early lowering of the velum in anticipation of the following nasal. Another example of the anticipation-lag syndrome of signal production is the introduction of an epenthetic stop between a liquid and a fricative as when the English word [pæls] becomes [pælts]. The dental stop is introduced due to the fact that the speaker cannot make the transition in place of articulation from [l] to [s] quickly enough. In the articulation of [l] the tongue tip is on the alveolar ridge, sides of the
tongue not touching. Production of [s] entails the reverse articulation—sides of the tongue making contact with the ridge, tip not in contact. In making the transition from [l] to [s], the speaker makes complete closure producing the stop [t]. Thus the tongue tip contact of [l] perseverates and the side closure of [s] is anticipated.

[n.b. We find that in spoken language the incidence of anticipatory assimilation far outweighs the incidence of perseverative assimilation. Interestingly, the same is true in ASL. I mention this because although as far as I know there is no explanation, either articulatory or perceptual, for this phenomenon, the explanation must lie outside of the realm of interference due to the production of the speech signal (because the same occurs in visual language).]

The second phonological process to be discussed here is neutralization. Neutralization in oral language entails the loss of distinction of two or more phones in a particular environment in a given language (e.g. loss of voice and voiceless distinction in final stops in German). By far the most common neutralizations in ASL are caused by what Frishberg calls "centralization" (the terms are not synonymous by any means).

The locations of a number of signs have moved historically to more central locations. It would appear that the center of the signing space is the hollow of the throat. By the process of centralization, the location of signs tends to move downward from the face, inward from the sides of the body, and upward from the waist. Frishberg offers the following examples. The sign DON'T-CARE which was originally made at the forehead, is now made at the nose. The locations for NOTHING, DENY and WRONG have all moved from the area of the upper lip in Old French Sign to below the chin in present day ASL. The signs for YOUNG and WILL (FUTURE) have changes such that the original waist level place of articulation has become one near the shoulders. The signs FEEL, LOVE, PLEASE (and other signs related to emotions "of the heart") have centralized to center chest (to the line of bilateral symmetry) from a location on the left side of the chest (over the heart).

Basically centralization is caused by the signer's desire to lessen effort (i.e. ease of articulation). It causes less physical energy to maintain the relatively smaller signing space resulting from a move of location from the extremities to a more central position.

What might also be called a tendency toward centralization can be seen in changes that occur in rapid signing (analog of rapid speech). In rapid or colloquial signing or when the signer is tired, the tendency is to
centralize location of signs. Again, locations tend to move downward, inward and upward to a more central location and outward to neutral space—that is, off the body. One functional result of the tendency for location to move off the body is loss of contact in contact signs. CONTACT is a distinctive feature of the movement parameter. By means of the centralization process (expanded in definition here), the loss of this distinction occurs at times. Moreover, loss of place of articulation distinction results in rapid signing, when for example the distinction between the locations forehead and neutral space are lost when both are articulated in neutral space. For example, the sign THINK—index finger on forehead—is often signed in neutral space, having moved downward and off the body in rapid signing. Thus we could state that the functional load of centralization is neutralization.

ASL has several morpheme structure conditions, one of which entails, in double contact signs (signs in which there are two places of articulation), that a. the choice of the location of the second contact is limited (and dependent on the location of the first contact) and b. that a neutralization of distinction occurs in the major location area of the second contact. That is, for example, that the choice of location of second contact may be at the central chest, but not at the shoulder. In this constraint, we also find a neutralization of distinction in the place of articulation parameter.

One striking example of neutralization in ASL is the addition of the extended thumb to hand configurations which previously did not entail this extension. The change is ongoing. Not all signs have this change nor do all signers extend the thumb. Thus we find a neutralization in progress. Signs to which the extended thumb is added are those which previously merely extended the index finger (G) or the index and middle fingers, unspread (H) or spread (V). When the G hand shape adds the extended thumb, it has the same shape as the L shape, which is considered distinctive. The change which entails the addition of the extended thumb to the H shape makes it resemble the 3 hand shape (thumb, index and middle fingers extended and spread). The change in V allows it to merge with 3. Thus the change in the shapes G, H and V causes a neutralization of distinctive features of the hand configuration parameter. Examples of signs which exhibit this change include RED (G hand drawn down mouth), VOICE (V hand moves down neck) and NAME (H hand taps H hand).

A third phonological process that occurs in both oral and visual language is deletion. In oral language, a segment may be deleted—with or without compensatory
change (e.g. loss of nasal following nasalization of preceding vowel as in French) or inserted (e.g. intrusive stop, epenthetic vowels, etc.). In visual language, however, the simultaneity (simultaneous articulation) of components and the necessity for one unit from each of the four parameters to be present in each sign prevent the deletion (actually make it impossible) of one component. Deletion occurs in ASL, but what is deleted is whole units or items.

Frishberg (1974) discusses the historical deletion of parts of compound signs and of facial expression and gross bodily movement when used for segmental purposes (as opposed to suprasegmental or prosodic use). The sign BIRD (a metaphorically iconic representation of a beak) originally was a compound of a sign for beak and one for wings. The 'wing' portion has been deleted. Another example is the sign DARK which is derived from the present day sign BLACK plus the present sign DARK.

Many signs originally required the use of facial expression and gross body movement for non-prosodic purposes. In the present phonological structure of ASL, only the hands may articulate segmental portions. The face and body are used extensively for the superimposition of prosodic features.

As an example of this change, which we might term deletion (of facial expression and body movement component, with compensatory change), consider the sign (BE)PATIENT, originally entailing pressing the forefinger against the lips and bowing "the head as if in resignation". Today the sign is made with the fist shape and the articulator (thumb toward signer) moves downward in the mouth area. The head does not move.

I have also discussed above the deletion or loss of contact that results from centralization in rapid signing.

Another striking type of deletion occurs in the deletion of one hand of a double articulator (two handed) sign in rapid or colloquial signing. It is possible, in colloquial signing to articulate all signs which are two handed in citation form with only one hand. The deletion of one hand occurs either when the signer is tired or more commonly when one hand is otherwise occupied (e.g. holding a book, etc.). There are two types of signs which entail the use of both hands: a. double articulator signs, in which both hands have a movement component and b. those signs in which one hand is the articulator, the other the place of articulation. When this type of deletion occurs in a double articulator sign, it is always the non-dominant hand which is deleted. When one hand acts as the place of
articulation, that hand is the non-dominant one. In rapid or encumbered signing (i.e. when one hand is otherwise occupied) it is the non-dominant place of articulation hand which is deleted. It is with the second type of sign that compensation is made subsequent to deletion, in that another unmarked place of articulation must be added (to account for the loss of the hand as location). This new location may be any convenient surface—e.g. table top, chair, thigh, etc.—if the signer is seated or, for example the hip or the book (or whatever) in the non-dominant hand if the signer is standing. I have witnessed the sign YEAR (normally made with double fists—dominant hand circles, then makes contact with non-dominant hand) made only with the single fist of the dominant hand, in a circular action in neutral space and then making contact with the signer's hip. This type of deletion is clearly the most common form of synchronic phonological alternation.

In regard to addition (or insertion), I have given the example above of the addition of the extended thumb in signs with the G (index finger extended), H and V (index and middle fingers extended, unseparated and separated) hand configurations. Again, I remind the reader of the impossibility in visual language of word internal insertion.

There are various types of phonological processes that occur in oral language that do not and in fact cannot occur in visual language. These include such processes as metathesis, syncope (of component or segment) and such purely segmental phonological changes as for example loss of final vowels or devoicing of final consonants. The reason for the lack of these and other similar processes in visual language is clear. All entail an alteration, or alternation in the case of metathesis, or deletion of linear segments within the word or syllable. I have discussed at some length (Friedman 1974b) the idea that no change can occur on the segmental portion in a visual language because there are no linear segments per se. Each "monosyllabic" sign requires one and only one component of each of the four parameters. One could hardly for example metathesize a movement and hand shape that are in addition articulated simultaneously. For those signs which entail two places of articulation or two hand configurations, one of the following must occur: a. in a double location sign the hand shape remains constant, or b. one of two hand shapes assimilates to the other, or c. in a single place of articulation sign, the hand shape change entails an opening or closing action, always either starting from or resulting in a neutral shape (e.g. B closing to A, A opening to 5, 5 closing to F, but never
for example 8 changing to F). If the sign entails a closing action, it must start with a neutral hand shape (i.e. 5 closing to F); if the sign entails an opening action, it must end in a neutral shape (i.e. 8 opening to 5). Rather than describing these signs as for example B closing to A or A opening to 5 (as Stokoe has done and I have continued to do), it may be well to describe only the salient hand shape (it can be demonstrated that there is only one) with the notion of beginning from or ending in that hand shape to or from a neutral, unmarked and possibly unspecified hand shape. Any change which entails an alteration of a particular segment, conditioned by the surrounding segments (note that the concept surrounding in the linear sense is anomalous as well in ASL) cannot occur in visual language. It is conceivable however, for the components of a single parameter to metathesize across word boundaries (and only across word boundaries as there are no inflectional affixes in ASL), but this type of metathesis would not only be unintended but incidental as it would not result in permanent phonological change in a given item.

In regard to a process such as suppletion, I would suggest that although it is conceivable for suppletion to occur in visual language, it is highly unlikely due to the fact that there are no inflectional paradigms per se in visual languages. There are other types of paradigms, such as those in which, for example by means of a change in hand configuration only, the compound signs for LAST-WEEK, TWO WEEKS-AGO or THREE WEEKS-AGO (also TWO, THREE DAYS-AGO, etc.) are formed. (WEEK is made by drawing the G hand—index finger extended—palm away from signer, across palm of non-dominant hand; LAST-WEEK is the compound of WEEK plus an index to past time, with an assimilation of the orientation of the articulator in WEEK to that of the index; TWO WEEKS-AGO entails the V hand shape—index and middle fingers extended—which is also the hand shape for the sign TWO—with the same location, movement and orientation as for LAST-WEEK). However, I would be unwilling to call this paradigm an inflectional one. To my knowledge, there are no cases of suppletion in visual language, however I would say that the potential exists.

Various phonological processes occur in visual language which cannot occur in oral language due to the nature of the signal. Included in these processes are:

1. Simultaneous articulation of components. Clearly in an oral language, segments are produced in a linear order. In visual language, in every sign, at least four components are articulated simultaneously. Although this is not within the scope of this paper, I would
mention that the potential exists in visual language for articulating more than one item at the same time. Of course, in oral language, it is quite common for intonational cues, e.g. question, to be superimposed on the segmental portion as for example in the echo question 'John?'. However, besides adding intonational cues by pitch variation, etc., it is clearly impossible to articulate more than one string of segments at one time—there is only one articulator. However, due to the fact that there are two articulators in languages in the manual/visual modality, it is possible and in fact common to momentaneously articulate two items with or without the addition of suprasegmental features. For example consider the Sign sentence EAT HE? in which the three morphemes EAT, HE and sentential question may all be articulated simultaneously: EAT by the dominant hand, HE by an index with the non-dominant hand, and question by raising the eyebrows and holding the signs for an extra beat. This type of construction, in terms of the physical realization, is of course impossible in oral language.

2. Scalar items. Because of the nature of the modality, language in the visual mode has the ability to convey nuance of meaning in a unique way. Because he is using a visual language, the signer has the ability to alter the shape of a sign in such a way as to convey degrees of meaning. Oral language has this ability only in limited ways (phonologically). In some languages, for example English, one can intensify the degree of an item (usually a surface adjective or durative verb) by stressing it, as for example in the sentence, 'The fish was BIG!'. In other languages, for example Hoysan, intensification or lessening of degree can be conveyed by various types of reduplication. The Hoysan word [hun] 'red' when reduplicated with a high rising tone followed by a low level tone [hun hun] means 'very red'; when reduplicated and the tones are reversed [hun hun] it means 'slightly red'. These phonological processes to show degree are limited in oral language.

In visual language what I would refer to as scalar items and the "phonological" processes by which these items are formed are plentiful. Visual language has the advantage of being able to use a much less discrete signal than can oral language. Any oral language is bound by its phonological structure: it must form its words from among available, acceptable (in the language) phones, in an acceptable combination. If a speaker wishes to convey various degrees of meaning (except in those cases cited above), he must carefully choose the lexical item representing that nuance, which rarely bears any phonetic resemblance to any other lexical item.
having the same "core" concept.

In many classes of items in ASL, it is possible to indicate degree of value by altering the "phonological" shape of the morpheme. If the signer wishes to convey, instead of for example 'big', 'very big', he simply makes the sign BIG bigger. If he wishes to convey 'extremely happy,' he articulates the sign HAPPY relatively larger than it would be for 'happy' and more rapidly and it may be reduplicated.

The scalar dimension can be seen in many nominal signs, as well. The nature of the phonological alteration of course depends on the semantic nature of the referent of the sign. For example, the signer can convey the meaning 'big table' simply by signing TABLE larger than he would in order to convey 'table'. The same can be done for other signs having referents in which spatial dimension can be iconically indicated. 'Small boy' may be articulated by signing BOY (fingers closed to tapered O shape at forehead) and drawing the hand outward and downward while maintaining the final hand shape of BOY. 13

3. Symmetry. Frishberg (1974) discusses the historical tendency toward symmetry in ASL. Signs have changed in such a way as to create symmetrical action of the two articulators. The tendency toward symmetry takes two forms. One is to cause the hand configurations of the two hands in two handed signs—either those originally monomorphemic or compound signs—to become the same. For example, the sign LAST (FINAL) originally had the index finger of the moving hand contact the extended little finger of the location hand. Today both hands have the little finger extended. [n.b. This is also an example of anticipatory assimilation.]

The second result of the symmetry tendency is to cause originally one handed signs in low acuity areas (i.e. below the neck) to become two handed, presumably for the purpose of adding redundancy to low acuity areas. 14 The signs DIE (DEAD) and HURRY, for example, which today are articulated in citation form with two hands, derive from one handed signs. 15 [n.b. This might also constitute an example of phonological addition.] It is clear that the tendency toward symmetry in ASL has no analog in oral language—a language can only insist on symmetry if there is more than one articulator.

1 For further description and discussion of formational structure and historical details of ASL see Friedman 1974b, 1975.
2 Frishberg quotes from Long, 1918, reprinted 1949.
3 See Friedman, 1974a,b for complete discussion of movement.
For further detail see Friedman 1974b, Friedman & Battison, 1973.

This variation and its ongoing change in ASL is discussed in detail in Battison et al., 1974.
I have discussed at length the idea that the components of a sign utterance are not comparable in certain respects to the segments of a spoken utterance in Friedman 1974b.

For further discussion of prosodic features see Friedman 1974a,b, 1975.

Frishberg quotes from Long, 1918.

For further discussion see Friedman 1975.

See Friedman 1973 for details.

See Friedman 1974a,b for further discussion of question intonation in ASL.

See Friedman 1973 for discussion of scalar reference in space and time locations and Friedman 1974b for discussion of other types of iconicity.

See Friedman 1974b for discussion of changes due to visual perception. Also see Siple 1973 for visual perception data.

References


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James Matisoff in his 1970 paper on glottal dissimilation and the Lahu high-rising tone has suggested that, if it could be proved that tonal contrasts are always secondary developments in the history of languages, it would be of the greatest typological interest. His examples from Lahu phonology show the complex tonal system in that language to have arisen from purely segmental circumstances. I will argue in this paper that the origin of the tonal accents in Low German and in the Scandinavian languages can be explained on the basis of segmental circumstances as well, and that they may be considered as secondary in the historical development of these languages. In Matisoff's analysis of Lahu, he concludes that it is the initial and/or final consonant which triggers the development of tonal contrasts and that such consonants are those segments which are most susceptible to change or loss. I will argue that in Low German and Scandinavian languages the segmental circumstances involve only vowels and diphthongs and that it is the redundant tonal transition in centering diphthongs which becomes distinctive when such diphthongs monophthongize. The sources of the centering diphthongs themselves will be looked for in terms of changing timing of disyllabic sequences.

I will examine the evidence in Low German dialects since the data offers what appear to be several intermediate stages in the process of tonogenesis. Parallel or similar developments in Scandinavian languages will be mentioned where appropriate.

Tonal accents in Low German dialects have attracted more attention from dialectologists than other linguists, perhaps because they are not as spectacular as the Danish stød or the musical accents of Norwegian and Swedish. The tonal accents of Low German occur in the North Saxon dialects of the Lower Elbe, that is, those nearest Denmark. The grammars call these accents "overlong" or Schleifton. Seelman describes them as involving a steep pitch drop in the vowel which he expresses in musical terms as at least a major third.

Within the grammar, the accents occur before final voiceless lenis sounds, namely, /v, z, d, g/ or a vowel in monosyllabic morphemes which have suffered apocope. The vowel which carries the Schleifton was originally long or lengthened in an open syllable before apocope occurred. Keller considers the Schleifton to be an allophone of the lost final reduced vowel since it "occurs in dialect where New High German, with which all speakers are conversant, has schwa." Schleifton can occur before fortis consonants only if these consonants do not belong to the same morpheme; therefore, in verb forms which have
suffered syncope the tonal accent occurs: 1st sg. pres. ik gääv' 'I give', 1st pl. pres. wi gääv't 'we give', 2nd sg. pret. du bleev'st 'you stayed', etc. (The apostrophe indicates the presence of Schliefton).

In terms of morphological categories, the tonal accents can occur in the singular of many feminine nouns of the ð- and n-classes, the singular of a few masculine n-class nouns, the plurals of many nouns with a former -e suffix, in some adjectives of the former ja-class, in some words with a former adverbial ending in -e, and in some verbal forms as already mentioned.

It would seem from this description, taken from Keller, that the tonal accent is a compensation for the final vowel lost in apocope. However, there exists other data which suggests that the solution is not quite so simple.

In Erich Seelmann's 1908 description of the Low German dialect of Prenden, spoken near Berlin, he notes that the tonal accent is optional: it arises in emphatic or excited speech and occurs when a centering diphthong is monophthongized, especially before nasals and liquids. In the Prenden dialect apocope has not occurred so that the tonal accent cannot be said to be a compensation for a lost final schwa or other vowel. There also exist a number of Frisian dialects, spoken on the north coast and in the northern coastal islands of The Netherlands, which have the Stosston in open-syllable lengthened vowels in words without apocope. It may also be noted that in the Jutish dialect of Danish described by Ringgaard the stød alternates with a schwa vowel in apocopated words. It seems clear that those dialects which have centering diphthongs do not have tones while those which have monophthongized these centering diphthongs do have tonal accents or stød. Clearly there must be a relationship between these phenomena.

Since centering diphthongs are contrastive in the Prenden dialect, as in many other dialects where they occur, it seems clear that they acquire tonal accent when monophthongized in order to continue to contrast with those long monophthongs with which they would otherwise merge. A similar development has taken place in the South Dutch dialect of Maaseik where short vowels lengthened in open syllables contrast by tonal accent to the old long vowels which do not have any tonal accent.

Observations of the correlation between tonal accents and short vowels lengthened in open syllables led Erich Seelmann to propose a solution to the problem of open-syllable lengthening in order to explain the tonal accents. He hypothesized that in disyllabic sequences with one medial consonant, i.e., when the first vowel was in an open syllable, the "end syllable accent weakened," causing the "accent" in the first vowel to become a Schliefton, after which a schwa-like "overshort" vowel appeared at the end of the first vowel, leading to the formation of a
full long vowel or a centering diphthong. Agathe Lasch in her Middle Low German grammar essentially follows the same line of reasoning.

Open syllable lengthening of short vowels occurred in all the Germanic languages in varying degrees. The primary difficulty in explaining open syllable lengthening is that CVCV words in older stages of Germanic appear to be stable, yet in later stages the first vowel is lengthened. Since the word accent is assumed to occur on the first syllable of native Germanic words and the position of the accent has not changed in CVCV words with lengthened first vowels, there seems to be no very clear phonetic explanation for the development. Peter Skautrup, writing about Danish, proposed that the lengthening was caused by a change in timing of the sequence such that the second syllable lost duration which was acquired by the first vowel. Seelmann seems to recognize that the reduction of the second vowel is related to the lengthening in his remark about the "weakened accent of the end syllable." English scholars such as Richard Jordan also suggested that the reduction of the final vowel was involved; however, along with most scholars of Germanic languages, he conceived of the relationship as an "increasing force of the accent" on the first vowel, that is, the accentual force of the second vowel was somehow transferred to the first vowel, causing it to lengthen.

Although "increasing force of the accent" is too vague a formulation to be useful, the phrase does suggest that we should look to the internal organization of CVCV words for a solution to the problem. Recent work by Ilse Lehiste suggests that such internal organization does exist in terms of stable vowel duration ratios in disyllabic words. In her 1971 article on the temporal organization of speech, Lehiste reports on a study indicating that in English the vowel of a CVC sequence is more closely related to the following consonant than to the preceding consonant in terms of negative correlation. Negative correlation is defined as holding between the durations of two successive segments if an error is made in the duration of one segment and the error is largely compensated for in the following segment. Such negative correlation "suggests that articulatory events are programmed...not in terms of single phonemes, but in terms of higher-level articulatory units."

Lehiste also studied disyllabic sequences, using the words steady, skiddy, skitty, spoken in the Midwestern variety of General American where the medial consonants would be normally pronounced with a flap. In these words the flap could not be said to be more closely related to the preceding or following vowel, based on negative correlations. Since Lehiste interprets the medial flap itself to mean that the articulatory program of such disyllables must obligatorily encompass the whole CVCV sequence, the negative correlations appear to be a
confirmation of that assumption.

More important to the argument of this paper is Lehiste's discovery of a constant vowel duration ratio, i.e., a negative correlation between the vowels of the CVCV sequences skiddy/skitty, even though skiddy was longer in absolute duration than skitty due to the voicing of the underlying medial consonant. Lehiste says, "The duration of the second vowel is adjusted to the duration of the first, and the sequence of two vowels constitutes a unit of programming at some higher level." In brief, she has discovered the principle of internal organization of disyllabic sequences in English.

If we extend Lehiste's discovery to the analysis of the problems involved in open syllable lengthening, we find it to be very useful.

First: We can see at once why the formulation of the change in length in open syllables as a function of accent was unsatisfactory. Accent, in itself, although it has been recognized as an organizing principle by Galton and Ladefoged, cannot tell us how a sequence subordinated to accent is organized. It should really be considered a "cover term" denoting groups of other features as suggested by Ladefoged in discussing the hierarchy of features in general.

Second: Lehiste's discovery gives us a positive and measurable means of testing Skautrup's hypothesis about a change of timing in disyllabic sequences that acquire open syllable lengthening. It certainly seems more useful to consider the change in terms of duration than in terms of "increasing accent force." If CVCV sequences are organized as a single unit of articulatory programming, then internal changes in vowel duration ratios will involve changes in the negative correlations, i.e., compensation will occur in one segment or the other. Lindblom (quoted by Lehiste, 1970) in working with Swedish, suggests that vowel reduction is due to timing, i.e., that a lack of sufficient duration results in the vowel in question being unable to reach its target of height and tonality. Although Lehiste remarks that Lindblom's hypothesis does not account for certain differences in quality between vowels in stressed and unstressed syllables in languages like Russian, the hypothesis may be viable for short vowels in open final syllables, that is, for the second vowel in CVCV words. I am not considering the possible motivation for a change in timing but it should be observed that loss of length in final syllables is characteristic of Germanic languages, including Gothic. Gothic, for example, has short final vowels corresponding to long final vowels in Indo-European and has no final vowels in words with short final vowels in Indo-European.

Third: The discovery of vowel duration ratios suggests a phonetic explanation of vowel lengthening itself. The concept of what actually happens when a vowel lengthens has not been
clear: How does a vowel lengthen? By simple prolongation? If so, when is the lengthened vowel recognized as distinctively long? How important is quality? What about the former contrast between long and short vowels in open syllables which lengthening obscures?

That the contrast of long and short vowels in open syllables remains important after open syllable lengthening occurs may be inferred from the development of the vowel systems of Germanic dialects: in the very conservative Westphalian dialects of Low German old long vowels and lengthened vowels still contrast. The lengthened vowel reflexes are centering diphthongs while the old long vowels have diphthongized with high closing finals, e.g., *iu*, *ui*, *ai*, *au*, etc. The same development occurs in Faroese dialects. In the West Norwegian dialect of Aurland the lengthened vowels are monophthongs but the old long vowels are diphthongs. In Frisian dialects the lengthened vowels are monophthongs with tonal accent or Stosston while the old long vowels are monophthongs without tonal accent except in exceptional circumstances. In the Dutch dialect of Maaseik the distinction between old long and lengthened vowels is maintained solely by tonal accents versus lack of tonal accents. In the North Saxon dialects the analogy would seem to break down since any vowel – old long or lengthened – which occurs before a final lenis unvoiced consonant has the tonal accent. However, since the same development occurs in dialects of Franconian, it appears that the identification of certain old long vowels with the lengthened short vowels in positions which are reflexes of old open syllables is a secondary development, according to Wiesinger. In English it is inferred that the vowels lengthened in open syllables were at first phonetically distinct from the old long vowels with which they later merged since, according to Dobson, careful Middle English poets such as Chaucer and Gower avoided rhyming long and lengthened vowels.

If we maintain that the lengthened vowels were necessarily phonetically distinct from the old long vowels because the contrast continued to be important, we must surmise how this contrast could be maintained in the face of changing negative correlations. Based on the conservative dialects of Westphalia and the Faroe Islands, the transition dialect of Prenden, and the tonal dialects of North Saxon, I suggest the following development. When the timing of the CVCV sequence changed for reasons that will not concern us, it appears that the final vowel lost duration. I suggest that the initial vowel compensated for this lost duration by delaying termination of the vowel by the addition of a schwa-like neutral speech sound, thus preserving the shortness of the distinctive vowel quality while accommodating the change in length of the second vowel. Since vowels have intrinsic pitch, the change in pitch between the distinctive portion of the lengthening vowel and its schwa-
like final portion would be a redundant accompaniment of the centering diphthong which this process created. Therefore, when the centering diphthong monophthongized, the redundant pitch change between the two components of the diphthong could become distinctive in order to continue to maintain the contrast of old long and lengthened vowels.

It has been suggested by Ringgaard that the pitch change is caused by a retraction into the first syllable of the original pitch change between the first vowel and the second reduced vowel in Danish words with apocope. However, this explanation would not fit the situation in Prenden where the centering diphthongs acquire tonal accent when monophthongized, whether or not there is a reduced vowel in the next syllable. Further, in Prenden the lengthened vowels have merged in the centering diphthongs with low-mid initial component while the centering diphthongs with high first component are reflexes of MLG eu and ø; therefore, it would appear that it is the structure of the diphthong itself, not the environment, which determines the appearance of the tonal accent. This analysis is dependent, of course, on the assumption that those vowels with which the new monophthongs would otherwise merge do not change their quality or their quantity (which is the situation in Prenden).

Since the first indication in documents that open syllable lengthening in Low German has occurred appears in the 12th century, I would like to account for the stability of the centering diphthongs in dialects such as those of Westphalia. It is known that the North Saxon dialects were once also characterized by centering diphthongs which monophthongized in the 16th century. I suggest that with the appearance of the schwa-final in the first vowel in response to the reduction of the second vowel to schwa, the two schwa elements became related by a new vowel duration ratio which stabilized the new sequence of lengthened vowel plus reduced vowel and thus prevented apocope. On the other hand, in North Saxon where apocope has occurred, I suggest that as the final vowel further reduced in duration, the redundant pitch change in the centering diphthong of the first syllable became increasingly prominent and began to substitute for the centering diphthong. As the final reduced to zero, the pitch drop in the first syllable intensified until, in Danish and Frisian dialects, the final pitch level of the falling tone became low enough to cause laryngealization: thus, the stød or Stosston came into existence. That the development to stød is not a necessary stage is shown by the apocopated words in North Saxon which still have steeply falling tonal accents. The fact that centering diphthongs in Prenden can monophthongize with accompanying tonal accent suggests that apocope may be developing optionally in the dialect although Seelmann doesn't mention such a development.

Is it possible to extend this analysis to the tonal accents
of Norwegian and Swedish? I think it can be done.

First, we notice that it is the East Norwegian and the Central Swedish dialects which have the distinctive tonal accents. Northern Norwegian dialects, Finnish dialects of Swedish, Icelandic, Faroese, and possibly the Western dialects of Norwegian do not have these tonal accents. Second, these tonal accents occur only in words of more than one syllable and in only those words which in Danish can never have stød but, in isolated dialects of Southeast Jutland, can have tonal accents. Third, Icelandic, Faroese, and the Western dialects of Norwegian have extremely complex diphthongization patterns while in the dialects of East Norwegian and Central Swedish which have the tonal accents, the old long vowels and the lengthened vowels are monophthongal. Fourth, it is the East Norwegian and Central Swedish dialects which acquired "syllable balancing", i.e., the development of CV.CV words with a kind of vowel harmony between the vowels which apparently made such words quite stable.

How can we relate these facts to the development of tonal accent from the pitch changes within a centering diphthong? I suggest that the Norwegian and Swedish dialects with tonal contrasts may have developed them in response to the loss of length in the second syllable of a heavy disyllabic sequence rather than the loss of length in the second syllable of a light disyllabic sequence as in Danish, North Saxon and Frisian. Although the tonal accents are generally assumed to have been present in Old Norse in polysyllabic words, there is no clear explanation for the lack of these accents in the most conservative modern representatives of Old Norse, namely, Icelandic and Faroese. Some linguists have proposed that the tonal accents arose as a result of the loss of length of the second syllable, i.e., the syllable following the initial accented syllable, but have not proposed a phonetic implementation of the change.

I suggest that after the period of syncopation (ca. 900 AD) brought a heavy first syllable into juxtaposition with a second heavy syllable or a sequence of two light syllables, a duration ratio was established between the two elements. Benediktsson expresses this relation by saying that the first syllable had primary stress and the second syllable had secondary stress. When the duration of the second element (either a heavy syllable or a sequence of two light syllables) was reduced, the vowel of the first heavy syllable responded by delaying termination in the same way as in light disyllabic disturbed sequences, i.e., by acquiring a schwa-like final. Since those dialects which developed tonal accents in these positions also had developed "syllable balancing" where optimal vowel duration ratios tended to delay final vowel reduction in CV.CV words, the retention of a tonal reflex of former heavy disyllabic sequences was not hampered. It follows, then, that heavy sequences contrasted
with light disyllabic sequences in the original language but that later changes in the duration of final elements of these phonological units discouraged the retention of the original contrast in terms of tonal accents since such a retention would require three accents: one for the heavy sequence, one for the light sequence, and one for monosyllables. That such a development was not impossible is illustrated by the existence of three accents in the Norwegian dialect of Oppdal where apocope is occurring in a tonal dialect and in isolated dialects of Danish in Southeast Jutland which have plain accent, stød, and tonal accent in contrast.

In summary, my argument not only proposes that the tonal accents of Low German, Frisian, Danish, Norwegian and Swedish are secondary developments, it also pinpoints the development in terms of specific segmental conditions, i.e., the rise of distinctive tonal accent from the redundant pitch change of a centering diphthong when it is monophthongized. Therefore, the explanation of these accents parallels that of Matisoff in his analysis of Lahu tonogenesis except that it deals with the interrelationships of vocalic elements instead of the interrelationships of vowels with consonants.

In addition, the development of tonal accents in Germanic dialects offers support for Ilse Lehiste's theory of phonological units larger than the syllable. If such phonological units exist, they may be expected to take part as such in historical change. If we look at the tonal accents of Norwegian and Swedish as reflexes of former heavy disyllabic sequences and the stød and tonal accents of Danish and Low German as reflexes of former light disyllabic sequences, we have in effect proved her point.

Finally, the analysis has been made without recourse to the concept of stress, but only in terms of changing duration and changing pitch or fundamental frequency patterns. Therefore, the analysis offers support for the proposal of Ladefoged that some features act as "cover terms" for groups of other features.

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The Perception of Sentences: A Linguistic and Perceptual Comparison
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Does how sentences are perceived relate to their structure? A number of cognitive psycholinguists, such as Carpenter and Just (1975) and Clark (1972), have argued that some transformations of sentences, such as Negatives, are more difficult than others, in that more processing time is needed to understand them. We wished to determine whether such transformations were also harder to perceive, and, if so, what the errors were like.

Does how sentences are perceived relate to their structure? This question can be explored in another way. Is a sentence more difficult to perceive if it contains a larger number of characters (a perceptual feature) or a larger number of words (a linguistic one)? If only a single glance is permitted the perceiver, will a particular place in the sentence (an aspect of perception) or a particular part of speech (an aspect of language) be reported?

Little is known about the perception of sentences. Perceptual psychologists are now debating whether perceptual strategies (i.e., where the eye looks next in a text) are related to the syntactic properties of the text at all. In fact, the majority of these scholars is that eyemovements over text are not related to its linguistic structure (for a full discussion of such models, see Haber, 1975). Our assumption, contrary to most current opinion, is that eyemovements and the perception of a text and its linguistic structure are interrelated.

Previous research on single sentences has tended to use auditory presentation (e.g., Fodor and Bever, 1965), or to permit multiple fixations for visual presentations (e.g., Wanat, 1971; Rayner, 1975). In order to set up a baseline to study the effect of varying syntactic structure, in a first experiment we held structure constant and varied the number of words and the number of character spaces they occupied. This enabled us to analyze the effects of sentence length, word position, and number of words independently. Then, with these results under our collective belts, in a second experiment we chose a constant sentence length and systematically varied syntactic structure by presenting each of a number of kernel sentences in 24 different syntactic configurations. For both studies, we permitted the viewer only a 200 millisecond glance at the sentence, so that he could not move his eyes to a second point of fixation.

Before describing the procedures and the results of the two studies in detail, some comments are in order about the variables we manipulated. Since it is known that the effective visual field is large enough to permit a viewer to perceive a number of words in a single glance, we wished to determine whether there is a systematic effect on the extraction of information as a result
of the number, the position, and the syntactic function of each of the words in the sentence, and of the syntactic structure of the sentence as a whole.

It seems reasonable that if a reader is given only a single glance of a sentence, so that he cannot move his eyes over it, then the longer the sentence the less of it he will perceive. But sentences can be made longer either (1) by inserting more words or (2) by having longer words (more character spaces). Do both of these variables have equal effects? Consider the three sentences:

1. Katherine hit Bartholomew
2. Fat Sue hit small weak Jo
3. Joe hit Sal

Sentences 1 and 2 are matched in character spaces but not in words, while sentences 1 and 3 are matched in the number of words but not in character spaces. We predicted that the number of words in a sentence would not effect performance, holding structure constant, but that the more space on the page the sentence occupied, given the limitation of a single glance, the poorer information extraction would be.

In the first experiment, all of our sentences were of the same form, based upon a kernel of a three-word simple declarative sentence: subject (common noun, proper, or pronoun) a verb (where Aux was tense only) and an object, (common, proper, or pronoun). To add words, adjectives were inserted before either or both the subject or/and the object (except when pronouns were used, which normally do not permit co-occurrence with adjectives). Some examples are given in Table 1. All words in each sentence

| John read it.                     | Ann hates big fat boys.                  |
| He brews beer.                    | Some old pens leak ink.                  |
| Tony avoided her.                 | Terry sacrificed her old dog.            |
| They confused us.                 | Nice old ladies knit sweaters.           |
| Banks purchase stocks.            | He teaches second year students.         |
| They demanded justice.            | New ideas clarify many problems.         |
| Jim lost his keys.                | He told his long sad story.              |
| Fat rats scare Dan.               | Some cats have big blue eyes.            |
| Tom tells funny jokes.            | Few bold actors earn much money.         |
| Hard work inspires Kim.           | Nice little green men inhabit Mars.      |
| They visited their friends.       | We utilized his clever new invention.    |
| Some bookstores sell posters.     | Large red Indian carpets frighten her.*  |
met a high frequency of usage criteria, at least according to
word frequencies computed for words in isolation (Kucera and
Francis, 1967). We should comment, however, that controlling
for frequency in isolation in no way controls for frequency in
context, and some of our sentences seemed very strange. In the
process of analyzing them, we tried to sort out the strange ones
and all the sentences got stranger and stranger, so we left well
enough alone.

In addition to an analysis of number of words and number
of characters, it is also possible to examine these data for
the perception of specific classes of words and for specific
positions of words. Since a verb always follows the subject in
these sentences, we cannot look at all permutations of word
classes, but we can determine whether, for example, adding an
adjective to a phrase affects perception of the noun it modifies,
or whether an adjective added before the subject is treated
similarly to one added before the object, and so forth. These
manipulations permit analysis of perceptual information extrac-
tion processes as a function of part of speech, position of a
word in a sentence, word length, and number of words; analyses
that to our knowledge have never been reported before.

In the second study we chose a fixed character space length
and systematically varied the syntactic configuration for a
number of kernel sentences. Table 2 gives an example of the
24 configurations tested for each kernel. This manipulation
allowed us to examine the relationship between the syntactic
structure of the sentences and the information extracted from
them. Are Negative sentences more difficult than Statements,
in that more processing time is needed to understand them? If
so, such sentences should be harder to perceive, and given a
single glance of limited processing time, less would be reported.
Further, from such analyses we hoped to determine which syntactic
configurations are more basic or fundamental. It is our expecta-
tion that these effects will be evident even in the processing of
single sentences from a single glance, and might even be clearer
here when the complicating effects of multiple fixations are
eliminated. Further research will have to carry these findings
into the area of normal multiple fixations and into context.

**Method**

We used the same method and procedures in both experiments.
The subjects, numbering 28 and 24 for each of the 2 experiments
respectively, were University of Rochester undergraduate college
students. They were recruited as volunteers to serve one hour
individually in an experiment described as studying the percep-
tion of sentences.

Each sentence to be read was presented on the 15 inch televi-
sion cathode-ray tube display operated by a Nova data acquisi-
tion computer. Initially a small fixation dot was lit on the
Table 2
The 24 Configurations of One Kernel Used in Experiment 2

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
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</thead>
<tbody>
<tr>
<td>1.</td>
<td>S</td>
<td>Elizabeth hit Julia.</td>
</tr>
<tr>
<td>2.</td>
<td>N</td>
<td>They didn't hit her.</td>
</tr>
<tr>
<td>3.</td>
<td>Q</td>
<td>Did she hit Tony?</td>
</tr>
<tr>
<td>4.</td>
<td>P</td>
<td>We were hit by him.</td>
</tr>
<tr>
<td>5.</td>
<td>QP</td>
<td>Were we hit by him?</td>
</tr>
<tr>
<td>6.</td>
<td>NP</td>
<td>He wasn't hit by it.</td>
</tr>
<tr>
<td>7.</td>
<td>NQ</td>
<td>Didn't they hit her?</td>
</tr>
<tr>
<td>8.</td>
<td>NQP</td>
<td>Wasn't he hit by it?</td>
</tr>
<tr>
<td>9.</td>
<td>PD</td>
<td>Christopher was hit.</td>
</tr>
<tr>
<td>10.</td>
<td>PDN</td>
<td>Jonathan wasn't hit.</td>
</tr>
<tr>
<td>11.</td>
<td>PDQ</td>
<td>Was Alexander hit?</td>
</tr>
<tr>
<td>12.</td>
<td>PDNQ</td>
<td>Wasn't Barbara hit?</td>
</tr>
<tr>
<td>13.</td>
<td>W</td>
<td>Why did he hit Sue?</td>
</tr>
<tr>
<td>14.</td>
<td>WP</td>
<td>Why was he hit by Jo?</td>
</tr>
<tr>
<td>15.</td>
<td>WPD</td>
<td>Why was George hit?</td>
</tr>
<tr>
<td>16.</td>
<td>I</td>
<td>Lou hit a red ball.</td>
</tr>
<tr>
<td>17.</td>
<td>II</td>
<td>Lou hit a ball far.</td>
</tr>
<tr>
<td>18.</td>
<td>NADV</td>
<td>He never hit Louise.</td>
</tr>
<tr>
<td>19.</td>
<td>Emp</td>
<td>Bernice did hit him.</td>
</tr>
<tr>
<td>20.</td>
<td>G</td>
<td>Katherine got hit.</td>
</tr>
<tr>
<td>21.</td>
<td>NG</td>
<td>John didn't get hit.</td>
</tr>
<tr>
<td>22.</td>
<td>QG</td>
<td>Did Esther get hit?</td>
</tr>
<tr>
<td>23.</td>
<td>NQG</td>
<td>Didn't she get hit?</td>
</tr>
<tr>
<td>24.</td>
<td>WG</td>
<td>Why did he get hit?</td>
</tr>
</tbody>
</table>
screen. When the subject pressed a button, the dot was replaced by one of the sentences, centered equally on either side of the dot, for a duration of 200 milliseconds (1/5 of a second). Following the presentation, the screen was blank for two seconds, and then the dot returned, signifying readiness for the next trial. One-fifth second is sufficiently long to easily see, though not necessarily to process, the entire sentence, but it does not permit more than the one fixation the subject is making when the sentence appears. The room lights were on throughout the experiment, so the screen was visible even when nothing was on it.

The subject sat in a chair 30" distant from the screen. At this distance, a sentence of 10 character-spaces in length was about 2-1/2° in visual angle, the same size that most normal readers see for that length sentence when they hold the text of a book at normal reading distance.

The subject was instructed to report as much of the sentence as he could, saying "blank" for words he could not name. In this sense each report contained as many items (words named or called "blank") as the subject thought appeared in the sentence. Subjects were encouraged to guess, but were not forced to name words of which they were unsure. Thus for the sentence "Girls love pretty dresses" a report might be "Girls love blank --- I think it was 'dolls'". Subjects were given a set of practice sentences before the experiment began.

All responses were both written down by the experimenter and also tape-recorded for reliability checking later. To score the data, the experimenter entered each response into a data file in the same computer. The computer then compared the presented sentence with each response, deleted all words that were literally letter-for-letter matches, and typed out for each sentence those portions of the 28 subjects' responses that were incorrect. Table 3 gives an example of how such a computer output looked for one sentence in the first experiment.

To check the reliability of the immediately transcribed responses the complete data for eight subjects were transcribed by another scorer from the tape recordings. When the new transcripts were compared to the ones made directly, only 30 of the 4000 words were scored differently, that is, less than 1%. For 21 of these 30, it was determined that the tape recording was ambiguous. We decided in these instances to rely on the experimenter doing the on-the-spot transcription. In any event, the overall procedure for scoring the sentence performance seems relatively error free.

The scoring done by the Nova computer used a strict criterion. Each word in the response had to match the presented word perfectly, letter for letter. Still looking at Table 3, it can be seen that some of the errors scored by the computer were not grievous at all. To take account of these we computed by hand a lenient score as well, in which we accepted as "correct"
Table 3
Computer Output of Scoring of a Sentence from Experiment 1
Only errors or omissions are reproduced. C indicates perfect.

<table>
<thead>
<tr>
<th>#56</th>
<th>THEY</th>
<th>VISITED</th>
<th>THEIR</th>
<th>FRIENDS</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.</td>
<td>C</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2.</td>
<td>C</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3.</td>
<td>C</td>
<td>invited</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4.</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>5.</td>
<td>C</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>6.</td>
<td></td>
<td>visit</td>
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<td>8.</td>
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<td>9.</td>
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</tr>
<tr>
<td>10.</td>
<td></td>
<td>twisted</td>
<td>feelings</td>
<td></td>
</tr>
<tr>
<td>11.</td>
<td>C</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12.</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>13.</td>
<td></td>
<td></td>
<td>her</td>
<td></td>
</tr>
<tr>
<td>14.</td>
<td></td>
<td></td>
<td>------</td>
<td>------</td>
</tr>
<tr>
<td>15.</td>
<td></td>
<td>invited</td>
<td></td>
<td></td>
</tr>
<tr>
<td>16.</td>
<td>Tim</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>17.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>18.</td>
<td></td>
<td>invited</td>
<td></td>
<td></td>
</tr>
<tr>
<td>19.</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>20.</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>21.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>22.</td>
<td>Tom</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>23.</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>24.</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>25.</td>
<td></td>
<td></td>
<td>relatives</td>
<td></td>
</tr>
<tr>
<td>26.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>27.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>28.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Totals 7 6 3 15
all mismatches in which a) one proper name was substituted for another of the same sex; b) one pronoun was substituted for another pronoun; c) number or person differed between stimulus and response; d) one adjective was substituted for another adjective, but the same semantic relations among the words was maintained (e.g., 'short' for 'small'); and e) the verb differed in tense or in the number from the stimulus form.

In the first experiment each subject was shown 125 sentences; of these, 107 were simple declarative sentences as previously described. The remaining 18 were "distractor" sentences using different syntactic configurations. Since the location of the sentence on the screen was important, all sentences of a given word length and character-space length were presented in a block together. The order of the 12 blocks was systematically varied from subject to subject so that some began with long sentences, some short, and so forth. Table 4 illustrates the arrangements

<table>
<thead>
<tr>
<th>Number of Words in Sentences</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>10-15</td>
<td>13</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-20</td>
<td>7</td>
<td>11</td>
<td></td>
<td></td>
</tr>
<tr>
<td>21-25</td>
<td>7</td>
<td>11</td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>26-30</td>
<td>8</td>
<td>10</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td>31-35</td>
<td></td>
<td>7</td>
<td>7</td>
<td>14</td>
</tr>
<tr>
<td>36-40</td>
<td></td>
<td></td>
<td>7</td>
<td>7</td>
</tr>
<tr>
<td>27</td>
<td>30</td>
<td>27</td>
<td>23</td>
<td>107</td>
</tr>
</tbody>
</table>

of number of words and number of character-spaces in the 12 blocks. Subjects were told how many words were in each block. Nevertheless, their reports did not always coincide with this number. All 125 sentences were stored in a presentation file in the computer. For each subject, the experimenter simply specified the actual presentation order to be followed by the computer program beforehand, and from that point on the computer ran the experiment.

In the second experiment, 24 kernel sentences were written in each of 24 different syntactic configurations, a total of 576 sentences. (The 24 configurations have been illustrated in
Table 2 above.) For these sentences, the number of character spaces was limited to a range of 18 to 23. The number of words varied from 3 to 6. Each of the 24 subjects was shown 2 of each of the 24 configurations of the kernels, a total of 48 sentences. The data were scored by the same stringent criterion as in the first experiment. The scored output provided by the computer was arranged by configuration. As an example, Table 5 gives the 48 responses, 2 for each of the 24 subjects, for the configuration NQP.

Results of Experiment One

The stringent and the lenient scores provided the same pattern of results throughout all analyses except that the error rates given by the former were between 5 and 10 percent higher. All analyses are based on the percent of words in each sentence incorrectly reported, and use the stringent scoring only.

Table 6 presents the results for the number of words and number of character spaces. As can be seen from the means given in the margins, increasing the number of words in a sentence, or increasing the number of character spaces in a sentence, or both together all increase errors. For the shorter sentences only about one-tenth of the words were incorrectly reported whereas nearly 2/3 of the words in the longest six word sentences were missed.

The cell entries in Table 6 show that, looking down, adding character spaces while holding number of words constant increases errors substantially. However, the converse is not true: holding the number of character spaces constant (that is looking across the table) adding more words to a sentence does not consistently cause more errors. The only dramatic exception to this is for sentences 16 to 20 character spaces in length. Going from three to four words, there is a substantial increase in errors. We shall have more to say about these four word short sentences below.

This general pattern can be seen more clearly in Table 7 where we expanded the data from Table 6 to include the position of each word in each sentence. Adding an extra word does not create additional errors. (Again, the same exception is for each of the word positions in the shortest four word sentences. Two of these sentences were particularly difficult, and if they were removed from the data, there would be no general exception at all. However, we cannot find any linguistic basis for their removal, so we just note this as an exception.) Thus, of the two general variables manipulated, number of words and number of character spaces, only the latter has any substantial effect on the perception of these sentences. This suggests that adding words to a sentence while holding syntactic structure constant leaves the perceptual and linguistic difficulty of processing the sentence unchanged.
Table 5

Computer output scoring of each of 2 responses of 24 subjects for the NQP transformations. First column is sentence number, third column is subject number, '=' means perfect response. Only incorrect or omitted words are indicated.

| 226 | WASN'T HE HURT BY IT?/ | 1 | HER? |
| 345 | WASN'T HE SOLD BY IT?/ | 1 | AT |
| 202 | WASN'T HE HELD BY IT?/ | 2 | HIM |
| 370 | WASN'T HE SHOT BY IT?/ | 2 | --- |
| 178 | WASN'T HE HIDDEN BY IT?/ | 3 | --- |
| 394 | WASN'T HE SLAIN BY IT?/ | 3 | --- |
| 154 | WASN'T HE EATEN BY IT?/ | 4 | --- |
| 418 | WASN'T HE STUCK BY IT?/ | 4 | --- |
| 130 | WASN'T HE DRAWN BY IT?/ | 5 | --- |
| 442 | WASN'T HE TAKEN BY IT?/ | 5 | --- |
| 106 | WASN'T HE LOST BY IT?/ | 6 | --- |
| 466 | WASN'T HE TOLD BY IT?/ | 6 | --- |
| 82 | WASN'T HE FED BY IT?/ | 7 | --- |
| 490 | WASN'T HE WON BY IT?/ | 7 | --- |
| 58 | WASN'T HE SEEN BY IT?/ | 8 | SHE | F** |
| 514 | WASN'T HE FOUND BY IT?/ | 8 | --- | --- |
| 34 | WASN'T HE CUT BY IT?/ | 9 | --- | OFF? |
| 538 | WASN'T HE BORED BY IT?/ | 9 | --- | --- |
| 10 | WASN'T HE HIT BY IT?/ | 10 | --- | --- |
| 562 | WASN'T HE KNOWN BY IT?/ | 10 | --- | --- |
| 562 | WASN'T HE KNOWN BY IT?/ | 11 | --- | --- |
| 10 | WASN'T HE HIT BY IT?/ | 11 | --- | --- |
| 538 | WASN'T HE BORED BY IT?/ | 12 | --- | --- |
| 34 | WASN'T HE CUT BY IT?/ | 12 | --- | --- |
| 514 | WASN'T HE FOUND BY IT?/ | 13 | --- | --- |
| 58 | WASN'T HE SEEN BY IT?/ | 13 | --- | --- |
| 490 | WASN'T HE WON BY IT?/ | 14 | --- | --- |
| 82 | WASN'T HE FED BY IT?/ | 14 | --- | --- |
| 466 | WASN'T HE TOLD BY IT?/ | 15 | --- | --- |
| 106 | WASN'T HE LOST BY IT?/ | 15 | --- | --- |
| 442 | WASN'T HE TAKEN BY IT?/ | 16 | --- | --- |
| 130 | WASN'T HE DRAWN BY IT?/ | 16 | --- | --- |
| 418 | WASN'T HE STUCK BY IT?/ | 17 | --- | --- |
| 154 | WASN'T HE EATEN BY IT?/ | 17 | --- | --- |
| 394 | WASN'T HE SLAIN BY IT?/ | 18 | --- | --- |
| 178 | WASN'T HE HIDDEN BY IT?/ | 18 | --- | --- |
| 370 | WASN'T HE SHOT BY IT?/ | 19 | --- | --- |
| 202 | WASN'T HE HELD BY IT?/ | 19 | --- | --- |
| 346 | WASN'T HE SOLD BY IT?/ | 20 | --- | --- |
| 226 | WASN'T HE HURT BY IT?/ | 20 | --- | --- |
| 322 | WASN'T HE PAID BY IT?/ | 21 | --- | --- |
| 250 | WASN'T HE KEPT BY IT?/ | 21 | --- | --- |
| 298 | WASN'T HE MET BY IT?/ | 22 | --- | --- |
| 274 | WASN'T HE LED BY IT?/ | 22 | --- | --- |
| 274 | WASN'T HE LED BY IT?/ | 23 | --- | --- |
| 298 | WASN'T HE MET BY IT?/ | 23 | --- | --- |
| 250 | WASN'T HE KEPT BY IT?/ | 24 | --- | --- |
| 322 | WASN'T HE PAID BY IT?/ | 24 | --- | --- |
Table 6
Percent Errors in Reporting Words, Broken Down by Number of Words and Number of Character Spaces

<table>
<thead>
<tr>
<th>Number of Words</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>10-15</td>
<td>11.2</td>
<td></td>
<td></td>
<td>11.2</td>
</tr>
<tr>
<td>16-20</td>
<td>16.8</td>
<td>29.2</td>
<td></td>
<td>23.0</td>
</tr>
<tr>
<td>21-25</td>
<td>40.7</td>
<td>34.7</td>
<td>42.8</td>
<td></td>
</tr>
<tr>
<td>26-30</td>
<td>53.9</td>
<td>50.3</td>
<td>52.9</td>
<td>52.4</td>
</tr>
<tr>
<td>31-35</td>
<td>62.9</td>
<td>62.1</td>
<td>62.5</td>
<td></td>
</tr>
<tr>
<td>36-40</td>
<td>62.4</td>
<td>62.4</td>
<td></td>
<td></td>
</tr>
<tr>
<td>22.9</td>
<td>39.2</td>
<td>52.0</td>
<td>59.1</td>
<td>43.3</td>
</tr>
</tbody>
</table>

Table 7
Percent Error in Reporting Words According To Position of Word in Sentence

<table>
<thead>
<tr>
<th>Character Spaces</th>
<th>Number of Words</th>
<th>Word Position</th>
<th>1st</th>
<th>2nd</th>
<th>3rd</th>
<th>4th</th>
<th>5th</th>
<th>6th</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>10-15</td>
<td>3</td>
<td>8.8</td>
<td>6.3</td>
<td>18.4</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>11.2</td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>14.8</td>
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<td></td>
<td></td>
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<td>16.8</td>
</tr>
<tr>
<td>16-20</td>
<td>3</td>
<td>23.3</td>
<td>16.9</td>
<td>25.3</td>
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<td></td>
<td></td>
<td></td>
<td>29.2</td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>42.9</td>
<td>28.1</td>
<td>51.0</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>40.7</td>
</tr>
<tr>
<td>21-25</td>
<td>3</td>
<td>32.1</td>
<td>18.5</td>
<td>30.8</td>
<td>57.5</td>
<td></td>
<td></td>
<td></td>
<td>34.7</td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>37.5</td>
<td>22.8</td>
<td>28.9</td>
<td>58.9</td>
<td>66.1</td>
<td></td>
<td></td>
<td>42.8</td>
</tr>
<tr>
<td>26-30</td>
<td>4</td>
<td>48.2</td>
<td>29.9</td>
<td>53.6</td>
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<td>26.4</td>
<td>34.6</td>
<td>57.1</td>
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</tr>
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<td></td>
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<td>57.5</td>
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<td>75.4</td>
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</tr>
<tr>
<td></td>
<td>6</td>
<td>73.5</td>
<td>52.6</td>
<td>26.0</td>
<td>50.5</td>
<td>78.6</td>
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</tr>
<tr>
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<td>62.2</td>
<td>31.6</td>
<td>32.7</td>
<td>82.1</td>
<td>98.5</td>
<td></td>
<td>62.4</td>
</tr>
</tbody>
</table>
Several other findings are apparent in Table 7. First, greater accuracy is always found for the words in the center of the sentence. This is undoubtedly because the fixation point was located in the center. Further, there is always a substantial asymmetry between the left and right sides of each sentence in which words to the left of center are reported more accurately than words to the right of center. This is true for 23 of 24 comparisons possible in Table 7, regardless of the number of words or the number of character spaces.

A part of speech analysis. Since all sentences have an SVO surface structure, part of speech and word position are usually confounded in these data. A different design would be needed to analyze these separately. However, several comparisons are possible and these will be briefly considered.

Subject nouns. The subject in each sentence tested is either a proper, common or a pronoun. Considering all the sentences, regardless of other variables, pronouns are reported more accurately by half than either common or proper nouns, with the latter two not differing from each other. This could be because pronouns have fewer letters or that there are so few of them that they are more redundant. To distinguish these, we compared pronouns with common and proper nouns matched for number of letters. The two-to-one difference remained. So redundancy seems a more likely explanation for this great advantage. This suggestion is further borne out by analysis of the adjectives.

Adjectives. Two general types of adjectives were used—quantifiers (cardinal numbers, plus FEW, MANY, SOME, ALL, etc.) and common adjectives. Again controlling for position in a sentence and number of letters, quantifiers were easier to report than common adjectives. This is also probably due to their restricted number, making them much more predictable from partial letters, syntactic or semantic information, as compared to the much greater number of possibilities among the common adjectives.

Placement of adjectives. The object noun is always the last word in the sentence. It is either preceded immediately by the verb or has one or more adjectives between it and the verb. Is the report of the object influenced by what precedes it? The answer is clearly yes. The object is reported better in every case in which it is preceded by an adjective than by the verb. This is probably also a redundancy process—the adjective reduces the number of alternatives the object can take.

Verb. Since these sentences are all SVO and the middle is reported better, naturally the verbs are reported best. To demonstrate that this is not due to part of speech but to fixation point, we compared all combinations in which the verb was the word on fixation with those in which the verb was either to the right or the left of fixation. In all comparisons tested, the verb is the best reported word only when centered on fixation. However, this result is partially confounded here with
the insertion and position of adjectives. Even so, we can have some confidence that the superiority of the center word is not due to the confounding of word position with part of speech, but rather with the fixation point placement.

In summary, these results show that a subject can see and report a number of words of a briefly flashed sentence. When syntactic structure is held constant the number of words he can accurately report depends primarily upon the number of character spaces occupied by the sentence, a perceptual variable resulting undoubtedly from the rapid fall-off of visual acuity on either side of the point of fixation. The same visual acuity seems to be the cause of the greater accuracy which occurs for the word nearest fixation. In addition to the visual acuity effects, we found a strong left-to-right asymmetry suggesting that, regardless where the subject is fixated, he either internally processes each sentence from left to right, or the redundancies of the syntax and meaning in the language are such that left hand items are more easily processed. There is already a substantial psychological literature in support of this type of finding (see Haber and Hershenson, 1973).

Results of Experiment 2

Unlike the first experiment, which was scored for errors in reporting each word, we now wished to consider the accuracy with which the subject maintained the syntax of the presented sentence. While we could still score for accuracy of words in first position, or in four-word sentences, these analyses are uninteresting variables given the syntactic manipulations we specified. Therefore, all scoring is for the accuracy with which the transformations were reported. We tested 24 configurations, but will report the results of only 15 of them here. (Of the remaining 9, 5 transformations involved GET, one was an Emphatic, one involved NEVER, and 2 contrasted adjective and adverb placement.)

Scoring presented real difficulties in that merky area where the informant, instead of saying he couldn't report any of the words, reported only one, or created an anomalous sentence. For example, in the statement frame, "Elizabeth [(hit) (told) etc.] Julia," 8 of the 48 responses reported only the first word. A ninth response was "Elizabeth old." These were all scored as Ø. In the Passive, the last two words were missed in 13 responses, resulting in a surface Deleted Passive: "We were [(hit) (told) etc.]" instead of the sentence as presented, "We were [(hit) (told) etc.] by him." These responses were scored as Passive since the transformation was correct.

In principle, we scored for as much information as we could. Thus for the WP question, "Why was he [(hit) (told) etc.] by Joe?" we scored the 2 word response, "Why wasn't" as a WN. We shall return below to the impact of some of these scoring decisions.
The main results will be presented in a series of confusion matrices in which the transformation of the presented sentence is listed across the top of the matrix and the transformation of the subjects' responses is listed down the side of the matrix. The first matrix, Table 8, includes the Statements, Negatives,

Table 8
Confusion Matrix for Accuracy of Reporting the Transformation of a Sentence. Entries of Number of Subjects Who Gave Each Response

<table>
<thead>
<tr>
<th>Transformation of Sentence Presented</th>
<th>S</th>
<th>N</th>
<th>Q</th>
<th>P</th>
<th>QP</th>
<th>NP</th>
<th>NQ</th>
<th>NQP</th>
</tr>
</thead>
<tbody>
<tr>
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<td>3</td>
<td>3</td>
<td>1</td>
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<td>1</td>
<td>46</td>
<td>16</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>4</td>
<td>4</td>
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</table>

Questions, Passives and all their 2- and 3-way combinations. We have arranged the columns by the number of optional transformations contained in the sentence.

As an aside, it might be thought that by placing the Statement configuration first, we are implying that it has fewer transformations than the next three listed: Negative, Question and Passive. Further, when we talk about "simplifying" transformations, as we will below, this might seem to imply an ordering to transformations, with implicit a φ kernel form. Transformational theory has several divergent views on this matter, and we do not wish to take sides on it. Thus, this organization of the data, and comments about simplification or losing transformations, should be taken as descriptive of the data, not derived from theory.
Several observations are apparent from Table 8. First, the configurations containing a single transformation (N, Q or P) are generally easier than those involving two or more. Second, the errors made are heavily dependent upon what was presented. Virtually all the errors are above the diagonal: that is, they represent a "simplification" of the configuration. Of the seventy errors in this matrix, only nine (12%) represent adding a transformation, while 88% of the errors represent deleting one or more of the transformations. The particular errors cluster also. Most striking are the combinations of N with P. For NP nearly all errors (16 out of 20) result from loss of the Passive, while for QNP 14 of the 18 errors lose the Passive.

We also tested the Passive form with the agent deleted. These are shown in Table 9. Clearly, when subjects report these

<table>
<thead>
<tr>
<th>Transformation of Sentence Presented</th>
<th>PD</th>
<th>PDN</th>
<th>PDQ</th>
<th>PDNQ</th>
</tr>
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<tbody>
<tr>
<td>S</td>
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<td>Ø</td>
<td>14</td>
<td>7</td>
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transformations inaccurately, they do so by losing the Passive altogether, not by making other kinds of errors. We also tested for the W question alone and in combination with the Passive as is shown in Table 10. By itself, W is handled quite accurately, but adding a Passive or a Passive Deleted makes the transformation much more difficult. Table 10, like Table 9, shows that Passives are easily lost, but, in this case at least, so is the W marker. Whether this would be true when W is
Table 10
Confusion Matrix for Accuracy of Reporting
WH Question Transformation

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<thead>
<tr>
<th>Transformation of Sentence Presented</th>
<th>W</th>
<th>WP</th>
<th>WPD</th>
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<tbody>
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<td>WN</td>
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<tr>
<td>Ø</td>
<td>1</td>
<td>12</td>
<td>5</td>
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</table>
combined with other transformations, such as Negative, is not known, since we did not test that configuration here. We do, however, have confidence in the conclusion that the Passive transformation, with or without the agent, is a weak appendage, and rather easily gets lopped off when processing demands are high. This finding is in line with the difficulty children have in acquiring the Passive form (Menyuk, 1969; Brown and Hanlon, 1970), and with the data that the Passive is lost from memory when sentences have to be retrieved (Savin and Perchonock, 1965).

We need to point out a bias introduced into these data by our rigorous method of scoring. When the presented sentence was "We were hit by him" and the response was "We were," we scored this as a Statement, since that is all we know from the informant. But the subject could have had a Passive in mind, but could not and therefore did not tell us what the next word was. We rescored all the data noting this ambiguity. In every case, taking whatever ambiguity there was into account, while numbers for the correct score were raised, responses below the diagonal were never added. For example in Table 9, 7 of the 15 Statements given for the PD sentence could have been PD if the subject had given more words. None could have been N or Q or any combination of these. Still, proportionally, the Passive retains its difficulty.

We have presented these results in confusion matrices rather than more traditional tabular form because of the extra power such organization of data provides. In addition to displaying overall level of difficulty, the types of errors can be clearly seen as simplifications in a highly patterned form.

These data taken together present strong evidence in support of the cognitive psycholinguists who have argued that sentences are processed as kernels plus transformation markers. Virtually all errors conform to a model in which a transformation has been "lost" and the resulting sentence contains fewer transformations. It is difficult to say which transformations are most easily lost, since we did not test all possible combinations. Clearly Q and N survive much better than P, while P and W are fragile.

This can be seen in the Venn diagram in Table 11 for the transformations N, Q and P. Most subjects got the single transformation correct, but any combination containing a Passive is markedly reduced in the accuracy with which subjects can report the complete combination.

We have said nothing about the absolute magnitudes of these numbers, because we have not controlled everything necessary in order to do so. Specifically, the fact that the Question transformation is reported more accurately than the Statement, does not imply that English is a VSO language any more than the reverse would have implied an SVO deep structure. To make such a claim, all of the words chosen in each configuration
Table 11
A Venn diagram showing the number of subjects out of 48 who maintained the correct transformation of N, Q, P, and their combinations.
would have to have been counterbalanced and we could not do this in this design.

A number of other comparisons and analyses are available in these data, but we do not have time or space to report them here. The most important set concern whether the particular configuration affects the accuracy with which different parts of the sentence are reported. For example, is the subject or noun equally perceived in a Statement, Question or Negative? Does the swapping of subject and object in changing from a Statement to a Passive affect the report of these two nouns? Does the insertion of DO in questions affect the main verb? Does the Negative marker affect the verb, the Aux, or the Subject? And so forth. If some transformations are harder to process than others, where specifically does this difficulty manifest itself?

The technique employed in the second experiment can be used for other contrasts, and we intend to pursue some of these in the future. For example, we can construct a set of sentences which differ only in their Aux configurations, as a way of extending the results reported by Kypriotaki (1974) on the pattern of acquisition of Aux.

Summary

In conclusion, the first of two experiments have shown that the difficulty of reporting the words in a sentence is generally independent of the number of words (holding syntactic structure constant) but is highly dependent on where the subject is looking during that one glance, how many character spaces are occupied by the sentence, and on a left-to-right internal processing strategy. In the second study we demonstrated that the subjects' accuracy in representing the syntax of the sentence is a function of the type and number of optional transformations contained in the sentence. Further, these results support a model which views word processing as separate from transformation marking.

Acknowledgments: The research for this paper was partially supported by research grants to the first author from NIMH (MH10753), NIE (NE-G-00-3-0090), and the Human Engineering Laboratories of the Department of the Army (DAAD05-71-C-0407).

Our grateful acknowledgments to Ola Selnes, Mark Waldman and Fran Weisberg who helped run the subjects and score some of the data.

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On 'In that'
Leora Herrmann
University of California, Berkeley

People have often spoken vaguely about stretching or limiting the meaning of a word. In English, we use the construction "in that" to do just this. "In that" clauses function as hedges, limiting the domain over which an assertion is held to be true and removing the speaker from responsibility for possible interpretations other than the one explicitly mentioned. Consider, for example, (1)-(3):

(1) He's sick in that he's unable to cope with reality.
(2) Jane's a good swimmer in that she wins every race.
(3) She's a friend in that I've known her for years.

The "in that" clause in each of these sentences specifies how the speaker believes the initial assertion to be true. In this way the "in that" clauses function as hedges, limiting the applicability of the initial assertions. In each case the speaker has left open the possibility that the initial assertion is false with respect to factors which are often considered to be part of its domain. This the speaker of (2) has only asserted that as far as her ability to win races is concerned, Jane is a good swimmer. (2) is felicitous and true even if Jane is a poor swimmer by any other criteria. Notice that (1a), (2a) and (3a), below, are acceptable:

(1a) He's sick in that he's unable to cope with reality, but otherwise he's just fine.
(2a) Jane's a good swimmer in that she wins every race, but her style is terrible.
(3a) She's a friend in that I've known her for years, but we're really not that close.

The unacceptability of (4) is further evidence that "in that" clauses are hedges which only assert the truth of a statement with respect to a specifically mentioned domain:

(4) *Tunas are fish in that they swim so well.

The problem with (4) is that tunas are fish, period. Given what we know about tunas, we cannot call them fish with respect to their swimming ability and leave open the possibility that they are not fish in other ways. (1), (2) and (3) could be followed by a statement which basically contradicted the initial assertion. For example, one could say, "She's a friend in that I've known her for years, but otherwise we're like strangers". One could not construct a parallel sentence for (4) because the initial assertion is so obviously true no matter how we look at it.

We see then that the use of "in that" rests in part on the
ability of the initial assertion to be true in some ways and false in others. A given assertion can be substantiated by a number of facts. We saw from (4) that the "in that" clause must limit the initial assertion by picking out some small subset of these facts and that this could not be done if the initial assertion was obviously true in all respects. Looking now at sentences like (5), we see that there is a second restriction on the use of "in that" clauses. In (5), we see that the initial assertion, "He's sick", cannot be followed by an "in that" clause which contains a fact that embodies the notion of sickness. In other words, (5) is unacceptable because the "in that" clause is automatically associated with the initial assertion:

(5) *He's sick in that he has the measles.

(5) is starred because having the measles embodies the notion of sickness to such an extent that the "in that" clause does not limit the initial assertion. Looking back at (1) we see that measles embodies the notion of sickness in a way that being unable to cope with reality does not. It seems that asserting that X is sick has a core meaning—that X has a sickness. We intuitively feel that measles is always a sickness, whereas being unable to cope with reality is only at times considered to be sick and is not a sickness per se. This distinction is borne out in (6) and (7) as well:

(6) He's sick with the measles.
(7) * He's sick with an inability to cope with reality.

All names of illnesses interact within such "in that" clauses in this way. Thus all sentences in (8) are unacceptable in a context-free environment:

(8) *He's sick in that he has a cold.
*She's sick in that she has the flu.
*He's sick in that he has schizophrenia.

Looking at (9), (10) and (11), we see that "in that" clauses can limit the domain over which a statement is held to be true while stretching the meaning of that statement. Compare (9)–(11):

(9) *He killed Alice in that he murdered her.
(10) He killed Alice in that he did nothing to keep her alive.
(11) He killed Alice in that he was always jealous.

(9) is unacceptable, again, because "he murdered her" does not limit the domain over which the initial assertion is being held to be true. One cannot murder someone without killing them in the process. In (10) and (11), the "in that" clauses are successful hedges. They not only limit the domain over which "he killed Alice" is believed to be true, but also stretch the mean-
ing of this initial assertion. If the speaker only said "he killed Alice", addressee would not normally interpret this to mean that he did nothing to keep her alive.

In (10) and (11), the "in that" clauses are adding something to the meaning of "kill". In these sentences, "he killed her" actually means something like "he indirectly caused her death". In addition to adding to the meaning of a word, "in that" clauses can be used to pick out connoted rather than literal meanings. For example, sentence (12):

(12) Joe's a fish in that he swims so well.

Sentence (12) is obviously not asserting that Joe is, in real life, a fish. On the contrary, it is obvious that he is a human being. (4), which differs from (12) only in that tunas are fish, is unacceptable and involves no stretching. Sentence (12) ascribes to Joe a particular characteristic of fish - good swimming. The "in that" clause limits the domain over which the initial assertion is true to include only swimming ability. It leaves open the possibility that Joe has no other fish-like qualities. Here we see that the "in that" clause is hedging on the initial assertion - the speaker of (12) could not be held responsible for insinuating that Joe smells like a fish, looks like a fish, or has a slimy personality.

"In that" clauses can pick out characteristics that are only metaphorically associated with the initial assertion. In (13), for example, the word "thief" does not mean a person who steals property, but has a more metaphorical meaning:

(13) My butcher is a thief in that he charges $2.00/lb. for ground chuck.

Sentence (14), although similar to (13), is unacceptable except in a limited context:

(14) *My butcher is a thief in that he's selfish and doesn't respect others' rights to property.

There are two differences between the "in that" clauses in (13) and (14). First notice that in (13) the "in that" clause mentions a thief-like action. This in turn brings to mind the connotative meaning of thief - one who gets things by unnatural, immoral means. In (14), however, the "in that" clause actually mentions attributes which are connotatively associated with thieves. In this way, (14) is on the other end of the spectrum from (15), which does not stretch the meaning of thief from its literal one, nor limit the domain over which "those men are thieves" is true:

(15) *Those men are thieves in that they're robbers.
Sentence (14) differs from the acceptable (13) in a second way. Its "in that" clause does not contain an adequate justification for the initial assertion. One cannot call someone a thief because they have personality characteristics in common with thieves. "Thief" describes a performer of certain types of activities, not a type of person. To be called a thief, one must do something thief-like. Thus, we find the response in (16) to be odd, while the response in (17) seems perfectly alright:

(16) A: Bill is selfish and disrespectful of others' rights to property.
B: *What a thief!
(17) A: My butcher sells hamburger for $2.00/lb.
B: What a thief!

Another example of an "in that" clause which stretches the meaning of a word is given in (18):

(18) All of us are students in that we're learning new things all the time.

Strictly speaking, the initial assertion in (18) is false; it is actually implied that those addressed are not in fact students. The "in that" clause in (18), like that in (13), gives us evidence that the initial is true as a metaphorical description. Because the "in that" clause shows what "all of us" do which is similar to what students do, (18) is acceptable. The "in that" clause in (19), however, is unacceptable:

(19) *All of us are students in that we're unsure of our futures.

Although most students are unsure of their futures (probably more than are learning new things all the time), this insecurity is not considered a central characteristic for the determination of whether or not someone is a student. Thus, there seems to be a continuum for studenthood which is roughly as shown in (20):

(20) in that + things synonymous to student or things which embody studenthood.
in that + things which students do, but which don't make one a student.
in that + things only tangentially associated with being a student.
unacceptable acceptable unacceptable
no stretching-----------------too stretched

Sentence (21) fits into the left-hand category while (19) fits into the right-hand category:

(21) *He's a student in that he's a registered student at U.C.
A continuum such as that in (20) could be made for any predicate. Each such continuum would vary in the amount of space between its poles. Words will have more or less flexibility for use with "in that" depending on how many characteristics there are which neither embody that word nor fall only tangentially within their domains. Thus a sentence like (22);

(22) Dad is alive in that he holds a permanent place in our hearts.

is acceptable because it picks out a metaphorical meaning of alive which has validity in terms of our conception of what "alive" normally means even though it implies that dad is in fact dead. (23),

(23) *Dad is alive in that his name (is ) Max.
    (was)

is unacceptable because the "in that" clause does not give ample justification for the claim that dad is truly or metaphorically alive. Sentence (24):

(24) * Dad is alive in that he's living.

is unacceptable because the "in that" clause does not limit the initial assertion, but rather is synonomous with it.

The right situation or context allows one to stretch the meanings of words even farther than they can be stretched in a context-free environment. Compare (25) with (26):

(25) *He's sick in that he's standing there without speaking.
(26) I really think Bob's sick in that he's standing there without speaking while John yells at him, and he never interrupts or tries to defend himself.

(25) is acceptable when found in the right context as provided by (26). This transition from unacceptablility to acceptability is related to two conditions on the use of "in that". These are given in (27):

(27) A. The speaker feels that X is a possible candidate for the domain over which the initial assertion may be true.
    B. The given "in that" clause limits the domain over which the initial assertion could be believed to be true.

Obviously these two are related.

Sentence (26) demonstrates that context allows condition B
to be met by extending the domain of the initial assertion, in this case, "he's sick". In (26), "sick" is stretched to include something like "not behaving in a manner that the speaker desires or approves of". The "in that" clause is picking this meaning out by specifically mentioning such a behavior. Notice, the stretched meaning of "sick" is related to the literal meaning of the word. One cannot extend a word so far that its stretched meaning is completely unrelated to its normal one. In fact, the usage of "sick" in (26) is similar to the use of "sick" in the expression of exasperation, "he's sick!", uttered after hearing that a person did something we do not approve of or would not do ourselves. The concept of abnormality which is associated with illness is thus carried over to behaviors which are alien or un-acceptable to the speaker.

We have seen, then, that "in that" clauses can enlarge the domain of a word, especially in a suitable context. Sentence (28) is similar to (5):

(28) *She's tall in that she's 6'4".

Tallness seems to have too few dimensions to allow us to pick out height as a limiting one. (28) is acceptable, however, when placed in an appropriate context, as in (29):

(29) A: I don't think she's tall.
    B: She's tall in that she's 6'4" and that sure isn't short.

The appearance of doubt that 6'4" implies tall is enough to allow "in that she's 6'4" to occur as a hedge on "she's tall". The context in (29) invites speaker B to use "in that" in this way for two reasons. First, because argumentative discourse forces one to explicitly state the domain over which one believes what one says to be true; and second, because arguments create doubt as to whether or not the speaker's domain is the entire domain as seen from the addressee's point of view.

Now compare (30) and (31):

(30) *There's a guy on our team who'll help us win tonight because he's tall in that he can reach the basket.

(31) A: He's not tall compared to the other players.
    B: Yeah, but he's tall in that he can reach the basket and that's all that counts!

Even though what is significant about the player's height in (30) is his ability to reach the basket, the "in that" clause is not limiting or hedging the initial assertion, "he's tall". In (31), however, there are two kinds of tall which are at issue - relative height and height as it relates to shooting baskets. Speaker B's comment is acceptable because the "in that" clause in (31) limits
the initial assertion to one type of tallness.

"In that" occurs also in sarcastic discourse:

(32) She's a swimmer in that she swims a lot.
(33) He's tall in that he thinks he is.

(32) is acceptable when read sarcastically because condition A (see (27), above) is violated; because swimming a lot does not make one a good swimmer. Using "in that she swims a lot" to justify the initial assertion, "she's a swimmer", implies that no better justification could be found, and therefore, that in the speaker's opinion, the statement is false.

The sarcastic readings with "in that" are made clearer by the addition of "only" as in (34) and (35):

(34) She's a swimmer only in that she swims a lot.
(35) She's sick only in that she thinks she is.

With "only", sentences like (28) are acceptable. (36), providing a context which is the basis for the sarcasm, demonstrates this:

(36) A: Gee, I didn't notice that Bill was particularly tall.
     B: Yeah right. He's only tall in that he's 6'7". I can see how you never noticed.

"Only" can also serve to expand the possible domain of the initial assertion - to make condition A apply. Thus, (37) and not (5) is acceptable:

(37) Dr.: You have no cause to worry. He's sick only in that he has the measles. The other tests were negative.

"In that", then, is a construction which works like a hedge to limit the domain over which an assertion is held to be true, removing the speaker from responsibility for interpretations other than those specifically mentioned. In the process of doing this, "in that" can stretch the meanings of words while limiting their applicability to a fixed and specified domain. In order to account for the occurrence of "in that" clauses in English, a device is needed which can determine the domain over which a given predicate can apply, the degree to which various metaphorical or connotative meanings are central or tangential to the predicate's basic meaning, and the way these things change in context. That "in that" interacts as it does with context once again demonstrates that pragmatics and syntax are not necessarily separate, but that each influences the other to create acceptable speech.
Variation in the Use of 'Front' and 'Back' by Bilingual Speakers
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Columbia University

This paper describes an empirical study of the linguistic encoding of spatial relations in Hausa and Djerma by 346 bilingual students in Niamey, Niger. These students were participating in a model school which had been organized for training teachers in Niger. They ranged

1) from 10 to 20 in age;
2) from classe sixième to classe première in school (seventh grade to twelfth grade in the American system).²

Before entering classe sixième, all had attended six years of primary school in which lessons had been conducted in French. Since French was also the language of instruction at the secondary level of education, all students were functionally bilingual. They were accustomed to a typical pattern of bilingualism in the third world: use of a European language at school, use of an indigenous language at home.

This study attempts to link current concerns in psycholinguistics and sociolinguistics, particularly as they relate to bilingualism:

1) How do opposing cognitive strategies interact with the linguistic choices of a bilingual speaker?
   a) Is there consistent matching of a cognitive strategy with a linguistic system for a bilingual speaker?

```
Strategy1 ----> Language1  
   ↓                  ↓
    Strategy2        Language2
```

b) Or does a single strategy emerge as the dominant one for both languages?

```
Strategy1 ----> Language1  |  Language2
    ↓                  ↓
         Strategy2    ∅
```

If so, which one emerges and why is it selected? Can the selection be predicted from the social background of a particular bilingual individual?

2) If opposing strategies are retained, may a single one play across both languages, matching up with factors in the communicative situation like setting, topic, addressee-addressee relationship, etc.?

A series of tasks were presented to the bilingual students in which they were linguistically constrained

1) to touch the side of an object which they considered to be its 'front' or 'back,' even though that object possessed no intrinsic front or back;

2) to respond with the terms for 'front' and 'back' in Hausa or Djerma in describing relations within various configurations of spatial stimuli (in the initial task they were free to describe the relations with any set of terms).

All the results of this study cannot be described in this preliminary report. Only six tasks will be selected for comparison. First, a red ball with no markings was placed in front of a student. The student was then asked:

*Kwallo yana da gaba da bayaa?
'Does the ball have a front and back?'

91.0% of the students answered,

*A'a.
'No.'

These students were then asked:

*Amma tilas a ba shi gaba da bayaa, ina gabanshi/bayanshi? Taba shi.*
'But if you had to give it a front and back, where's its front/back? Touch it, please.'

Gaba 'front' and bayaa 'back' were alternated throughout the study in this kind of task; such alternation will be consistently represented by gaba/baya 'front/back'.

The 9.0% of the students who answered 'yes' to the initial question were simply asked:
Taɓa gaban/bayan kwallo.
'Touch the front/back of the ball, please.'
The responses to these tasks were as follows:

A. 'Touch the front.'
1. 63.1% touched the far side. \((z=3.89, p<.001)\)
2. 36.9% touched the near side.
B. 'Touch the back.'
1. 60.8% touched the near side. \((z=3.86, p<.001)\)
2. 39.2% touched the far side.

The two patterns of response reflect opposing strategies that the students have encountered in their bilingual experience. An adult speaker of French (or of English, for that matter) typically assumes that any object without an intrinsic front-back is facing in:

This strategy appears to reflect the face-to-face norm of social interaction (Clark:1973) and, in addition, man-machine interaction as well.5

An adult speaker of Hausa and Djerma without western education typically assumes that an object is facing in the same direction he is facing:6

In effect, they give their own spatial orientation to the object; it is as if the object possesses its own deictic center.

These opposing strategies are also evidenced in encoding the relations between two objects, neither possessing an intrinsic front-back. A French adult will typically describe the nearer of two objects as in front of the farther, the farther as in back of the nearer:

\[
\begin{array}{c}
\text{A est devant B.} \\
\text{'}A is in front of B.'
\end{array}
\]

\[
\begin{array}{c}
\text{B est derrière A.} \\
\text{'}B is in back of A.'
\end{array}
\]

No matter which object is taken as the reference point,7 it is assumed to be facing ego. If A serves as reference point, its front is conceived as towards ego and so its back is towards B:

\[
\begin{array}{c}
\text{B est derrière A.} \\
\text{'}B is in back of A.'
\end{array}
\]

If B serves as reference point, it is conceived as facing ego and so A as well:

The Hausa or Djerma adult will typically report the farther object as in front of the nearer, the nearer as in back of the farther:
A yana bayan B.
'A is in back of B.'
B yana gaban A.
'B is in front of A.'

No matter which object is taken as the reference point, it is assumed to be facing in the same direction as ego itself. If A serves as reference point, it is conceived as facing away from ego and hence towards B:

If B serves as reference point, it is conceived as facing away from ego and hence A is at its back:

Four tasks were presented to each bilingual student which forced a choice between the opposing strategies. They are represented in the following matrix:

<table>
<thead>
<tr>
<th>SENSORILY PERCEIVED OBJECTS</th>
<th>CONCEPTUALIZED ENTITIES</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nearer object serving as reference point</td>
<td></td>
</tr>
<tr>
<td>1.</td>
<td>2.</td>
</tr>
<tr>
<td><img src="image1" alt="Diagram" /></td>
<td><img src="image2" alt="Diagram" /></td>
</tr>
<tr>
<td>Farther object serving as reference point</td>
<td></td>
</tr>
<tr>
<td>3.</td>
<td>4.</td>
</tr>
<tr>
<td><img src="image3" alt="Diagram" /></td>
<td><img src="image4" alt="Diagram" /></td>
</tr>
</tbody>
</table>

The sensorily perceived objects were unmarked balls and rocks, the conceptualized entities were Nigerien cities. For each of the four tasks the students were asked in Hausa or Djerma:

O yana gaban ko bayan X?
'Is 0 in front or in back of X?'

The following represents the percentage of students whose responses reflect an underlying strategy that is typical of the one used by a Hausa or Djerma adult without western education (i.e., ![Diagram](image5)):
Nearer object as reference point

1. $\text{Ball} \rightarrow X \rightarrow \text{Rock}$ : 69.0%  
   ($z=8.83$, $p<.001$)

2. $\text{Maradi} \rightarrow X \rightarrow \text{Damagaram}$ : 79.1%  
   ($z=10.30$, $p<.001$)

Farther object as reference point

3. $\text{Rock} \rightarrow X \rightarrow \text{Ball}$ : 73.4%  
   ($z=14.03$, $p<.001$)

4. $\text{Dogon} \rightarrow X \rightarrow \text{Maradi}$ : 64.6%  
   ($z=5.10$, $p<.001$)

If the first two tasks involving a single object are numbered 5 and 6, the percentage of students using the indigenous strategy for each of the six tasks may be represented in a line graph:

It may be seen that the percentage of students using the indigenous strategy is somewhat higher for the tasks involving two objects, particularly if the object to be located is farther away. It has been argued that the farther object may attract the nearer (which serves as reference point), hence increasing the tendency of ego to create a delictic center in the nearer one which parallels its own. This attraction may become even stronger if the two objects are conceived as attracted towards an even farther point:

The projection of a third point may explain the particularly high percentage of students using the $[X \rightarrow O]$-strategy in responding to the geographical task based on a $[X \times O]$-configuration. Since Nigerien society is strongly Muslim, it tends
to project in an easterly direction in space towards Mecca; for example, this field-dependent projection is realized in architectural orientation of mosques and in bodily orientation during the daily ritual of prayers.

\[ \text{Maradi} \rightarrow \text{O} \rightarrow \text{Damagaram} \rightarrow \text{Mecca} \]

Further support for the above hypothesis of 'attraction' may be found in the pattern of response to the \([\text{O} \ x \ o]^{-}\)-configuration, where O was a small rock which was not visible because of the bulk of X, a large basket with no intrinsic front-back.

\[ \text{basket} \quad \text{O} \quad \text{rock} (\text{o not visible}) \]

Only 60.2% of the students made use of the \([\text{O} \rightarrow \text{O} \rightarrow \text{X}]^{-}\)-strategy in responding to this task, suggesting that the attraction of X to O in a \([\text{O} \ x \ o]^{-}\)-configuration depends to some degree, on the visibility of O.9

An initial hypothesis that students might make more use of the imported \([\text{O} \rightarrow \text{O} \rightarrow \text{X}]^{-}\)-strategy when dealing with the geographic tasks (i.e., more school-like tasks) was not supported by the results. Some increased use of this strategy was shown when the reference point was farther (tasks 3 and 4), but it was not significant from a statistical point of view. Whether or not the real-world configuration of stimuli was immediately present appears then not to be of significance in determining the choice of strategy. It should be remembered that the choice of gaba 'front' or baya 'back' is normally constrained by the immediate location of the speaker at the moment of his speech act, whether or not the stimuli are immediately present. In other words, the use of gaba 'front' and baya 'back' is deictically anchored in the immediate location of the speaker in typical acts of speech.10

It is evident from the results of this study that a significant proportion of the bilingual students reflected a \([\text{O} \rightarrow \text{O} \rightarrow \text{X}]^{-}\)-strategy in making responses in an indigenous language, whether in Hausa or in Djerma. The use of this strategy may have been motivated by a number of factors in the communicative situation:

1) setting (a western-style school);
2) task (problem-solving);
3) mood (relatively formal);
4) discourse frame (artificial, a test-like situation);
5) audience (interlocutor was a relatively un-educated native speaker of Hausa and Djerma);
however, educated Nigeriens and Americans manipulated the objects which presented the varying patterns of stimuli.

No doubt, these factors contributed to the significant use of a \([\overrightarrow{x} \rightarrow \overleftarrow{x}]\)-strategy. The communicative environment called for a response based on the strategies entailed in speaking French.

It should be noted, however, that many bilingual Nigeriens (and Nigerians, too, for that matter) claim to use the same strategy for both languages. Indeed, they claim never to have been aware of conflicting strategies. Some insist that the \([\overrightarrow{x} \rightarrow \overleftarrow{x}]\)-strategy is used by native speakers of Hausa and Djerma, just as it is used by native speakers of French and English. Others argue that the \([\overrightarrow{x} \rightarrow \overleftarrow{x}]\)-strategy is used by native speakers of French and English, just as it is used by native speakers of Hausa and Djerma. For example, the \([\overrightarrow{x} \rightarrow \overleftarrow{x}]\)-strategy was consistently reflected in responses made in English to the above tasks on the part of an eighteen-year old Djerma student who had lived with American missionaries in Niamey since she was a young child.11

Thus it is possible that any use of the \([\overrightarrow{x} \rightarrow \overleftarrow{x}]\)-strategy in response to the tasks may have reflected

1) a relatively stable strategy that the bilingual individual uses in responding in Hausa or Djerma, as well as in French:

```
Model_1

\[ \overrightarrow{x} \rightarrow \overleftarrow{x} \]

French Hausa/Djerma

\[ \emptyset \]
```

2) a strategy that was elicited by factors in the communicative situation (in indigenous communicative situations a \([\overrightarrow{x} \rightarrow \overleftarrow{x}]\) -strategy would be used, irrespective of what language was spoken:

```
Model_2

\[ \overrightarrow{x} \rightarrow \overleftarrow{x} \]

Non-indigenous communicative situation

Indigenous communicative situation
```
Since language itself is so crucial a factor in a communicative situation, it would, of course, be reasonable to assume a strong correlation between choice of language and kind of communicative situation. However, as the results of the study indicate, there is no consistent matching of a $\mathcal{F} \rightarrow \mathcal{X} \rightarrow \mathcal{Y}$-strategy with use of Hausa or Djerma in a significant proportion of the responses. This lack of matching suggests that a static model (like Model 3 below) does not adequately account for the cognitive strategies of these bilingual speakers:

![Diagram](image)

Whether model 1 or model 2, or some synthesis of the two, best explains the strategies of bilingual students in Niger awaits further research.

**Notes**

1. The school was organized at the lycée Kasaï by the Niger Peace Corps. I would like to thank the following persons who made this study possible: James Eckstrom, director of the Niger Peace Corps; Bob Vivolo, Sue Rasmussen, and all other Peace Corps volunteers who helped execute the project; Mohammadou Yacouba who spent long hours conducting the interviews; and, most of all, the Nigerien students who responded willingly to the interviews.

2. Age and school class do not correlate as closely in Niger as they do in most western countries.

3. The order in which these six tasks will be discussed does not reflect their order of presentation to the students. Tasks were systematically varied in order except for the one which allowed the students to choose their own terms in describing a spatial relation; this task was always presented initially so that the linguistically free choice would not be affected by the subsequent linguistically constrained choice between *gaba* 'front' and *baya* 'back.'

4. The linguistic cues used in the tasks will be given only in Hausa with an English gloss. The Djerma cues correspond closely to the Hausa ones.

5. Although the face-to-face norm of social interaction may be the ultimate model to which we refer, I suggest that the culturally patterned interaction with machines like typewriters and telephones may be more immediately instrumental
in our assuming that non-fronted objects are facing us. After all, Hausa and Djerma culture did not traditionally provide these face-to-face patterns of man-machine interaction. Furthermore, preliminary investigation seems to indicate that non-industrial cultures, lacking these interaction patterns, typically make use of a strategy like that of the Hausas and Djermas.

6. It is not claimed that these strategies govern the speech of French children or of Hausa and Djerma children. There is some evidence to suggest that competence in using the terms 'front' and 'back' in relation to objects lacking a front-back may be acquired fairly late (Kuczaj and Maratsos: 1974). The testing of pre-school Djerma children seems to indicate that the adult strategy has not been internalized in any systematic way at all.

It should also be noted that an empirical study has not been made of the strategies of adult speakers of Hausa and Djerma without western education. Informal testing, however, provides strong evidence that the $\[ \begin{array}{c} \rightarrow \\ \rightarrow \end{array} \]$-strategy is as typical for these speakers as the $\[ \begin{array}{c} \rightarrow \\ \rightarrow \end{array} \]$-one is for a French or American adult (cf., Harris and Strommen: 1972 for a study of American adults using 'front' and 'back' in relation to non-fronted objects). A study of the strategies of Hausa adults in a rural village near Damagaram, Niger will be carried out in the coming year.

7. The object that is selected as reference point is encoded in complement position.

\[
\begin{array}{c}
\hat{A} \\
\hat{B}
\end{array}
\]

\(A \text{ est devant } B.\) \(A \text{ yana bayan } B.\)

'A is in front of B.' \(A \text{ is in back of } B.\)

Of course, in a question like "Where's A?" B is not encoded at all. Its salient presence in the environment makes such encoding unnecessary. Throughout the rest of the paper X will be used to represent the object taken as reference point, \(\hat{O}\) to represent the object whose relation to X is to be specified.

8. As mentioned earlier, the order of tasks in this study was systematically altered. At one point, it was suggested that the ordering of \(\text{gaba 'front'}\) and \(\text{baya 'back'}\) in the linguistic cue should be altered as well. Such alternation, however, would have violated a natural constraint in language, namely, that the unmarked category precede the marked. Usually, we do not say, 'Is it in back or front?' just as we do not say, 'Is it little or big?' In each instance the unmarked takes precedence, unless, for some reason, we choose to mark the unmarked category.

9. In a natural speech act it is quite likely that the percentage of use of \(a[\begin{array}{c} \rightarrow \\ \rightarrow \end{array}]\)-strategy would be even lower. The use of that strategy in responding to the other tasks probably
led to some kind of carry-over to this task. In a previous publication I had suggested that Hausas would typically describe 0 as baya 'in back' of X in a \[ \overset{\text{Back of}}{ \begin{array}{c} 0 \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} } \text{Front of} \begin{array}{c} \overset{\text{room}}{X} \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} \text{room} \] configuration if 0 were not visible. This suggestion was based on informal interviews with a limited number of informants (Hill: 1974).

As to the effects of field-dependence, it has been suggested that some native speakers of English shift to a \[ \overset{\text{Back of}}{ \begin{array}{c} 0 \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} } \text{Front of} \begin{array}{c} \overset{\text{room}}{X} \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} \text{room} \] strategy if they are facing the intrinsic front of, say, a schoolroom in responding to a \[ \overset{\text{Back of}}{ \begin{array}{c} 0 \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} } \text{Front of} \begin{array}{c} \overset{\text{room}}{X} \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} \text{room} \] configuration.

An informal test that I conducted with adult students at Teachers College suggests that such shifting is very slight. However, more testing needs to be done on field-dependence effects in the use of 'front' and 'back' in encoding spatial relations.

10. The deictic point of reference may, of course, be shifted. For example, the addressee's spatial location is used as the deictic point of reference in the following speech act:

'John, please give me that book in front of you.'

John book speaker

Or it may be shifted to a point in space not at all related to the spatial location of the speaker and hearer at the moment of the speech act. For example, two Hausas conversing in Niamey could say,

Daga Paris Bruxelles tana gaban ko bayan Amsterdam?
'From Paris is Brussels in front or in back of Amsterdam?'

As shown by the gloss, 'front' and 'back' are not naturally used in English (nor French either) to describe the relation of one point to another in geographic space. Any tendency for the bilingual students in Niger to shift to a French strategy in dealing with a school-like task (i.e., relating points in geographic space) may have been counteracted by the fact that devant 'front' and derrière 'back' are not normally used in this way.

11. It is of great interest that the 78 girls included in the study made significantly greater use of the [ \[ \overset{\text{Back of}}{ \begin{array}{c} 0 \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} } \text{Front of} \begin{array}{c} \overset{\text{room}}{X} \\ \rightarrow \rightarrow \rightarrow \rightarrow \end{array} \text{room} \] configuration.
strategy than the boys did on all six of the tasks. In fact, on certain of these tasks more than half the girls made use of this strategy. This shift to the non-indigenous strategy is not surprising since in a strongly Muslim society only girls whose social background reflects western values attend secondary schools. Boys, on the other hand, are drawn from a wider section of society. This correlation between social background and choice of strategy will be presented in detail in a forthcoming publication in Studies in African Linguistics.

It should be noted that a group of eight students, either Nigers métis or Africans from surrounding countries, responded to the same tasks in French, since they spoke neither Hausa nor Djerma. For the most part, the Nigerien métis used the \( \frac{\text{fruits}}{\text{fruits}} \) strategy; however, the Africans from neighboring countries (speakers of Fon, Wolof, etc.) tended to make use of a \( \frac{\text{fruits}}{\text{fruits}} \) strategy. As pointed out in a previous publication, the \( \frac{\text{fruits}}{\text{fruits}} \) strategy appears to be widespread in West Africa (Hill: 1974). More research needs to be done on the choice of these strategies by West Africans when speaking English or French.

References


On Incorporating Grassmann's Law Into Sanskrit Phonology

James E. Hoard

University of Oregon

1. Within the generative phonological framework a number of proposals have been advanced to incorporate Grassmann's Law into Sanskrit phonology in a natural way. The proposals of Kiparsky (1965) and Zwicky (1965) have been criticized by Anderson (1970) for, among other things, a failure to characterize exceptions in a straightforward manner. Anderson's solution to the problem, which involves a scheme for local reordering of rules, has recently been criticized by Phelps and Brame (1973) who themselves reject local reordering and offer two additional solutions preserving linear rule order. However, neither of their solutions seems to me to be acceptable since both involve theoretical claims that are very questionable.

At the meeting of the Western Conference on Linguistics held at Victoria, B.C. in October, 1973, I presented a new solution to the problem which, like that of Phelps and Brame (and of Zwicky), preserved linear ordering but which used rather simple rules along the lines of Anderson and Kiparsky and took into account some of the sandhi phenomena in Sanskrit. Since the proposal given in this paper differs only in minor ways from the earlier one of 1973, I have retained the same title.

In response to the analysis that I gave at the WECOL meeting, Phelps (to appear) has abandoned the proposals given in Phelps and Brame (1973), adopts the suggestion that Sanskrit sandhi phenomena play a crucial role in the solution, and advances a new proposal which accounts for a range of data well beyond that previously considered in the recent literature. Sag (1974) has also offered a solution to this problem. I take up both of these proposals below in §7.

2. Anderson (1970) claims that local reordering of Grassmann's Law (GL) must be invoked to derive correctly Sanskrit forms which involve also Bartholomae's Law (BL) and the rule for deaspiration of consonants (DaC). Anderson follows Kiparsky in formulating GL; except for changes in feature names the rule formulations are identical. Anderson formulates GL as follows:

\[
[+\text{cons}] \rightarrow [-\text{asp}] / \_\_ [+\text{seg}]_0 \left[ +\text{cons} \atop +\text{asp} \atop +\text{Root} \right]
\]

The application of GL in reduplicated forms may be seen in (first and third person singular) perfect forms of such roots as phal 'burst' and dhāv 'run/wash'. The (third person singular) present forms show the underlying aspirate initial of these roots.
Anderson formulates his rule for the deaspiration of consonants (DaC) as:

\[ [+\text{cons}] \rightarrow [-\text{asp}] / \{ +\text{obstr} \} \]

This formulation is equivalent to both Zwicky and Kiparsky's rules for deaspiration. The application of DaC may be seen in the nominative singular and instrumental plural forms of such roots as -stubb 'raising' and -bhudh 'waking'. The accusative singular forms show the underlying aspirate finals of these roots. The final t of bhut is due to a rule of obstruent devoicing; this devoicing rule is left out of account here as the rule does not bear on any of the arguments presented.

<table>
<thead>
<tr>
<th>root</th>
<th>nom. sg.</th>
<th>acc. sg.</th>
<th>instr. pl.</th>
</tr>
</thead>
<tbody>
<tr>
<td>/stubb/</td>
<td>-stup</td>
<td>-stubham</td>
<td>-stubbhis</td>
</tr>
<tr>
<td>/bhudh/</td>
<td>-bhum</td>
<td>-budham</td>
<td>-bhudbhis</td>
</tr>
</tbody>
</table>

That DaC precedes GL may be seen in various of the forms involving diastrate roots (those roots whose underlying forms begin and end with an aspirate). The roots and endings are here separated by a hyphen. The o of bhodh is the guna- grade of the root.

<table>
<thead>
<tr>
<th>DaC</th>
<th>phonetic</th>
</tr>
</thead>
<tbody>
<tr>
<td>/bhudh/</td>
<td>bhud</td>
</tr>
<tr>
<td>/bhudh-am/</td>
<td>budham</td>
</tr>
<tr>
<td>/bhodh-syati/</td>
<td>bhotsyati</td>
</tr>
<tr>
<td>/bhudh-bhis/</td>
<td>bhudbhis</td>
</tr>
<tr>
<td>GL</td>
<td>phonetic</td>
</tr>
<tr>
<td></td>
<td>bhut</td>
</tr>
<tr>
<td></td>
<td>budham</td>
</tr>
<tr>
<td></td>
<td>bhotsyati</td>
</tr>
<tr>
<td></td>
<td>bhudbhis</td>
</tr>
</tbody>
</table>

Anderson formulates Bartholomae's Law (HL) as:

\[ [-\text{cont}] \rightarrow \begin{cases} [+\text{voi}] \\ [+\text{voi}] \end{cases} \]

\[ +\text{asp} / [-\text{voi}] \]

Except for changes in feature names, Anderson's formulation of HL is the same as Kiparsky's. For roots like ru(n)dh 'obstruct' and labh 'catch', the application of HL before suffixes like -tha (second plural active) and -ta (past participle) gives forms such as:

<table>
<thead>
<tr>
<th>root</th>
<th>2nd pl. active</th>
<th>past participle</th>
</tr>
</thead>
<tbody>
<tr>
<td>ru(n)dh</td>
<td>runddda</td>
<td>ruddha</td>
</tr>
<tr>
<td>labh</td>
<td>labdha</td>
<td>labdha</td>
</tr>
</tbody>
</table>

It is easy to see that (as Anderson has stated them) HL precedes DaC in forms such as the following:
Anderson argues that the order BL-DaC is the 'unmarked' order for the application of these rules (it is counter-bleeding) and that the order GL-BL is extrinsic. This establishes the order GL-BL-DaC when all three rules apply. When BL does not apply, however, Anderson argues that DaC-GL, despite the fact that it is a bleeding order, is the 'unmarked' order of these two rules. His reasoning is that the class of diaspirate roots would disappear altogether with the order GL-DaC, an order which would otherwise be the 'unmarked' one. According to Anderson, GL would also be lost as a phonological rule and would become merely a fact about reduplication.

3. Phelps and Brame (1973) consider Anderson's local ordering principles in some detail and conclude that Anderson's procedures for determining the order in which n rules apply to any given form result in n! possible rule sequences to examine. Moreover, each of these n! orders must be compared with the n! possible orders for every other form in the given language to see that rule loss will not be the result of particular orders. Phelps and Brame conclude that "the inclusion of rule loss in the scale of rule order markedness has questionable implications for language acquisition and language change, and ... dramatically alters the theory of local ordering in such a way as to render the theory virtually unconstrained (399)."

Phelps and Brame propose two solutions to the problem of incorporating GL into Sanskrit phonology. One of them involves combining GL with BL. GL and BL can be combined in a transformational rule GL-BL:
This gives derivations for diaspire roots as follows:

| /bhudh-ta/ | /bhudh-am/ | /bhudh/ | /bhudh-bhis/ |
| GL | GL | GL | GL |
| budh+dha | budh-am | buhd | buhd-bhis |
| DaC | DaC | DaC | DaC |
| bud+dha | | | |

As Phelps and Brame admit, the putative rule 'GL and BL' makes a very questionable use of angle brackets and also raises questions about exceptions to only one expansion of a rule, since some forms undergo GL but not BL.

Phelps and Brame offer a second solution with a revised BL which not only aspirates and voices the t and th endings but assigns them to the category 'Root'. The revised rule is BL':

\[
\begin{align*}
\text{[-voice]} \rightarrow \\
\text{[+voice]} \\
\text{[+asp]} \\
\text{[+Root]} \\
\end{align*}
\]

If DaC and GL follow BL' (Phelps and Brame 'combine' DaC and GL with braces), then we have derivations such as the following:

| /bhudh-ta/ | /bhudh-am/ | /bhudh/ | /bhudh-bhis/ |
| GL' | GL' | GL' | GL' |
| buhdh-a | buhdh-a | buhd | buhd-bhis |

While BL' 'works' in the limited sense that correct forms are derived, it seems to me that BL' is totally unacceptable from a theoretical standpoint. The theoretical principle I have in mind can be stated simply: Categories are inviolable. To be able to claim that an ending or a part of one is a 'root' is an innovation that leaves phonological theory utterly incoherent since derivations and P-rules are no longer constrained by the morphological facts of a given language. If categories are inviolable, then one cannot move boundaries about and/or change morphological labels in mid-derivation. Categories and boundaries simply have to be 'natural' or there is no way whatever to relate morphology to surface phonetics in an explanatory fashion.

Incidentally, the inviolability-of-categories principle also includes the notion that distinctive feature definitions assign values to segments naturally and automatically. It follows, for example, that [p] cannot (arbitrarily) be assigned to the category [-cont]. We can, of course, change [p] to [f] by a P-rule. If so, then the feature [+cont] will be automatically assigned to the new segment [f]. Assigning [p] to the category [+cont] is no more unnatural than assigning the first segment of a suffix to the category [+Root]. In brief, the categories (however defined) of morphology and phonetics are constants so far as the phonology is concerned. The only items affected by P-rules are segments and classes of segments. Morphology and phonetics are then the bases
for the phonological theory.

4. At this point, it is perhaps well to consider how Whitney (1889) formulates equivalents to DāC, BL, and GL. On permitted final consonants, Whitney states: "141. Of the non-nasal mutes, only the first in each series, the non-aspirate surd, is allowed; the others---sandpir aspirate, and both sonants---whenever they would etymologically occur, are converted into this." On deaspiration, he states: "153. As aspirate mute is changed to a non-aspirate before another mute or before a sibilant..." Whitney gives Bartholomae's Law clearly: "160. If, however, a final sonant aspirate of a root is followed by...t or...th of an ending, the assimilation is in the other direction, or progressive: the combination is made sonant, and the aspiration of the final (lost according to 153, above) is transferred to the initial of the ending." Whitney recognizes Grassmann's Law as a part of reduplication: "590a. A non-aspirate is substituted in reduplication for an aspirate..."

At several places in his grammar, Whitney comments on the diaspirate roots: "155. In a few roots, when a final sonant aspirate...loses its aspiration, the initial sonant consonant becomes aspirate... 141a. In a few roots, when their final (sonant aspirate) thus loses its aspiration [by 141, JEH], the original sonant aspiration of the initial reappears... 160b. In this combination [where BL applies, JEH], as the sonant aspiration is not lost but transferred, the restoration of the initial aspiration (155) does not take place..."

Whitney's section 141 is a statement of external sandhi; in current terminology, we would say that only voiceless unaspirate stops occur before #. On the matter of internal versus external sandhi Whitney says: "109. The rules of combination (sandhi putting together) are in some respects different, according as they apply---a. to the internal make-up of a word, by the addition of derivative and inflectional endings to roots and stems; b. to the more external putting together of stems to make compound stems, and yet looser and more accidental collocation of words in the sentence... 111a. Moreover, before case-endings beginning with bh and s (namely, bhvām, bhīs, bhvās, su), the treatment of the finals of stems is in general the same as in the combinations of words (pada) with one another---whence those endings are sometimes called pada-endings, and the cases they form are known as pada-cases."

5. While Whitney does state the facts of the matter, we can note first that the deaspiration process he gives in section 153 is unnecessary since all cases of deaspiration are actually already accounted for in section 141. Section 141 accounts for deaspiration before external suffixes like bhīs (and it is external according to Whitney, 111a). Similarly, deaspiration before external suffixes beginning with s are also accounted for by 141. In fact the only deaspiration rule that we need is the following:

Deaspiration of Consonants (DC)

[+asp] → [-asp]/___#
At first glance it seems that one cannot posit external sandhi before s suffixes because of the sandhi rule, usually considered to be internal, that retroflexes s after vowels other than a (and after k, r, l). But this sandhi rule also applies over # (as in agni#su = agunsu 'fire', loc. pl. This retroflexion rule applies, then, over either a + or a # formative juncture (see Whitney, §§180-185) and cannot bear in any way on the formulation of a deaspiration rule.

To prevent misunderstanding about the application of rules across morphological boundaries, it should be pointed out that there are actually three different situations which obtain when two formatives are concatenated. 1) An internal + boundary separates the formatives; 2) an external # boundary separates the formatives; 3) two external sharp boundaries separate the formatives. Both 1) and 2) are intraword environments and the symbols + and # are merely a formal way of showing that formatives may be 'tightly' (+) or 'loosely' (#) concatenated. Case 3) is the interword environment and the ## notation indicates formally that an external juncture is associated with each of two concatenated formatives.

The rule that retroflexes s, as in agunsu, applies over either + or #, but not over ##. That is to say, retroflexion of s occurs over an internal or an external juncture, but not between words.

The terminology involving +, #, and ## (now fairly widespread in phonology) must be distinguished from the terminology used by Whitney and others for Sanskrit. For Whitney, both the # and the ## environments are 'external sandhi' (§109b) and he apparently does not recognize a distinction between the interword ## and the intraword # environments so far as sandhi rules are concerned.

GL can be formulated in an even more general way than that of Kiparsky and Anderson.

Grassmann's Law (GL):\[
\begin{align*}
\text{-syl} & \rightarrow [-\text{asp}]/\underline{\text{X}} \\
\text{+cons} & \\
\text{+obst} & \\
\end{align*}
\]

The formulation given here differs from Anderson's and Kiparsky's in that [+Root] is not specified in the environment. GL is stated as in Jensen (1974:682) and the X in the environment indicates that only 'irrelevant' segments may intervene between the two aspirate obstruents. The 'irrelevant' segments according to Jensen's 'relevancy condition', are the non-obstruents. We should also note that, by convention, # is not included in X, but that + is.

Since GL is right-handed (i.e. the crucial environment is to the right of the environment bar) and GL is self-bleeding, it applies iteratively from left to right. For example, GL applies to the reduplicated perfect form /bhabhandh+a/ as follows:
GL - 1st application  babhand+h+a/
GL - 2nd application  baband+h+a

The root specification given in earlier formulations of GL has been dropped because, in fact, the imperative suffix /dhi/ can be an environment for GL. A relevant example is bhindhi 'cleave' (2nd sing. imp.), cited in Monier-Williams (1899:756), apparently from underlying /bhi+n+d+dhi/. The d of the root must be deleted before GL applies. If the d of the root were not deleted we would expect bhinndhi since the d of the root is excluded from X and GL would not apply. The form bhindhi is given by Sag (1974: 599). I have, however, been unable to find a citation in any of the dictionaries at my disposal.

Another example of GL application involving this suffix is jahi, the imp. form of /ghan/ 'kill, slay' from /ghn+dhi/. The derivation apparently involves vocalization of n before a consonantal affix as well as palatalization and deaspiration (cf. ghnanti, 3rd pl. pres. indic.). The dh of dhi usually becomes h before vowels. Here the dh must not become h before GL has applied.

The form bodhi, 2nd sing. imp. of /bhudh/ is somewhat more problematical. We cannot tell by inspection whether the surface dh is the dh of the root or the dh of the imperative marker. The underlying form, with guna of the root, should be /bhodh+dhi/.

Now it simply does not matter for present purposes whether the dh of the root or the dh of the suffix is deleted. In either event, GL will apply to deaspirate the initial consonant.

BL can be formulated in approximately the way suggested by Whitney, section 160. The second segment need not be restricted to voiceless stops. BL is an internal sandhi rule.

Bartholomae's Law (BL)

\[
\begin{array}{c}
+\text{voice} \\
+\text{asp} \\
+\text{obst}
\end{array} + \begin{array}{c}
-\text{cont} \\
-\text{obst}
\end{array} \rightarrow \begin{array}{c}
+\text{voice} \\
-\text{asp}
\end{array} + \begin{array}{c}
+\text{voice} \\
+\text{asp}
\end{array}
\]

For a deaspirate root like /bhudh/, we now have derivations which preserve linear ordering and which do not require rules utilize questionable theoretical devices. DC must apply before GL in a bleeding order. The order GL-BL is a counter-bleeding order.

/bhudh+ta/  /bhudh+am/  /bhudh/  /bhodh#syati/  /bhudh#bhis/
DC         bhud  bhud#syati  bhud#bhis
GL         budh+ta  budh+am  ---  ---  ---
BL         bud+dha   budham   bhut    bhotyaati  bhudbhis
buddha

The rule order DC-GL-BL is the same as Kiparsky’s. However, his solution differs from the one proposed here in several ways:
1) Kiparsky formulates BL (as does Anderson) so that only the progressive aspiration and voicing is accounted for; a separate rule of regressive deaspiration must somehow apply to give the final result. 2) To achieve regressive deaspiration, Kiparsky invokes a cycle. His version of DC, which deaspirates consonants before s, dh, and #, can then apply on a second pass through the rules to produce correct outputs. He will derive buddha as follows:

1st cycle
DaC /bhudh+ta/ GL budh+ta BL budh+dha
2nd cycle DaC bud+dha

There is, however, no reason to believe that deaspiration applies anywhere except in the external # environment, at least so far as these forms are concerned. Hence, there is no reason to suppose that BL should be effected piecemeal rather than as a single unified process. There is, then, no reason to cycle the rules and, in fact, since deaspiration is apparently restricted to #, it would not in any event reapply to BL forms (which all have internal sandhi).

There are a few cases of exceptional external sandhi. Whitney comments: "667. ...In combination with a final t or th, the final dh of dadh does not follow the special rule of combination of a final sonant aspirate (becoming ddh with a t or th: 160) but-- as also before s and dhv-- the more general rules of aspirate and of surd and sonant combination..." The "more general rules" are those of external sandhi. Sample derivations are as follows:

/dhad#tha/ /dhad#ta/ /dhad+ã+mi/ DC

/ /dhad#tha /dhad#ta /dadh+ã+mi
GL

BL dhattha dhatta dadhãmi

Although not mentioned by Whitney in his section 111a and barely touched on in 667, the endings dhve and dhvam (second plural middle suffixes) are external. With the root duh 'to milk' (underlying /dhugh/) and dhã 'put, do' (reduplicated stem /dhadh/) sample derivations are as follows:

/dhugh#dhev/ /dhugh#dhvam/ /dhugh+thãm/ /dhadh#dhvam/ DC
dhug#dhev dhug#dhvam dugh+thãm dhad#dhvam

GL

BL

For reduplicated forms with bhr 'bear' and bhudh we have such derivations as:
The innovations I have suggested with regard to the sandhi of certain Sanskrit affixes are modest. The declensional endings $bhvam$, $bhis$, $bhvas$, and $su$ have traditionally been considered to be preceded by $\#$. I have suggested that $sya$, $dhve$, and $dhvam$ from the conjugal system are also preceded by $\#$, that $dhi$ is normally preceded by $+$ (as in Phelps, to appear), and that the conjugal markers $tha$ and $ta$ are normally preceded by $+$ but are exceptionally preceded by $\#$ (for $dhva$ and $bhr$, for example). None of the sandhi rules stated in the literature appear to go against this analysis.3

It is perhaps necessary to point out that sandhi phenomena are widespread. Not only does Sanskrit show a difference between internal and external sandhi, so do such diverse languages as English, the Salish languages, and Quileute. It seems to me not unlikely that every language with a well developed morphology has two ways of joining affixes and roots. Moreover, it is clear why there are two ways of joining formatives: external sandhi tends to preserve the discreteness of the items being joined; internal sandhi leads to words which, although morphologically complex, have more nearly the phonological structure of unit morphemes. It is not at all surprising to find the contrary needs of communicability and pronouncability reflected in the way formatives are joined.

On the other hand, it would be very surprising if local ordering and the cycle, two recently suggested theoretical innovations that come immediately to mind, had any basis in fact. Neither enhances communicability or pronouncability (except by accident) and both place severe burdens on language learners unless they are very much predisposed toward communication systems with strange and apparently useless properties.

However, we must examine the solution given above in light of some recent proposals for eliminating extrinsic ordering (Kisseberth (1972), Koutsoudas, Sanders, and Noll (1971)). Given that DC, GL, and BL are formulated as I have stated them above, then the rules must be extrinsically ordered, in the order DC-GL-BL, or else we must find some very subtle principle which selects DC-GL, a bleeding order, but also GL-BL, a counter-bleeding order. It is not obvious that there is any such principle. As I understand Kisseberth's proposal, it will select the order DC-GL, but not GL-BL.4 Simultaneous application of DC and GL, as advocated by Koutsoudas, Sanders, and Noll, will give wrong outputs. This can be remedied by building in the converse of the environment of DC into GL so that $[+asp] \rightarrow [asp]/\_X[+asp]\_\#$. DC and GL are now unordered, but at the price of complicating GL (and, in fact, repeating in GL information already in DC).

Historically, extrinsic ordering is well supported by the
fact that rules arise sequentially at particular times and places. There is no reason to assume that only certain rule relations (feeding, counter-bleeding, etc.) are permitted to occur as sequential historical events or that re-ordering must occur if certain rule relationships arise as a result of rule addition. In the Sanskrit case there is every reason to think that DC was introduced before GL and that the introduction of BL followed GL; that is, there is every reason to think that the synchronic and historical orders are the same; namely, DC-GL-BL (see Kiparsky 1965:66ff. for discussion). I am not arguing that the synchronic order of rules must always be in agreement with the historical order of rule addition. I am merely noting that this seems to be true in the present instance and that one would expect the historical order of rule addition to be maintained for the most part.

In the question of rule ordering, observations and conclusions concerning rule markedness are not at issue. It is perfectly possible to understand that some rule orders are more natural than others without being able to predict whether a particular order (marked or unmarked) will occur. Inabilities to predict the order of rule application leaves us with no alternative but to invoke extrinsic ordering or some equivalent device to establish proper priority statements. Since there seems to me to be no well motivated way to eliminate extrinsic ordering or its equivalent in the present instance, I prefer to maintain the simple rules I have formulated together with extrinsic order than to complicate the rules and/or to advance ad hoc ordering principles.

7. Phelps' new solution to the diapirate problem (Phelps, to appear) produces correct outputs, and does so without appeal to fantastical theoretical devices like cycles and local recording, but it is not without difficulties. Her solution depends on five important rules, which apply in the following order:

1) BL, stated as in Anderson (1970)
2) Regressive Voicing Assimilation (RVA)

\[ [+\text{obst}] \rightarrow [\alpha\text{voice}] /\underline{(\#)} [\alpha\text{voice}] \]

3) External deaspiration (ED) \([=\text{DC}]\)

4) GL

\[ [+\text{asp}] \rightarrow [+\text{asp}] /\underline{[+-\text{seg}]} _{1} [+\text{asp} +\text{Root} \alpha\text{voice}] \]

5) Internal Deaspiration

\[ [+\text{asp}] \rightarrow [+\text{asp}] /\underline{[+\text{obst}]} \]

Rule 2), RVA, is required in anyone's analysis of Sanskrit. In the analysis given above, RVA does not figure in the diapirate problem. For Phelps, however, it is crucial that RVA apply before GL, for RVA bleeds her version of GL, now restricted to apply only in an \([\alpha\text{voice}]\) environment. To derive dhatta, Phelps requires
that dhadh\textsuperscript{+}ta be an exception to BL. Thus, BL is skipped; but RVA applies and gives dhath\textsuperscript{+}ta. GL cannot now apply since RVA has bled GL and the [\textit{q}voice] requirement is not met. Finally, Internal De-aspiration gives dhat\textsuperscript{+}ta.

The formulation of GL with [\textit{q}voice] is not well motivated. There are no disaspirate forms in Sanskrit which differ in voicing. Such forms are prohibited by morpheme structure constraints and are not produced by any morphological reduplication process. Independent evidence for including [\textit{q}voice] in the statement of GL is lacking because there are no roots like *bhath which would simply go through the rules unaltered (say before an internal suffix beginning with a vowel) nor any reduplicated forms like *tha+dhan, which would also emerge unscathed. While ED (=DC) is independently well motivated and bleeds the independently well motivated version of GL that contains no \textit{a}-variables, one is led to conclude that adding [\textit{q}voice] to GL is merely a device which allows the independently well motivated rule RVA to bleed a now parasitic version of GL.

Complicating GL so that RVA bleeds it may seem an innocuous strategy. But it is not. On the contrary, it must be a basic tenet of (natural) phonology that each phonological rule of a language be independently motivated in its entirety; that is, each rule must be stated in the most general way. Only if each rule is independently motivated in its entirety can questions concerning how rules interact in a phonology have empirical significance. And only if empirically significant answers to these questions are possible can we hope ever to ascertain the organization of a phonology. But this completes the argument, since a fundamental goal of linguistics is to ascertain how phonologies are organized. In brief, if we wish in principle ever to say how a phonology is organized, we must have the basic tenet that rules are independently motivated in their entirety. Moreover, the whole enterprise of determining what constitutes a natural process or rule is also ultimately pointless in the absence of this tenet.

It seems to me, then, that, because GL has been 'degeneralized' to allow RVA to bleed it, Phelps' solution does not meet this very basic criterion of phonologies.

Incidentally, the argument applies with equal force against the Unordered Rule Hypothesis. As I mentioned above, complicating GL with the unnecessary specification \# only appears to save the Unordered Rule Hypothesis. The cost of such a strategy is to deprive the hypothesis of any possible empirical significance.

In my analysis, unlike that of Phelps, I posit a lexical exception feature associated with dhadh. The exception feature is (informally): + \rightarrow \#/[t,th]. All of the t and th initial suffixes normally have internal sandhi. I am claiming, then, that dhadh is an exception to this generalization. This stem has perfectly regular sandhi elsewhere. The expected \# occurs before se, dvhe, etc. and the expected + occurs before sonorant-initial suffixes and before dhi.
Phelps' apparently disagrees with the claim that an exception feature can have an environment which is partly phonological and wishes to "disallow the insertion of # before t and th, a phonological context." However, exception features are not phonological readjustment rules of the type I argued against in Hoard (1972:134). For exception features the environment is always morphological (in part or entirely) since exception features apply only to specific formats.

For example, in English the past and past participle marker d is normally #d. But keep, hear, deal, leave and a number of other verbs have +d. The lexical notes are: keep: # → +d; hear: # → +d; deal: # → +d; leave: # → +d. With internal sandhi, keep, hear, deal, and leave are subject to Cluster Shortening of their vowels. Deal and leave are also subject to the rule of Cluster Naturalization. These two rules result in the surface forms: kept, heard, dealt, and left. Both of these rules are needed independently of internal sandhi verb forms to derive such words as stealth, width, and fifth. Positing irregular internal sandhi for keep, hear, deal, leave results in a straightforward analysis. (For details, see Sloat and Hoard (1973).) If we do not utilize lexical notes specifying exceptional sandhi, then we will be forced to postulate that there are two different and quite unrelated English past and past participle suffixes for weak verbs: #d and +d. Hence, given what is known about exceptional sandhi in English, positing an irregular # in Sanskrit before t and th endings as a lexical note of dhadh is quite acceptable.

The treatment of the entire problem advanced by Sag (1974) does not involve incorporating Grassmann's Law into Sanskrit as do all the others. His approach is to reject GL as a synchronic rule and simply to cast Pāṇini's rules for the Sanskrit data in question into a generative phonological framework. There is, however, a fundamental objection to Sag's analysis: The rules he gives as Regressive Aspiration Assimilation and Deaspiration (p.604) are almost totally unnatural and use the braces notation to include dissimilar items in what are actually discrete and incommensurate environments. Thus, the rules he gives force us to give up any generalizations about the lack of diaspares within a root in the surface phonetics of Sanskrit. Moreover, after following Pāṇini in assuming that there are no underlying diaspire roots, it is curious that Sag's rule of Regressive Aspiration Assimilation has only the effect of creating diaspire roots in three separate environments as intermediate forms. This is a 'false step' in the sense of Zwycky (1974), but hardly a very good one. Since later rules simply conspire to undo all the diaspares created by the unnatural rule of Regressive Aspiration Assimilation, Sag's proposal serves, then, to reinforce the proposition that Grassmann's Law is a synchronic rule of Sanskrit and that there are underlying diaspares.

8. I have tried to show in this paper that very general and in-
dependently motivated formulations of Grassmann's Law, Bartholomae's Law, and the rule for Deaspiration of Consonants are the correct synchronic rules of Sanskrit. If Sanskrit sandhi phenomena are taken into account, then the linear rule order DC-GL-BL accounts for the data with no theoretical innovations.

Footnotes

1. This statement is perhaps the source of Phelps and Brame's formulation of BL'. They 'transfer' the feature [+Root], however, as well as aspiration.

2. Kiparsky (1965:64) inadvertently omits bh from the environment of his rule.

3. There are, however, certain consonant alternations in Sanskrit which need to be mentioned just because it might be supposed that they have some bearing on the determination of + or # before these suffixes. For instance, root final ś is usually changed to ṭ before dh conjugation markers and before declensional bhvām, bhīs, bhīyas, au, and nominative s; but ś becomes k before the s of conjugation markers. From viś 'settler' we have, then, viś nom., viṣu loc. pl., viṣta 'entered', and veṣyam 'I shall enter'; and from dvīś 'hate' we have dvēṣṭi 'he hates', dvēkṣi 'thou hastest' dvīḍṭhe 'you hate', dvīḍhī 'hate' imp., and advēṭ 'he hated' imperf. Sometimes, however, k appears instead of ṭ. So we find dik as the nom. sing. of dīś 'cardinal point'. The appearance of k in final position in dik guarantees that there is no substance to the claim that ṭ occurs before # and certain consonants while k occurs before + and certain (other) consonants. It appears that these consonant alternations have nothing whatever to do with the sandhi of the affixes in question but are conditioned solely by the presence of a given affix (or by the initial consonant of an affix).

4. The non-opaque order BL-GL which is selected requires a global rule to the effect that GL applies (not only in its stated environment) but just in case BL has applied. The global rule cannot be formulated in such a way that GL applies to any string whose ancestor had an aspirate stop since, in that case, GL will apply erroneously also to bhut and bhotsyati.

References


THE PERCEPTION OF CONTOUR TONES

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0. Introduction

In this paper, I shall address myself to the perception of speech-like acoustic signals with changing frequency contours. The paper is divided into three parts: 1) a brief discussion of the relationship between pitch and fundamental frequency; 2) the presentation of my data from a perception experiment using synthesized speech; and 3) the linguistic relevance of these data for a proper understanding of tone languages.

1. Pitch and Frequency

Pitch and frequency are often used interchangeably in the linguistic literature. In fact, however, they refer to two different entities and there is not a one to one relationship between them. Pitch is a psychological attribute of tone, referring to the perception of a stimulus by a subject. Fundamental frequency, on the other hand, can be measured by instruments without any reference to our auditory mechanism. In the case of pure tones, it has been shown by Stevens (1935) that at high intensity levels low tones (frequencies up to 1000 Hz) are perceived lower and high tones (above 3000 Hz) are perceived higher. More recent studies (Cohen 1961) show that pitch is affected by loudness in the direction indicated by Stevens, but to a much smaller extent (less than 2%) than originally claimed. Because of the spectral characteristics of speech sounds, the effect of intensity is even lower than for pure tones and consequently can be judged as irrelevant for the determination of pitch. For speech, the main correlate of pitch for voiced sounds is fundamental frequency (which corresponds to the rate of vibration of the vocal cords), although we have evidence (Schouten 1940, Licklider 1956) that higher frequency regions (the fourth and higher harmonics; Plomp 1967) are used for this pitch perception. Klatt (1973) indicates in fact that pitch perception is improved when the fundamental frequency region is filtered out of the acoustic signal. Unfortunately, most of the experiments dealing with pitch perception have been done with pure tones. Because of the acoustic characteristics of the stimuli generally used in these experiments (wide frequency range, steady state frequencies) the results are difficult to extrapolate to speech.
2. Perception Experiment

In order to obtain data more closely related to speech in general and to tone languages in particular, the following experiment was conducted. Ten American subjects participated in this experiment which was divided into two parts.

2.1. Part I

2.1.1. Experimental Paradigm. The stimuli consisted of 30 instances of the vowel [i] synthesized with different fundamental frequency patterns. As seen in Fig. 1 the stimulus was composed by a slope followed by a level tone maintained constant at 120 Hz. The onset frequency was one of the following values: 70, 100, 110, 130, 140 and 190 Hz; that is, 3 values below and 3 values above 120 Hz. The duration of the slope (Δt) was varied at 40, 60, 100, 150 and 250 msec. In other words, 15 stimuli (3 F₀ onset X 5 slope duration) had a falling fundamental frequency and 15 stimuli had a rising fundamental frequency. The overall duration of the stimulus was fixed at 250 msec. Each time the stimulus was presented, it was followed by a 500 msec pause and a second vowel [i] with a steady state fundamental frequency. The duration of this vowel was also 250 msec. The level of its fundamental frequency was adjustable by a knob controlled by the subject. The task of the subjects was to match the pitch of the second vowel to the beginning of the first vowel. The rate of stimulus presentation as well as the number of trials for a given presentation were controlled by the subject. Each one of the 30 stimuli was presented 3 times in a randomized order. The subjects heard the stimuli through earphones at a comfortable level (about 70 dB).

2.1.2. Results. The results of this part are presented in Fig. 3. On this graph, the subjects' responses (i.e. the steady state fundamental frequencies of the second vowel adjusted by the subjects) are plotted as a function of the duration of the slope. The thin straight lines represent the ideal responses; the wider lines with corresponding symbols represent the subjects' actual responses for this fundamental frequency onset.

This graph suggests that 1) falling patterns (i.e. vowels with fundamental frequency onset above 120 Hz) are perceived more accurately than rising patterns (i.e. vowels with fundamental frequency onset below 120 Hz); 2) the longer the slope, the more accurate the matching, but correlation between slope duration and accuracy of matching is not linear.
FIGURE 1. Stimulus Format (Part I)

250 MS 500 250 MS

FO ONSET

120 HZ

ΔT

STIMULUS
SUBJECT'S RESPONSE

FIGURE 2. Stimulus Format (Part II)

250 MS 500 250 MS

120 HZ

FO OFFSET

ΔT

STIMULUS
SUBJECT'S RESPONSE
FIGURE 3. Subjects' Responses (Part I)
FIGURE 4. Subjects' Responses (Part II)
2.1.3. Discussion. These results can be explained by forward masking. Masking is the obscuring of a sound by another. Forward masking is the masking that occurs when the masker tone is terminated before the test tone (maskee) is begun. Forward masking is the most effective when the frequency of the earlier tone is lower than but close to the frequency of the second tone (Lüscher and Zwislocki 1949, Licklider 1951, Jeffress 1970). If we extrapolate the results obtained with steady state tones to contours, we can understand why the onset region of the rising ramp was not accurately perceived, since each frequency was masked by the previous lower frequency (since the frequency is going up). This is not the case for the falling tone in which each frequency is followed by a lower frequency.\(^3\) Data from the psychoacoustic literature (Brady et al 1961, Heinz et al 1968, Pollack 1968, Nabelek and Hirsh 1969, Nabelek et al 1970, Tsumura 1973) can be interpreted as supporting my claim concerning the role of masking in the perception of changing frequency contours.

In these experiments subjects were asked to match the pitch of a steady state signal with a changing frequency signal. They consistently adjusted their steady state tone closer to the final point of the contour. This fact already shows the role of masking which attenuates the effect of the onset region in favor of the offset region, but furthermore there is a tendency to match closer to the final point when the stimulus is a rising contour (as opposed to a falling contour). This indicates, as I have suggested, that the masking of the onset region is more effective in the case of rising contours (as opposed to falling contours), and consequently, this leads to the perception of an averaged pitch closer to the offset frequency.

These data are also in agreement with the study of Brady et al (1961) with respect to the role of the rate of frequency change. They found that the matching of a steady state frequency with a contour frequency is closer to the end point of the contour when the rate of change is high; in other words, the onset region is less salient at high glide rate. This is shown on Fig. 3 by responses close to 120 Hz when the slope duration of the stimulus is short (i.e. the rate of frequency change is high). This is also in agreement with Pollack (1968) and Nabelek and Hirsh (1969), whose results indicate that optimum discriminability of relatively small frequency changes is obtained at relatively slow glide rates.

2.2. Part II

2.2.1. Experimental Paradigm. The experimental
paradigm used here is similar to the one used in Part I, but instead of having a slope followed by a level tone, now the level tone precedes the slope (see Fig. 2). As in Part I, the level tone was maintained constant at 120 Hz; the slope duration had one of the following values: 40, 60, 100, 150 or 250 msec, and the values for fundamental frequency were identical to the values used for fundamental frequency onsets in the previous part of the experiment (i.e. 70, 100, 110, 130, 140 and 190 Hz). The subjects were asked to match the adjustable steady state fundamental frequency of the second vowel to the end point of the first vowel.

2.2.2. Results. The results are presented in Fig. 4. As on the previous graph, the subjects' responses are plotted as a function of the length of the slope. The thin straight lines represent the ideal responses, and the wider lines with similar symbols represent the subjects' actual responses.

Three points should be emphasized: 1) The matching is more accurate with the end point of the stimulus vowel than with the onset of the stimulus vowel (i.e. Part I of the experiment). This can be easily visualized by the fact that the subjects' actual responses are closer to the ideal response line (offset frequency) in Part II than to the onset frequency in Part I. 2) It was mentioned in Part I that the onset of a falling pattern was more accurately perceived than the onset of a rising pattern. This is not the case for the offset point. 3) As before, the longer the slope, the more accurate the matching, but it is obvious on this graph that there is an upper limit in duration above which the accuracy of matching does not improve; in particular, the change of slope duration from 150 to 250 msec has very little effect on the subjects' responses.

2.2.3. Discussion. The explanation based on forward masking proposed in the first part of the experiment to account for a different degree of accuracy in the perception of onset frequencies of falling vs. rising contours cannot be applied here (otherwise one would predict a higher degree of accuracy for the matching of the offset falling ramp). Rather, it seems that the final auditory image (the offset frequency) can be processed equally well no matter what the preceding frequencies are. The fact that the matching is more accurate than in Part I can be accounted for by considerations based on memory. The effect of glide rate on pitch perception is consistent with the first part of the experiment.

3. Linguistic Implications and Directions of Further Research
These data are of potential interest in the understanding of tone languages in two respects: 1) the development of tones; and 2) the distribution of tones.

3.1. The Development of Tones

The historical development of tone systems from the reinterpretation of intrinsic perturbations caused by neighboring consonants on the pitch of the preceding or following vowel has been suggested for a number of genetically unrelated languages (Beach 1938, Haudricourt 1954, 1961, 1972; Matisoff 1973, Ohala 1973, Purcell 1972, 1974; Purcell et al 1975). In order to validate such historical developments, two sets of data are necessary: first, data from production tests showing that these intrinsic perturbations caused by pre- and postvocalic consonants do exist (House and Fairbanks 1954, Lehiste and Peterson 1961, Mohr 1969, Löfqvist 1973, Hombert 1974); and second, perceptual data showing that these intrinsic perturbations can be perceived (Haggard et al 1970, Fujimura 1971, Abramson 1974, Hombert 1974, 1975a).

3.2. The Distribution of Tones

Data such as those presented in this paper represent a preliminary step in our understanding of the distribution of tone shapes in tone languages. For instance, the fact that languages have a tendency to favor falling tones rather than rising tones (Cheng 1973, Hyman 1973b) has been explained in articulatory terms (Ohala and Ewan 1973, Sundberg 1973). It was suggested earlier in this paper that falling tones are probably more salient perceptually than rising tones because they cover a wider frequency range (since the onset is more accurately perceived). This indicates a perceptual correlate to the articulatory constraint generally proposed to account for the asymmetry between the number of rising tones vs. the number of falling tones in languages.

Our understanding of the distribution of tones in tone languages would be significantly improved if we would be able to develop a theory of tone space based on perceptual distance analogous to the model proposed for vowel space by Liljencrants and Lindblom (1972). For the construction of such a model data from three sources seem to be relevant: 1) acoustic descriptions of tones from tone languages; unfortunately, only a limited number of such studies have been done (Howie 1972, Abramson 1962, Chuang 1972, Han 1969, Gandour 1974, LaVelle 1974); 2) confusion matrices from tone languages showing how similar or dissimilar different frequency contours are judged; 3) controlled investigation
of the different parameters involved in pitch perception: fundamental frequency onset, speed of pitch change, fundamental frequency offset.... It is in this area that the perception data presented in this paper will be useful.

4. Conclusion

The data presented in this paper indicate the following: 1) The onset of falling tones is more accurately perceived than the onset of rising tones. This asymmetry can be explained by forward masking. This explanation accounts for most of the data reported in the psycho-acoustic literature. 2) There is no difference in the perception of the offset of a falling vs. a rising tone. The explanation based on masking does not hold for offset frequencies. It seems that the final auditory image is processed equally accurately independently of the preceding frequencies. 3) The perception of onset and offset frequencies is more accurate for slow rate of frequency change. Finally, the relevance of these data for the development and distribution of tones in tone languages is mentioned.

5. Acknowledgements

I would like to thank John Ohala for his constant, helpful guidance throughout my experimental investigations of the perception of tone. In addition, I am grateful to Michelle Mieusset for her aid in preparing the photographs and to Harold Clumack, Hector Javkin, Larry Hyman, C.K. Chuang, Bob Krones and Steve Greenberg for their comments on an earlier draft of this paper.

6. Footnotes

1For low frequencies, Stevens' results are in agreement with Zurmühl (1930) and Snow (1936).
2For a review of theories of pitch perception and their relevance for speech, see Hombert (1975b).
3Obviously this explanation holds only if we consider forward masking. Backward masking would make the opposite prediction (Massaro 1968).

7. References

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Crossover and About-Movement in a Relational Grammar
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One of the central claims of a theory of relational grammar, as proposed by Perlmutter and Postal, is that underlying structures consist of unordered items. Consequently linear order plays a much less crucial role in relational grammar (RG) than it does in a non-relational grammar (which I will call linear grammar, or LG). So for example Passive is not a movement rule in RG, but rather a rule which changes grammatical relations. Since a number of phenomena have been described by crucially assuming the principles of LG, it is worth seeing whether or not these can be described within a relational framework.

One such case is the Crossover constraint (Postal, 1971), which has been formulated as a constraint on movement rules. In this paper I will propose a reformulation of this constraint within a relational framework; and will then show that the rule of About-Movement appears to be a counterexample to the reformulated constraint. It would therefore seem that RG cannot capture a generalization that can be captured in LG. But a closer look at About-Movement leads to a reformulation of this rule in such a way that the relational version of Crossover will work. It should be noted though that this paper does not constitute an argument for RG over LG, since either framework can handle the facts I will discuss.

1. Crossover in Linear Grammar

The term Crossover has been used to refer to a number of different phenomena. Postal's original constraint covered three kinds of cases:

A. For some speakers, Wh-Fronting and Relative Clause Formation cannot front an NP which is preceded by a coreferential NP:

1. a. *What did the man who built it destroy?
   b. *the house which the man who built it destroyed

B. For all speakers, however, these rules are blocked if the fronted NP is preceded and commanded by a coreferential NP:

2. a. *Who does he think it will please?
   b. *the man who he thinks it will please

C. Third, rules like Passive and Dative cannot re-
verse the order of two coreferential NP's which are clausalmates:

3. a. The snake bit itself
   b. *The snake was bitten by itself

4. a. Sally sold the slave to himself
   b. *Sally sold the slave himself

The fact that non-clausalmates are not affected by the constraint is shown by the following:

5. a. The monster was destroyed by the scientist who created it
   b. The scientist who created it was destroyed by the monster

Postal (1972) argues that these three cases are all separate phenomena. I will be assuming that this is correct, and by Crossover I mean only the constraint that blocks the C-cases. Of course there is a good deal of overlap between these cases; for example, any reasonable formulation of the constraint ruling out A will also rule out B. I will also be assuming that the C-cases are not blocked by a constraint on Reflexives, as in Jackendoff (1972), but, in LG terms, by some kind of movement constraint.

Postal’s formulation of the part of the constraint that applies in C was very roughly: No rule can reverse the order of two coreferential NP’s if they are clausalmates. However, another formulation could be:

I. If NP precedes and commands a coreferential NP, then no rule can cause NP to precede and command NP.

This formulation differs from Postal’s in that it does not mention clausalmates; the grammaticality of (5) as opposed to the ungrammaticality of (3) and (4) is accounted for by the command condition. I will not argue for this formulation as opposed to one mentioning clausalmates; the point relevant to this paper is simply that the notion of linear order is crucial in either formulation.

However, two problems should be mentioned. Lakoff (1968) noted the ungrammaticality of the following:

6. *Ted was believed by himself to have finished and used this to argue that Raising must precede Passive. If (6) were derived via Passive-Raising:
then no violation should result. Neither the application of Passive nor that of Raising reverses precede-command. (Lakoff was actually assuming that the constraint blocks the application of a rule if it reverses the order of clausemes; but such a constraint is likewise not violated in (7)). A derivation in which the rules applied in the order Raising-Passive, on the other hand, is blocked by either version of the constraint, since the application of Passive violates the constraint:

But if we assume that there is no extrinsic rule ordering, then (7) is a possible derivation. The constraint must therefore be formulated not as a constraint on rule application, but as a derivational constraint on trees which are not necessarily adjacent in a derivation:

II. If NP₁ precedes and commands a coreferential NP₂ at some point in a derivation, then at no later point can NP₂ precede and command NP₁.

This constraint will block both of the above derivations. (In a theory in which all constraints on rules are essentially derivational constraints, then the only difference between I and II is that I refers to adjacent trees in a derivation, while II does not).
A second problem concerns cases involving Tough-Movement:

9. a. To see himself is hard for Oliver
   b. *Oliver is hard for himself to see

If we assume that (9b) is derived from an underlying structure (10a) by raising the object:

10. a. 

then the constraint wouldn't hold, since the order of the two coreferential NP's hasn't been reversed. This problem can be solved by assuming that the dative for Oliver is the underlying subject, and that Flip has applied. Given this, the derivation of (9b) is exactly parallel to the Passive-Raising case, and violates constraint II:

11. 

Since there is little syntactic evidence for Flip (and especially for its application in these cases), it may be objected that this is not a real solution. In (3) I will reformulate the constraint in such a way that Flip need not be assumed.

2. Relevant Principles of Relational Grammar

Before looking at this constraint in a relational framework, I will briefly sketch some of the relevant principles of RG that Perlmutter and Postal (hereafter P&P) have proposed:
A. A sentence consists of a verb and various associated nouns. Some of these nouns bear a grammatical relation to the verb, these are terms. Subjects, Direct Objects and Indirect Objects are terms; all other nouns, such as Locatives and Benefactives are non-terms.

B. There is a hierarchy among nouns as follows:

Sub. > D.O. > I.O. > Non-Terms

C. Linear order does not exist in underlying structure. Rather it is fixed at some point in the derivation by language-particular rules. Thus the rule for word order in English will yield:

Sub. - V - D.O. - I.O. - Non-Terms

With these principles, certain rules which have standardly been formulated as movement rules can be reformulated as relation-changing rules. So P&P have proposed that the universal formulation of Passive is:

D.O. → Sub.
The old subject becomes a non-term by the principle:

D. If a term is displaced by the application of a rule, it becomes a non-term.

Passive therefore need not mention the fate of the underlying subject, since D is a convention applying to all rules.

Principle C together with D predicts the fact that there is a word order difference between actives and passives in English. An active sentence is linearized as Sub. - V - D.O.; whereas a passive is linearized as Sub. - V - Non-Term. Both are predicted by the English word-order rule.

Dative Movement has also been formulated by P&P as a relation-changing rule: I.O. → D.O. Again the old direct object automatically becomes a non-term, and since terms precede non-terms, the word order difference is predicted. P&P also formulate raising rules, such as Raising and Tough-Movement, as relation-changing rules. In all raising rules, the raised NP assumes the grammatical relation of the "host" NP (the NP out of which it is raised), the host then becomes a non-term by Principle D.

However, it is not the case that all rules are reformulated as relation-changing rules, and thus RG makes a distinction between relation-changing rules and movement rules. The latter only change linear order, while keeping grammatical relations constant. P&P have claimed that Topicalization, for example, is a movement rule; Heavy NP Shift is presumably another.

The aspect of relational grammar which is crucial here is that rules like Passive and Dative apply to unordered structures. P&P have suggested, and Johnson (1974) has explicitly argued for, a principle by which linear order is fixed post-cyclically. This principle entails that
all movement rules are post-cyclic, since they cannot apply to unordered structures. But another theory, which is consistent with all of the above principles, would be one in which linear order was fixed cyclically. This theory would allow movement rules to be cyclic. For example, it could be that linear order is fixed on each cycle after the application of all relation-changing rules; at which point movement rules are free to apply. In either theory though, relation-changing rules apply to unordered structures.

3. A Relational Formulation of Crossover

Given a theory in which Passive and Dative apply to unordered structures, the Crossover constraint as formulated in I or II — in which the reversal of precede and command is blocked — will have no effect. Neither constraint will rule out (3b) or (4b).

There is however a natural translation of the constraint into relational terms. Basically, it would block a reversal of the hierarchy of two coreferential NP’s, rather than an order reversal. To state this more formally, P&P have given the following definition:

An NP_a outranks an NP_b if:

1.) NP_a assymetrically commands NP_b, or
2.) NP_a commands and is higher in the hierarchy than NP_b

Thus a subject outranks a clausemate object; but a subject does not outrank the object of a higher clause.

With this definition, we can reformulate the constraint:

III. If at some point in a derivation NP_1 outranks NP_2, then at no later point can NP_2 outrank NP_1

This is a reformulation of II; i.e., the constraint is not restricted to adjacent trees in a derivation. But we should note that with the above definition of rank, there is no evidence for this kind of constraint (as opposed to a constraint on rule application, or, equivalently, a derivational constraint referring to adjacent trees). That is, we were led to a formulation like II by the fact that in a derivation of (6) in which Passive precedes Raising, no single rule reverses precede-command. But let us look at this derivation in a relational framework:
The above constraint is violated simply by the application of Raising. At the input to Raising, \textit{Ted}_1symmetrically commands and thus outranks \textit{Ted}_2, whereas at the output \textit{Ted}_2 outranks \textit{Ted}_1, since it commands and is higher in the hierarchy than \textit{Ted}_2 (Sub. \hspace{1em} \textgreater \hspace{1em} Non-Term). We could then restate the constraint as:

IV. If an NP\textsubscript{1} outranks a coreferential NP\textsubscript{2}, then no rule can cause NP\textsubscript{2} to outrank NP\textsubscript{1}.

Moreover, it is no longer necessary to assume that Flip applies in a Tough-Movement case like (9b). The application of Tough-Movement itself, like the application of Raising in (12), will cause a rank reversal:

At the input, \textit{Oliver}_2 outranks \textit{Oliver}_1 (by assymetrical command), and at the output \textit{Oliver}_1 outranks \textit{Oliver}_2 (by commanding and being higher in the hierarchy).

Thus, depending upon one's feelings about Flip, it may appear that a RG formulation is actually preferable to the LG formulation. But this is simply a consequence of the fact that the notion outrank is broader than the notion precede and command, since the former also includes assymetrical command.

The facts could therefore be captured in an equivalent way in LG. We can in LG define the notion outrank as follows:

An NP\textsubscript{a} outranks NP\textsubscript{b} if:

1.) NP\textsubscript{a} assymetrically commands NP\textsubscript{b}, or
2.) NP\textsubscript{a} commands and precedes NP\textsubscript{b}

This definition is equivalent to the RG definition; we have simply substituted precede for be higher in the
hierarchy. With this definition, the same constraint - IV - can be the LG formulation. The application of Tough Movement in derivation (10) will violate IV; since at the input Oliver₁ assymetrically commands Oliver₂, while at the output Oliver₂ precedes and commands Oliver₁. Thus in LG as well as in RG the assumption that Flip has applied is unnecessary. Likewise, the application of Raising in (7) will violate the constraint, and so we need not assume that the constraint refers to non-adja- cent trees.

In other words, the constraint is the same in the two grammars, it is simply the definition of outrank which differs. It begins to look as if we have equivalent theories - at least for this portion of the grammar - where we are simply using two different terms: precede vs. be higher in the hierarchy.

But the two grammars are making different predictions. Since RG does have movement rules in addition to relation-changing rules, the relational formulation predicts:

Movement rules which do not reverse assymetrical command will not be affected by the constraint.

In other words, if a rule keeps grammatical relations con- stant, but reverses the precede-command relationship of two coreferential NP's, then no violation should result. LG does not make a parallel distinction, and so the LG constraint would hold in a larger class of cases.

4. The Apparent Problem

Earlier it was mentioned that Heavy NP Shift is pro- abably a movement rule; another similar looking rule is About-Movement (Postal, 1971), which applies in

14. a. I talked to everyone about that movie
    b. I talked about that movie to everyone

Since there is a fair amount of word-order freedom among items occuring after the verb, we might suppose that there is no separate rule of About-Movement, but a gen- eral post-verb Scrambling rule. Whether or not this is the case, the assumption that (14b) is related to (14a) by movement means that the grammatical relations are held constant. Thus in both sentences, everyone is an I.O., while that movie is a non-term.

This assumption, combined with the RG formulation of Crossover, makes the following prediction:

The constraint will not apply to About-Movement (i.e., this rule can reverse the order of core- ferential NP's).

But this prediction appears to be wrong. Postal (1971) argued that About-Movement is subject to the constraint
on the basis of examples like:

15. a. I talked to him about himself
    b.*I talked about him to himself

Within RG there is an alternative explanation for
the above contrast. P&P have proposed that only terms
can trigger Reflexive. Since in this analysis the about-
phrase is a non-term, the ungrammaticality of (15b) is
predicted. But this explanation will not account for:

16. *I talked about himself to him

and so the problem remains. The LG formulation of Cross-
over does account for both (15b) and (16); since in both
cases About-Movement reverses the precede-command re-
lationship of two coreferential NP's. But the RG formula-
tion fails because the assumption that About-Movement is
a movement rule means that relational hierarchy is not
reversed.

In other words, in LG, Passive, Dative and About-Move-
ment are all movement rules, and so it follows that they
behave the same with respect to the constraint. In RG
we are assuming a distinction between Passive and Dative
on the one hand, and About-Movement on the other. But
this distinction makes exactly the wrong prediction. In
order to account for (16), RG would have to either (a) po-
sit a separate constraint that applies to About-Movement,
or (b) keep the same constraint, and add precede and com-
mand to the definition of outrank. Either way represents
a complication not necessary in LG, and makes the distinc-
tion between movement rules and relation-changing rules
look dubious.

5. A Reformulation of About-Movement

But the problem may lie not in the distinction per se
between movement rules and relation-changing rules, but
in the assumption that About-Movement is a movement rule.
P&P have proposed that there are rules which make terms
out of non-terms. Such rules are needed to handle pseudo-
passives like (17b):

17. a. George Washington slept in this bed
    b. This bed was slept in by George Washington

Since Passive can be formulated in LG to front the first
NP after the verb, the existence of (17b) presents no prob-
lem for LG. But this sentence looks like a counterexample
to the RG formulation (D.O. → Sub.), since the passivized
NP is apparently a locative. To account for this sentence,
P&P have suggested that there is a rule: Loc. → D.O.
Once this rule applies, Passive is free to apply. (17a) is
therefore derivationally ambiguous; this bed is either a locative, or an ex-locative turned object.

A slightly different approach to pseudo-passives is taken by Johnson (1974). In his account, the preposition incorporates into the verb (by Predicate-Raising, where in is treated as a predicate). A consequence of this process is that the object of the preposition becomes the object of the derived verb - sleep in. This account predicts that whenever incorporation takes place, there could be a lexical item corresponding to the derived verb. Thus Johnson suggests that inhabit is derived from live in by this process.

We can now ask whether About-Movement might not be a similar kind of rule. The answer is not only that it can be, but that given the P&P formulation of Passive, it must be. That is, About-Movement feeds Passive, as is shown by the contrast between (18b) and (19b):

18. a. I talked to everyone about that movie
    b.*That movie was talked to everyone about

19. a. I talked about that movie to everyone
    b. That movie was talked about to everyone

In LG, where we can formulate Passive to front the first NP after the verb, the grammaticality of (19b) is predictable. But if we assume a movement formulation of About-Movement, this sentence is a counterexample to the RG formulation of Passive. Since grammatical relations are not changed by movement rules, that movie is a non-term in both (18a) and (19a). This means that a non-term has become a subject by Passive.

Therefore, within this framework we must conclude that About-Movement is parallel to the locative case. Either there is a rule which takes about-NP's and turns them into D.O.'s, or, as in Johnson's formulation, there is a rule which incorporates about into talk. This incorporation yields the derived verb talk about, whose object in (19a) is that movie.

By formulating About-Movement as an object-creating rule, we predict that it feeds Passive. In addition, the following predictions are made:

1. It follows that About-Movement appears to be a movement rule in the same way that Passive and Dative do. Thus, in (14a) the items are linearized: Sub. - V - I.O. - Non-Term; in (14b) we have the order: Sub. - V - D.O. - I.O.

2. In the incorporation analysis, it is predicted that there is a possible lexical item corresponding to the derived verb talk about. The existence of discuss bears out this prediction.

3. Most pertinently, the relational formulation of
Crossover now makes the correct prediction. Since About-Movement is a relation-changing rule, it will obey the constraint in the same way that Passive and Dative do. Thus in

15. a. I talked to \( NP_1[\text{him}] \) about \( NP_2[\text{himself}] \)

\( NP_1 \) outranks \( NP_2 \), since it commands \( NP_2 \) and is higher in the hierarchy (I.O. \( \geq \) Non-Term). In (15b):

15. b.*I talked about \( NP_2[\text{him}] \) to \( NP_1[\text{himself}] \)

the rank has been reversed; \( NP_2 \) now commands and is higher in the hierarchy than \( NP_1 \) (D.O. \( \geq \) I.O.). We see then that RG does not need to include an additional constraint to cover About-Movement; nor is there any need to include precede in the definition of outrank.

One more case should be noted. The following pair shows the same contrast as (15a) and (15b):

20. a. I talked with him about himself

b.*I talked about him with himself

The ungrammaticality of (20b) is predicted by the LG formulation of the constraint, since About-Movement has reversed the precede-command relationship of the coreferential NP's. At first glance, the ungrammaticality of (20b) looks like a counterexample to the RG formulation. With \text{him} appears to be a non-term (as opposed to \text{to him in (15)}. If so, then neither of the coreferential NP's are terms in (20a). Hence, neither outranks the other, and so the constraint wouldn't hold. But given the P&P principle that only terms can trigger Reflexive, we must conclude that with \text{him} is in fact a term, since it has triggered Reflexive in (20a). We can assume then that it is an I.O., and so the constraint will block the derivation of (20b) in the same way that it blocks (15b).

Whether we consider the RG or the LG formulation of the constraint, there is an interesting consequence to the interaction of Crossover and About-Movement. It was hypothesized that \text{discuss} is a lexicalization of talk about, where about has incorporated into the verb. And just as (20b) is bad, the corresponding sentence with \text{discuss} is also bad:

21. *I discussed him with himself

Given the central assumption of this paper - that Crossover is a constraint on the application of syntactic rules - the constraint will not block (21) in a theory in which all lexical insertion occurs before the application of syntactic rules. In such a theory, the existence of \text{discuss} would not depend on the prior applica-
tion of About-Movement (or About-Incorporation); and so the ungrammaticality of (21) cannot be attributed to a constraint which is violated by this rule. But of course this argument works both ways; and could be used instead as evidence that Crossover is not a constraint on the application of syntactic rules.

Footnotes

1 All references in this paper to Perlmutter and Postal are from their course on relational grammar at the LSA Institute, Summer, 1974.

2 Evidence for this assumption comes from cases involving Tough-Movement and Dative Deletion. Thus the ungrammaticality of (i):

i. *Sam is hard for himself to shave

can be attributed either to a constraint on reflexives, or to a movement constraint (which is violated by Tough-Movement). But if the dative for himself is deleted, the sentence is still bad:

ii. *Sam realized that he'd be hard to shave

(ii) is of course good on an irrelevant reading, where the deleted dative is unspecified. But it cannot have the reading of:

iii. Sam realized that it would be hard for him to shave himself

To show this more clearly, we can put (ii) in a context where the (iii)-reading is preferred:

iv. Sam will be late because he still has to shave { himself, and

(a) he says it's always hard to shave { himself in less than five minutes

Bill

(b) he says that { *he is always hard

Bill to shave in less than five minutes

A constraint on reflexives won't account for the above contrast, or for the ungrammaticality of (ii) on the (iii)-reading.

3 I am assuming that Tough-Movement is a raising rule, rather than a deletion rule. The interaction between
Crossover and Tough-Movement bears on this issue. If we assume that Crossover is, in LG terms, a movement constraint, then we must conclude that (i):

1. *Sam is hard for himself to shave

is derived by movement.

But in view of the ungrammaticality of (ii):

2. *Sam is too hairy for himself to shave

where deletion is clearly involved, it would seem that our assumption about the nature of Crossover is wrong. But this follows only if the derivation of (ii) does not involve movement in addition to deletion. There is a plausible analysis of (ii) involving Tough-Movement. That is, the too Adj. to V construction is closely related to a construction with impossible. A rough paraphrase of (iii) is (iv):

3. The rock is too heavy to move
4. The rock is so heavy that it's impossible to move the rock

But impossible is a Tough-Mover; and so (iii) might be derived from something like (iv) where Tough-Movement applies in the lower S to give

5. The rock is so heavy that the rock is impossible to move

The derived subject would then be deleted.

This analysis is not meant to be taken too literally, since the so construction itself needs further analysis. The crucial point is simply that in a decomposition analysis of the too Adj-construction, Tough-Movement will apply in the derivation. Given this, a movement analysis of Crossover will predict the ungrammaticality of (iii), since the derivation of this sentence is in part the same as the derivation of

6. *Sam is impossible for himself to shave

References


Frame Semantics for Motion Verbs with Application to Metaphor
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I want to propose a method of doing descriptive semantics for motion verbs. The basic tool I will use is called a graph of interaction. These graphs have been introduced by the topologist Rene Thom. They represent simple models of interactions which can be used to specify a basic framework for understanding the invariant properties of motion verbs. Let me give an example. In (1) I present a graph Thom calls "the gift." The graph describes an interaction between three actants, each represented by a line. We may say that each actant performs a role in the interaction. In (1) we have a sender, S, the bottom line; a receiver, R, the top line; and line G that goes from S to R, representing the transferred actant. Time is thought to move from left to right in (1).

(1)
\[
\begin{array}{c}
R \\
\downarrow \\
G \\
S
\end{array}
\]

It is easy to use (1) to specify the content of motion verbs such as give and put. Following an observation of Leonard Talmey, (1) specifies the content of give provided the receiver role is filled by an animate noun, but if the receiver is inanimate (1) represents put. Examples are in (2) and (3).

(2) I gave flowers to Alice.
(3) I put the flowers in a vase.

I will call this specification the basic frame of give and put. Other, less literal uses will be derived by substitutions of one sort or another. We will come to these after presenting some evidence in support of my analysis.

There are six verb-particle constructions in English in which give and put correspond in just the manner I have claimed, animate versus inanimate receiver.

(4) away
a. I gave my worldly goods away to the poor.
b. I put my books away in the bookcase.
(5) back
a. I gave the pen back to Bill.
b. I put the pot back on the stove.
(6) off
a. The flowers gave off a pungent perfume.
b. I put off my work until tomorrow.
(7) out
a. Ziegler gave false information out to the press.
b. I put the clothes out on the line.
(8) over
a. I gave my gun over to the policeman.
b. My contribution put the total over the goal.
(9) up
a. Mary gave her gun up to the policeman.
b. John put strawberries up in jars.
A few comments on (4)-(9) are necessary. First, the pronoun test shows these are all verb-particle constructions. That is, if a pronoun is substituted for the noun in direct object position (here corresponding to the actant $G$), it must come between the verb and the preposition. I verify this for (4) in (10); the rest are similar.

(10) a. I gave away my worldly goods.
    I gave them away.
    *I gave away them.

b. I put away my books.
    I put them away.
    *I put away them.

Notice that in (10) I have suppressed the phrase designating the receiver. Yet any representation of the acceptable sentences in (10) must include mention of the receiver actant. This can be argued in two ways. First, a discourse as simple as (11) is incoherent unless we infer the presence of a receiver actant, and that it is the table$_j$ rather than John.

(11) I put away my books$_i$. John picked them$_i$ up off the table$_j$.

A second argument comes from the anomaly in (12); we cannot cancel the receiver actant. The cancellation test is a good way to find an unmentioned actant.

(12) *I put my books away nowhere.

It should be noted in connection with this test that its application to (6a) gives a version of a traditional philosophical question, as in (13).

(13) *The flowers gave off a pungent perfume to noone.

To be sure a philosopher might argue that (13) makes sense, but a science fiction writer might make a similar claim for (12). In either case the important linguistic fact is that these starred sentences require very considerable discourse support to undo the anomaly.

One of the functions of frame semantics is to identify various levels of anomaly. Anomaly can only be defined with respect to a given context. The notion of context embodied in my theory is the graph of interaction. Metaphors, which are considerably less anomalous than (12) and (13), result from substitutions in the basic frame of these verbs. These substitutions leave the graph of interaction invariant. One way of characterizing the badness of (12) and (13) is that they express contradictory information about the structure of the interaction described. Metaphors are merely ontologically peculiar. Already in (6a) we have had a sentence in which an inanimate subject is being said to give something. Let us consider some other simple metaphors using give and put.
(14) I gave in to temptation.
(15) John gave himself to the theater.
(16) Bill put strange ideas in Mary's head.
(17) I put the question to Bill.

The distinguishing basic frame properties of the verbs give and put have been reversed in (14)–(17). We need to express facts such as this in our theory. I believe the best way to capture the essential quality of these metaphors lies in the "case" notions of Fillmore. I will generally follow the 1971 version, but with the variations that are called for. By the term "case frame" for a verb, I understand a list that includes the interaction described and case assignments for each actant. A basic frame is then a particular case frame in my expanded sense of that term. The basic frames for give and put are given in (18) and (19) respectively.

(18) give: gift interaction, S=Agent, G=Object, R=Experiencer
(19) put: gift interaction, S=Agent, G=Object, R=Goal

Sentences (14) and (15) have inanimate nouns in the receiver role. This changes the case frame from (18), the receiver is a Goal in the metaphorical expressions instead of an Experiencer. Similarly (16) and (17) differ from (19) with an Experiencer instead of a Goal. One might want to go further in characterizing the metaphors by representing the semantic transformation itself as part of the meaning. By this I mean that (14) and (15) could be said to "personify" the receiver nouns. This might be considered a residue of the basic frame, the animateness somehow hangs on, survives the substitution. I will not explore this refinement. It is more important to discuss substitutions in the sender role.

We already have in (6a) an expression with give that does not have an Agent in the sender role. The case of flowers in (6a) is Source presumably, because there is no intention in the interaction. But this assignment requires that we stretch Fillmore's original notion quite a bit. Source was intended to be temporal or spatial. Perhaps the latter sense applies to (6a), but this solution will not work for more abstract senders. I give some examples of this phenomenon below; (20) is taken from Georgia Green (1974).

(20) Mary's behavior gave John an idea.
(21) Poverty gave John a saintly feeling.
(22) Mary's behavior put John on guard.
      in seventh heaven.

If we want to stay within Fillmore's inventory of cases, the subjects of (20)–(22) must be assigned Source. But these sen-
tences involve a notion of psychological motivation that does not naturally fit into the spatial/temporal notion of Source. A variety of solutions are possible for this problem. I will outline one which seems attractive to me.

Sentences like (20)-(22) and (23) and (24) involve a kind of causality that is qualitatively different from that associated with the notion of Agent.

(23) Ambition drove John to greatness.
(24) John is given to abominable practices.

The standard causal situation involves only one locus of intention, situated in the animate actant Agent. The metaphors I have been considering complicate the standard situation in one way or another. In (20) one might argue that either two intentional actants are present, or that the causality resides in the abstraction "behavior." A similar complication is involved in (21)-(24). I propose that we add to the inventory of cases a category for non-standard causation. This idea has been suggested in a different context by Huddleston (1970), who suggested that the case "Force" be introduced to handle non-intentional activity such as (25).

(25) The wind opened the door.

The version of a non-standard causality notion that I prefer may be called Agitator. By this term I understand a broader notion than non-intentional Force, but one which includes the latter. As I will use the term Agitator is something of a wastebasket. I throw into it anything that will not fit into the standard causal situation. The advantage to making up this new category is that we can use properties of the graphs of interaction to characterize properties of the Agitator case. It will take some argument to get to this point. For now, I will begin by looking again at (20)-(24).

What is needed to characterize the semantic structure of these metaphors is a concept like incitement, inducing motion in an actant. The psychological term motivation, after all, means to cause to move. It is not clear whether Huddleston means his Force case to designate compulsion, but such an interpretation would not go happily with (20) or (21). Agitator should be thought of as involving degrees of compulsion. Some degree of constraint is implied by (22)-(23). The grammar of (24) is elusive on this point. (24) looks like a passive, but the former subject of the sentence has become so unemployed as to be absent. Presumably, however, a reason clause, i.e. an abstract sender, is the missing actant. In (26) I suggest some possibilities.

(26) John is given to abominable practices due to *nothing
witchcraft insecurity
Again the case we assign to the sender in (24) and (26) must specify the psychological source. Moreover we understand that John is not entirely in control of the situation described by these sentences. Wood, in his helpful collection *English Verbal Idioms*, glosses expressions like (24) and (26) as conveying addiction. Perhaps if abominable were changed to harm- less the notion would be closer to inclination. In any event, the causal character of these sentences can be indicated by assigning Agitator to the sender.

An argument for the necessity of this additional case must be based on examples that cannot be handled without it. All our examples so far would be called Instrument by Fillmore. The crucial evidence involves interactions more comp- licated than (1). The sentence (27) describes an interaction of excision whose graph is given in (28).

(27) I sliced off a piece of salami with a knife.
(28) Patient
  \[ \text{Instrument} \]
  Metastable part
  Sender

The basic frame for verbs like *slice*, *cut-off* and *wash* is an animate Agent in the sender role, an inanimate Instrument, the Patient role filled by an Object NP and the Goal case as- signed to the Metastable (or excised) part. (27) exhibits this basic frame and so does one reading of (29). There is

(29) The boy's arm knocked the glass off the table.

a reading of (29), however, in which the action is unintentional. On this reading we cannot call the boy an Agent, so we say he is the Agitator. The importance of this example is that it removes one of Fillmore's objections to Huddleston's Force. Fillmore says that Force is unnecessary because it never co-occurs with either Agent or Instrument. On the un- intentional reading of (29) we have a counter-example to this claim. Rejecting a notion like Force or Agitator makes it im- possible to represent the two senses of (29) or any other sen- tence in which intentionality is unspecified.

It will be instructive to compare a frame analysis of a non-basic excision with the analysis of traditional case grammar. Consider (30).

(30) The wind threw me against the aerial, breaking it off the car.

The clausal complexity of (30) suggests breaking it down into a higher sentence in which wind is Instrument and the pronoun is Object. In the lower sentence the pronoun is the Instrument.
The result of such a standard analysis is that the actant named by the pronoun is assigned two cases, Object and Instrument, albeit in different clauses. The net effect of this procedure is to code the dual notion of Agitator onto this actant. If there is an Agitator, there must be an Agitatee also. The double case role of me is nothing other than a way of saying that its instrumental function is due to an outside influence; in short, that it is agitated into action. Let us consider another non-standard excision as in (31).

(31) The governor inspired the chancellor to slash the budget by dropping the Linguistics Department.

I will confine myself to the interpretation of (31) in which the dropping is done by the chancellor. Then (31) maps onto (28) in a simple way: governor is the Agitator and sender, the chancellor is in the Instrument role and case, budget is Object and the Goal is Linguistics Department. Again there is a representation of this in the standard analysis. This time the Agitatee gets assigned Goal in the higher sentence and Agent in the lower. I conclude from such comparisons that a notion like Agitator is already present in case grammar, but it is buried in too narrow a notation. My quarrel is not with assigning more than one case to a noun, that will be inevitable for sentences like (32).

(32) Bill had me give him a gift.

The difficulty with the standard solution for (30) and (31) is that it does not generalize.

The kind of frame semantics I have been advocating allows the Agitator case to be assigned to a range of examples in a principled manner. As an approximation to a rule of Agitator assignment, I offer (33).

(33) Assign NP\textsubscript{i} to Agitator case if either
   i) an animate NP\textsubscript{i} intentionally emits a non-coreferential animate actant NP\textsubscript{j}.
   ii) a non-concrete, non-intentional actant NP\textsubscript{i} emits any actant.

(33) will account for all examples of non-standard causality that have been discussed here. There are examples of sentences for which (33) is not perfectly clear; one such is (34).

(34) Mary gave up John to those abominable practices.

Although (34) meets (33i) I am not entirely comfortable with labelling Mary's role to be that of Agitator. I will not pursue this matter here. The important thing about a formulation such as (33) is that it allows case to be assigned on the basis of a property of the graph of interaction and well known semantic
distinctions such as animateness, concreteness and intention. Moreover this formulation allows some metaphorical expressions to be brought into the framework of semantic theory. Metaphors and idiomatic uses of motion verbs are still quite a difficult problem, but some of their problems can at least be formulated by the use of the graphs of interaction.

A rule like (33) shows that frame violations do not have to be treated one at a time, but that generalizations about non-literal meaning are possible. This possibility depends upon the assumption that models of interaction play an essential role in semantics. Such an assumption amounts to a claim about the structure of the lexicon. I formulate a naive and too strong version of this claim in (35).

(35a). Motion verbs invariably refer to canonical representations of interactions.

b. Metaphors change the ontological status of actants, but keep the interaction constant.

(35b) does not account for the elliptical quality of many idioms. For examples I return again to the verbs give and put.

(36) John put Bill down.
    Mary put Harry on.
(37) I give up.

In (36) we know that the subject is inciting the direct object to move toward a certain goal, but there is no noun phrase to specify what that goal is. In (37) we know even less. The missing information concerns what is being given and to whom. Saying this is equivalent to trying to fill the basic frame of give. Understanding (36) and (37) requires further context, the latter is even more context-dependent than the former. If, however, we were interested in building a language understanding system that could process a discourse that contained (37), the basic frame (18) would be a reasonable heuristic to guide the search of context.

As a final caveat (35a) must be amended to account for expressions in which a motion verb gets another actant added onto the interaction. An example of this phenomenon is the verb take, which appears in its basic frame in (38) and in its "complexified" form in the expressions in (39).

(38) I took the pencil in my hand.
(39) I took advantage of Bill by lying.
    I took a picture of the baby.

The graph of (38) looks like (40). The sentences in (39), however, map onto the excision interaction (28), advantage and the picture being metaphorically removed from the patient.
I only want to raise the problem of classifying related families of interactions, rather than say anything very concrete about the principles of such a classification. Rather than considering the "complexification" of (39) with respect to (38), I will offer an example of the problem in the context of the verb see.

In his article "Look and See," Jeffrey Gruber offers a paraphrase of (41) by (42).

(41) John sees the cat.
(42) John's gaze goes to the cat.

These sentences map onto the interaction (1). Thus an actant such as gaze or glance is postulated as an incorporated element in the motion verb see, and it plays the role of gift. Now let us suppose we want to describe an act of seeing in which there is an obstacle or difficulty which complicates the action. Sentence (43) describes a situation in which the problem has been successfully been surmounted.

(43) I saw Harry through the mist.

Assuming a paraphrase of see as above, (43) may be glossed as (44).

(44) I sent a glance through the mist to Harry.

The graph which characterizes such an interaction is (45).

(45) Receiver

message...

Messenger

Sender

This interaction, called "envoyer" by Thom, has a structure that is identical to (1) with the addition of the actant called Messenger (message and gift correspond). The additional actant symbolizes the difficulty of communication and it makes (45) more complex than (1). The Messenger actant is represented in (43) by the object of the preposition through. The name Messenger suggests that this actant may also represent an aid to communication; (46) is an example of this sense.

(46) I saw Harry through the window.

The case usually taken by the messenger is Fillmore's Path,
this actant characterizes the itinerary of the motion. The
subject of see takes the Experiencer case. Although (42) and
(44) might suggest Agent as a more likely assignment, Gruber
argues convincingly that see normally is non-agentive. In the
metaphors considered below this question will emerge again.

Let us consider a non-literal see-through expression.

(47) a. Money saw Harry through the crisis until things got
Courage better.

b. Bill saw Harry through the crisis until things got better.

If we consider the basic frame of the verb complex see-through
to be that of (43) and (46), then (47) preserves only the case
of the messenger and receiver. The object of through, crisis,
is an abstract version of Path and the Goal in (47) is also
abstract, being the state change described by the until-clause.
The case of the subject is another matter. By (33ii) the
subject in (47a) should take the Agitator case since the NPs
money and courage are abstract in the latter case and non-in-
tentional in the former. This seems reasonable. But (33i)
also makes the prediction that the subject of (47b) should
be an Agitator. This too is reasonable provided we are able
to specify that Bill is inciting, not compelling.

There is an expression with see-through in which the
subject is clearly an Agent; an example is (48).

(48) I saw through Harry's disguise.

The problem in analyzing (48) is the possessive expression
Harry's disguise. Disguise plays the role of Messenger, but
it does not take the Path case. Rather the case of this noun
seems to be Object. We can paraphrase (48) by (49).

(49) I saw through the disguise to Harry.

From (49) it appears that Harry is the Goal and (48) should
be thought of as another example of a single NP carrying two
actants with different cases.

My final example of a see-through expression shows the
heuristic value of the graph (45) for representing both am-
biguity and closely related aspects of a situation. Consider (50).

(50) Harry saw Bill's point through the example.

On one interpretation (50) looks like (48). The subject of
(50) is in the sender role, the direct object NP is the re-
ceiver and the object of through is the Messenger. The case
of the subject can be either Agent or Experiencer, depending
on the context. Not even contrastive stress will decide which
case should be assigned. The case of example is clearly Instru-
ment, and the NP Bill's point is the Goal. Let us call this
interpretation IA or IE depending on our choice of case for
Harry.

In previous examples possessive NPs have not been analyzed in a uniform manner. Sometimes such an NP is treated as a unit ((20)-(22)), other times such phrases are treated as complex, as with the boy's arm in (29). The analysis is a function of the particular interaction involved. In (20) the possessive plays no role in the situation, i.e. cannot be mapped onto one of the actants. Whereas in (29) it is crucial. Excision requires an actant to "motivate" the Instrument. (50) is an intermediate example. Having just given an analysis in which Bill's point is treated as a unit, I will now show that it can be treated as complex with a subtle change in meaning.

Considering the reading of (50) in which the subject Harry is in the Experiencer case. There is another case frame for (50) besides IE also having Experiencer as subject. We can conceive of Harry as the receiver in (45). Such a choice determines a new distribution of nouns and actants, call it J. As in IE and IA example plays the messenger role. But J has Bill in the sender role and point as the message. There is a natural case frame for J, point being Object, Bill Source and example Instrument. Assigning Bill to Source corresponds to our intuition that Bill is not "doing" anything in (50). Since senders are often Agents, however, there is a related causative version of the situation J, such as (51).

(51) Bill made Harry see his point through the example.

Of course (51) is not a paraphrase of (50). Fillmore's case hierarchy correctly predicts that Bill cannot be Agent in (50).

It remains to characterize the difference between the J and IE readings of (50). In my dialect the J reading describes mental reflection and the IE reading describes an interaction occurring in real time. There are two kinds of cognitive see, one that is fast and one that occurs in slow motion. On the IE reading the presence of the Goal case emphasizes the completion of the action. But the case frame for the J reading has no Goal, but rather a Source and Object. Separating Bill from his point is a step away from the immediacy of the IE situation in which Bill is present in the Goal. The IE situation is one in which the "sight" comes quicker than with the J situation. Needless to say I do not have any way of expressing this difference as a formal rule. It is of more than passing interest however, to notice that the most non-standard versions of this interaction with respect to causality have the J distribution of nouns and actants, namely (51) and the J reading of (50). This lends some support to recognizing a category of non-standard causation.

I would like to conclude with a summary of the goals and scope of what I have been calling frame semantics. My basic claim is that metaphorical uses of motion verbs exhibit certain regular relationships to the literal sense of these verbs. The graphs I have been using to characterize the invariant elements are suggestive heuristic devices for repre-
sentiing case frame variations. To a certain extent rules describing metaphor structure can be given. Doing this requires some broadening of case grammar. In the future I think it would be profitable to look at the Benefactive case from this point of view. This case seems to depend upon global properties of interactions, so perhaps the frame perspective would allow a description of some of the constraints. Graphs of interaction and frames are a new and still unexplored tool. Still they allow us to describe phenomena which are ubiquitous, but which have eluded linguistic theory. This alone is sufficient reason to recommend them.

FOOTNOTES

1. This work was supported by NEH grant F-74-156. I have pro-
fitted greatly from conversations with Charles Fillmore, Wallace Chafe, and Leonard Talmy. Rene Thom has also been generous with his time. No one but the author, however, is responsible for the contents.

2. I have fudged on the exact specification of the basic frame of see-through, because one might want to say that the obstacle sense of the Messenger actant triggers an Agent in the sender role if that actant is animate. This point of this proposal is that only with obstacles can the sender be described as "doing" something, i.e. being an Agent. I have not explored this possibility.

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GREEK APOCOPE - A RULE THAT PLANS AHEAD*

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0. Introduction

This paper deals with a rule that is very powerful in two different ways. It looks forward to avoid producing outputs that the next rule cannot handle, and it looks ahead to the surface to avoid producing disfavored clusters. The rule is traditionally known as apocope (Ap) in the handbooks of ancient Greek. In its simplest, and, I believe, ultimately correct formulation, apocope deletes a vowel at the end of a preposition or preverb before the initial vowel or consonant of the next word, if the two words are sisters. I will assume this syntactic relationship has its phonological reflex in the presence of a single word boundary.

\[
\text{Apocope} \quad V \rightarrow \emptyset / \quad \# \quad [\text{seg}]
\]
\[
\begin{array}{l}
[\text{preverb}] \\
[\text{preposition}]
\end{array}
\]

Thus we find:

\[
\text{kata}^{h}\text{tît}^{h} \text{e}^{h}:\text{mi} \quad \text{alternating with} \quad \text{kat}^{h}(\#)\text{tît}^{h} \text{e}^{h}:\text{mi}
\]

's down-put'

\[
\text{kata}^{h}\text{tôus nômous} \quad \text{alternating with} \quad \text{kat}^{h} \text{tôus nômous}
\]

't according the laws'

There is some reason to believe that some weak, word-like boundary exists even between preverbs and their verbs (although they are graphically one unit), since the preverb can be scrambled away ('tmesis').

The vowel-dropping phenomenon is uninteresting when it occurs before another vowel, but the facts get more intriguing when we consider what sort of consonants the Ap rule allows to end up adjacent, and what happens to these clusters before they reach the surface.

In ancient Greek, the two consonants which Ap brings together must become a geminate, through the operation of a regressive assimilation rule (RA), even if the unassimilated cluster is permissible word-internally. The RA rule, in its most general form, looks like this for obstruents:

\[
\text{Regressive Assimilation} \quad C_{1} \rightarrow C_{2} / \quad \# C_{2}
\]

(obligeratory)

For instance, if we apply apocope to the word apokope: 'cut off' (literally 'off-cut') we will get *apkope: which then obligatorily becomes akkope: by RA.

We get different outputs from consonant clusters at weaker boundaries\(^2\) so we know the rule must make reference to #. It is hard to find examples in attested Greek where RA applies without
the previous application of Ap to feed it, because Common Greek had a rule deleting all word-final obstruents except s. It is rare then to find two obstruents adjacent across a single word boundary in underlying form. However, as we will see in section 2.1, it can be fairly conclusively proven that Common Greek did have a rule of RA. Furthermore, RA also applies to sequences of n or r followed by #C, and these cases do arise in synchronically underlying forms of attested Greek.

l. Restrictions on the application of apocope

The apocope rule might be presumed to apply generally, since all it does is remove a semantically unloaded, unstressed vowel. But in fact it doesn’t. There is a whole complicated set of restrictions on the phonological domain of its application in the five dialect groups of ancient Greek. These restrictions can be divided into two conditions which may apply more or less stringently, according to the dialect:

(C1) The first consonant (the one before the doomed vowel) must be a dental.

(C2) The consonants which will end up adjacent must be homorganic.

Peculiar conditions on a vowel-dropping rule, but more on that later. If a dialect requires that both conditions be met, relatively little apocope will take place, and, consequently, fewer geminates will be produced. If only one condition must be met, there will be correspondingly more apocope and gemination; if fulfilling either condition will do, there will be still more. The dialects can be quite neatly divided in the way they impose these conditions. In Attic-Ionic, apocope never gets to apply (except in the uninteresting prevocalic environment.) In most of Western and Southwestern (Doric) Greek, (C1) must obligatorily be met, and (C2) acts as a part of a variable rule, so that if it is fulfilled the rule applies more frequently. So only prepositions of the form ...TV- lose their final vowels, and they lose them most often before a word starting with another dental. However, a few Western dialects and Arcadian drop (C2) entirely, and will apocope any vowel preceded by a dental, no matter what the following consonant may be. Proceeding along a hierarchy of increasing application of apocope and geminate-production, we come to most of the Aeolic dialects. These stipulate that either (C1) or (C2) must be met; that is, any vowel after a dental will drop (regardless of what consonant follows) and a vowel after a labial will also drop if the following consonant is labial. (There are no prepositions of the form ...KV-.) Finally we come to the Aeolic dialect Thessalian, which ignores both conditions and will apocope anything anywhere. Perhaps a chart of these restriction will make them more manageable:

<table>
<thead>
<tr>
<th>Type</th>
<th>Example</th>
<th>Apocopates In</th>
</tr>
</thead>
<tbody>
<tr>
<td>dental V#dental</td>
<td>kata tas</td>
<td>Western, Arcadian, all Aeol.</td>
</tr>
<tr>
<td>dental V# C</td>
<td>kata ballo</td>
<td>some West., Arcad., all Aeol.</td>
</tr>
</tbody>
</table>
type labial V#lab labial V#C
example apo ballo apo tas
apocopates in all Aeolic Thessalian only

As we said, Attic-Ionic, the dialect one learns in Greek classes, has no apocope of the preconsonantal type at all. The very early Greek attested in the Mycenaean inscriptions in Linear B, although related to Arcadian, does not appear to have Ap either, although such things are hard to be certain about in that script.³

The important things to abstract from the above mass of data are:

(1) The vast majority of dialects only allow apocope if the first consonant in the cluster that will be produced is a dental.

(2) There is a hierarchy of frequency and generality of apocope's application, with Attic-Ionic at the bottom and Thessalian at the top.

The significance of (1) will soon become apparent. We will deal with (2) in section 3.

2.0 The regressive assimilation rule

Ap produces new clusters at single word boundary, and these must undergo RA. Several questions about RA present themselves. Is it demonstrably a separate rule form Ap? Do all the dialects, including those without Ap, have it? Can it assimilate any consonant to any other consonant, or is it less powerful than that? What did it look like in Common Greek? Since, as we mentioned above, Greek lost final obstruents quite early, it is necessary to go back into prehistory to tell what RA was like when it had input other than what Ap gives it.

2.1 The Common Greek regressive assimilation rule

There are several forms occurring in attested Greek that can only be explained as the result of the assimilation of a Common Greek t or d to a following consonant. Thus:

| hopposios | hod-pos | 'the which' |
| hoppos | hod-ti | 'the how' |
| hotti | hod-ka | 'the what' |
| hokka | from | 'the when' |
| pokki | pot-ki | 'for what' |

',p-p'i

These are all collocations where one expects single word boundary. Hod seems to have acted as an enclitic, pot is in fact probably the apocopated form of a preposition, and ',p-p'i is an archaic case ending which, preserved in Sanskrit, behaves as if separated from nouns by single word boundary. Notice that in all these rules, RA is dealing only with dentals as focus. No other consonant is assimilating. This is, in a sense, an accidental gap, in that Greek never had many consonant-final clitics it could juxtapose. But I do think that the list is representative of all the types of C#C clus-
ters that actually arose in Common Greek. I do not believe that anything other than a dental was likely to have formed the first member of a C\&C cluster. We must now ask if this meant that the Common Greek regressive assimilation rule could only handle dentals. This seems the reasonable conclusion to draw, though it raises the question of whether a speaker, confronted with data that a weak but general rule can handle, will posit the weak rule or the most general possible formulation of that rule, lacking any evidence of its total generality. Did Common Greek have a rule assimilating dentals to a following consonant over word boundary, or one assimilating all obstruents? I opt for the former, given the generality of the rule in the limited formulation (not just voiceless dentals, for example) and the absence of any evidence of greater strength. This conclusion draws support from the fact that word-internal obstruent clusters in Greek may not begin with a dental, only with a labial or a velar. It would therefore make sense for the language to adopt a rule which could eliminate dentals from C\&C clusters but which would make no mention of other obstruents, since these were acceptable as the first members of other clusters.

2.2 Regressive assimilation in Attic-Ionic

Another piece of evidence for the existence of a rule of RA in Common Greek, separate from apocope, is that such a rule continues to exist in Attic-Ionic, although that dialect group has no apocope. Not only does Attic-Ionic have the reflexes of the C\&C clusters listed on the previous page (with the geminates simplified), it also has some word-final consonants that the obstruent-deletion rule did not affect: s, r, and n. We can therefore find prepositions in Attic-Ionic like syn 'with' which undergo the same process before consonants as apocopated prepositions like an from ana 'up' in the other dialects. The reflexes of an assimilated sonorant are not quite as simple of those of an assimilated stop. Before liquids we get complete assimilation: syn\#le\text{go} \rightarrow syl\text{le}go just as (ana\#le\text{go} \rightarrow ) an\#le\text{go} \rightarrow alleg\text{o}, Before obstruents, only place assimilation takes place: syn\#pat\text{ho} \rightarrow sympat\text{h}o just as an\#pauo \rightarrow am\#pauo. Clearly, this must be the same rule in Attic-Ionic as the one we have termed RA elsewhere.

Attic-Ionic therefore provides evidence that RA is a separate rule from Ap. It also provides no counter-evidence to the claim that RA refers to dentals only as the focus. Notably, the Attic-Ionic preposition ek\text{(s)}, which, preconsonantally, appears with final k, does not assimilate. We find ekpat\text{h}o, etc.

2.3 The global relationship between apocope and assimilation

If we now go ahead and assume that the RA rule the dialects inherit referred only to dentals, a striking similarity between the most usual restriction on apocope and the possible inputs to RA presents itself. Ap removes only those vowels which stand between consonants the inherited RA rule could handle, should they become adjacent! We might still choose to list the restrictions in the environment of the apocope rule itself:
Restricted Apocope \( V -- \emptyset / C \)

\[ [\text{alv}] C \]

\[ \langle [\emptyset \text{ features}] \rangle_{C_2} \]

But are we not making a counter-intuitive claim here? Apocope (dropping vowels before consonants) in all the languages in which I have seen it discussed, is an allegro-speech phenomenon, concerned with the deletion of unstressed, functionally unloaded vowels. The fact that Greek apocope is optional, that it makes reference to close syntactic relationships, and other possible properties (see ftn. 5) identifies it with this class of rules. To put restrictions on the sorts of consonants in its environment is to imply that those consonants (and, if C2 applies, their interrelationships) are actually conditioning the dropping - or, even worse, that the Ap rule is functioning in order to bring these two consonants together. This latter position is not, in fact, a straw man. Lejeune (1955) has explained the restriction to homorganic consonants (C2) as due to the fact that the vowel is getting in the way of the formation of a geminate. The absurdity of such an assertion may become more apparent if we apply it to a language we have more intuitions about, like Italian. Italian also has preconsonantal apocope rule. Since almost every Italian word ends in a vowel, producing an even more un-clustered surface configuration than Greek's, the idea that Italian is deliberately seeking derived clusters seems ridiculous. What we have, more reasonably, is a conflict of goals. Italian (and Greek) likes words to end in vowels, but it also likes to get rid of unstressed, unfunctional vowels in informal speech.

So what I am claiming is that apocope is essentially a context-free, allegro speech phenomenon. It compromises, however. It restricts its application, despite the actual irrelevance of the surrounding consonants, to those environments which the succeeding application of RA can fix up. It makes very little sense to say that vowels are more susceptible to loss after dentals than after some other segment. It does make sense to say that a RA rule may only be strong enough to affect one class of segments, those which elsewhere in the language are not permitted to begin a cluster. The conditions we have stated on apocope are therefore, really restrictions we should state on RA. We may continue to put them into the structural description of Ap, but this is essentially to cover up the real state of affairs. Apocope looks ahead in order to avoid producing unassimilable clusters.

In some ways, this is a revolutionary sort of rule to propose. Kiparsky (1973) has proposed that the only global rules in phonology look backwards, perhaps backwards only to the underlying form. Very few instances of forward-looking (peeking) rules have ever been mentioned in the literature as such. But, in another sense, the proposal is not very iconoclastic at all. Dressler (1972) has suggested that in shifts from formal to faster, more informal speech, rules may become global (though all his instances are, again, backward
types); more to the point, Zwicky (1972) has noted that fast speech rules may either have their unacceptable outputs fixed up before they reach the surface or apply less generally. In Greek, apocope makes both sorts of compromise with the surface. What can get fixed gets fixed, what is irreparable is not produced. In section 3 we will see another sort of compromise apocope makes with the surface configurations of the various dialects. If one should balk at the weakest spot in this argument about the relationship between apo- cope and regressive assimilation — namely the proof that regressive assimilation is actually too weak to handle clusters that do not begin with a dental — one is still left with a rule that is in principle concerned with the input to the next rule. For apocope will have to specify in its environment exactly those segments (the consonants) that have everything to do with the operation of assimilation and nothing to do with the phonetic likelihood of dropping a vowel.

2.3 The dialects with apocope occurring in environment after other than a dental are easily explained if we think of them in terms of generalized regressive assimilation rules. Those which allow Ap to bring any consonants together so long as they are homorganic have generalized the original RA rule to one of manner assimilation for all clusters. This is easily done, since manner assimilation was already needed in the original rule for sonorants. (Section 2.2) Thessalian has generalized the rule to effect both place and manner assimilation for all consonants. Apocope, which is by nature completely general, then allows more types of clusters to get through as the RA rules become equipped to handle them. If we assume that the original RA (and Ap) rule(s) were general, then it is hard to understand why the dialects would have put such unlikely restrictions on them. Thessalian almost surely represents the dialect that has undergone the most change.

3.0 A slippery conspiracy

There is another, perhaps even more mysterious and hard to pin down, way in which apocope manifests its concern for the future. Not only does it seem to care what the next rule will do, it also seems to participate in a compromise with each of the dialects' tolerance for the presence of geminates on the surface. To make the claim more specific: there is a hierarchy of increasing tolerance for and production of geminates which matches up to an astonishing extent with the other hierarchy of increasing application of apocope. Recall the chart of increasing application of apocope in section 1. Attic-Ionic was lowest down, with no Ap at all. Then came Western Greek, then a few anomalous Western dialects (Cretan, Laconian, and Elean), then most of Aeolic, and, finally, Thessalian, with unrestricted apocope. The result of applying apocope is, ultimately, the production of geminates. Now let us compare this hierarchy with the presence or absence of geminates from other sources in the dialect groups we have defined. Attic-Ionic, first of all, avoids geminates the most of all the dialects. This is not to say it has none. But where all other dialects have inherited geminates or have reflexes of various
clusters that emerge as geminates, Attic will often have eliminated
the geminate. One example is the old *mod- compounds discussed in
section 2.1. These have geminates everywhere but in Attic-Ionic. An-
other is reflexes of *ss and *ty clusters, which emerge as ss
in most Greek, but as s in Attic-Ionic. Furthermore, Attic-Ionic
has no rules which produce geminates.

The next group, Western Greek, is like Attic-Ionic in having
no geminate-forming rules. But it does allow those geminates pro-
duced by very old rules like hod- compounding to remain, and it
does also allow all the inherited geminates to continue unmolested.
This is the group which allows a very limited number of geminates
to be produced by a restricted apocope rule.

Elean, Cretan, and Laconian, next on the apocope hierarchy,
have introduced some new rules that actually produce some new ge-
minates. What is elsewhere written zd is written dd in these dia-
lects, though this may simply be a graphic difference. More con-
vincingly, pt, kt, and st emerge as tt in this group.

The alert reader will notice that Arcadian was lumped in with
these few Western dialects in the apocope hierarchy. Arcadian does
not, however, produce geminates like they do, and would at first
blush appear to be an exception to the proportion I am buildling up.
However, Arcadian simplifies the geminates it produces with its
fairly free apocope, and therefore causes less conflict with its
surface dislike of geminates than it otherwise would.

The Aeolic dialects have as reflexes of the very frequently
inherited sequences *s+sonorant, sonorant+*s, glide+sonorant,
geminate sonorants. That is, compensatory lengthening for the loss
of *s or *glide is realized on the consonant next to the lost seg-
ment in Aeolic. In all other Greek, it is realized on the preceding
vowel. So we find emmi from *esmi (Attic e:mi) and estella from
*estelsa (Western este:la). This gives Aeolic a very characteristic
geminate-y surface structure, much more so than even Cretan, Elean
and Laconian.

But the most striking case by far is to be made in Thessalian,
the dialect with unrestricted apocope. Thessalian, being an Aeolic
dialect, has all the geminates produced by s- and glide-loss just
described. And it has the reflexes we noted for the few Western dia-
lects: tt for pt, and dd for zd. Not only that, it has its own pecu-
liar geminate -producing process. It doubles consonants occurring
before i or y, even doubling voiced stops, the geminates of which are
very rare in the rest of Greek. We find iiddian for idian, poliios for
polios, ekklessia, proxenniou, etc. I am sure this is more than a
graphic phenomenon, for, I am told, the same process occurs in West
Germanic.

There we have it. The frequency of apocope and its concomitant
production of geminates lines up directly with the frequency of gemi-
nates allowed through on the surface from other sources. Apocope seems
to be compromising again. This time it agrees to limit its applica-
tion in accordance with the surface configuration preferred by the
dialect in question. Attic-Ionic hates geminates to such an extent that
Ap gives in completely and applies only before vowels. Thessalian appears to have no objection to geminates at all, so apocope applies without restriction. The other dialects tolerate geminates to a greater or lesser degree; Ap kindly produces more geminates to a similar degree. How this compromise might get instantiated in a grammar is a mystery to me, but the case seems quite strong that it has, particularly for the dialects at the extreme ends of the hierarchy.

4. Conclusion

It has been noted before, notably by Zwick, that rules of casual speech tend to apply less generally if their output is disfavored on the surface. In this paper I have made this observation more concrete and more formal. The Greek rule of apocope looks ahead to avoid producing irremediable clusters, and it looks to the surface to avoid producing outputs not in accord with the general surface configuration of the dialect.

FOOTNOTES

*I would like to thank Joachim Schindler for his valuable help on this paper. The usual disclaimer of course holds.

1. The handbooks generally treat the prevocalic case under the separate rubric of 'elision,' but I see no reason offhand to treat the two cases separately. If elision is a separate rule, it makes little difference to my argument, in any case.

2. For example, word-internal *nr comes out as ndr, but remains nr when the consonants are separated by a morpheme boundary. *p+t/ remains as such (as in the 3p sg pf of labial-final verb stems), but must assimilate to tt when # is the boundary between p and t.

3. See Chadwick and Baumbach (1963) for a fuller discussion. There are no certain cases of apocope in Myc., but a few have been proposed. It is not altogether clear that the writing system would have reflected apocope in the spoken language, even if there had been any, so we can draw no conclusions about dating of the inception of Ap.

4. There are other equally plausible explanations for the failure of ek(s) to undergo RA. The s may be present in the underlying form at the time assimilation would apply (not too likely); or RA may fail to apply to avoid homonymy with the past tense prefix e-, the preposition en-, etc. I said 'equally' but I don't believe it.

5. See Dressler on Breton (1972) and Latin (1973), Zwick on Welsh (1972). Other properties of Greek apocope held in common with accepted allegro rules are (possibly) inhibition when homonymy will result (such as ana 'up' being confused with a(n)'not'c and phonetic output constraints irrelevant to the rule itself (as in Breton vowel-dropping.)
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Conventional Implicature in Montague Grammar

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In this paper we try to spell out some of the content of the notion of conventional implicature, and we propose to treat many of what have been called presuppositions as conventional implicata.

The notion of conventional implicature is due to H. P. Grice (1975). We hope that we are using his term in the original spirit, but we cannot, of course, be absolutely sure of this. In any case, we try to integrate this notion with the syntactic and semantic framework of Montague grammar.

An implicature in Gricean terms means the following. If the uttering of a sentence $S$ in a given context licenses the inference that $p$, although the proposition $p$ is something over and above what the speaker actually says, then he has implicated that $p$ and $p$ is an implicatum of the utterance of $S$. Grice discusses two kinds of implicatures: conventional and conversational. The latter sort is intimately connected with his notion of cooperative conversation, in which the participants observe certain conversational maxims. Grice himself is primarily interested in conversational implicatures. He has very little to say about conventional implicatures, which is the kind we are going to discuss.

Conventional implicatures arise, not from the interplay of what is said with the conversational maxims, but from the conventional meanings of words that are used to say it. Grice gives the following example.

If I say (smugly) "He is an Englishman; he is, therefore, brave," I have certainly committed myself by virtue of the meaning of my words, to its being true that his being brave is a consequence of (follows from) his being an Englishman. But while I have said that he is an Englishman, and said that he is brave, I do not want to say that I have said (in the favored sense) that it follows from his being an Englishman that he is brave, though I have certainly indicated, and so implicated, that this is so. I do not want to say that my utterance of this sentence would be, strictly speaking, false should the consequence in question fail to hold.

So some implicatures are conventional --

Grice 1975, p. 66

One typical characteristic of conventional, as opposed to conversational, implicatures is that in the case of conventional implicature the implicatum is detachable from what is being said. It is possible to find another way of saying the same thing which does not give rise to the implicature. In Grice's example, the speaker could have expressed the same proposition by uttering
"He is an Englishman and he is brave," which does not implicate that one follows from the other.

In the following, we will identify Grice's notion of what is actually said with the logical form of the sentence that was uttered. To say that the two sentences cited above have the same logical form is to say that they translate to equivalent formulas in some model-theoretically interpreted language, such as Montague's intensional logic, which in turn means that they express the same proposition.

Conventional implicatures seem to us to a large extent co-extensive with phenomena that have been called "pragmatic presuppositions". For the purposes of this paper we identify the two notions; however, we certainly do not claim that everything that has been called a case of pragmatic presupposition is a case of conventional implicature. More likely, the former term, as it has been used in the literature, covers a heterogeneous class of phenomena. In any case, what is said above about Grice's therefore-example, which is the only one of this kind that he discusses in detail, applies equally well to sentences with words such as manage, fail, again, even, etc., which are said to induce pragmatic presuppositions.

For example, it is clear that whoever utters

(1) John managed to find a job.

commits himself to the view that it isn't easy to find a job, or at least not easy for John. By asserting (1) the speaker warrants the conclusion that finding a job must have taken some trying, some directed effort on John's part. But this is not what the speaker actually says when he utters (1). If it wasn't so, he would not have said anything false. The truth of (1) depends solely on whether John actually found a job, the rest is a conventional implicatum to which the speaker commits himself by using the word manage. Had he chosen not to do this, he could have expressed the same proposition by uttering (2) instead of (1).

(2) John found a job.

In addition to particular lexical items, conventional implicatures may also be associated with certain grammatical constructions, such as the cleft construction in English. For example,

(3) It wasn't Rosemary who got the job.

commits the speaker to the view that someone got the job. This is what he conventionally implicates by using the cleft construction to express the proposition that Rosemary didn't get the job. Thus we do not require that conventional implicatures be always attributable to the meaning of individual words.

We also allow for the possibility that what is conventionally
implicated by uttering the sentence may, at the same time, be semantically entailed by it. For example, we assume that by uttering

\[ (1) \text{It was Rosemary who got the job.} \]

the speaker says that Rosemary got the job and conventionally implicates that someone got the job. In this case, the conventional implicatum is also semantically entailed by the proposition that the sentence expresses.

To round out the picture of pragmatic presuppositions as conventional implicata, we can show how they play a role in the ordering of discourse in terms of a notion which is often used in defining pragmatic presupposition. This is the idea of common ground.

Imagine a group of people engaged in an exchange of talk. At each point in their conversation there is a set of propositions that any participant is rationally justified in taking for granted, for example, by virtue of what has been said in the conversation up to that point, what all the participants are in a position to perceive as true, whatever else they mutually know, assume, etc. This set of propositions is what we call the common ground or the common set of presumptions.\(^2\) In the course of the conversation these presumptions may change; indeed, if the purpose of the conversation is to exchange information, enlarging the common ground may be thought of as one of the participants' goals. When a participant says something, thereby advancing the conversation to a new point, the new set of common presumptions reflects the change from the preceding set in terms of adjunction, replacement, or excision of propositions depending on the exact relation of what was said to the previous common ground.

Definitions of pragmatic presupposition that have been given in the literature make use of the notion of common ground in the following way (e.g. see Karttunen 1974).

\[ (5) \text{Sentence S pragmatically presupposes proposition p} \]

\[ = \text{def} \]

\[ \text{it is felicitous to utter S in order to increment a common ground } \Gamma \text{ only in case p is already part of (= entailed by) } \Gamma. \]

In essence, this definition says that each time the common ground is to be changed as a result of the speaker uttering a new sentence the presuppositions of this incremental sentence should be satisfied by the existing set of presumptions. This way of thinking about presuppositions gives us a very natural account of presuppositions of compound sentences, which pose a difficult problem for any purely semantic theory of presuppositions.

We now think that it is better to regard most cases of pragmatic presupposition as conventional implicatures, not as felicity conditions. However, we do not claim that the earlier approach is entirely misguided. On the contrary, we hope to show how the
import of conventional implicatures for the felicity of utterances can be explained. To do this, we want to point out the following generalization and to explain why it holds.

(6) As a general tendency, it is in the interest of the participants in a discourse to organize their contributions in such a way that the conventional implicata of the sentence uttered are already part of the common ground at the time of the utterance.

In commenting on this observation, we rely on ideas put forth by R. H. Thomason (1973) and R. C. Stalnaker (1974). The argument is based on the cooperative nature of conversation. All parties to a conversation have an interest in avoiding disruption of the flow of discourse. If an utterance makes a controversial point which is not set apart where it can be challenged directly, disruption is likely to follow. For example, if the speaker asks

(7) Did you forget to call Harry?

either one of the two possible casual answers signals the answerer's tacit acceptance of the proposition that he intended to call Harry. To disassociate himself from this proposition, he has to digress from answering the question. The conversation is disrupted because there is no simple way to indicate the rejection of the controversial proposition. If the speaker believes that the addressee might disagree with the point, he should perhaps ask

(8) Did you call Harry?

instead, to avoid potential disruption. So it is usually uncooperative to put forward in conversation a sentence to which the addressee cannot make a simple response without committing himself to something he may not wish to accept at all. As sentence (7) illustrates, conventional implicata are not set apart so they can be challenged in a simple way. Consequently, in cooperative conversation a sentence ought to be uttered only if nothing it conventionally implicates is a subject of controversy in the conversational setting. Of course, the least controversial propositions of all are those in the common ground, which all participants already accept. In the limit, then, every conventional implicatum of a sentence belongs to the common set of presumptions which the utterance of that sentence is intended to increment. This may be the source of the temptation to regard conventional implicata as felicity conditions, that is, as pragmatic presuppositions in the sense of the definition in (5).

Hitherto, conventional implicatures have not received much attention, and no formal treatments exist to our knowledge. We will try to show that they can be studied with the same explicitness and rigor that we have come to expect elsewhere in semantics.
We will of course make use of all the insights that have been obtained in the course of studying pragmatic presuppositions and we hope to make those largely informal results more precise.

To carry out this program we will need, in describing the meaning of sentences in a language, to associate with each sentence two functions, defined on assignments of values to deictic expressions. One function should map such assignments into semantic values appropriate to record the sentence's semantic content, to specify its logical form. This aspect of meaning has received study by Stalnaker, R. Montague, D. Lewis, among others. The second function should have as values propositions which specify the conventional implicata of the sentence; this is where we propose to extend current conceptions of meaning. Accordingly, our next task is to present a mechanism by which a finite system of rules can recursively associate with each of a language's infinitely many sentences the two required functions.

For this purpose, it is very convenient to make use of the framework for linguistic description developed by Montague. We adopt this approach strictly for reasons of convenience; for all we know, another descriptive framework might yield just as satisfactory an account of conventional implicature. In the following discussion, we presuppose some familiarity by the reader with Montague's theory and, in particular, with the description of a fragment of English which he presented in "The Proper Treatment of Quantification in Ordinary English" (henceforth, PTQ). Moreover, to facilitate understanding we key our examples as much as possible to that description so that only our innovations will be new to those familiar with PTQ. Rather than define with strict formality here the apparatus we are introducing, we rely on a semi-formal exposition supplemented with examples to convey our intentions.

Recall that in PTQ rules come in pairs. A syntactic rule applies certain operations to members of specified grammatical categories and determines the syntactic category of the resulting expressions. The corresponding translation rule assigns to each expression thus obtained a translation which is a function of the translations of the expressions operated on by the syntactic rule. Such rule pairs apply recursively, beginning from a list for each category of its basic members with their corresponding translations. In PTQ the translations are expressions in an interpreted formal language called intensional logic.

We will modify this to have each translation consist of more than one such logical expression, since we wish to specify not only the extension of each English phrase we generate but also its potential for conventional implicature. In addition to providing each phrase with a more complex translation than PTQ does, we shall enlarge the sets of basic expressions (lexical items) in order to discuss more examples of conventional implicature. Otherwise we shall not change the linguistic description presented in PTQ in any essential way; in particular, we leave the syntactic rules of Montague English almost completely unaltered.
One of the expressions our translation procedure will assign to each generated phrase will be called the extension expression of that phrase. It will be identical to the single expression Montague's translation assigns. As in PTQ, the extension expression of the phrases produced by a syntactic rule will be obtained by applying certain formal operations to the extension expressions of the rule's input phrases.

Another of the expressions we will assign to each generated English phrase we call the implicature expression. The determination of this expression for a non-basic phrase is a somewhat more complicated matter than with the extension expression. Our method of obtaining it reflects the general fact that the implicatures incipient in a complex phrase can arise in two ways: (i) they may be induced by the dominant functor phrase, or (ii) they may originate in the subordinate argument phrase. For example, the implicatures incipient in regret that John has failed to win are in part due to the main verb being regret that, and in part due to the complement sentence John has failed to win. Those of the former kind are obtained by letting the implicature expression of regret that apply to the sense of the complement, those of the latter variety are derived by applying the "heritage expression" of the main verb to the sense of the complement's implicata.

The third expression, the heritage expression, associated with each English phrase is needed only to facilitate the assignment of implicature expressions by helping to determine the form in which the conventional implicata of argument phrases are "inherited" by the complex phrase which is constructed from them. It is the heritage expression which determines whether a functor phrase is a "hole" or a "plug" or a "filter" in terms of Karttunen 1973.

At this point it is best to look at a detailed example. Consider the sentence

(9) John fails to win.

which says that John doesn't win and conventionally implicates that he either tries or is expected to win. The syntactic derivation of this sentence is illustrated by the analysis tree in (10), where "4" and "8" refer to the syntactic rules of PTQ that were used to derive the expression in question.

(10) \[ \text{John} \rightarrow \text{fail to win, 8} \]

\[ \text{fail to win} \rightarrow \text{win} \]

The basic lexical items in (10) are John, fail to, and win. In order to make it more convenient to present their translations, we introduce the notational conventions given in (11).
(11) John' = the translation of John
John^e = the extension expression of John
John^i = the implicature expression of John
John^h = the heritage expression of John

Thus John' = ⟨John^e, John^i, John^h⟩, fail-to' = ⟨fail-to^e, fail-to^i, fail-to^h⟩, etc. Let us define John' and fail-to' as follows.

(12) John^e = λPP(^j)  \quad \text{win}^e = ---
John^i = λP ∨ u □ u = j  \quad \text{win}^i = ---
John^h = λPP(^j)  \quad \text{win}^h = ---
fail-to^e = λP2-P{x}
fail-to^i = λP2[try-to^e(P)(x) ∨ \forall y \text{ expect}-that^e(y,^P{x})]
fail-to^h = λP2P{x}

We have left the translation of \text{win} unspecified in (12) since it is not germane to the present discussion.

Let us first examine the translation of \text{fail to}. The heritage expression, fail-to^h, guarantees that each complex phrase, such as \text{fail to win}, which is formed by combining \text{fail to} with a verb phrase inheres all the conventional implicatures inherent in the latter. It makes \text{fail to} a hole in terms of Karttunen 1973. The implicature expression, fail-to^i, is such that an implicature of trying or expectation is associated with any sentence whose main verb is \text{fail to}. Finally, the extension of \text{fail to}, fail-to^e, reflects the fact that semantically this verb behaves like a negation operator that applies to verb phrases.

In the translation of \text{John}, the extension expression comes straight from Montague; the implicature part appears in PTQ as a meaning postulate stipulating that \text{John} is a rigid designator. These do not concern us here. The only relevant part is the heritage expression, \text{John^h}, which is identical to the extension of \text{John} but has an entirely different purpose. It guarantees, among other things, that any sentence formed by combining \text{John} with a verb phrase conventionally implicates that \text{John} has all the properties specified by the implicature expression of the verb phrase.

To show how all this works, we have to present the two syntactic rules and the corresponding translation rules needed to derive \text{John fails to win} in the manner shown by the analysis tree in (10). We will start with Rule 8 that forms \text{fail to win} by combining \text{fail to} and \text{win}. For the sake of convenience, we will present the syntactic and translation rules together.
(13) **Rule 8.** If $\delta$ is a phrase of category IV//IV and $\beta$ is a phrase of category IV, then $\delta\beta$ is a phrase of category IV.

If $\delta$ translates to $\langle \delta^e, \delta^i, \delta^h \rangle$ and $\beta$ to $\langle \beta^e, \beta^i, \beta^h \rangle$, then $\delta\beta$ translates to $\langle \delta^e(\hat{\beta}^e), \exists[\delta^i(\hat{\beta}^e)(x) \land \delta^h(\hat{\beta}^i)(x)], \exists x=x \rangle$.

The syntactic part of the rule comes unchanged from PTQ. Our modified translation rule looks rather complicated but the idea is actually fairly simple. It seems best to discuss it by considering our example *fail to win*.

First of all, the rule assigns to *fail to win* an extension which is identical to what the translation of this phrase would be according to the original PTQ system. This is given in (14).

(14) $\text{fail-to-win}^e = \text{fail-to}^e(\hat{\text{win}}^e)$

The conventional implicature of *fail to win* consists of two parts. The first is formed by taking the implicature expression of *fail to* and by applying it to the sense of the extension of *win*. The second part is obtained from the heritage expression of *fail to* as applied to the sense of the implicature of *win*. For certain trivial technical reasons we need to introduce an additional variable, "$x$", in order to conjoin these two parts. The result is given in (15).

(15) $\text{fail-to-win}^i = \exists[\text{fail-to}^i(\hat{\text{win}}^e)(x) \land \text{fail-to}^h(\hat{\text{win}}^i)(x)]$

The first half of $\text{fail-to-win}^i$ says that the trying or expectation which *fail* conventionally implicates has to do with winning. The second half guarantees that all the implicatures inherent in *win* are carried on, for example, being eligible to win.

What remains to be explained is the heritage expression of *fail to win*. As shown in (13), this is given directly without any consideration of the particular phrases that are involved. Thus we get (16).

(16) $\text{fail-to-win}^h = \exists x=x$

This makes *fail to win* a plug. If there were some rule that combines an intransitive verb phrase with some other phrase in such a way that the verb phrase is treated as the functor expression, then the implicatures of the argument phrase would not become implicatures of the resulting phrase. However, this is a moot point since there are no such rules in PTQ. Hence it doesn't actually matter here what *fail-to-win* is.

We are now finally in the position to give the precise translation of *fail to win*. By making use of the equivalences and notational conventions of PTQ, we can reduce it to the form given in (17).
\[(17) \text{fail-to-win}' = \langle x, \text{fail-to-win}(x) \rangle, \]
\[\langle x, \text{try-to-win}(x, \text{win}(x)) \rangle \land \text{win}(x), \]
\[\langle x = x \rangle \]

The extension of fail to win comes out to be the same as that of not to win (assuming that this were derivable in PTQ). The implication part is the same as the extension of try to win or be expected to win conjoined with "being such as winning implicates" (whatever that is). The heritage expression, as we noted above, is a matter of arbitrary choice.

Let us now pass on to the second syntactic rule we need to use in deriving John fails to win. Our version of this rule is given in (18) combined with the corresponding translation rule.

\[(18) \text{Rule 4. If } \delta \text{ is a phrase of category t/IV and } \beta \text{ is a phrase of category IV, then } \delta \beta \text{ is a phrase of category t, where } \beta \text{ is the result of replacing the first verb in } \beta \text{ by its third person singular present tense form. If } \delta \text{ translates to } \langle \delta^e, \delta^i, \delta^h \rangle \text{ and } \beta \text{ translates to } \langle \beta^e, \beta^i, \beta^h \rangle, \text{ then } \delta \beta \text{ translates to } \langle \delta^e(\beta^e), [\delta^i(\beta^e) \land \delta^h(\beta^i)], [p \lor \neg p] \rangle. \]

As before, we leave Montague's syntactic rule in PTQ unchanged. The general form of the translation rule in (18) is identical with that of (13). This is also true of the corresponding translation rules in PTQ.

The rule in (18) makes the extension expression of a sentence identical to what its full translation would be in PTQ. This is illustrated in (19).

\[(19) \text{John-fails-to-win}^e = \text{John}^e(\text{\textasciitilde fail-to-win}^e) \]

The conventional implicature of the sentence is a conjunction, as shown in (20).

\[(20) \text{John-fails-to-win}^i = \text{John}^i(\text{\textasciitilde fail-to-win}^e) \land \text{John}^h(\text{\textasciitilde fail-to-win}^i) \]

The first conjunct in (20) is the conventional implicature created by the subject phrase, John, which is of little interest to us here. The second conjunct is formed by applying the heritage expression of John to the sense of the implicature expression of fail to win. In effect, this amounts to forming the proposition
that John has all the properties this verb phrase implicates.

As the heritage expression of John fails to win, the rule in (18) gives a trivial tautology.

(21) John-fails-to-win^h = p \lor \neg p

Since there is no rule in PTQ that treats sentences as functor expressions, this is a matter of arbitrary choice, it plays no role in the generation of more complex sentences.

The full translation of John fails to win is given in (22) in reduced form.

(22) John-fails-to-win' = \langle \neg \text{win}^e(^j), \; [\forall u \Box u = j \land
\begin{align*}
\text{try-to}^e(^j, \text{win}^e) \lor \forall y \text{ expect-that}^e(y, \text{win}^e(^j)) \land \text{win}^i(^j) & ,
\end{align*}
\begin{align*}
[p \lor \neg p]\rangle
\end{align*}

As we see in (22), the extension expression says that John doesn't win. The second part of the translation gives us a conjunction of three implicatures: (i) John is a rigid designator, (ii) either John tries to win or someone expects John to win, and (iii) John has the properties implicated by win, say, eligibility. This is exactly the result we wanted. Note that, as the example shows, any word in a sentence can give rise to implicatures; (i) is contributed by John, (ii) by fail to and its complement, and (iii) by the verb win. Note moreover that the specific form of the implicature created by fail to is influenced by the heritage expression of John, and the implicature brought in by win is influenced by the heritage expressions of both fail to and John.

At this point it is time to recall what it is that we are doing here. We start from the assumption that a lexical item may contribute in two ways to the meaning of a sentence in which it occurs. First of all, it may help to determine the logical form of the sentence, what it is that the sentence literally says. Secondly, a lexical item - as well as certain grammatical constructions - may be a source of implicatures. By asserting the sentence, the speaker commits himself equally to the proposition that the sentence expresses (logical form) and to the proposition that the sentence conventionally implicates (implicature expression), as well as to all of the propositions that these jointly entail.

By using the framework of Montague grammar, we have outlined a theory which treats both aspects of meaning equally explicitly. In a forthcoming paper we will expand our remarks to cover all of Montague's PTQ. We will also discuss a wider variety of conventional implicatures, for example, those that accompany words such as again, whole, regret, fortunately, etc.

As a final example of what can be done along these lines, consider the verb phrase adverb almost, for which we give the following translation.
(23) \( \text{almost}_1 = \langle \lambda \varphi \text{almost}_2^e(\varphi \text{P}(x)), \lambda \varphi \neg \text{P}(x), \lambda \varphi \varphi \text{P}(x) \rangle \)

Syntactically we treat \text{almost} here as a verb phrase adverb, let us call it \text{almost}_1. Consequently, the extension expression in (23) is of the type appropriate to translate a member of syntactic category IV/IV. We assume that there is a related sentence adverb, \text{almost}_2, whose extension is made use of in specifying the extension of \text{almost}_1. \text{almost}_2^e ought to mean something like "come close to being the case". As the implicature expression of \text{almost}_1 tells us, asserting that something almost has a given property implicates that it doesn't (cf. barely, which gives the opposite implicature). The heritage expression of \text{almost}_1 tells us that this adverb is a hole; all implicatures of the phrase it modifies are inherited by the resulting phrase.

The syntactic rule of PTQ that generates phrases such as almost win and almost fail to win is given in (24).

(24) Rule 10. If \( \delta \) is a phrase of category IV/IV and \( \beta \) is a phrase of category IV, then \( \delta \beta \) is a phrase of category IV.

If \( \delta \) translates to \( \langle \delta^e, \delta^i, \delta^h \rangle \) and \( \beta \) to \( \langle \beta^e, \beta^i, \beta^h \rangle \), then

\( \delta \beta \) translates to \( \langle \delta^e(\beta^e), \exists \delta^i(\beta^i)(x) \land \delta^h(\beta^i)(x) \rangle \), \( \exists x = x \).

As in PTQ, the translation rule in (24) is identical to the corresponding rule in (13), and its general form matches that of the translation rule in (18).

With the three syntactic rules we have discussed, we can derive (25) in the manner shown in (26).

(25) John almost fails to win.

(26) \[
\begin{array}{c}
\text{John almost}_1 \text{ fails to win, 4} \\
\text{John} \quad \text{almost}_1 \text{ fail to win, 10} \\
\text{almost}_1 \text{ fail to win, 8} \\
\text{fail to win} \\
\end{array}
\]

The resulting translation is given in (27) in reduced form.

(27) \( \text{John-almost}_1 \text{-fails-to-win'} = \langle \text{almost}_2^e(\neg \text{win}_e(\neg j)), \text{[win}_e(\neg j) \land \text{John-fails-to-win}_1], [p \lor \neg p] \rangle \)

As (27) shows, (25) commits the speaker to the view that (i) John comes close to not winning, (ii) John wins, and (iii) all the implicatures of \text{John fails to win} hold, in particular, John tries or is expected to win.
It is important to realize that this intuitively correct result depends crucially on distinguishing between the logical form (extension expression) and the conventional implicatures of fail to win. As the extension expression in (27) reflects, the proposition which is asserted by (25) to be a close call is simply that John doesn't win. If John loses resoundingly, then even if the additional propositions implicated by John fails to win come close to being true, (25) is false. By the same token, the implicature of falsehood arising from almost applies only to the proposition that John doesn't win. The implicata of John fails to win are shared by (25), rather than the contrary. So clearly it would not do to suppose that all conventional implicata of the sentence are semantically entailed by it, and accordingly to regard the meaning of fail to win simply as a compound property, conjoining the extension with the implicature presented in (17).

The capability to deal with facts of this sort provides strong support for the mode of analysis we are proposing. By deepening and extending this explicit account of conventional implicature we anticipate that a number of long-standing problems about presupposition can be solved.

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Footnotes

* The first author's research for this paper was supported in part by a workshop on the semantics and syntax of non-extensional constructions conducted by the Mathematical Social Science Board and the second author's research by the Institute for Advanced Study.

1 In a paper in this same volume, L. Coleman points out that the implicature associated with manage may in fact be less specific than what we here take it to be. She gives persuasive examples which indicate that manage can implicate a number of things ranging from trying and difficulty to mere unlikelihood.

2 R. C. Stalnaker (1974) prefers the term "presupposition" for what we call "common presumption". Our notion of common ground is similar to his concept of presupposition set.

3 We do not mean that (6) is a principle governing cooperative conversation. It merely summarizes a trend in the conversational practice of talkers who are cooperating rationally. This tendency is, we maintain, a result of people generally observing a Gricean maxim of manner, perhaps: Set points which may be controversial apart; say them so that they can be easily challenged. Some reasons for supposing this are sketched in the text below.

4 This is a very rough attempt to specify the conventional implicatures of fail. We are presenting it only to make it possible to give a concrete example of how conventional implicatures of
complex phrases are derived in accordance with our rules. It has been pointed out to us by L. Coleman, J. J. Katz, J. D. McCawley, and others that it is difficult to pin down exactly what the implicature of fail is. Perhaps fail implicates no more than that there is some reason for someone to take seriously the possibility that the proposition in question might be true. For an interesting discussion of this question, see Coleman's paper in this same volume.

5 We deviate here slightly from PTQ syntax by letting the rule place the adverb in front of the verb rather than the other way around.


Bibliography


MAKING IT LAST: REPETITION IN
CHILDREN'S DISCOURSE

"The counterfeit Is poorly imitated after you"
(Shakespeare) Sonnets liii

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I. Introduction:

One of the most commonplace observations in the psycholinguistics literature is that many young children often repeat utterances addressed to them. Just as commonplace are generalizations concerning the importance of this behavior to the development of language in the child. We have on the one extreme those who consider all linguistic knowledge to be obtained through this vehicle. And at the other extreme, we have those who place no importance whatsoever to the repetitions of young children.

Throughout the 60's and into the 70's the literature is dominated by studies which purport to show that language does not develop through repetition. Typically the class of repeated utterances of the child is compared to the class of spontaneous or free utterances. Over and over these studies show that, with the exception of the child's repetition of adult expansions (Slobin 1968, Brown and Bellugi 1968), repeated utterances are not longer nor transformationally more complex than spontaneous utterances. (Ervin-Tripp 1964, Menyuk 1970, Bloom 1970).

If repetition is irrelevant to language development, we are left with the question: Why do young children repeat the utterances of others with such frequency? This question has not been seriously addressed. At this point in time, we still do not understand what children are doing when they repeat a given utterance. This state of affairs exists because, until quite recently, psycholinguists have been insensitive to the status of utterances as social acts. With some exception (Bloom 1970, Weir 1970, Scollon 1974, Slobin 1968) they have focused on the form of repeated utterances to the exclusion of their function in real communicative situations. An expressed intention of this paper is to remedy this state of affairs. I present here an analysis of repetition
in child language from a pragmatic perspective. By pragmatic perspective, I mean simply one that relates an utterance to its context of use. Context, of course, is an infinitely extendable notion but can include such things as the speaker's communicative intention, the speaker-hearer relationship, the extralinguistic setting of the utterance, the linguistic setting of the utterance (e.g. prior discourse, topic at hand etc.) and other areas of background knowledge such as knowledge of conversational norms and conventions.

Data used to substantiate this presentation are drawn from a number of existing sources. However, I will rely primarily on observations carried out by myself on the spontaneous conversations of twin boys (2 yrs 9 mos at the outset). Their conversations were recorded (video and audio) on a monthly basis over a period of a year.

II. Children As Communicators:

It is no accident that the positive function of repetition in children's speech has not been investigated. For one thing, perspectives adopted in developmental psycholinguistics are heavily influenced by current paradigms in linguistics. It is only in the past five years that pragmatics has been seriously considered within the field. Secondly, within developmental psycholinguistics, there has persisted a stereotype of the child as a non-communicator. Over and over, we find attempts to set children apart from adults in their verbal activity. We are told that children are egocentric in their speech; that is, they are not interested in directing their talk to an addressee. Co-present individuals are merely used as sounding boards for the child, as the child has no interest in obtaining a response to his utterance. Furthermore, when others talk, the child experiences difficulty in attending and evaluating their communicative intentions. In short, we are told that, unlike adults, children typically do not engage in dialogue. More characteristic of their speech are collective monologues. (Piaget 1926)

With this prejudice in hand, the psycholinguist quite naturally believed that the primary motive of the child in interacting with adults was mastery of the adult code. In line with this, it was quite natural for researchers to associate repetition with this goal. Why did children repeat? Behaviorists claimed that young children repeated utterances as an attempt to produce the same utterance themselves. That is, they repeated because they wished to imitate the adult form of an utterance. Repetition in the speech of young children
became strongly associated with imitation. In fact, throughout the rationalist counter-argument, the association of repetition with imitation was never challenged. It was tacitly accepted that children repeated as an attempt to copy a prior utterance; what was denied was that the attempt was successful or a means by which mastery was obtained.

Notice here that contextual grounds have subtly entered into the psycholinguist's categorization of repetitions as imitation. The psycholinguist perceives these repetitions as imitations because the repeater is a young child and the initial speaker is an adult. Constrained by the current paradigm, the relationship is translated into that between master of the code and learner of the code. This is important to note as most psycholinguists try to define imitation in terms of repetition alone. That is, they try to treat imitation as a formal relation between two utterances and not as a social act.

I have argued in a earlier paper (Keenan 1974b) that attempts to define imitation on formal grounds alone have been unsuccessful and inconsistent. The constraints on what counts as a repetition vary enormously from investigator to investigator. Rodd and Braine (1970), Freedle et al (1970), Ervin-Tripp (1964) for example consider only immediate responses to an utterance as possible imitations. Bloom (1974) on the other hand, is willing to look to the next 5 to 10 utterances for a candidate imitation. Then there is the problem of cross-utterance similarity. Just how much of the initial utterance must be repeated in order for it to count as an imitation? For many investigators, the repeated utterance could omit but not substitute items of the initial utterance. Further, the repeated utterance had to be a more or less telegraphic version of the adult string, omitting the function words but retaining some or all of the content words. For other investigators (Rodd and Braine 1970), it was sufficient that the child repeat a particular construction under investigation for the utterance to count as an imitation.

On top of these practical difficulties is the fact that repetition alone is neither necessary nor sufficient to characterize imitation. It is not a necessary criterion in that attempts to copy may not in fact repeat the prior utterance. Hence there may be innumerable unsuccessful imitations—e.g. inarticulate mutterings, wild stabs, false starts, and the like—which are not repetitions in any accepted sense of the word. Note here, then, that when psycholinguists address themselves to the role of imitation in language development, they are con-
sidering only successful imitations in their data base. We have no idea whatsoever of the character of these unsuccessful imitations. We don't know what type of adult utterance is responded to in such a way, and we do not know the nature of the distortion. It is evident that even on its own terms, the imitation literature stands on shaky ground.

Repetition is not a sufficient criterion for imitation in that it is possible to find repeated utterances that are not attempts to copy. Slobin illustrated this beautifully in his 1968 article on imitation. He provides the following dialogue between Adam (2;6) and his mother:

Mother: It fits in the puzzle someplace.
Adam: Puzzle? Puzzle someplace?
Mother: Turn it around.
Adam: Turn it around?
Mother: No, the other way.
Adam: Other way?
Mother: I guess you have to turn it around.
Adam: Guess turn it round. Turn round.

In this dialogue, Adam appears first to be using repetition as a vehicle for querying a prior utterance and then as a vehicle for informing himself and/or agreeing with the mother's comment.

It is clear that all repetitions are not imitations and all imitations are not repetitions. In order to establish a given utterance as an imitation, contextual criteria must be provided as well. Further, it is not sufficient to define the context as simply that of a child interacting with an adult. We have seen that this relationship may be held constant through a variety of social uses of repetition (imitation, query, self-informing). In order to establish that an imitation has taken place, the investigator must somehow contend with the communicative intentions of the child. This is not to say that for an imitation to have taken place, the child must have the conscious intention to reproduce a prior utterance. There may be degrees to which the child is aware of his own behavior. It is only to say that the presence or absence of the intention to imitate must be reckoned with. In particular, we can not accept that a repetition overtly elicited in an experimental situation can be equated in all cases with a repetition uttered in spontaneous conversation between caretaker and child. The overtly elicited repetition counts as an imitation because the child has been asked to copy the experimenter's utterance. While this sometimes may be the case
in spontaneous conversation, we can not assume all repeats to be of this character. Claims made about the nature of repetition in the laboratory situation, then, should not automatically extend to ordinary verbal interactions between caretaker and child.

Once we address ourselves to the communicative intentions of the child, we can begin investigating a variety of interesting questions. For example, we know that children who repeat utterances increase this activity until about two and a half and then it begins to decline. It would be interesting to follow a repeater through this cycle, indicating the ways in which the repetition was used in discourse. We could begin asking in what order the different communicative uses of repetition emerge. It may be the case that the child first uses repetition to imitate and later comes to use it to perform other communicative tasks. It may be the case that, as Slobin (1973) has suggested for syntax, the child uses an old form for new functions. That is, some children may latch on to repetition quite early as a device for participating in discourse and use this device to perform novel communicative tasks. Further, it may be the case that repetition is more appropriate or more efficient for some tasks than others. For example, if you want to copy the utterance of another speaker, then repetition is a good device to employ. Similarly, if the child wishes to let his caretaker know that he has understood ("communication check") the caretaker's utterance, then repetition is appropriate. On the other hand, there are only a few types of questions one can ask by repeating all or part of a prior utterance. It may be the case that as the child becomes competent in a greater number of speech acts, he finds repetition a less and less satisfying device.

A second area of inquiry opened up concerns the differences and similarities between children who rely heavily on repetition and those who rarely repeat (Bloom 1974). The distinction has been posed in the literature as those children who are imitators and those children who are non-imitators. Addressing ourselves to the communicative intentions of children, we may discover than this dichotomy misses the mark. It may be the case that "imitators" are not in fact imitating and that all of these children do similar communicative work; they simply differ in the formal devices used to carry out this work.
III. Repetition and Prior Discourse:

I would like now to examine in some detail the varied uses of repetition in conversational discourse. In investigating these uses, I look for clues in prior discourse and in subsequent discourse. Here I consider the relation of repetition to prior discourse.

One of the characteristics of the literature on imitation is that it generally ignores the illocutionary force of the utterance that the child is responding to. The utterance repeated by the child is not described as a request for information, request for services, an assertion, a greeting, a rhyme or song. All utterances are lumped together under the cover term "model sentence". The use of this term of course reflects the general assumption that all repetitions are imitations. Furthermore, in comparing an utterance with its repetition, the investigator judges only the extent to which the repetition succeeds as an imitation. It is typical of repetitions in fact not to succeed completely. Ervin-Tripp (1964), for example, mentions that only a small percentage of the spontaneous "imitations" in her data were exact repetitions. As imitations, then, the repetitions of young children are inferior reproductions.

If, on the other hand, children are repeating not to imitate but to satisfy some other communicative obligation, then inexact repetition might be the intended not unintended desire of the child. The fact that the child, particularly the child from 2 - 3 yrs, fails to copy in entirety a previous utterance in conversation may reflect the child's competence and not his incompetence. Consider, for example, the model sentences used by Rodd and Braine (1970) in their study of imitation. In this study, the investigator directed to a child of 25 mos the sentence "Is the baby sitting down?". The child's response was "Uhhuh, baby down." Here, it is perfectly appropriate for the child not to repeat the previous utterance. In fact, it would be inappropriate for the child to produce an exact copy. Clearly, the child has grasped the communicative intentions of the investigator. The child's response shows that the child treats the investigator's utterance not as a model to be imitated but as a question to be answered. The repetition is far more successful as an answer than as an imitation.

Repetition with omissions are appropriate in response to utterances other than information questions as well. For both adult and child alike, it it appropriate to repeat just one or two words from the utterance of a conversational partner to comment attitudinally:
Example 1:
(Toby and David at 2:09 conversing with their nanny, Jill)

Jill: And we're going to have hot dogs.
Toby: Hot dogs! (excitedly)
Jill: And soup.
David: Mmm soup!

to agree with:

Example 2:
(Toby and David at 2:09 with their nanny, Jill)

Jill: And we're gonna build a fire.
David: Mmm.
Toby: Oh yeah/build fire.

to self-inform:

Example 3:
(Toby and David at 2:09 with their nanny, Jill)

Jill: And we're going to cook sausages.
Toby: cook sausages.
Jill: And bacon.
Toby & David: bacon.
Jill: And eggs.
Toby & David: eggs.

to query:

Example 4:
(Toby and David at 2:10. Toby engaged in sound play)

Toby: /diɔt/ tʃiʃu/ i/ u/bɔ/ ɔt/
David: /boʊt /

Example 5:
(Toby and David at 2:11)

David: 'My hands are cold.
Toby: /cold.

and, yes, to imitate:

Example 6:
(Toby and David at 2:09 with their nanny, Jill)

Jill: Aren't I a good cook? Say "Yes, the greatest!"
Toby: yes the greatest (softly)
Jill: That's right.
David: the greatest! (loudly).

Even in the case of explicit imitation, the child repeats selectively. For example, the child does not repeat the performative verb "say" in the previous utterance. The child has shaped the repetition to satisfy his obligations as a conversational partner. In each case the shaping reflects the child's orientation to the expectations of the prior speaker.

We have established, then, that children are sensitive to the illocutionary force of prior utterances in discourse. They repeat as an attempt to
respond appropriately to particular types of utter-
ances. I have mentioned some of these types in the
above discussion, but this mention by no means ex-
hausts the list. In addition to its usefulness in
answering information questions, commenting, affirm-
ing, self-informing, querying and imitating, repeti-
tion may be used to make counter-claims of the
following sort:

Example 7:
(Toby and David at 2:09)
David: you\silly/you\silly/you\silly/you
\silly/you\silly/
Toby: \you/you silly/you silly/you silly/
\no you silly/

Further, repetition may be used to match a claim made
by a previous speaker (Keenan and Klein 1974). That
is, the second speaker may claim what was predicated
by the first speaker holds for the second speaker as
well.

Example 8:
(Toby and David at 2:09 with their nan\ry, Jill)
David: Doggie bib. (I have) doggie bib. (See).
I have doggie bib (2X). (?) bib.
Jill: Davids got brown flowers in his.
David: Yeah.
Toby: (I) have doggie bib.
Jill: (You've got a) doggie bib.

Example 9:
David: I get them off.
Toby: I get them off.

In counter-claims and matching claims, we see
that an utterance which replicates another in form
does not replicate it in meaning. The utterances
differ in meaning precisely because they differ in
context. In each case, the meaning of the deitic
item (I, you) depends on who the speaker is and who
the addressee is. Such examples indicate the diffi-
culty involved in earlier claims that imitations
must preserve the meaning of the model utterance.
(Ervin-Tripp 1964). Preservation of meaning must
surely be the exception rather than the norm in
repeated utterances. Even if the repeated utter-
ance contains no deitic items, the position of the
utterance as a response (i.e. second pair part,
c.f. Sacks & Schegloff 1973) makes it pragmatically
distinct from the initial utterance.
In addition to the above mentioned uses of repetition, there are examples in the data of repeating to greet back, to reverse the direction of an order, to reverse the direction of an information question, and to request clarification of an utterance:

Example 10:
(Toby and David at 2:11)
David: [fae:b]/
Toby: [fae:b]/ You mean that/

In short, there appears to be no end to the ways in which cross-utterance repetition is employed in conversational discourse. Repetition is probably one of the most misunderstood phenomena in psycholinguistics. It is associated only with the language of children who, in turn, are underated as communicators. It is obvious, however, that with some exceptions, the kind of repetition described here is quite characteristic of adult speakers as well. Any of the following exchanges could appear in adult discourse:

Example 11: (Greeting)

Example 12: (Self-informing and/or displaying knowledge)
A: That's Haley's comet.
B: Ah, that's Haley's comet.

Example 13: (Agreeing)
A: That's dreadful.
B: Dreadful.

Example 14: (Matching claim)
A: I'm fat. B: I'm fat.

Example 15: (Counter-claim)
A: You're thinner than I am.
B: You're thinner than I am.

Example 16: (Querying)
A: Yes. B: Yes?

Example 17: (Answering)
A: Yes? B: Yes.

Example 18: (Reversing direction of question)
A: Well? B: Well?

Example 19: (Imitating)
A: Say 'cheese'. B: Cheese.

Example 20: (Commenting)
A: But my diet.
B: Diet schmiet. Let's eat.
What then is going on when a child repeats the utterance of a co-present speaker? Is the child learning anything about his language? Is there any way in which repetition is developmentally progressive with respect to language? We can say that in repeating, the child is learning to communicate. He is learning not to construct sentences at random, but to construct them to meet specific communicative needs. He is learning to query, comment, confirm, match a claim and counter-claim, answer a question, respond to a demand and so on. In short, he is learning the human uses of language, what Dell Hymes has called "communicative competence" (1971).

IV. Repetition and Subsequent Discourse:

I would like to turn now to the relation between repetition and discourse subsequent to a repetition. It has been often noted in the literature (Slobin 1968, Brown and Bellugi 1968) that when caretakers repeat and expand the utterances of children, they often do so a kind of "communication check". The caretaker presents his or her interpretation of the child's utterance to the child for verification.

Example 21:
(Toby and David at 2:09 with their nanny, Jill)
Toby: Gramma Ochs/
Jill: Gramma Ochs?
Toby: yeah/

Example 22:
(Toby and David at 2:09 with their nanny, Jill)
Toby: airplane/
Jill: Oh. She went on an airplane, did she?
Toby: yeah/

It is similarly the case that children repeat the utterances of adults to let them know they have understood their utterances at some basic level. (Examples 1-3 illustrate this point.) It is characteristic of some adults that they in fact wait for such repetitions by the child before proceeding with the discourse. These communication checks are not unique to adult-child interaction, however. They are also prevalent in child-child conversational discourse as well.
Example 23:
(Toby and David at 2:11)
David (putting head on Toby's bed): ..help me/
David's falling/ help me/ David's falling/
help me/help me/help me/ Its got me/ help
me/ help me/ oooo/
Toby: /help me/ /you saying help me/
(See Example 10)

Children often experience enormous difficulty in
getting their message across (Ryan 1974) and many
of them come to expect verification of their message
through repetition. In the case of Toby and David,
when verification was not expressed by a co-conversa-
tionalist, the child would solicit it.
(Keenan 1974, Keenan and Klein 1974) The child
would repeat his utterance over and over until it
was acknowledged.

Example 24:
(Toby and David at 2:10 with their nanny, Jill)
(Toby and David in the process of making a
picture:)
Toby: Put it Tobys room/
Jill: Toby's got a worm?
Toby: No/ Put it Tobys room/
Jill: Toby's what?
David: room /
Toby: Tobys room/ | (simultaneously)
Ji : Tobys room?
Toby: yeah/
David: (?)
Jill: Oh. Put it in Toby's room.
Toby: yeah /
(See example 23 for child-child interaction)

And/or the child would accompany his utterance with
an explicit request to attend and acknowledge:

Example 25:
(Toby and David at 3:00)
Toby: my big tractors coming/
David: no/ (?)
Toby: its coming/look its coming/ its coming/
David: Now its coming/ Its coming/ Its coming/
look its coming/
Toby: I see/

In short, the children observed in this study
established a convention whereby given an utter-
ance by one partner, some evidence of attentiveness
or base comprehension from the other was expected to follow. It is certainly the case that adults in our society depend on communication checks (nods of the head, eye contact, "umhum"'s etc.) in talking with one another. However, the dependence does not appear to be as extreme or as frequent as is the case for young children. For example, when one adult native speaker converses with another such speaker, he or she usually assumes that the message has been successfully decoded by the addressee. Adult speakers usually take it for granted that conversational partners "know" in some sense (e.g. are aware of) the messages previously exchanged in the course of a particular conversation. In the absence of a challenge from the addressee, a speaker can treat these utterances as shared knowledge (Givon 1974). And in subsequent discourse, he or she can consider these utterances to be known or old information.

Children, on the other hand, can not make these assumptions. Because of the production difficulties they experience on all levels (phonological, syntactic, semantic), they can not assume that their utterances have been decoded. Simply uttering a proposition does not assure that it is "shared knowledge" between speaker and addressee. Hence, what communication checks do is to precisely turn an utterance into shared knowledge. That is, when an addressee repeats (expands) an antecedent utterance, he evidences his knowledge of that utterance. Henceforth, both interlocutors can treat the propositions contained in the utterance as given or old information.

It is often the case in adult discourse that known or old information emerges as the topic of a subsequent utterance. The topic is the unchallengeable or presupposed element about which some new predication ("comment") is made. Similarly, in the discourse of young children, information made known through repetition, may serve as future topics in subsequent discourse. It is often the case that an utterance is produced by one speaker, part or all of it is repeated by the addressee, and the repeated information becomes the topic of a next utterance. For example,

Example 25:
(Toby and David at 2:10, eating lunch)
Toby: piece bread then/
David: no piece bread/ piece bread/ Its gone/
Example 26:

(Toby and David at 2:11 in bedroom)
(An alarm clock rings)
David: bell/
Toby: bell/
David: bell/ its mommys/
Toby: (?) it/
David: was mommys alarm clock/
Toby: 'larm clock/ yeah/ goes ding dong ding dong/
David: no/ no/ goes fip fip/ fip fip/

These 2 examples bring out a number of points. Example 25 illustrates the way in which the repeated information may become the topic of a subsequent utterance in the form of a pronoun. Pronouns normally refer to an established or already known referent. In this case, it is perfectly appropriate for the speaker to use a pronoun, because repetition has given the referent this status. In example 26, we see that the initial utterance "bell" is repeated and treated as the topic of the following utterance "Its mommys". Again the known information is represented in the form of a pronoun. On the other hand, the repetition of "alarm clock" later in the dialogue is incorporated directly as topic of "goes ding dong ding dong" without the mediation of a pronoun. Further, example 26 illustrates nicely the recursive nature of topic-comment sequences in conversational discourse. We see that the new information "bell" serves as old information topic for the comment "was mommys alarm clock." However, part of this predicate "alarm clock", becomes old information through repetition by the other child. Having achieved this status, it then becomes the topic of the subsequent utterances "goes ding dong ding dong" and "goes fip fip/ fip fip/". Whole stretches of discourse are linked in this way: New information is transformed into old information through repetition, yielding topics for subsequent discourse. One positive role of repetition in discourse is, then, to establish topic candidates (Keenan 1974). The topic candidates can be utilized in the discourse of either conversational partner. In example 25, the child who repeats the utterance exploits it as a topic. In example 26, we have a case in which the child who introduces the new information is the one who topicalizes it in later discourse. (David first points out the existence of a "bell" and later makes a claim about it: "its mommys" etc.)

Two additional points need to be made with respect to the role of repetition in establishing topic candidates: 1) The first is that such sequences are characteristic of many adult-child interactions as well as
child-child interactions. It is often the case that an adult will present new information, the child will repeat some or all of it, and will use it as the topic of utterances:

Example 27:
(Toby and David at 2:09 with their nanny, Jill)
Jill: Jiji's going camping this afternoon.
Toby: Oh yeah/
David: camping/ oh exciting/} simultaneously

Or the child will initiate an assertion, the adult will repeat it and use it subsequently as a topic:

Example 28:
(Toby and David at 2:09 with their nanny, Jill)
Toby: Jiji's wonderful/
Jill: Wonderful. I know it/

With respect to the earlier mentioned topic of children who are imitators and those who are not, it may be worth investigating if the so-called non-imitators engage in conversations primarily like example 28, whereas the so-called imitators engage in conversations primarily like example 27. That is, it may be characteristic of some caretaker-child interactions that the caretaker takes an utterance of a child and makes it old information through repetition, using it as a topic in further discourse. This kind of discourse would give a "non-imitative" look to the child's utterances. In other caretaker-child interactions, however, the child himself or herself may transform the utterance of another into old information through repetition ("imitating"), providing either the caretaker or the child with a topic candidate.

Secondly, now that we understand some of the work that is being carried out through discourse, we can understand more clearly the meaning of any single utterance of an interlocutor (child or adult). For example, we can retrace the history of the discourse to isolate the communicative work of an utterance. In many cases (though by no means in all cases), the first mention of a referent by a child or by an adult talking to a child is simultaneously a claim and a request to be ratified as a topic candidate. The second mention of the referent (the repetition) ratifies the information as known, and subsequent mentions take-for-granted that it is established, old information.
Furthermore, without discourse history, it would be difficult to separate what is new information from what is old information in any single utterance. That is, it would be difficult to isolate what is being asserted from what is already taken-for-granted or presupposed. The linguist can not, for example, rely on the range of syntactic cues expressing old information in adult speech. The use of pronouns to express old information is a relatively late development in child language (Bloom 1975). Further, even if pronouns are available for this purpose as in the speech of Toby and David, there still is an absence of definite articles, relative clauses nominalizations, and other syntactic means for codifying taken-for-granted information. For many children, taken-for-granted information is marked through discourse and not through syntax. Ratification of a word, phrase, etc. in discourse is sufficient in itself to establish these items as presupposed in subsequent utterances. This is the case in example 26, where "alarm clock" is the old information or topic addressed by the next two utterances "goes ding dong ding dong" and "no/no/ goes fip fip/ fip fip/". We end this paper with the hypothesis that cross-utterance repetition anticipates the syntactic marking of old information and that heavy reliance on repetition gives way once syntactic devices for topicalization emerge in the child's speech corpus.

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INTRODUCING COGNITIVE GRAMMAR

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It has been taken for granted in transformational linguistics that transformational grammars are abstract objects which are represented somehow in the mind, but which are utilized only in an extremely indirect way in actually producing and understanding sentences. However, psycholinguistic studies in the past decade have shown transformational grammars to be so remote from actual production and recognition as to be just about indiscernible. Fodor, Bever, and Garrett, in The Psychology of Language, which is as orthodox a transformationalist treatment of the subject as one can find, put the case as follows:

We have proposed a model which does not posit a very intimate relation between the form in which grammars represent linguistic structure and the form in which sentence recognizers do. In particular, we have seen that both the theoretical and experimental arguments for a perceptual model in which the grammar is concretely recognized appear dubious.

-- page 368

Given that Fodor, Bever and Garrett take "grammars" to be "transformational grammars" of the standard type, we would not dispute their conclusions. However, if one gives up on transformational grammars and instead assumes a version of relational correspondence grammars as outlined below, we believe that there is a direct and intimate relation between grammars and mechanisms for production and recognition. In fact, we suggest that grammars are just collections of strategies for understanding and producing sentences. From this point of view, abstract grammars do not have any separate mental reality; they are just convenient fictions for representing certain processing strategies. Likewise, abstract transformational derivations are just convenient fictions for representing aspects of linguistic structure. From our perspective, linguistic structure is fully determined by the cognitive representation of meaning, the form of the linguistic signal, and the processing mechanisms that relate the two. The remainder of this paper is an attempt to show how such a conception of grammar might be actualized in a primitive and highly tentative (but we hope suggestive) way.

Incidentally, the idea that grammars are just collections of processing strategies is by no means novel. It
is implicit (so far as we can tell) in Hilary Putnam's convincing rebuttal to Chomsky's argument for the innateness of purely linguistic structure. The proposal is made more or less explicitly in two papers by William Watt. In the case of recognition, various proposals of this sort are formalized in works in the field of artificial intelligence, especially in recent work by William Woods, Ron Kaplan, Martin Kay, and others. Our ideas were directly inspired by the Woods-Kaplan work on Augmented Transition Network Grammars (ATN's). Bever, Langendoen, and Kimball have also investigated processing strategies. Our dissatisfaction with their basic theoretical assumptions (although not the problems they address) will be evident throughout this work.

There is a single characteristic shared by all the diverse approaches mentioned above which both unifies them and distinguishes them from the traditional approach; the concept of incremental processing. Rather than operating on whole structures, transforming one into another, these approaches all construct structures a piece at a time, modifying and expanding them as more and more of the signal is considered. It is this procedure we would like to use as the defining characteristic of left-to-right grammar, or, less chauvinistically, cognitive grammar. Thus a cognitive grammar is one which attempts to characterize and explain grammatical phenomena in an incremental, signal-driven fashion.

We can distinguish two main types of cognitive grammars; perception grammars, where the signal is the spoken or written sentence and the incrementally constructed structure is the semantic content, and production grammars, where the signal is the progressively more detailed intended semantic content and the incrementally constructed structure is the surface utterance. It is our contention, which we hope to motivate in the balance of this paper, that it is possible to construct perception and production grammars for English which are related to each other in a principled way, and which are furthermore directly related to a grammar which looks very much like an abstract well-formedness grammar based on a relational model à la Keenan, Comrie, Postal, Perlmutter, and Johnson.

By way of a concrete introduction to the concepts discussed above, we will present a small part of a perception grammar for English and exemplify the use of that grammar by analyzing the sentence

(1) The aardvark was given a bagel by Irving.

The notion "current semantic hypothesis" is basic to this conception of recognition grammar. It derives from
a similar approach used by William Woods and others in natural language recognition grammars for use on computers. Essentially what rules of grammar do is make hypotheses about the grammatical relations borne by elements in the surface utterance as they are processed. These hypotheses may later be changed under the influence of information derived from subsequent processing of further elements. Thus we would say that in processing sentence (1) above, we first hypothesize that the noun phrase the aardvark is the underlying subject. After encountering a verb in participle form with the auxiliary be, however, we change that hypothesis and consider it to be the direct object. Finally, on encountering another noun phrase, a bagel, we reconsider once more and decide that the aardvark is really the indirect object, and that a bagel is the direct object.

Hopefully even this rough description demonstrates the possibility of a relationship between this sort of processing grammar and relational grammar rules; we have first said 'subject goes to object', then 'object goes to indirect object', which are inverse analogues of the relational grammar rules of passive and dative respectively.

A formal statement of these and the other rules of our mini-recognition grammar can be found in the left-hand column of Table I. We will discuss the other columns later. Let us emphasize right away that this is a primitive grammar; it is not meant to be complete either in scope or detail. It is intended only to demonstrate certain salient characteristics of a cognitive grammar, and thus many areas which would require extensive and detailed treatment in any adequate grammar are here glossed over or completely ignored. Specifically, the analysis of parts of the surface utterance as noun phrases is assumed, although such analysis requires a complex set of rules of its own. No attempt is made here to deal with problems of subordination of any kind. Nor are any semantic or pragmatic restrictions included. The prolific use of relations of a distinctively "Aspects" flavor (e.g., Aux and Type) is not intended as a commitment to a particular brand of linguistic theory, but rather derives from a desire to avoid areas of inquiry not central to the issue we are concerned with in this paper. In fact, it is only rules 9-11 which are central to our discussion; the others are required to give a complete grammar and are not our main concern.

The general form of the rules of the grammar is given in Figure 1. The semantic hypotheses are represented in relational terms. V designates the predicate of a sentence, A the auxiliary, T the type (i.e.,
### Background

**Relational conditions**

<table>
<thead>
<tr>
<th>Conditions on current relational hypotheses</th>
<th>New relational hypotheses</th>
</tr>
</thead>
<tbody>
<tr>
<td>Conditions on current analysis of surface string, where * indicates current element</td>
<td></td>
</tr>
</tbody>
</table>

#### Figure 1  GENERAL FORM OF A RECOGNITION RULE

Question or Declarative), and the other capital letters the various relations borne by various terms to the predicate. Arabic numerals are variables over elements in the utterance. "*" always refers to the element of the utterance currently being processed. $\emptyset$ is used to represent non-occurrence of any kind. The conditions symbolized in the lower left-hand quadrant should be interpreted as follows:

- $\text{NP}(x)$  'x is a noun phrase'
- $\text{M of } (x) = y$  'y is a marker of the role the $\text{NP}(x)$ plays in the sentence'
- $\text{FIRST}(x)$  'x is the first NP to be processed'
- $\text{Verb}(x)$  'x is in the lexical category Verb'
  (same for $\text{PastPart}(x)$ and $\text{Aux}(x)$)

#### Figure 2  WHAT THE SYMBOLS MEAN

Thus Rule R3 can be read as saying that, provided the current element is a noun phrase whose (prepositional) marker is to and that some element is currently hypothesized to be the object, hypothesize that the current element is the indirect object.

The first six rules in the grammar represent a first approximation to the language-specific ordering rules, whose existence is postulated in relational grammar but which have not been investigated yet in any detail. They hypothesize the grammatical relations borne by elements based essentially on position in the sentence. These rules alone will successfully recognize simple declarative sentences. The addition of rules R7 and R8 permit a discrimination between Yes-No questions and declaratives on the
basis of the order of appearance of the auxiliary (if any) and the subject. Rule R7 states that if the current element has been hypothesized as the subject, and no element is currently hypothesized as the auxiliary, then the sentence is of type declarative. Rules R9, R10, and R11 introduce more complexity into the grammar, by recognizing passive sentences with or without overt agents and dative sentences.

At this point we would like to discuss briefly the notion of a derivation in this kind of recognition grammar. Essentially we view our rules as well-formedness conditions for a sequence of structures, each consisting of a set of semantic hypotheses and some contiguous portion of the utterance beginning at the beginning. Thus rule R5 says that the pair of structures given in Figure 3a is a well-formed pair, given an appropriate lexicon. "*" is always considered to refer to the last element of that portion of the utterance found in a given structure, and there is a global well-formedness condition on derivations which says that the portion of the utterance contained in each structure must be non-decreasing from structure to structure; that is, "*" must either stay in place or move to the right through the utterance.

Figure 3b gives a shorthand form of the derivation of sentence (1) using the grammar we have been discussing. A derivation is successful provided it ends with a structure whose * is #, the end of the sentence, and furthermore, a predicate has been found and sufficient relations hypothesized to satisfy the set of arguments required by that predicate.

\[
\begin{array}{l}
S(1), \quad T(DCL), \quad A(2) \\
\hline
[\text{the aardvark}] = 1, \text{NP}(1), [\text{was}] = 2, \text{AUX}(2) \\
\end{array}
\]

\[
\begin{array}{l}
O(1), T(DCL), A(2), V(3), S(\Delta) \\
\hline
[\text{the aardvark}] = 1, [\text{was}] = 2, [\text{given}] = 3 \\
\text{NP}(1), \quad \text{AUX}(2), \text{PastPart}(3) \\
\end{array}
\]

Figure 3a A PAIR OF SUCCESSIVE ANALYSES IN A DERIVATION
Though these are recognition rules, strictly speaking, their content matches the content of certain rules that have been proposed as rules of abstract grammar. For instance, as we noted above R10 is clearly closely related to the dative rule as proposed in relational grammar. Our principal claim is that all of the recognition rules we have discussed (and any recognition rules of this general form) can be related directly and in a principled way to rules of an abstract grammar. The kind of abstract grammar we have in mind is correspondence grammar, as described by G. Lakoff (1970 and 1973), with the addition of the apparatus of relational grammar. Examples of such rules are given in column 2 of Table I. The general form of such rules is given in Figure 4. In an abstract relational grammar, these rules each characterize a well-formed pair of successive stages in a derivation. Each stage is a labelled dependency diagram of the sort characterized by relational grammars. Condition I of a correspondence rule (see Figure 4) corresponds to the input condition of a transformation; condition II corresponds to a condition on the output of a transformation. For example, rule A10 is our version of the relational grammar dative rule. It says that a possible pair in a well-formed derivation may have as its first member a
AE 'Affected Element' The most significant thing which is different between the first and second members of the pair.

SS 'Surface Signal' The element whose appearance in the utterance signals that this rule has applied.

COND Conditions on both members of the pair which are necessary for the application of this rule; these conditions are unaffected by the rule.

I Conditions on the first member of the pair.

II Conditions on the second member of the pair;

   a = relational conditions
   b = surfacy conditions

In addition to the notations explained in Figure 2, P(x,y) is a precedence relation which partially orders the dependency diagrams. It may be read as 'x precedes y'.

Figure 4 GENERAL FORM OF A CORRESPONDENCE RULE

dependency diagram containing a direct object and an indirect object, and as its second member a dependency diagram in which the term which is the direct object in the first member is a chômeur not marked by a preposition and the term which is the indirect object in the first member is the direct object. The affected element is that which remains a term, the indirect object. But the surface signal is the occurrence of the second unmarked NP after the verb. For example, compare (2) and (3).

   (2) John gave the dog a bone.
   (3) John gave the dog to Susan.

The occurrence of "a bone" in (2) -- the second unmarked NP after the verb -- perceptually signals the occurrence of the dative rule.

Similarly, in A9 (our version of passive) the affected element is the direct object in condition I and the subject in condition II. But what signals the application of passive is the verb in its past participial form. Thus, given the substring of (1) "The aardvark was", there is no signal of passivization; the signal first appears in the substring "The aardvark was given". Incidentally, this is the first time, to our knowledge, that the notion of "surface signal" has been suggested
in the theory of generative grammar. We think it is an important notion, worthy of considerable attention, especially in the area of universal grammar.

We should note, incidentally, that we were forced to split passivization into two parts, rules 9 and 11. It seems to us somewhat ironic that this formulation, though given in relational grammar terms, seems to share much with Chomsky's third-generation formulation of passive (which is why we have used the symbol $\Delta$ in his honor). The reason we were forced to do this is that in a recognition grammar which is strictly left-to-right, you can't process something you haven't gotten to yet; in this case, the by-phrase. Another way of interpreting the use of $\Delta$ is as an expression of a constraint imposed by the Keenan-Comrie hierarchy. That is, $\Delta$ marks an unfilled relation which must be filled. Our formulation of passive is thus different in this respect from that of Perlmutter and Postal. In their passive rule, the direct object ousts the subject, which is retired to chômeurhood by a general principle. In our rule, the subject abdicates and the direct object is promoted to subjecthood by a general principle. It is clear that our dative rule could be cast in a similar form, as could any relation-changing rule. From the point of view of abstract grammar, there seems to us to be no difference between these two approaches. However, the fact that processing considerations forced us to this formulation of passive suggests to us that our approach of abdication and subsequent promotion may be the best way of stating relation-changing rules in general.

Rules $A1-A8$ impose ordering relations on the surface forms. If we compare these abstract rules with their corresponding recognition rules, an interesting difference emerges. First consider $R2$, the object recognition rule. The expression 'V(1)' in $R2$ indicates the presence of a predicate by the point in the derivation where the rule applies. In other words, the predicate must be processed before the direct object NP, which is represented by * in $R2$. Since there is no implicit correlate of processing in the abstract grammar, an overt indication of processing order must be provided. Traditional transformational grammar provides an ordered base and transformations which utilize and modify that order. We think this is too strong. We propose instead the P-relation, which is introduced by rules. Thus, in $A2$, we introduce the P-relation between the predicate and the direct object to reflect what is in $R2$ a consequence of left-to-right processing. Moreover, we are limiting the use of the P-relation by permitting the surface signal only to be the second argument of the relation. This reduces the two-place P-relation to a one-place relation, which is equivalent to the notion "processed".
Now consider rule R7, which states that if you have processed a subject, but have not processed an auxiliary, then the sentence is declarative. The notation 'A(∅)' in R7 indicates that there is no auxiliary present at the time the subject is being processed. This is translated into abstract grammar as condition IIa in rule A7: A(1) ⇒ ¬P(1,2). This says that if there is an auxiliary in the (declarative) sentence, it doesn't precede the subject.

The general form of the correspondence between the rules of the recognition grammar and the rules of the abstract grammar is given in Figure 5.

```
<table>
<thead>
<tr>
<th>COND</th>
</tr>
</thead>
<tbody>
<tr>
<td>IIa</td>
</tr>
<tr>
<td>I</td>
</tr>
<tr>
<td>IIb</td>
</tr>
</tbody>
</table>

where * = SS
```

Figure 5  RELATION BETWEEN A RECOGNITION RULE AND A CORRESPONDENCE RULE

In accordance with the discussion above, the appearance of 'R(n)' -- where R is any relation and n is any number -- in the part of a recognition rule reserved for conditions on current or background relational hypotheses corresponds to a 'P(n,SS)' in condition IIa of the corresponding abstract rule. When 'R(∅)' appears there, a corresponding 'R(n) ⊢ P(n,SS)' appears in condition IIa.

To exemplify the use of the abstract rules, we have provided an abstract derivation of sentence (1) in Table 2 at the end of the paper. This is to our knowledge the first formal derivation in relational grammar terms to appear, and should be perused bearing its novelty in mind.

Though we know nothing whatever about production, for the sake of completeness we submit for the reader's consideration a set of production rules. Figure 6 gives the general form of such production rules, a sampling of which are found in column 3 of Table 1.

<table>
<thead>
<tr>
<th>Conditions on relations persistent from semantic structure or on present relational commitments derived therefrom</th>
<th>New derived relational commitments</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Future constraints on surface elements</td>
</tr>
</tbody>
</table>

Figure 6  GENERAL FORM OF A PRODUCTION RULE
Such rules might be interpreted as expressing the method by which that portion of the utterance produced up to a given point, together with the speaker's intentions at that point, constrain the balance of the utterance. Figure 7 gives the correspondence between these rules and rules of the abstract grammar. The P-relation translates in a way similar to that given for the recognition rules.

\[
\begin{array}{c|c}
\text{COND} & \\
I & IIa \\
& IIb
\end{array}
\]

where * = AE

\text{Figure 7 RELATION BETWEEN A PRODUCTION RULE AND A CORRESPONDENCE RULE}

This should be shown clearly in the examples provided. The question of production is so wide open at this point that our proposal can only be seen as an extremely tentative working hypothesis.

What we have done so far is to define very primitive versions of recognition and production grammars and to re-define abstract grammars of a relatively primitive sort in such a way that each rule of an abstract grammar uniquely characterizes a recognition rule and a production rule, and conversely. From this perspective, an abstract rule of grammar is simply another way of organizing the content of a recognition rule and/or a production rule. In this scheme of things, what mental reality do abstract grammars have? Only the mental reality of the corresponding recognition and production rules. They have no independent mental reality at all. Abstract grammars in such a theory are merely convenient fictions. Correspondingly, the derivations characterized by abstract grammars are also convenient fictions and are claimed in our theory to have no mental reality. Only the sets of computations characterized by the corresponding processing grammars are claimed to be psychologically real.

Perhaps at this point we ought to make clear some things that we are not claiming. We are not claiming that all perceptual strategies for natural language correspond to production strategies and/or abstract rules of grammar. For example, Bever and Kaplan have proposed strategies to account for why it is easier to process the following (b) sentences than the corresponding (a) sentences.
(4) a. The boat floated down the river sank.
b. The boat taken down the river sank.
(5) a. I told the man that my mother knew in 1968
   the story.
b. I told the man that my mother knew the
   story in 1968.

Both of the (a) cases are garden-path sentences that
require re-processing. Bever has suggested that "floated
down the river" in (4a) is first processed as the main
clause predicate, and then re-analyzed as a reduced
relative clause. Kaplan has suggested that the that-
clause in (5a) is first processed as a complement, and
then re-analyzed as a relative clause. Both claims are
subsumable under the recognition principle:

(6) Process so as to minimize islandhood.

As Ross has shown, main clauses are less islandy than
complements, which are less islandy than relatives. We
have as yet no way of stating (6) in terms of recognition
rules. Indeed, it seems to be a principle governing the
application of recognition rules, rather than a rule
itself, just as Ross' island constraints govern the
application of transformations. We have as yet no reason
to believe that (6) corresponds to any principle of
abstract grammar and/or production. The theory we have
outlined thus far takes no stand on the issue.

The primitive theory that we outlined above requires
strict left-to-right processing and makes no mention at
all of memory. We view these as shortcomings of the
theory which we hope to overcome in future versions.
Overall sentence processing has basically a "left-to-
right" character, since information is processed as it
is received. But that does not mean that sentence pro-
cessing takes place one word at a time, as the words are
received. Phonetic information is received continuously,
and studies indicate that it is stored and processed in
chunks, and that morphological, syntactic, semantic, and
pragmatic information may all be used just in figuring
out what the words are. This indicates that principles
at all linguistic levels are accessed simultaneously to
"see what fits". It is for this reason that we have set
up our grammars as well-formedness grammars. As such,
they do not commit us to strict left-to-right processing.
They are consistent, as they are now formulated, with
the position that incoming information is processed in
chunks (even overlapping chunks). To provide for such
processing, the concept of a "recognition derivation"
would have to changed, but the rules we have so far
postulated could be left intact. What we envision is a
theory in which the overall, but not strict, left-to-right orientation of processing is imposed by the continuous reception of phonetic information, chunk storage and processing, and the shift from short-term to longer-term memory. We have nothing whatever to say about any of these matters. All we have done with respect to recognition is to formulate some more-or-less plausible-looking recognition rules that generate and change hypotheses about what is being said and to show their relation to the study of abstract grammar.

We have also said nothing thus far about simultaneous or parallel processing, or about semantic and pragmatic conditions on the application of grammatical rules. We view these as factors to be included in future versions of the theory. Sentence (1) is a case where such additions seem to be needed. One might plausibly argue that, on perceiving the substring "The aardvark was given...", one would be likely to guess, for semantic reasons, that "The aardvark" would ultimately turn out to be the indirect object rather than the direct object, simply because animate objects are more likely to be indirect than direct objects of "give". The probability would be even higher for a substring such as "Susan was given...", since humans are unlikely to be given to anybody. Such observations are not necessarily in conflict with our claims, though they might appear to be, since they might be taken as a challenge to the claim that "The aardvark" in (1) is ever given an intermediate analysis as a direct object.

The apparent conflict arises from a confusion between two sorts of hypotheses: semantically and pragmatically conditioned hypotheses about the ultimate role of a noun phrase in semantic structure, on the one hand; and syntactically motivated tentative hypotheses for use in figuring out grammatical structure, on the other. So far we have talked only about hypotheses of the second sort. But any hypothesis that "Susan" will ultimately turn out to be an indirect object is clearly of the first sort. Our ultimate intention is to include hypotheses of the first sort in cognitive grammars, and to extend the notion of a derivation to permit such hypotheses to be made simultaneously or in parallel with grammatical hypotheses. It will still be necessary in such cases, however, to use the syntactic derivational process discussed above to validate such pragmatic hypotheses, or invalidate them as would be the case in sentences such as "Susan was given in marriage to Seymour".

Perhaps the most important aspect of what we are claiming is that the idea of linguistic generalization that is implicit in studies of abstract grammar is basically right. The idea is that (i) the same rules
needed to handle relatively simple sentences will handle complex sentences due to their recursive application; and (ii) if you have the "right" rules -- the mentally real rules -- then you shouldn't have to state the same rule twice in a grammar. This is in conflict with certain processing approaches to linguistic analysis. The typical reasoning behind such approaches goes like this: One thing people have lots of is long-term memory. But short-term memory and processing capacity is at a premium. If mentally real grammars are collections of processing strategies, these strategies should be such as to minimize the use of short-term memory and processing capacity. And if that takes more long-term memory, that's fine. There's lots of it. Therefore, we should not worry about "capturing generalizations". If you need to "say the same thing twice" -- or even three times or thirty-seven times in order to state processing rules that will minimize the use of short-term memory and processing capacity, then that's fine. All other things being equal, non-general rules that minimize short-term memory and processing capacity are psychologically real, while general rules that fail to minimize short-term memory and processing capacity are not psychologically real.

We are not prepared to accept such a position. To our knowledge, there is no solid evidence whatever that there is a necessary conflict between linguistic generalizations and the minimization of short-term memory and processing capacity. We believe that the assumption that such a conflict existed came out of the widespread belief that transformational grammar provided the right framework in which to state linguistic generalizations. We agree that there is a conflict between transformational grammar and any processing theory with reasonable claims to psychological reality. Our solution is to abandon transformational grammar and instead adopt some version of cognitive grammar as the appropriate framework for the statement of general linguistic principles.

It should be clear from the above discussion that we reject summarily the putative distinction between "acceptability" and "grammaticality", as proposed by Chomsky and more recently defended by Bever, Fodor, Langendoen, Kimball, and others. The study of cognitive grammar is part of the study of cognitive capacity as a whole. This includes at least the representation of knowledge, memory, processing strategies and mechanisms, reasoning, principles of social interaction, and whatever other abilities or knowledge enable one to use language. It is important to recognize the distinction between a grammar, which is a collection of rules for use in pro-
cessing, and perception and production mechanisms, which employ a grammar to do processing. Grammars are language-specific (though all grammars may share certain universal principles), whereas the perception and production mechanisms which make use of these grammars are common to all normal human beings.

Finally, cognitive grammar should be viewed as another extension of the proper domain of linguistic inquiry. The force of the generative semantics movement was to integrate semantic, and later pragmatic, considerations into the study of syntax. In that spirit, we view this paper as the beginning of the integration of processing considerations into the generative semantics paradigm.

Footnotes

(1) The work reported herein was partially supported National Science Foundation Grant no. SOC75-03435. Many thanks for numerous helpful ideas, comments, and suggestions to Herb Clark, Charles Fillmore, Ron Kaplan, Martin Kay, Judy Kegl, Mike O'Malley, Dan Slobin, Len Talmy, William Watt, Terry Winograd, and William Woods.

(2) We have adopted the view that there is a general principle which prevents such rules from operating if the relation in question is already filled, rather than including such a restriction in each rule; e.g., we do not need to add "and no element has previously been analyzed as V" to R6.

References


(3) Kaplan, R., Augmented Transition Networks as Psychological Models of Sentence Comprehension, Artificial Intelligence, 3, 1972, 77-100.


(8) Lakoff, G., Correspondence Grammars, unpublished University of Michigan manuscript, 1970.


### TABLE 1

<table>
<thead>
<tr>
<th>RECOGNITION GRAMMAR</th>
<th>ABSTRACT GRAMMAR</th>
<th>PRODUCTION GRAMMAR</th>
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<td><strong>R1:</strong></td>
<td><strong>A1: Subject</strong></td>
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<td>NP(*)</td>
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<td>M of * = ∅</td>
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<td>b. NP(1)</td>
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<td>FIRST(1)</td>
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| **R2:**             | **A2: Object**   | **P2:**            |
|                     | CH(∅)            |                    |
|                     | V(1)             |                    |
|                     | O(*)             |                    |
| NP(*)               |                  | NP(*)              |
| M of * = ∅         |                  | M of * = ∅        |
| AE: 2               |                  |                    |
| SS: 2               |                  |                    |
| **COND:**           |                  |                    |
| V(1)                |                  |                    |
| I: O(2)             |                  |                    |
| II:a.               |                  |                    |
| b. NP(2)            |                  |                    |
| M of 2 = ∅         |                  |                    |
| P(1,2)              |                  |                    |
| CH(3) ⊨ ¬P(3,2)    |                  |                    |

| **R3:**             | **A3: Indirect Object** | **P3:**            |
|                     | O(1)                  |                    |
|                     | IO(*)                 |                    |
| NP(*)               |                       | NP(*)              |
| M of * = to        |                       | M of * = to       |
| AE: 2               |                       |                    |
| SS: 2               |                       |                    |
| **COND:**           |                       |                    |
| O(1)                |                       |                    |
| I: IO(2)            |                       |                    |
| II:a.               |                       |                    |
| b. NP(2)            |                       |                    |
| M of 2 = to         |                       |                    |
| P(1,2)              |                       |                    |

<p>| <strong>R4a:</strong>            | <strong>A4a: Unmarked Chômeur</strong> | <strong>P4a:</strong> |
|                     | V(1)                    |          |
|                     | M(<em>) = ∅                |          |
|                     | CH(∅)                   |          |
|                     | CH(</em>)                   |          |
| NP(<em>)               |                       | NP(</em>)    |
| AE: 3               |                       |          |
| SS: 3               |                       |          |
| <strong>COND:</strong>           |                       |          |
| V(1)                |                       |          |
| M(3) = ∅            |                       |          |
| I: CH(3)            |                       |          |
| II:a.               |                       |          |
| b. P(1,3)           |                       |          |
| CH(2) ⊨ ¬P(2,3)    |                       |          |
| NP(3)               |                       |          |
| CH(∅)               |                       |          |
| CH(<em>)                |                       |          |
| NP(</em>)                |                       |          |</p>
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<td><strong>A5: Auxiliary</strong></td>
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<td><strong>A8: Subject-Verb Inversion</strong></td>
<td><strong>P8:</strong></td>
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|                     | \[
| S(\emptyset)       | AE: 2            | S(\emptyset)     |
| A(*)                | SS: 2            | A(*)             |
|                     | COND: A(2)       | T(Q)             |
|                     | I: T(Q)          |                  |
|                     | II:a.            |                  |
|                     | b. S(1) \rightarrow P(1, 2) |                  |
|                     |                  |                  |
| **R9:**             | **A9: Passive**  | **P9:**           |
|                     | \[
| A(2)               | AE: 1            | A(2)             |
| V(*)                | SS: 3            | V(3)             |
|                     | COND: A(2)       |                  |
|                     | V(3)             |                  |
|                     | I: S(\Delta)     |                  |
|                     | O(1)             |                  |
|                     | II:a.            |                  |
|                     | b. PastPart(3)   |                  |
|                     | be(2)            |                  |
|                     | PastPart(3)      |                  |
|                     | be(2)            |                  |
| S(1)                |                  |                  |
| O(1)                |                  |                  |
| PastPart(*)         |                  |                  |
| be(2)               |                  |                  |
|                     |                  |                  |
| **R10:**            | **A10: Dative**  | **P10:**          |
|                     | \[
| O(1)                | AE: 1            | O(2)             |
| CH(*)               | SS: 2            | IO(*)            |
|                     | I: IO(1)         | CH(2)            |
|                     | O(2)             |                  |
|                     | II:a.            |                  |
|                     | O(1)             |                  |
|                     | CH(2)            |                  |
|                     | b. M of 2 = \emptyset |         |
| M of * = \emptyset  |                  |                  |
|                     |                  |                  |
| **R11:**            | **A11: \Delta Introduction** | **P11:**         |
|                     | \[
<p>| S(\Delta)          | AE: 1            | S(\Delta)        |
| CH(<em>)               | SS: 1            | CH(</em>)            |
|                     | I: S(1)          |                  |
|                     | II:a.            |                  |
|                     | S(\Delta)        |                  |
|                     | CH(1)            |                  |
|                     | b. M of 1 = by   |                  |
| M of * = by         |                  |                  |
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Lxicography and the count-mass distinction
James D. McCawley
University of Chicago

To my knowledge, the only published dictionary of English
which explicitly indicates whether each noun is a count noun or a
mass noun is Hornby’s Advanced learner’s dictionary of current
English. This paper is concerned with issues raised by the ques-
tion of whether a dictionary in fact should indicate explicitly
whether a noun is count or mass, i.e. the question of whether
counthood or masshood is predictable from the meaning of the word.

I emphasize that I am speaking of the meanings of the words
and not about characteristics of the entities that can be described
using those words. There is clearly no difference between noodles
and spaghetti that can be held responsible for the fact that
noodles is a plural count noun but spaghetti is a mass noun (in
English, this is, not in Italian), nor is there any such difference
between garlic and onions or between rice and beans, or between
the data that are referred to by a plural count noun and the data
that is referred to by a mass noun. The same entities can be
described as footwear or as shoes; as furniture or as chairs. How-
ever, this does not rule out the possibility that the words garlic
and onion or the words footwear and shoes or the words datacount
and datamass differ in meaning in a way that allows one to infer
that the one is a mass noun and the other a count noun. The most
obvious aspect of meaning to examine for a basis for the count/
mass distinction is individuation, and the obvious hypothesis to
try is that the meaning of a count noun specifies an individua-
tion, whereas the meaning of a mass noun is neutral as to individu-
ation.

The fact that a particular type of thing always comes in
individual quanta does not imply that the meaning of a word describ-
ing those things has to make reference to that individuation, e.g.
from the fact that rice comes in grains or that footwear comes
individuated the same way that feet are, nothing follows about the
meanings of the words rice and footwear. Actually, I am not
completely sure that I am making a claim that has any content when
I claim that the meanings of count nouns specify an individuation
but those of mass nouns do not. In fact, I am encountering head-on
Quine’s problem of ‘radical translation’. In a language such as
Japanese, in which nouns are normally unspecified as to grammatical
number and a classifier must be used both in expressions of
cardinal number and expressions of quantity, do I have any basis
for deciding whether a given noun is a count noun or a mass noun,
e.g. do I have any basis for treating Japanese hito’tubu no mu’gi
‘a grain of barley’ as semantically like English a grain of barley
rather than like English a bean? If Japanese conceive of beans the
way that we conceive of barley, how could we tell? There is an area
of research that may eventually provide the basis for solid answers
to such questions, namely the logic of mass expressions. However,
at present that area is grossly underdeveloped. The few existing
treatments of quantification of mass expressions (e.g. Parsons
1970) reduce it to quantification over individuals by
interpreting an expression such as **all gold** as if it were an abbreviation for **all objects that result from partitioning a mass of gold**. I feel strongly that this approach is misguided and that it is doomed to failure when any attempt is made to apply it to expressions such as **most gold** or **much gold**. However, until I put my typewriter where my mouth is and produce a satisfactory account of the logic of mass terms, I will have to content myself with an expression of my visceral feeling that there is content to the claim that the words **rice** and **bean** differ in meaning by more than just information as to the biological species.

I should also at this point observe that when the meaning of a word does not specify an individuation, it is not necessarily the case that what it refers to is indefinitely divisible. It has occasionally been objected (Antley 1974) against the claim that mass nouns are unspecified as to individuation that many mass nouns cannot be combined with measure expressions that refer to minute quantities, e.g.

(1) **a molecule of footwear/rice.**

However, all that that observation shows is that there can be a lower limit to the quantity of matter that can possess a given property. There are impeccable mass nouns which refer to mixtures or suspensions rather than to chemically pure substances and which thus cannot be combined with molecule:

(2) **a molecule of**

- [Irish coffee](#)
- [butter pecan ice cream](#)

If, as I conjecture, the meaning of **footwear** is simply 'to be worn on the feet', the oddity of **a molecule of footwear** will come from the fact that a single molecule is too little to be worn. There is in fact a gradient as regards the smallest quantity that is describable by a given mass noun:

(3) a. an atom of **sodium/water**
   b. a molecule of **water/ice**
   c. a milligram of **ice/garbage/excelsior**
   d. an ounce of **excelsior/garbage/ballast**.

**Excelsior** is wood shavings used as a packing material, and you can't call something excelsior unless there is enough of it to use as a packing material. Ballast is anything used to weigh down the bottom of a floating object and thus cause it to float upright, and unless you have enough to lower the center of gravity of the object, it isn't ballast.

It is quite easy to find dictionary definitions in which a count noun is defined in terms of an individuation and a semantically similar mass noun is defined without reference to an individuation, for example:

(4) **noodle.** A thin strip of food paste, usually made of flour and eggs.

**pasta.** Paste or dough made of flour and water, used dried, as in macaroni, or fresh, as in ravioli.
The definition of *noodle* specifies an individuation: a noodle is a strip, two noodles are two such strips, etc. The definition of *pasta*, on the other hand, indicates no individuation: it indicates what pasta is made of, what its texture is, and how it is used, but it does not indicate a quantum of pastahood. It is also easy to find pairs of words that differ as to counthood but are defined by a dictionary in a way which provides no information from which that difference could be predicted:

(5) *rice*. 1. A cereal grass, *Oryza sativa*, ...  
   2. The starchy edible seed of this grass.  

   *bean*. 1. Any of several plants of the species *Phaseolus*, ...  
   2. The edible seed or pod of any of these plants.  

In either case, the definition amounts to 'the edible seed of X', which provides no hint of the fact that one seed of *Phaseolus vulgaris* is a *bean*, whereas one seed of *Oryza sativa* is not a *rice* but rather a *grain of rice*. In this case, the failure of the lexicographer to provide the information is partly due to the fact that he chose to include a non-restrictive modifier ('edible') in the definition, which forces a definite article on him and thus robs him of the possibility of using an indefinite article as a means of indicating the individuation of beans ('A seed or pod of any of these plants'). It is less clear how the definition of *rice* could be fixed up so as to make it clear from the definition that *rice* is a mass noun. If the predicate use of nouns is taken to be the basic one, a definition of *rice* (that is, of *is rice*) could be given along the lines of 'consists of seed(s) of *Oryza sativa*', where 'seed(s)' is intended to be unspecified as to number. I offer this definition only to show that it is possible (albeit by brute force) to cast a definition of *rice* into a form which makes it refer not to grains of rice but to the substance of those grains.

Before leaving *rice* and *bean*. I should touch on the senses listed as '1' in these definitions. In what sort of sentences does sense 1 appear -- perhaps in sentences like (6)?

(6) We grow beans in this field.  

A lot of rice is grown in southeast Asia.

While both of these sentences are used with reference to the planting and cultivation of whole *Phaseolus* or *Oryza* plants, and in large numbers at a time, the grammatical number of the noun is still singular in the case of *rice* and plural in the case of *bean*. Thus, even when the cultivation of whole plants is referred to, the choice of number is made as if it were the food for which that plant is grown that was being referred to. This fact leads me to conjecture that, contrary to the apparent opinion of the lexicographer responsible for the definitions (5), sense 2 is more basic than sense 1, if sense 1 is even a real sense of the words *rice* and *bean*. In fact, a case can be made that the object of grow can just as easily refer to the product that the plant yields as to the plant itself and thus that *rice* and *bean* in (6) are really used in sense 2; note, for example, that one can say
(7) I always grow my own parsley.
I always grow my own parsnips.

in which the object NP clearly refers to the vegetable (in the culinary sense) that results, not to the whole plant or to the biological species, since parsley and parsnips are two parts of the same plant but the sentences of (7) differ in meaning.

The nouns cold and headache are clear cases of count nouns whose countness can be attributed to an individuation specified in their meanings, as contrasted with flu, diarrhea, and tuberculosis, which have meanings that are neutral as to individuation. Colds and headaches can be counted:

(8) I had two colds last winter.
I've had two headaches within the last three days.

A cold is a 'case' of a particular infection. A 'case' must be distinguished from an 'attack': the attack is the onset of the disease, whereas the 'case' endures from onset to cure. The observation that a cold is a case of something is supported by the following facts:

(9) I have a cold.
I have a case \{ \*an attack \} of the flu.

Do you have the same cold/\*flu that you had last week?
Do you have the same case of the flu that you had last week?
I had a sudden \{ \*case of the flu. \*cold. \}

Cold appears to pattern exactly the way that case of X does.

There are also ambiguous words whose different meanings correspond to different individuations. Job has the three senses that are illustrated in the following sentences:

(10) a. Let me finish this job, and then I'll join you in the bar.
b. Harry and I have the same job -- we're both file clerks.
c. They not only fired me, they abolished my job.

In (10a), the job is a specific piece of work, and the job ceases to exist when that work is completed. In (10b), the job is an ongoing activity which the agent does as a profession or means of livelihood, and two persons who are hired to perform the same activity have the same job, in this sense. In (10c), the job is an institution, which comes into being when the employer creates such a position in the organizational structure of his firm, and the job continues in existence, regardless of who is employed in the job or even of whether anyone is employed in the job and regardless of any changes that are made in the duties that the holder of that job must perform, until either the job is abolished by the
employer or the firm ceases to exist. In this sense of job, the
job you have today in which you are paid $15,000 a year to teach
Montague grammar may be the same job in which 30 years ago some-
one was paid $5,000 a year to teach freshman German and coach the
squash team. Different words may share just one of these individ-
uations. For example, task has only a sense parallel to the job
of (10a), profession only a sense parallel to the job of (10b),
and chairmanship only a sense parallel to the job of (10c). And
the most common sense of work involves no individuation at all.
The individuation is part of the meanings of all of these nouns
(except work), and their countness is predictable from the
presence of the individuation in the meaning.

Let us now turn to the case of footwear, which has been held
(Katz 1970) to have the same meaning as 'articles of apparel to be
worn on the feet', i.e. to have the same meaning as a count
expression. It can of course be claimed (as I have, in McCawley
1971) that footwear rather has the meaning of the mass expression
'apparel to be worn on the feet', and that 'articles of apparel to
be worn on the feet' makes reference to an individuation but
'apparel to be worn on the feet' does not. Due to the fact that
the apparel that people wear on their feet is individuated in the
same way that their feet are, it is not easy to come up with
evidence that has a bearing on the question of which (if any) of
these expressions differ in meaning. Relevant facts might be
sought in the imaginary situation in which, through the miracles
of modern technology, non-individuated footwear is put on the
market, e.g. socks that you can spray on your feet and then peel
off later, in the fashion of the spray-on bandages that have been
developed in recent years. However, it is not clear that it is the
liquid inside such an aerosol can that constitutes footwear,
rather than just the sock-like units that are formed as it is
sprayed on the feet. A similar situation prevails in the case of
furniture, though the possibility of non-individuated furniture is
much less fanciful; indeed, there are dozens of stores where you
can buy crates full of modules that can be assembled to fit your
fancy into one gigantic structure or several small ones. To the
extent that (11) is a normal thing to say with reference to such
modules, furniture will have to be taken to be unspecified as to
any individuation:

(11) I've just ordered two crates of furniture.

However, there is great variability among informants as to the
extent to which they are happy with (11).

There are in fact some sentences that seem to show individu-
ation lurking in the meaning of furniture. For example, while
(12b) would be true in a situation where Fred has one 2000-foot
long piece of rope, (12a) is not true in a situation in which Fred
has one 40-foot long sofa but no other furniture:

(12) a. Fred has a lot of furniture.
    b. Fred has a lot of rope.

(12a) seems to imply that Fred has at least several pieces of
furniture, despite the fact that a single piece of furniture, unlike a grain of rice, can be enormous. Likewise, furniture admits adjectives referring to the size of the piece much more readily than do more hard-core mass nouns such as rice, and even footwear is not outlandish in such a usage:

(13) a. large furniture
   b. ?large footwear (accepted by some informants as a way of referring to ski boots, etc.)
   c. *large rice (cf. long-grained rice)
   d. **large sand

However, the a lot in (12a) does not just mean 'a large number', though it appears to imply it. (14) does not allow the interpretation that the number of pieces of furniture that Fred has exceeds the number that I do.

(14) Fred has more furniture than I do.

It is easy to come up with situations in which (14) would be judged false even though Fred has more pieces of furniture than I do; for example, if Fred has 4 chairs, 3 magazine racks, 2 coffee tables, and 1 lamp, and I have 2 chairs, 1 desk, 1 bed, 1 sofa, and 1 table, my six pieces of furniture would constitute more furniture than Fred’s 10 pieces do. The situation is similar with

(15) Fred has more clothes than I do.

If Fred’s wardrobe consists of 2 pairs of shoes, 2 pairs of socks, 1 pair of swimming trunks, and 4 T-shirts, and mine consists of 1 pair of shoes, 1 pair of socks, 1 pair of levis, 2 shirts, and 1 jacket, it would be strange to say that Fred has more clothes than I do, even though Fred has 9 or 13 articles of clothing (depending on whether a pair of shoes counts as 1 or as 2 articles of clothing) but I have only 6 or 8. I conjecture that the reason for this is that Fred’s 9 or 13 articles of clothing don’t clothe him as fully as my 6 or 8 articles clothe me; likewise, Fred’s 10 pieces of furniture don’t furnish his apartment as fully as my 6 pieces furnish mine. I thus maintain that the reason that (12a) is not true unless Fred has a large number of pieces of furniture is that a really small number of pieces of furniture is not enough to make an apartment fully furnished. Or at least, that is the case with regard to conventional furniture. George Williams informs me that modular furniture has been taken to such extremes in Germany that one can purchase modules from which one can construct a 'Wohnungslandschaft', which may be a continuous structure constructed out of modules and incorporating sleeping space, sitting space, storage space, work space, etc. I do not find (12a) odd if it is used with reference to a single very elaborate Wohnungslandschaft which furnishes the apartment as thoroughly as could a large number of pieces of conventional furniture.

I thus conclude that the meanings of furniture, clothing, and presumably also footwear, though that is a less clear case, are not of the form 'articles for doing X' but are rather simply 'for doing
X', e.g. *clothing would mean *for wearing* or *to be worn*, *footwear would mean *to be worn on the feet*, and *furniture would mean something like *to support persons and objects* (or at least, that supplemented by additional conjunctions such as *is moveable*), and these words thus do not conflict with the claim that a noun is a mass noun if and only if its meaning does not provide an individuation for the things that it describes.

But wait a minute -- in the above argument, specifically, in example (15), the noun involved was not *clothing but clothes*. I could just as well have used *clothing* instead of *clothes* in that argument, and nothing would have been any different. But what about *clothes*? Is it a mass noun? One fact might suggest that it is really a count noun, namely the fact that it takes plural agreement:

(16) *My clothes are/is in this locker.*

However, other facts show much more clearly that it isn't a count noun; for example, it can't be combined with cardinal number expressions:

(17) *I've just bought several/five clothes.*

*Many clothes are too expensive for me to buy.*

*Clothes* evidently is a plural mass noun, as are such words as *brains, guts, intestines, and hemorrhoids*:

(18) *His guts were splattered over the wall, (*all five of them).*

So we have plural mass nouns, a fact that we'll have to live with, just as we have to live with the fact that there are count nouns with only a plural form, due to either pure idiosyncracy (as with Russian *lazy 'clock*) or to a minor regularity (as in the case of *trousers, tweezers, and goggles*).

But then what about some of the count nouns that I talked about at the beginning of this paper? Is there any way of telling whether the plural forms *noodles* and *beans* are ambiguous between a plural count sense (as in (19a)) and a plural mass sense (as in (19b))?*

(19) a. How many *noodles* did you eat?
   b. How much *noodles* did you eat?

I am at a loss as to how to tell whether these are distinct senses. If this is a real ambiguity, it can be incorporated into a grammar in the form of a redundancy rule which would predict the mass sense from the count sense, e.g. it would predict a sense 'food consisting of seeds of X' for any noun which has a sense 'seed of X' (though this extra sense would be in use only for those species whose seeds were used as food). But that raises the question -- why should the prediction go in that direction rather than the opposite? Why not take the mass sense as basic, recognize a lot of mass plurals, and have a redundancy rule predicting a count sense for the singular of any mass plural with a meaning 'consisting of
X's or Y'?

Offhand, I can't think of any solid reason for preferring either analysis over the other. However, either way, the courthood or masshood of a noun will still be predictable from its meaning. But also, either way, my hope expressed above that the logic of mass expressions might provide a basis for setting up a semantic distinction between *rice* and *beans* would be shattered: it could at best provide a basis for a distinction between *rice* and one sense of *bean*, but it would not help answer the question of which sense of *bean* is more basic nor, thus, the question of whether *bean* basically differs from *rice* in by more than the difference between *Phaseolus vulgaris* and *Oryza sativa*. Answers to those questions would be available if someone could show that *beans* really is not ambiguous between a count sense and a mass sense. A way of telling whether it is ambiguous that way ought to be lurking in any grammatical theory that is worth *Phaseolus vulgaris*.

FOOTNOTES

1 Do these facts imply that *water* is ambiguous between the sense 'H₂O and the sense 'Liquid H₂O'? Any of the molecules in a block of ice is 'a molecule of water'. The fact that the whole block cannot be described as water is probably due to conversational implication: you could be more informative by using the word for 'solid H₂O'.

2 The definitions in examples 4 and 5 are quoted from *The American Heritage Dictionary of the English Language* (New York, 1966).

3 The definition ought also to incorporate the fact that whether the pod is regarded as edible determines whether it is the seed or the pod that is called a bean: when the pod is normally eaten, it is the pod that is called a bean, whereas for those kinds of bean whose pods are not normally eaten, it is the seed that is called a bean.

4 Lurking individuation may also be seen in the fact that a *piece* of furniture can refer to a chair or a magazine rack but not to a leg of a chair or a drawer of a cup board, let alone a chip off of a table.

5 I have not investigated the conditions under which spatial adjectives can be combined with mass nouns in such expressions as *thick cloth*, *long hair*, *deep mud*.

REFERENCES


On Some Tibeto-Burman Sound Changes
Boyd Michailovsky
UC Berkeley

0. In this paper a number of sound changes involving the influence of final consonants on vowel quality are examined. In general, it is found that final consonants in the denti-alveolar region have caused all but high front vowels to be either fronted $\langle a, o, u \rangle \varepsilon, \phi, y \rangle$ or to acquire an -i glide $\langle a, o, u \rangle ai, oi, ui \rangle$. First I will present the data, from Tibetan and some Tibeto-Burman languages of Nepal, and then I will discuss the acoustic and articulatory aspects of the problem. The discussion will focus on the direction, rather than the fact, of sound change.

1.0 In considering the data to be presented below, it is useful to bear in mind the systems of final consonants and of vowels of Written Tibetan (WT), whose orthography was established in about the eighth century. The consonant finals are spelled -b, -d, -g, -m, -n, -ŋ, -r, -l, -s. Conservative dialects of all the languages discussed here have similar systems, although some or all of the non-nasal continuants may be missing. The WT vowels are i, e, a, o, u. Conservative dialects discussed below generally have five or six vowel qualities. The influence of neighboring consonants and vowel harmony have led to a proliferation of vowel qualities in other dialects.

1.1 Tibetan. Comparison between WT and the modern dialect of Lhasa described by Chang and Shefts (1964) reveals the following regular changes:

\[
\begin{align*}
\text{WT } & \{a, o\} > \text{LT } \{\varepsilon\} \quad /\_d, n, l, s \text{ (WT syllable final).}
\end{align*}
\]

WT -d, -n, -s have no consonantal reflexes in Lhasa. In general, where stops or -s have been lost at the end of a word, the "falling tone" is produced $\langle \text{v} \rangle$, which B. Chang (1968abd) describes as a "glottal incident" followed by lowered pitch. Where nasal word-finals $\langle -n \rangle$ always and $\langle -\eta \rangle$ sometimes have dropped, a long nasal vowel results. The changes in vowel quality are illustrated on the left side of Table 1. 

Olson (1974) has noted that an eastern (Kham) dialect shows the same changes as Lhasa, except that the fronted WT a merges fully with reflexes of WT e, and -l tends to be lost without trace.

Data on a few other Tibetan dialects has been included on the right side of Table 1. Balti and Lahul are western dialects; Balti provides modern evidence for the presumed WT system. Lahul shows few vowel
<table>
<thead>
<tr>
<th>Gloss</th>
<th>WT</th>
<th>Lhasa</th>
<th>Balti</th>
<th>Lahul</th>
<th>Amdo</th>
</tr>
</thead>
<tbody>
<tr>
<td>sleep</td>
<td>g놀</td>
<td>ῖ недо</td>
<td>ngid</td>
<td>ni</td>
<td>ῖ недо'</td>
</tr>
<tr>
<td>is</td>
<td>red</td>
<td>ῖ 대</td>
<td>bgyad</td>
<td>giad</td>
<td>giel'</td>
</tr>
<tr>
<td>eight</td>
<td>brgyad</td>
<td>ῖ 쿨</td>
<td>bgyad</td>
<td>kад</td>
<td>skel'</td>
</tr>
<tr>
<td>language</td>
<td>skad</td>
<td>q온</td>
<td>skat</td>
<td>kad</td>
<td>wォ尔'</td>
</tr>
<tr>
<td>Tibet</td>
<td>bod</td>
<td>ῖ 폐</td>
<td>bodh-</td>
<td></td>
<td>ῖ 폐'</td>
</tr>
<tr>
<td>demon</td>
<td>bdud</td>
<td>ῖ 툩</td>
<td>ῖ'</td>
<td></td>
<td>ῖ 툩'</td>
</tr>
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<td>price</td>
<td>rin</td>
<td>ῖ 립</td>
<td>rin</td>
<td></td>
<td></td>
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<td>otherwise</td>
<td>gsen</td>
<td>seGetProperty(&quot;bank&quot;)</td>
<td>sman</td>
<td>m안-men</td>
<td>xmen</td>
</tr>
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<td>medicine</td>
<td>sman</td>
<td>ῖ 쿨</td>
<td>sman</td>
<td>ton-</td>
<td>xton</td>
</tr>
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<td>autumn</td>
<td>ston</td>
<td>тён</td>
<td>ston</td>
<td></td>
<td></td>
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<td>seven</td>
<td>bdun</td>
<td>т우</td>
<td>bdun</td>
<td>dun-dun</td>
<td>din</td>
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<td>mule</td>
<td>drel</td>
<td>thee</td>
<td>ῖ 트리</td>
<td>ῖ 트리'</td>
<td></td>
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<td>wool</td>
<td>bal</td>
<td>흔</td>
<td>bal</td>
<td>bal'</td>
<td>wa-wal'</td>
</tr>
<tr>
<td>extra</td>
<td>thol</td>
<td>ῖ 토</td>
<td></td>
<td></td>
<td></td>
</tr>
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<td>country</td>
<td>yul</td>
<td>y 닥</td>
<td>yul</td>
<td>jul'</td>
<td>j으-jıl</td>
</tr>
<tr>
<td>two</td>
<td>ῖ 닥</td>
<td>ngis</td>
<td>ῖ 닥</td>
<td></td>
<td>ῖ 닥- 닥</td>
</tr>
<tr>
<td>know</td>
<td>ῖ 쿨</td>
<td>ῖ 쿨</td>
<td>shes-</td>
<td></td>
<td></td>
</tr>
<tr>
<td>cloth</td>
<td>ras</td>
<td>ῖ 쿨</td>
<td>ras</td>
<td>rai-ra</td>
<td>re</td>
</tr>
<tr>
<td>religion</td>
<td>chos</td>
<td>ῖ 쿨</td>
<td>chhos</td>
<td>씨</td>
<td>씨</td>
</tr>
<tr>
<td>incense</td>
<td>spos</td>
<td>pォ</td>
<td>poi</td>
<td></td>
<td>ῖ 흔</td>
</tr>
<tr>
<td>body</td>
<td>lus</td>
<td>l 닥</td>
<td>lui</td>
<td>l 닥</td>
<td></td>
</tr>
</tbody>
</table>

Elsewhere, vowel quality is largely preserved:

| west  | nub | nub | nubkha | ῖ | nィb |
| needle | khab | q호p | ka깝 | khab | k'ап-k'аб |
| six    | drug | thう | truk | drug | ῖ rk-дриг |
| roof   | thog | th호 | thqsa | thog | ῖ og' |
| yak    | gyaq | yァa | hyaq | ja | ῖ jaɟ |
| three  | gsum | 꾸m | khsu 꾸m | sum | гsim-sim |
| bear   | dom | thom | dom | tom |       |
| sky    | gnam | 꾸m | khan 꾸m | 꾸m | 꾸m- 꾸m |
| wind   | rлуq | 꾸m | hlung | 꾸m-po | 꾸m |
| price  | gon | q호s | gon | 꾸m-po | 꾸m |
| beer   | char | char | chhang | 꾸m-an | 꾸m-an |

LT: Goldstein 1968; Balti: Read 1935; Lahul, Amdo: Roerich 1933, 1958. Only LT may be regarded as pho-
nemized (Chang + Shefts 1964).

changes, mainly those induced by WT -s. It is interesting that the change is usually a diphthongization ra-
ther than simple fronting as in Lhasa. Amdo, an east-
ern dialect, shows still another effect of *-s: fronting 
with unrounding. (Note that *u unrounds to i anyway in
Amdo.) In Amdo, only *a is fronted by *-d, *-n.
1.2 Newari. In the Newari of Kathmandu, all consonant finals have been lost, with compensatory vowel length (and nasality in the case of nasal finals). The dialect of Dolakha conserves the finals of Classical Newari (to 18C.). Vowel quality is generally preserved, except before -*s in Kathmandu.

<table>
<thead>
<tr>
<th>Classical</th>
<th>Dolakha</th>
<th>Kathmandu</th>
<th>gloss</th>
</tr>
</thead>
<tbody>
<tr>
<td>hnas</td>
<td>nas</td>
<td>nhaː</td>
<td>nose</td>
</tr>
<tr>
<td>hnas</td>
<td>nes</td>
<td>nhɛː</td>
<td>seven</td>
</tr>
<tr>
<td>gvey(*gos)</td>
<td>gɔs</td>
<td>gwɛː</td>
<td>moustache</td>
</tr>
<tr>
<td>kwos</td>
<td>kose</td>
<td>kwɛː</td>
<td>bone</td>
</tr>
</tbody>
</table>

1.3 Bantawa. The Bantawa dialects of Dilpa and Khawa are located on opposite sides of a small stream. They are virtually identical. But Dilpa's final -t has the reflex -i? (except after i) in Khawa. Similarly, Dilpa -n corresponds to Khawa ɪ. Final stops in both dialects (and in Bahing, below) are unreleased and accompanied by a simultaneous glottal stop. (Michailovsky 1972b).

<table>
<thead>
<tr>
<th>Dilpa</th>
<th>Khawa</th>
<th>gloss</th>
</tr>
</thead>
<tbody>
<tr>
<td>tit</td>
<td>tiʔ</td>
<td>clothing</td>
</tr>
<tr>
<td>set</td>
<td>seiʔ</td>
<td>he kills</td>
</tr>
<tr>
<td>sat</td>
<td>saiʔ</td>
<td>he spins thread</td>
</tr>
<tr>
<td>dot</td>
<td>doiʔ</td>
<td>he begs</td>
</tr>
<tr>
<td>tshut</td>
<td>tshuiʔ</td>
<td>he sends</td>
</tr>
<tr>
<td>pwt</td>
<td>puiʔ</td>
<td>he bathes</td>
</tr>
<tr>
<td>bin</td>
<td>bɪʔ</td>
<td>he flies</td>
</tr>
<tr>
<td>phen</td>
<td>phɛʔ</td>
<td>he undoes</td>
</tr>
<tr>
<td>nan</td>
<td>naʔ</td>
<td>he rests a load</td>
</tr>
<tr>
<td>ton</td>
<td>toʔ</td>
<td>he pushes</td>
</tr>
<tr>
<td>rwn</td>
<td>rwaʔ</td>
<td>he scour</td>
</tr>
</tbody>
</table>

1.4 Sunwar and Bahing: Sunwar and Bahing are neighboring, mutually unintelligible languages of east Nepal. Dental finals of Bahing have diphthongization of non-front vowels as a reflex in Sunwar. The correspondence is quite parallel to that of the Bantawa dialects above, except that e is not diphthongized.

1.5 Tamang-Thakali: Tamang-Thakali (and Gurung) are not to be regarded as Tibetan dialects but as related to Tibetan as a whole (Shafer 1966). The examples below are monosyllabic nouns, illustrating correspondences in word-final position; verb-syllables behave a bit differently due to the influence of suffixes. Except where modified by finals, vowel qualities are largely preserved across Tamang and Thakali. Tamang has a 5-vowel system with distinctive length; in Tha-
kali, long and short vowels have merged generally, but a new vowel quality, \( \Lambda \), appears as the reflex of Tamang short a, distinguishing it from the reflex of Tamang long a. The Eastern Tamang dialect of Risiangku (Mazau-don 1973b) may tentatively be regarded as close to Common Tamang-Thakali (CTT).

CTT *-t causes a shift a > e in Sahu (W. Tamang: Taylor and Everitt 1972) and also o > e in Thakali (Hari 1971). *-n has had an effect only in Sahu. Note that in Sahu the final consonant is preserved.

<table>
<thead>
<tr>
<th>Tamang</th>
<th>Thakali</th>
<th>Sahu</th>
<th>Tukche</th>
<th>Marpha</th>
<th>Gloss</th>
<th>WT</th>
</tr>
</thead>
<tbody>
<tr>
<td>2thet-mai</td>
<td>2thet</td>
<td>2the</td>
<td>2the</td>
<td>2the</td>
<td>siblings, etc.</td>
<td></td>
</tr>
<tr>
<td>2kat</td>
<td>2ket</td>
<td>2k(\Lambda)</td>
<td>2k(\Lambda)</td>
<td>voice (skad)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4prat</td>
<td>4pret</td>
<td>4pre</td>
<td>4pre</td>
<td>4pre</td>
<td>eight (brgyad)</td>
<td></td>
</tr>
<tr>
<td>4tot</td>
<td>4tot</td>
<td>4te</td>
<td>4te</td>
<td>4te</td>
<td>load</td>
<td></td>
</tr>
<tr>
<td>1man</td>
<td>1men</td>
<td>?(m)((\Lambda)</td>
<td>?(m)((\Lambda)</td>
<td>medicine (sman)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1kan</td>
<td>1ken</td>
<td>?k(\Lambda)</td>
<td>?k(\Lambda)</td>
<td>cooked grain</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Tamang has few final -s; cognates of WT words in -s and often -l show fronted vowels in all dialects.

4ni: 4nji: 4ni 4ni two (gnis)  
2ne: 'altar' gnas 'abode'
2poina 2poi 2poi incense (spos)
3li/3lii 3li 3li body (lus)
3paim 'Newar' 1phai 2pai 2pai wool (bal)
4kle 4mwi 4mwi king (rgyal-po)
4mwi 4mwi 4mwi silver (dul)

Finally, Marpha in particular shows a change *ap > o; Tukche seems to be in an intermediate stage.

4tap 4tap 4taw 4to needle (khab)
3kap 3kap 3kap 3ko cover
3ko 3ko 3ko 3ko back

2. Acoustic Discussion: It is well known that the influence of consonants on neighboring vowels takes the form of transitions, or rapid changes in vowel quality, measurable as movement in the formant frequencies during the part of the vowel immediately adjacent to the consonant. Perceptual experiments have shown that the transitions of neighboring vowels contribute significantly to the recognition of stops (Cooper et al. 1952; Schatz 1955), although the stop bursts or explosions also have an important role. In languages with unexploded final stops, we would expect the transitions to play a critical role. In fact, it is found experimentally that transitions accompanying all final consonants are more pronounced (Lehiste + Peterson 1960) or
more uniformly realized (Halle et al. 1957) than those accompanying initials. This may be related to the general tendency of assimilative processes to be anticipatory in nature. In the present study, we will investigate whether the observed changes in vowel quality could have their origin in characteristics of the transitions induced by finals in the denti-alveolar region and not shared by finals at other points of articulation.

In the original version of their well known paper, "Transitions, Glides, and Diphthongs", Lehiste and Peterson give the F1, F2, and F3 frequencies of transitions in CVC utterances by English speakers (L&P 1960 Tables V-X). Some of these values have been used to construct Table 2 below. What the data shows, for our present purpose, is, first of all, that -t, -n, and -s do indeed form a natural group with respect to their F2 transitions (perceptually the most important) with values in the 1400-1600 Hz. range. In addition, we

<table>
<thead>
<tr>
<th>Final</th>
<th>Vowel:</th>
<th>F2t Avg:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>i</td>
<td>e</td>
</tr>
<tr>
<td>-p</td>
<td>-240</td>
<td>-160</td>
</tr>
<tr>
<td>-t</td>
<td>-95</td>
<td>+90</td>
</tr>
<tr>
<td>-d</td>
<td>-240</td>
<td>-225</td>
</tr>
<tr>
<td>-k</td>
<td>+45</td>
<td>+150</td>
</tr>
<tr>
<td>-m</td>
<td>-445</td>
<td>-310</td>
</tr>
<tr>
<td>-n</td>
<td>-300</td>
<td>-250</td>
</tr>
<tr>
<td>-g</td>
<td>/ -55</td>
<td></td>
</tr>
<tr>
<td>-s</td>
<td>-200</td>
<td>-215</td>
</tr>
<tr>
<td>F2v:</td>
<td>2200</td>
<td>2015</td>
</tr>
</tbody>
</table>

F2t averages based on all 15 vowels studied by L&P. Following L&P, separate averages are calculated for velar and palatal articulations of k, p. Values after i, I, eI, e, æ, aI, oI are considered palatal, others velar.
find a negative F2 transition after i, ei, and e (eI is an exception) and a positive one after all other vowels, suggesting a basis for the split found generally in the Tibeto-Burman data: fronting of a, o, u; no effect on i, e.

Similar data for Swedish, from Öhman 1966, is given in Table 3. The Swedish -d is dental, like the -t of western Tibetan, Tamang, Bantawa, and Bahing.

Table 3: Sequences Vba, Vda, Vga by speakers of Swedish. Transitions before the consonant for V=y, b, a, o, u. The target F2 frequencies of the vowels V have been measured separately for each sequence. From Öhman 1966 Tables II and IV.

<table>
<thead>
<tr>
<th>V</th>
<th>Vba F2v</th>
<th>Vda F2t</th>
<th>Vga F2v</th>
<th>Vga F2t</th>
</tr>
</thead>
<tbody>
<tr>
<td>y</td>
<td>2000</td>
<td>-500</td>
<td>1990</td>
<td>-260</td>
</tr>
<tr>
<td>b</td>
<td>1650</td>
<td>-355</td>
<td>1620</td>
<td>-210</td>
</tr>
<tr>
<td>a</td>
<td>990</td>
<td>-170</td>
<td>970</td>
<td>+125</td>
</tr>
<tr>
<td>o</td>
<td>670</td>
<td>-25</td>
<td>690</td>
<td>+330</td>
</tr>
<tr>
<td>u</td>
<td>670</td>
<td>-135</td>
<td>660</td>
<td>+345</td>
</tr>
</tbody>
</table>

It is clear that non-front vowels before -t, -d, -n, and -s have rising F2 transitions. A rise in F2 corresponds to movement forward in the vowel space. This is precisely the direction of the vocalic quality changes observed in the Tibeto-Burman data: u > u, y, i; o > we, oi, b, e; a > ai, a, e, e. The one diachronic change involving movement toward the back of the vowel space, CTT *ap > Thakali Aw, o, is associated with a negative F2 transition. To illustrate the relation between the transitions and the vowels, Figure 1 gives some of the Lehiste and Peterson data for English transitions plotted on the acoustic vowel space, with the English diphthongs ai, oI, aU included for comparison.

The formant movements of the transitions shown in Figure 1 are clearly smaller than those of diphthongs with similar starting points and directions. But the differences in magnitude in the cases of -s or -ot vs. oI, or -at vs. ai hardly seem sufficient to account for the difference in auditory quality between the vowels aI, e.g., sauce and tot on the one hand and the diphthongs of soy and tight on the other. What Figure 1 does not show is the difference in timing between the transitions and the diphthongs. According to Lehiste and Peterson's measurements, the offglide (including the transition) after the steady-state portion
Figure 1: English transitions and diphthongs. Vowels are placed at target positions. Solid lines indicate transitions to final consonants. Broken lines show diphthong glides. (Data: L+P 1961)

of the vowels i, ə, o, and u only occupies about 25 per cent of the total duration of the vowel. But they find that the glide, second target, and offglide of the diphthongs make up over 60 per cent of their total duration (L+P 1961 Table IV). A schematic representation of this difference is given in Figure 2. The data on the diphthong oI is taken from Holbrook and Fairbanks (1962 Table 2). They did not find the "second steady-state target" portion described by Lehiste and Peterson.7

It may be noted that English final -l is always preceded by a negative F2 transition (Table 2), whereas in Tibetan and Tamang-Thakali, -l has had a fronting effect on preceding vowels. It is reasonable to suspect that the vowel changes observed were produced by an -l with acoustic properties quite unlike those of English "dark" -l. In fact, where Tamang -l is preserved, in some verb syllables, it is found to be a
Figure 2: F2 change over time for the English vowel ə with transition to -s (solid line) and for the English diphthong əI (dotted line).  

Dental -l. An acoustic discussion of the diachronic effects of various types of -l is Essen, "An acoustic explanation of the sound shift [ɪ] > [u] and [l] > [i]" (in Abercrombie et al. 1964).

3. Articulatory and Acoustic Models: It may be asked why the sound changes discussed here could not be described simply as anticipatory articulatory assimilations. Such a description would have speakers beginning to assume the articulatory position for the final consonants progressively earlier, thus altering preceding vowels. There are some difficulties in accepting such a model, however. It should be noted that a [t] requires articulatory movements quite different from those of an [-i] diphthongization or front vowel. The transition to -t should involve movement mainly of the tip of the tongue, whereas the diphthongization involves movement of the body, not the tip, of the tongue. In the case of the transition of -ap leading to a back vowel, Thurgood and Javkin have pointed out that the transition to -p should not involve any tongue movement, whereas the back vowel requires movement of the back of the tongue toward the velum. Thus both sound changes would involve some movements that are not assimilatory from an articulatory point of view.

The acoustic approach helps in several ways. The transition has been described acoustically, and experiment has proved that it is heard and used perceptually. This gives a sound phonetic basis for the process of phonologization that may then take place. The hearer, or learner, reinterprets the perceived formant shifts from being a feature of the following consonants to being a feature of the vowel. In his own pronunciation, he performs the articulatory movements necessary to
produce the altered formant values earlier, starting either at the middle of the preceding vowel, producing a diphthong, or at the beginning of it, producing a new vowel. The actual articulatory movements used may differ from those involved in the earlier transition, as long as they produce the desired auditory effect. The result is a typical case of phonologization. (Note that loss of the final consonant, which does not occur in all cases, is a separate phenomenon.) The acoustic approach by no means minimizes the importance of articulatory factors, but it provides the link between speaker and hearer, or more importantly, learner, that is essential for sound change.

Notes
1. Some of the data quoted in this paper was collected while I was a Research Associate of the Institute for Nepal and Asian Studies, Tribhuvan University, Kirtipur, Nepal. I am grateful to that Institute and to its Dean, Dr. P.R. Sharma. Earlier drafts of this paper benefitted from suggestions by John Ohala.

2. The aspects of Tibetan dialectology touched on here are by no means original; they were commented upon by Jäschke (1867, 1881).

3. B. Chang (1968e) has published formant frequency data based on spectrograms of Lhasa Tibetan vowels:
   i: F1: 300 Hz. F2: 2300; o: F1: 500 Hz. F2: 900-1000
   e: 400 22-2300; u: 300 700-900
   a: 7-800 13-1500; o: 7-5-600 900-1000
   Also (1968c): ε: F1: ?600 Hz; F2: 19-2100 Hz. And finally, approximating from table 5c (1968f): F2 of ö: ~1500 Hz.

4. Newari sources: Classical, Jørgensen (1936); Kathmandu, Hale (1971) (I have used my own transcription); Dolakh, Michailovsky (1972a).

5. On Sunwar, see Bieri and Schultze (1971). On Bahn see Hodgson (1857-8) and Michailovsky (1973).

6. The transcription of Sahu and Tukche has been modified by Mazaudon to accord with Risiangku (Mazaudon 1973b) and Marpha (Mazaudon 1972). Some of the sets given here (and many others) may be found in Mazaudon (1973a).

7. Gay (1968) shows that the second steady-state target for əI, əU, əI tends to disappear in rapid speech, and argues that neither it nor the precise offglide frequency is essential to the recognition of these diphthongs. The first target value and the speed and direction of formant movement are the important parameters.
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f. Voice and aspiration in Lhasa Tibetan. (Aug.)
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THE POSITION OF INCORPORATION TRANSFORMATIONS IN THE GRAMMAR

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University of Washington

In Newmeyer (1974a) I argued that the rules of PREDICATE RAISING and NOMINALIZATION, assuming that they are transformations, have to apply precyclically. I concluded from this that the claims of generative and interpretive semantics about grammatical organization turn out not to be as distinct as is generally believed. I further speculated that all incorporation (i.e. word-formation) rules which are not clitic-placement rules might be universally precyclic. If this can be substantiated, then, at least as far as claims about strictly nonglobal processes are concerned, generative and interpretive semantics are true notational variants — those rules which a generativist treatment would label 'precyclic' may simply be thought of as 'presyntactic' under an interpretivist treatment.

In this paper, I will assume that compound nouns and adjectives are formed by a transformational incorporation rule. I have chosen to study compounds because their derivation seems rather troublesome for the precyclic hypothesis. I say 'troublesome' because it has often been assumed that compounds are derived from reduced relative clauses — that phrases like (1) are derived from relative clause reduced structures like (2):

(1) the tree-house

(2)

```
      NP
       |  
      NP  PP
       |   |     
  DET  N  P  DET  N
     the  house  in a tree
```

(from the house which is in a tree)

If compounds are derelativized, then, of course, their formation cannot be precyclic, since RELATIVIZATION, RELATIVE CLAUSE REDUCTION, and PREPOSING are clearly not precyclic.

I will argue, however, that the class of compounds whose first element is intuitively related to a prepositional object cannot be derived from reduced relative clauses. Instead, I will demonstrate that their syntactic and semantic properties are much closer to those of noun complement constructions like (3) below than to those of reduced relatives, and will conclude that they must be derived from such constructions:
Since there is no reason to believe that structures such as (3) do not exist as inputs to the cycle, there is no reason to believe that compounds derived from them could not be formed by precyclic incorporation.

(4) below lists some typical relative clause reductions and (5) some typical noun complements. It is important to point out that ambiguities are often possible between the two:

(4) a. the book - on the table  
b. the tree - nearest the river  
c. a present - for my uncle  
d. the pan - on top of the stove  
e. the sign - to the right of the other one  
f. the chicken - now in the oven  

(5) a. the - house in the woods  
b. the - girl next door  
c. the - boy down the street  
d. the - pot for cooking  
e. the - road to Mandalay  
f. an - arrow through the heart

Several distinct criteria support the constituency assigned in (2) and (3). First, English speakers' intuitions support a major constituent break between the noun phrase and the prepositional phrase in the phrases of (4), but immediately following the determiner in the phrases of (5). Second, the simplest statement of the rule of RELATIVE CLAUSE REDUCTION (or 'WHIZ-DELETION') leaves both the head noun phrase and the prepositional phrase under the immediate domination of the highest NP node, supporting (2) as a structure for reduced relatives. Third, as Chomsky (1970) pointed out, noun complements but not reduced relatives can often take a contrastively stressed possessive determiner, a fact consistent with (2) as a structure for reduced relatives and (3) for noun complements:

(6) a. *[JOHN'S book - on the table] is boring  
b. *[JOHN'S - house in the woods] is falling apart
Fourth, in careful speech, reduced relatives take secondary phrasal stress on their head nouns, while noun complements do not:

(7) a. the book - on the table  
    b. the - house in the woods  

This fact also seems to support the difference in constituent structure assigned. Reduced relatives and noun complements also differ in their gross semantic properties. Compare (8) and (9):

(8) [the book - on the table] belongs to Jones  
(9) [the - house in the woods] belongs to Jones  

While the subject of (8) involves the predication of an incidental and possibly temporary position of the book, the subject of (9) is a type of house. That is, there is a far greater degree of semantic cohesiveness between the head noun and the prepositional phrase in complements than in reduced relatives. For this reason, sentences like (10) with reduced relatives are always contradictions, while sentences like (11) with noun complements are often not:

(10) a. [the book - on the table] is on the floor  
    b. [the pan - on top of the stove] is in the cupboard  
(11) a. [the - girl next door] is in Miami  
    b. [the - pot for cooking] is for storing cookies  

There are associated differences in presupposition as well. As Bach (1974 - citing Asa Kasner) points out, 'restrictive relative clauses presuppose the existence of entities of which the description given in the relative clause is not true. Thus, the man that I saw presupposes at least one man that I didn't see'. Reduced relatives work the same way as full relatives. Thus (12a) presupposes that there exist books which are not on the table and (12b) that there exist men who are not standing on the corner:

(12) a. [the book - on the table] is green  
    b. [the man - standing on the corner] is bald  

Noun complements, however, do not work this way. (13a) does not presuppose that houses exist which are not in the woods, and (13b) does not presuppose that roads exist which are not to Mandalay:

(13) a. [the - house in the woods] needs repair  
    b. [the - road to Mandalay] is muddy
Another difference is that reduced relatives, but not noun complements, allow internal modifiers. Thus we have (14a) but not (14b):

(14) a. [the book - obviously on the table] is green
    b. *[the - house obviously in the woods] is in need of repair

The fact that (14a) but not (14b) can take internally a sentence adverb such as obviously points to the desententi-
ality of the prepositional phrase in reduced relatives but not in noun complements. Notice also that the subject of
(15a) is ambiguous between a reading where the existence of men not from India is presupposed and a reading where it is
not. In the latter case, man from India functions as a semantic unit - being from India uniquely characterizes that
particular man to the speaker. (15b), however, due to its internal modification, has only the reading associated with
reduced relatives:

(15) a. the man from India sold me a flute
    b. the man obviously from India sold me a flute

Finally, noun complements have restrictions on their prepo-
sitional objects not found in reduced relatives. This object
may be a proper noun or a generic common noun, but never a
common referential noun, as illustrated in (16):

(16) a. the - boat to China
    b. the - food for horses
    c. *the - food for the horse

Reduced relatives have no special restrictions on their
prepositional objects.

I will now demonstrate that, following each criterion
for distinguishing noun complements from reduced relatives,
compounds behave like the former, not the latter.

First, take constituency. Obviously, in the phrase
the tree-house the main constituent break is between the
determiner and the compound. All other things being equal,
this supports a derivation from a noun complement, where the
main constituent break follows the determiner. However, even
stronger evidence in support of a complement origin is poss-
ible. (17) and (18) below are the two candidates for the
origin of the tree-house:
In every case of incorporation with which I am familiar, the element moved and incorporated is in construction with (see Klima 1964) the node under which incorporation takes place. We may therefore hypothesize:

(19) An element present in a P-marker may be moved and incorporated under a lexical node only if it is in construction with that node.

But constraint (19) automatically rules out the reduced relative source (17) for tree-house. Tree is in construction with the N of house in (18) but not in (17).

Semantically, compounds are much more like noun complements than like reduced relatives. A 'tree-house' is a type of house, a 'garbage can' is a type of can, and so on. In each case, the semantic connection between the two nouns of the compound is the intrinsic one which we found in noun complements, not the accidental one which we found in reduced relatives. This is related to what Zimmer (1971) described as the 'naming function of compounds, which...is based on the potentially classificatory nature of the relation between their constituents'. (see also Bolinger 1973) Noun complements perform this naming function as well, while reduced relatives do not.

Likewise, (20) does not presuppose that houses exist which are not in trees, nor does (21) presuppose that cans exist which are not for garbage:

(20) My tree-house is nice
(21) My garbage can is full

Finally, as pointed out by Levi (to appear), the first element in a compound may be a proper noun or a generic common noun, but not a common referential noun. Thus we have (22a) and (22b), but not (22c) if a specific horse is being referred to:

(22) a. a China-scholar
b. a horse doctor
c. *a horse doctor
The similarity of compounds to noun complements in this respect is, in my opinion, too striking to be accidental. It points to a complement origin for compounds that seems to me to be indisputable.

On the basis of this evidence, then, we may assume that a class of compound nouns is derived from noun complements. Since, as far as I know, there is no reason to believe that noun complements, whatever their ultimate origin, do not have the structure represented by P-marker (3) cycle-initially, we may assume that at least some compound formation is precyclic. We therefore have more reason to believe the strong claim that all incorporation transformations are precyclic.

Given the strong correlation between the cyclicity of a movement rule and its reference to grammatical relations and the precyclicity of a rule and its word-forming function, it seems to me that, all other things being equal, we should prefer compound formation to be precyclic to its being cyclic. In other words, the burden of 'proof' falls more heavily on anyone who would wish to argue for a cyclic compound formation process. Such an argument could not be an easy one to put through - it is difficult for me to conceive of two compounds which differ only in that DATIVE MOVEMENT applies in the derivation of one but not the other. Yet a cyclic rule of COMPOUND FORMATION would allow for this possibility.

Aside from the rules involved in relative clause formation and reduction, only one cyclic rule has ever been claimed to apply in the derivation of compounds - the rule of PASSIVE. Levi (to appear) suggests, not implausibly, that the application of PASSIVE accounts for the past participles in (23a) and (23b) below:

(23) a. virus-caused diseases
    b. tide-caused waves

I will now argue that PASSIVE is not responsible for the _ed suffix. First note (24a) and (24b):

(24) a. cancer-causing substances
    b. China-watching diplomats

Levi accounts for the _ing suffix in (24) by an ad hoc insertion; thus the postulation of PASSIVE to account for (23) does not lead to a general explanation of the morphology of participial compounds. However, a general explanation is possible which does not involve PASSIVE. Note the underlying grammatical relations of these compounds:

(25) virus-caused diseases
    S    V    O

(26) cancer-causing substances
    O    V    S
When the order is SVO we get a past participle. When the order is OVS we get a present participle. Now consider sentences with so-called 'flip' or 'psych-movement' verbs. Under the most popular analysis, (27) represents the underlying order and (28) the derived order:

(27) I was surprised at you
    S  V  O

(28) You were surprising to me
    O  V  S

When the order is SVO we get a past participle. When the order is OVS we get a present participle. If we are to relate this generalization to that governing the morphology of participial compounds, there is no reason at all to assume that PASSIVE applies in the derivation of compounds. There is further evidence which suggests that it is correct to relate compounds and 'flip' sentences in this way. Levi points out that compounds with -ing never refer to 'one-time only' activities or properties, but only to repeated, habitual, or customary ones. Thus, a pipe-smoking man can only refer to a man who habitually smokes pipes, not to one who has lit up for the first time. But compounds with -ed are not so restricted: there is no habituality implied in a university-initiated project or a revisionist-inspired slogan. 'Flip' sentences, as it turns out, work the same way. (28) implies a nonmomentary state of surprise on the part of the speaker, while (27) allows for a momentary reading.

What all of this suggests is that the -ed suffixes in (25) and (27) have the same origins. Since we know that the suffix in (27) could not have arisen from the application of PASSIVE, it follows that the suffix in (25) could not have either. Thus, compounds like virus-caused are not counter-examples to precyclic COMPOUND FORMATION.

One side benefit of a precyclic compound formation process is that it becomes possible to pin down the notion 'anaphoric island' (Postal 1969) more precisely. As Postal noted, NP's 'inside' of lexical items may in general not be linked to anaphoric pronouns:

(29) a. *Tom is an orphan and he deeply misses them
    b. *The French attempt to beautify their country has
       made it a tourist attraction
    c. *The best wombat meat comes from the young ones

However, if a lexical item is composed solely of an NP and a cliticized inflectional suffix, that inside NP may be linked to an anaphoric pronoun:

(30) Johni's brother hates him
Since there is good reason to believe that cliticization processes are postcyclic (Kayne, to appear; Perlmutter 1973), the inclusion of compound formation rules in the precyclic component of the grammar allows us to state the following generalization about anaphoric islands:

(31) No pronoun occurring in surface structure may be linked anaphorically to an NP contained in a lexical item if that lexical item was formed by a precyclic incorporation process.  

I will conclude with a speculation about the derivation of compounds. Until now I have been assuming (along with most grammarians who have studied compounds) that the structure prior to the application of the compound formation transformation either is the meaning of the compound or is at least closer to the meaning than the structure following the application. In an orthodox generative semantics framework, of course, it is necessary that this be the case. But when we look at the meanings of a wide cross-section of compounds, we find that these meanings are not particularly well represented by the input structure to the transformation. To cite only a few examples, an armchair does not really mean a 'chair with arms': many chairs with arms are not properly termed 'armchairs', and even a proper armchair could be for a time without arms - say if they had been broken and sent out for repair. Even more obviously, a foxglove is not a 'glove for a fox', but a flower; an earmark is not a 'mark on the ear', but a sign; a silverfish is not a fish at all, but an insect; being 'black' and being 'a bird' are neither necessary nor sufficient conditions for being a blackbird; and so on. In each case, the actual semantic representation of the compound is, to one degree or another, independent of the lexical meanings of the words which make it up. Yet at the same time, as Lees (1960) and many others have observed, speakers, as part of their linguistic competence, have the ability to reconstruct a set of semantic representations for compounds which they have never heard. Actually, it is the rules involved in this reconstruction process - not the rules involved in relating the surface realization of the compound to its true meaning - which are those discussed by Lees, Levi, and by myself in this paper. In other words, at least two semantic representations are relevant to the grammar of compounds - that of the true meaning and those of the reconstructable meanings. In this sense, a compound is in some respects like an idiom, whose behavior is governed by a transderivational constraint relating both its literal meaning and its true meaning to its surface form (see Newmeyer 1974b). I have nothing concrete to offer in this paper in the way of a formalism which captures the essential similarities as well as the essential differences.
between the derivations of idioms and compounds. However, since compounds have, to a greater or lesser degree, idiomatic properties themselves, I feel that an investigation of this matter might well lead to some useful insights into the nature of idiomatization.

FOOTNOTES

1 This paper has benefited from suggestions by Judy Levi and a number of students and colleagues at the University of Washington. Errors are my own.

2 In Kuiper (1972) and Berman (1974), the hypothesis that compounds are derived transformationally is explicitly challenged. Some of their arguments are countered (effectively, in my opinion) in Levi (to appear). Their main point, which deals with the 'messiness' and idiosyncratic properties of any compound formation transformation, seems to me to have force only if this rule belongs to the same component as that of the 'familiar' syntactic rules. Since this the conclusion which I explicitly reject in this paper, there does not seem to be any empirically decidable issue at stake as far as that goes.

3 The most recent analysis to assume this is Levi (to appear). While I reject certain aspects of Levi's derivations, her work is by far the most insightful and useful study of compound formation produced to date.

4 It is not clear to me whether this node should be labeled 'N' or bear some label 'higher' than N but 'lower' than NP, such as the 'NOM' or 'N' of various lexicalist studies.

5 It is necessary to specify that the pronoun be present in surface structure since, as Ross (1971) points out, sentences such as I approve of America's attempt to justify herself; but I don't approve of the British; attempt to, where the pronoun has been deleted, are grammatical. Some speakers (see Lakoff and Ross 1972 and Corum 1973) apparently allow (31) to be violated if there is a strong morphological resemblance between the antecedent and the NP in question.

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Verbal semantics and sentence construction
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Dans la langue il n'y a que des différences sans termes positifs.

It has long been observed that the ergative languages of the Caucasus have a special sentence construction which contrasts with both ergative and intransitive types. This is the 'affective construction', used with verbs of perception. 1) Closer to home, traditional grammars abound in references to distinctive behavior of verba sentiendi et dicendi. The two phenomena have nothing in common except their governing lexical class. It is precisely such facts that will concern us here. A number of well-defined lexicosemantic classes of verbs will be shown to govern such surface patterns as the failure of subject selection, the failure of direct-object selection, and the appearance of otherwise unexpected reflexive, passive, intransitive, mediopassive, deponent, and similar forms of verbs. 2) The governing lexical classes will be consistent across languages and language stocks; the governed syntactic phenomena will vary. For this behavior I will use the term displacement — distinctive treatment of a salient class, with departure from the dominant scheme accorded more significance than the material treatment of the deviating category.

One of the clearest examples concerns five Russian prepositions: nad 'over, above', pod 'under', za 'behind', pered 'in front of', meždu 'between'. These five are unique among spatial-locative prepositions in governing the instrumental case. The choice of the instrumental is semantically unmotivated, the only coherent fact being the semantic properties of these prepositions. A feature of perspective, exclusive to these five, is singled out for emphasis in unusual case government.

Distinctive treatment of verbal classes produces the three displacement patterns just mentioned: subject displacement, object displacement, and voice displacement. Object displacement should be distinguished at the outset from mere intransitivity in two-place verbs. No language achieves uniform transitivity in the surface treatment of its two-place predicates. In other words, every language will use oblique complements rather than direct objects for nominal relations low on the object-selection hierarchy. For instance, every language will have some expressions such as go in, cross over; but transitives such as enter, cross, follow are a marked type. Every language will have constructions like look at, listen to; but transitives such as watch, Fr. écouter are a marked class. These facts are interesting in themselves, but we will be concerned with failures of subject and object selection at or near
the top of the scale: with verbs having agents or experiencers, and with verbs having patients.

The first hierarchy of displacement classes centers on what are known as psychological predicates, experiential verbs, or -- the term used here -- psych-verbs. These comprise verbs of perception, verbs of emotion, and verbs of cognition. They also tend to include verbs of modality and related notions such as ability and difficulty. 3) Representative East Caucasian examples:

Bats

\[\text{4) son } \hat{\text{ho}} \quad \text{w-abc'o} \quad '\text{I know you}'\]
\[\text{lng dat} \quad 2\text{sg nom} \quad \text{verb} \quad (\text{Dešeriev 232-4})\]

Avar

\[\text{insu-da } \hat{\text{zindirgo} } \text{was} \quad \text{w-ixana} \quad '(\text{the}) \text{ father}\]
\[\text{Fa loc own} \quad \text{So nom verb saw his son}' \quad (\text{Bokarev 36})\]

(For an overview of Caucasian sentence-construction types see Meščaninov 1967: Ch. IV; Klimov 1965:55, 62.) Many nominative languages show parallel subject displacement. Slavic languages display a sizable set of psych-verbs with dative experiencers (Russian examples): 5)

emotion:

\[\text{mne nравится} \quad 'I \text{ like}'\]
\[\text{mne grустно} \quad 'I'm \text{ sad}'\]
\[\text{mne весело} \quad 'I'm \text{ happy}'\]

Cognition:

\[\text{mne известно} \quad 'I \text{ know}'\]
\[\text{mne кажется} \quad 'it seems to me; I think'\]
\[\text{mne понятно} \quad 'I \text{ (can) understand}'\]

Perception:

\[\text{mne видно} \quad 'I \text{ (can) see}'\]
\[\text{mne холодно} \quad 'I \text{ am cold}'\]

Modality:

\[\text{mne надо} \quad 'I \text{ must}'\]
\[\text{mne могу} \quad 'I \text{ may}' (permission)\]
\[\text{mne нужно} \quad 'I \text{ need}'\]

Finnish shows the genitive in an analogous construction: 6)

\[\text{minun on janoi} \quad 'I \text{ am thirsty}' \quad (\text{minun, gen.} ; \text{on, 'is') }\]
\[\text{minun on tarve/minun тHytty} \quad 'I \text{ must'} \quad (\text{Eliot 138})\]

Similar examples abound. Iranian languages, like their Caucasian neighbors, may exhibit the 'affective' sentence construc-
tion as the result of subject displacement, utilizing one or another local case, with psych-verbs (Bel'man 1974). (For more on Indo-European dative-impersonal constructions see Guzman 1967.)

The psych-verbs may also be set apart by voice displace-
ment. Latin deponents, formally passive but active in meaning, include such verbs as \text{fruor} 'enjoy', \text{vereor} 'fear', \text{obliviscor} 'forget', and the related \text{loquor} 'speak', etc. In those lan-
guages of the stative/active type whose verb classes have lex-
ical exceptions, psych-verbs may figure prominently even in a much-reduced class of statives (e.g. Bats, with only a handful of statives: Dešeriev 221 ff.; also Tunica: Haas 1946:355-7).
On the other hand, psych-verbs may be among the first stative verbs to be displaced into an (expanding) active class. These diametrically opposed treatments support the definition of displacement as a purely negative device without symbolic value. Psych-verbs are a salient lexical class, and therefore apt to receive special treatment.

Displacement is seldom exhaustive: other members of the same semantic class will typically show normal voice and subject selection. E.g., Russian has ordinary surface transitives in ljubit' 'like, love', videt' 'see', znat' 'know', and a number of others which take nominative subjects and accusative objects. Their transitivity, and their very basic lexical status, prove that the displacement of the other psych-verbs is not simply a matter of borderline transitivity.

Within the generic class of psych-verbs, the subset of emotion verbs may be singled out for special treatment. Conspicuous among the Russian lexical reflexives, the Slavic equivalent of deponents, are such verbs as: 7)

ja seržus' 'I'm angry' ja interesu jus' 'I'm interested'
ja bojus' 'I'm afraid' ja bespokoju s' 'I'm worried'

(The final -s is the reflexive particle.) In Finnish a group of transitive verbs of (explicit or implicit) emotion govern the partitive in the object (Eliseev 1959:68-9):

| simple emotion: causative: agentive: | rakastaa 'love' vihata 'hate' kiusata 'annoy' onnitella 'congratulate' |
| pelätä 'fear' kadehtia 'envy' ilahduttaa 'make glad' kiittää 'thank' tervehättä 'greet, welcome' |

Three distinct syntactic classes are involved, and three pairs of nominal relations — evidence that the shared verbal feature, and not nominal roles, determines this use of the partitive. 8)

In Latin a group of five verbs indicating negative emotion govern an accusative experiencer and a genitive object of emotion. From this core, the genitive government is occasionally extended to personal verbs, including some of positive emotion (Lane 207-8):

| impers.: pers.: | tuī me miseret 'I pity you' fastīdit meī 'he disdains me' 2sg gen 1sg acc verb verb 1sg gen (Plautus) quamquam domī cupid 'although I yearn for conj. 1gen verb home' (ib.) |

In Avar the verbs 'like' and 'love' take the dative in subject displacement — a small class, but conspicuous in using the pan-East-Caucasian dative experiencer, in contrast to the locative used with general psych-verbs.
Russian distinguishes an adjunct to the class of emotion verbs. Like the Finnish set above, they are an internally diverse group. They are united only by the presence of a semantic component WANT, explicit or implicit. All govern the genitive with greater or lesser regularity.

WANT: xotet' 'want' želat' 'wish' žaždat' 'thirst for'
imPLICIT
WANT: dostigat' 'attain' iskat' 'seek'
trebovat' 'demand' ždat' 'wait for'
WANT: izbegat' 'avoid' lišat' 'deprive'

Any general definition of the notion emotion verb must mention a conspicuous component of evaluation (as defined by Osgood et al. 1957), either positive (love) or negative (hate). A similar parameter figures in the definition of WANT. (That the two are not the same can be demonstrated by introspection: love, high in positive evaluation, entails no assumptions about a time-coreferential WANT; and vice versa.)

In summary, the first series of displacement classes is headed by the general category of psych-verbs cum modals. Once the generic class has been singled out for displacement, a subgroup of emotion verbs may be distinguished. An even more specific class may be set apart, as with Russian WANT. As a result the unmarked, generic group of displaced psych-verbs may consist most conspicuously of modals and related notions, as in Finnish and Slavic; or verbs of perception and cognition may dominate, as in Avar. It is difficult to rank the markedness of perception vs. modal verbs. (Another Caucasian language, Lezgin, is reported to distinguish verbs of possibility, with elative 'subject', from general psych-verbs, which take the dative: Meščaninov 1967:66ff.)

The special treatment of emotion verbs must be distinguished from simple metaphor as in the ablative of separation with verbs of negative reaction, Engl. at and Russ. na with verbs of aggressive emotion, etc. Each of the sets above is characterized by extension of the government pattern to causatives, conversives, and other derivatives of the presumed historical core. Synchronically, we are dealing with verbal features and not with case assignment based on semantic roles of nominals. Diachronically, however, the role of metaphor is important. The rise of displacement classes seems to proceed by formal renewal of an important subclass, 9) with subsequent spread to the generic class. A prominent source of formal devices is the variety of figurative oblique complements with verbs of emotion. We can trace the Russian WANT verbs back to an ablative of separation, with subsequent generalization. The Finnish partitive must have had a similar source.

Verbs of authority, ruling, disposition, and the like are a sufficiently conspicuous displacement class in Indo-European to warrant reconstruction as governing the genitive in the proto-language (Kuryłowicz 1964:184, following Delbrück). The
independence of the displacement class from its material realization is demonstrated by the formal renewals of the governed case in the various daughter languages. Although the genitive pattern is documented in archaic Latin usage (Lane 209), a replacement in the form of the ablative governed by such verbs as utor 'use' seems to have emerged in at least an incipient stage. In Slavic a number of verbs, including Russ. vladet' 'rule, own', upravljet' 'govern', rukovodit' 'direct, manage' take the instrumental; the pattern shows some productivity in Russian. In Lithuanian, a quite restricted subset of the same verbs governs the dative (vadovauti 'direct, manage', diriguoti 'conduct, direct').

The implicational hierarchy established so far is outlined below. To the left are the more general and widely attested groups; those to the right are more specific and less frequently found. Implication may be read from right to left.

<table>
<thead>
<tr>
<th>general</th>
<th>general</th>
<th>evaluation,</th>
<th>verbs of</th>
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<tbody>
<tr>
<td>psych-</td>
<td>emotion</td>
<td>WANT</td>
<td>authority,</td>
</tr>
<tr>
<td>verbs</td>
<td>verbs</td>
<td></td>
<td>ruling</td>
</tr>
</tbody>
</table>

The diagram suggests that verbs of authority are in principle related to psych-verbs. A subtle link does seem to exist in the evaluative component characteristic of the higher-ranked classes. Verbs of authority have a self-benefactive component: to rule or have disposition over something suggests action in one's own interest, status beneficial to oneself. The precise role of this factor in semantic structure varies from extra-linguistic connotation to contextual implication to explicit semantic component. Whatever their precise connection, authority verbs appear to be a specifically Indo-European displacement pattern and hence may not belong in the universal chart at all.

There is a second, separate displacement hierarchy whose outlines are more speculative than those of the first and will only be sketched here. Because the verbs involved are one-place predicates, the formal realization of classes centers on voice displacement and the choice between parts of speech.

Many languages distinguish stative from active verbs among intransitives. For such languages, verbs of motion and verbs expressing assumption of position (sit down as opposed to sit, be sitting) stand at the head of an implicational hierarchy: the active verbs will always include at least verbs of motion, and virtually all of them (although I know of no language whose active verbs comprise only verbs of motion). 10

Next we encounter at least a few verbs of position such as to be sitting. These are states susceptible to voluntary control. Also at this point we may observe a few strictly locative notions: live (in a place), be (in a place). (The locative complement, obligatory with these verbs and frequent with verbs of position, is evidently the shared feature.) In Bats, for instance, locative 'be', 'live', and verbs of motion are active, while the copula 'be' is stative (Dešeriev 22ff.).
The next two classes create distinctions between parts of speech. At this point, for instance, we find that Indo-European and its neighbors will begin to use adjectives rather than verbs. For languages with parts of speech not isomorphic to our own, we need to make a further distinction of relatively discrete figure-ground shapes (round, square, crooked, grooved) from formless properties such as color, texture, and the like. Whorf, for instance, draws roughly this line between Hopi verbs and adjectives (1946:164).

These four stages --- motion, position, shape, formless qualities --- form a reversible hierarchy. Any stage may be entered from below, by the relatively more stative verbs, or from above, by active verbs. It is a strict hierarchy in that stages cannot be skipped, either in the synchronic composition of formal classes or in their diachronic expansion. One source of apparent exceptions lies in interference from the first implicative hierarchy: voice displacement of psych-verbs will be a consistent source of relatively more stative verbs. Georgian, for instance, has a class of 'static verbs' (Čikobava 1967:51), morphologically defective in that they coincide formally with passives of causatives. They may be likened to denominets and lexical reflexives in Indo-European languages. On the present hierarchy they are limited to the second class, the verbs of position and location, but they include a number of psych-verbs as well. This is a stative class, and should include all relatively more stative levels. It fails to include representatives of the third and fourth levels only because those notions are normally lexicalized as adjectives and thus not susceptible to the voice-displacement properties of verbs. Lexicalization of this type is a second source of apparent violations of hierarchical ordering.

Creek exhibits a similar lexicalization of certain statives, in this case as participials of active verbs. They include predicates of color, texture, and the like, and a few of shape (Haas 1974). They extend from the fourth into the third classes.

Balto-Slavic inherited from Proto-Indo-European a class of verbs with a long *ē suffix in the infinitive stem, thought by some to have comprised the stative verbs of a stative/active protolanguage (e.g. Perel'muter 1974). The early Slavic representatives of this class, based on Old Church Slav vocabulary, included principally the following types. 11)

| psych-verbs  | *zirēti 'look at' | *mēnēti 'think' |
| position     | *sedēti 'sit'     | *visēti 'hang' |
| motion       | *bēgēti 'run'     | *letēti 'fly'  |

If the *ē verbs were indeed originally statives, they have expanded beyond the second class into the first, the verbs of motion. Ordinarily when stative/active systems yield to subject/object systems it is the active verbs that intrude into the stative domain. If the Balto-Slavic irregularity is not just the natural result of a system breaking down, these verbs deserve a careful, semantically-based search for internal chronology.
These examples can be schematically represented as follows. Arrows indicate presumed or attested directions of expansion; heavy vertical lines indicate at least partial class membership.

<table>
<thead>
<tr>
<th>Bats active verbs</th>
<th>Georgian 'static' verbs</th>
<th>Slavic verbs</th>
<th>Creek active ppls.</th>
<th>Hopi parts of speech</th>
</tr>
</thead>
<tbody>
<tr>
<td>motion</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>position (location)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>shapes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>colors, textures, etc.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

In summary, we have seen two separate implicational sequences. The first is headed by psych-verbs, and often uses subject or object displacement. The second centers on decreasing agency and discreteness (potency and activity, in the terms of Osgood et al. 1957), and is manifested in voice displacement distinctions in part of speech. Psych-verbs may function in both hierarchies. Viewed as perceptual states, they enter into the second; viewed as two-place relations with animate subjects, they initiate the first.

Several other verb classes are candidates for inclusion in one or the other hierarchy. In Slavic, verbs expressing motion of a body part govern the instrumental: Russ. maxat' rukoj 'wave the hand'. In several languages (Indo-European, Caucasian) verbs such as 'hit', 'touch' exhibit object displacement. Both examples are suspect insofar as a spatial relation can be invoked: to wave with the hand, to hit or touch on. Verbs of possession may trigger special sentence constructions, e.g. genitive plus copula as used in the Caucasus and its analogs in nominative languages lacking a transitive have (Russian u menja est' 'I have', lit. 'to me is'). Where a transitive have exists it is usually a stative verb. Rather than genuine displacement, these facts seem to indicate borderline transitivity (for possession as location see Lyons 1968:388ff.). Voice displacement with verbs denoting weather and related phenomena is uninteresting in that such predicates lack logical arguments. The pan-Slavic instrumental with verbs 'smell', 'waft', etc. is conspicuous and tenuously connected to meteorological phenomena: Russ.

paxnet rozami  'it smells of roses (instr.)'
veet vesnoj    'there's a breath of spring (instr.)'
(See Mrázek 1964:193ff. for examples and a broader classification.) In Balto-Finnic such verbs would fit into the wider group of verbs of perception taking the ablative (allative, in eastern dialects) in the predicate nominal: Fi. tuoksua 'smell', nHyttää 'seem, look', kuulostaa 'sound' (Eliseev 92ff.). We are back among the psych-verbs.

Rigorous attention to displacement per sé, independent of its material realization, loses some explanatory value to the structuralist ideal. Clearly the morphosyntactic realization of displacement is not always arbitrary. It may have a transparent historical source, as when an ablative of separation is extended to evaluative or emotion verbs. Frequently it will have a synchronically motivated derivational source. For instance, languages having a dative case will almost invariably use it in the early phases of displacement. The category dative has some cross-linguistic validity as a morphological focus (although its peripheral functions will not always coincide): It has a double source: it is semantically assigned to nouns functioning as experiencers, goals, and/or beneficiaries; and it is syntactically assigned to the subject of an infinitive created by Equi. These rules, of course, overlap to a considerable extent. It is this substantive semantic fact that explains the parallel subject displacement of psych-verbs in both nominative and ergative languages. In both instances, the experiencer receives an oblique treatment as a result of displacement. Some of the early displacement of modals would then be due to syntactic assignment of the dative. (Since true modals and many peripheral modals may be used only with sentential complements, perhaps no modals will need to be analyzed as reflecting true displacement.) Otherwise we seem to find mirror-image displacement, although here we need more data. Emotion verbs, for instance, trigger object displacement in Finno-Ugric and Indo-European, but subject displacement in Avar.

Some languages are more prone to use displacement than are others, as is especially true of the first hierarchy. Uralic, Indo-European, and East Caucasian tolerate much more displacement than do West Caucasian, Basque, Paleosiberian, and apparently Turkic. Sometimes this absence of displacement may be internally supported. Paleosiberian languages exhibit a variety of 'subject' cases in response to animacy or gender-like properties of nouns (see Moščaninov 1967:Ch. V for a survey of construction types). Verbal displacement rules might overload such a system. The West Caucasian languages lack the variety of morphological cases required for subject and object displacement.

Displacement, when it occurs, follows universal lines. But the choice of whether or not to use it is language-specific, and language families seem to be remarkably conservative in this respect: recall the Indo-European verbs of authority, with a displacement pattern still visible after millennia of syntactic change and morphological renewal. Where not
internally conditioned, specific displacement patterns or a
general absence of displacement could be diagnostic for genetic
reconstruction. This conclusion has implications for the lin-
guistic situation in the Caucasus. West Caucasian may well not
tolerate subject displacement, but what is striking is its
apparent failure to achieve with any other device the psych-
displacement so characteristic of East and South Caucasian.
Similarly, East Slavic fails to show certain minor displacement
patterns of Balto-Finnic despite centuries of syntactic conver-
gence. If all of these facts are not just illusions created by
omissions from published grammars, we may have discovered an
area of syntax sufficiently stable to support genetic
hypotheses.

Footnotes

1 This paper originated in an effort to place the Slavic
facts in areal and typological perspective. Thus the bias in
the data base toward Eurasian languages and case languages.
The quotation from Saussure appears on p. 172.
2 Obviously there are interdependencies among these three.
Subject-selection failure typically alters voice in a nomin-
ative language; object-selection failure produces intransitive
constructions in ergative languages.
3 It is admittedly unorthodox to include modals among the
psych-verbs; but if the latter are defined by their experiencer
argument, then modals also strongly govern a nominal which is
better classed as an experiencer than as any other of the more
or less standard nominal functions.
4 Bats also permits the standard ergative construction with
these verbs: as (erg.) w-abc°o-gə̀o 'I (erg.) know you' (ibid.).
ə̀o is a pharyngeal fricative (Cyrillic x). In both examples in
the text, the direct object is in the unmarked case and the verb
agrees with this noun.
5 The first-person dative is used only to simplify the exam-
examples, some of which may sound stilted in this form.
6 The modern genitive may have syncrtized an older dative,
reconstructed partly on the basis of these constructions (pro-
on: Oinas 1961:8; con: Hakulinen 1961:69). The allative and adessive
cases used with some of these same verbs probably represent a
recent replacement under Balto-Slavic influence: minulla (adess.)
on kyrimà 'I am cold', minulle (all.) sopii 'it suits me'.
7 The reflexive form entails object-selection failure, since
reflexives cannot normally govern accusative direct objects.
This pattern is best analyzed as voice displacement, with ob-
ject displacement only a by-product, because with very few ex-
ceptions it seems triggered by verbal semantics and is not
contingent on transitivity and intransitivity.
8 Much the same displacement pattern is pan-Balto-Finnic and
occurs in Hungarian as well: ibid.
9 For formal renewal and its mechanism see Kuryłowicz 1964: Ch. I. In Avar the result of formal renewal of emotion verbs among psych-verbs was the appearance of the areally unmarked dative experiencer. This favored morphological structure could have been achieved in many ways, however. Of interest here is only the fact that precisely the emotion verbs were singled out: displacement, following universal semantic lines, facilitated conformity to an areal tendency in morphology.

10 This statement can be worded more strongly. In some, but not all, stative/active languages, sentence construction types are lexically governed by verbs and subject to many lexical exceptions. In such languages, it is the stative verbs that are the marked, minor class. If only the verbs of motion were active, they would be the marked class; but this pattern seems not to occur.

11 The psych-verbs included also some verbs of speaking. Like 'look at', these are the agentive derivatives we have seen in displacement classes mentioned earlier. Also included were the stative two-place verbs *dgereti 'have, hold' and (with a different present stem) *jimeti 'have'. Verbs of position, and perceptual phenomena corresponding to the fourth level of the hierarchy, are the most numerous representatives of the *m class (as is also true of Baltic and Latin).

12 See Kuryłowicz 1964:185ff. for an interpretation of the genitive with 'touch', 'hit', etc. as a reduced construction.

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Cyclicity and Extractability of Extraposition constructions in French
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Harvard University

0. There are in French two rules of extraposition of a sentential subject: Il-Extraposition which leaves the pronoun Il in subject position, and Ça-Extraposition which leaves ça or ce(c' before vowels) as the subject:

Il-Extraposition (Il-E)
(1) Il est tout à fait possible que Jean vienne.
   (It is very possible that Jean will come.)
(2) Il est probable que j'achète une voiture.
   (It is probable that I will buy a car.)
(3) Il serait intéressant de visiter Avignon.
   (It would be interesting to visit Avignon.)

 Ça-Extraposition (Ça-E)
(4) C'est tout à fait possible que Jean vienne.
(5) C'est probable que j'achète une voiture.
(6) Ça serait intéressant de visiter Avignon.

Although these two extrapositions look similar, they differ as to extractability: it is possible to extract from the extraposed clause in the Il-E ((7)&(9)), but not from the extraposed clause in the Ça-E construction ((8)&(10)).

(7) La voiture qu'il est probable que j'achète
   (The car that it is probable that I will buy
   est vieille mais pas chère.
   is old but cheap.)
(8) *La voiture que c'est probable que j'achète
   est vieille mais pas chère.
(9) Que serait-il
    Ou'est-ce qu'âil serait
       intéressant de visiter
   (What would it be interesting to visit?)
(10) *Que serait-ce
    *Qu'est-ce que ça serait
       intéressant de visiter

This correlates with the fact that
a) Il-E, like Extraposition from subject position in English (cf. arguments presented in Hankamer & Sag, forthcoming: "On the Cyclicity of Extraposition") can be shown to be cyclic. I will give three arguments to this effect in section 1.3.

b) For Ça-E on the other hand there are no arguments that it is cyclic, and in fact I will present in section 2. three arguments that it is postcyclic.
1. Cyclicity of Il-Extraposition

1.1 First argument

(11) Il a l'air d'être arrivé une fois de plus
(It looks like it happened once again that
she got lost.)

This sentence can be derived in two ways. If Il-E is
cyclic then we must be able to derive it by applying
Il-E on the lower cycle, followed by Raising into Sub-
ject position on the upper cycle.
The other derivation would be to apply Il-E post-
cyclically on the upper clause after Raising has applied.
In other words Raising would give (12), to which Il-E
would apply to give (11). However this derivation is
impossible, because the verb avoir l'air does not allow
Il-E, as can be seen in (13).

(12) Que Marie se soit perdue a l'air d'être arrivé
une fois de plus.
(13) *Il a l'air que Marie s'est perdue.
(It looks like Marie got lost.)

(The verb avoir l'air only allows Raising.)
The same argument can be made on the basis of other
sentences(14&15) where the upper verb also does not al-
low Il-E (16&17).

(14) Il est foutu d'arriver que Marie se perde.
(It is bound to happen that Marie get lost.)
(15) Il commence a m'arriver de me tromper sans
(It is beginning to happen to me that I make
arrêt,
mistakes all the time.)
(16) *Il est foutu qu'elle se soit perdue.
(17) *Il commence que je me trompe sans arrêt.

The argument given above crucially depends on the exis-
tence of the constraint on certain verbs that they do
not allow Il-Extraposition. In order to strengthen the
argument it is necessary to show that the constraint
holds for derived subjects as well as for deep-structure
subjects:

(18) *Il est foutu d'étonner Jean que Marie lui don-
ne un cadeau.
(It is bound to surprise J that M give him a
present.)
In sentence (18) the lower verb cannot extrapose (Cf. 19) therefore we must assume that the sentential subject gets raised. If être foutu allowed extraposition of derived subjects, (18) would be good, but it isn't. This shows that the constraint on être foutu holds for derived subjects as well as base subjects. The same argument can be made for (20) and (21).

This will be an important point to keep in mind throughout the arguments dealing with Extraposition rules: if a verb has certain constraints on the movement of its subject, these constraints are as true of derived subjects as they are of deep structure subjects.

1.2 Second argument.

Consider the following sentences:

(22) Il semble s'en suivre que J a décidé de partir.
(23) *Que J ait décidé de partir semble s'en suivre.
(24) Que J ait décidé de partir semble étonnant
      Que J décide de partir semble arriver souvent.
(25) Il s'en suit qu'il va partir.
(26) *Qu'il va partir s'en suit.

If Il-E is not cyclic then Raising would apply first to give (23) and then Il-E would apply over sembler, which allows Il-E. But (23) is ungrammatical. This means that if (22) is really derived by first raising and then Il-E we must claim that sembler takes obligatory Il-E. However this is clearly wrong as seen in (24). Ssembler does allow a sentential subject if it got into subject position by Raising.

If we adopt the derivation for which Il-E is cyclic we can account for *(23): the embedded verb s'en suivre requires Il-E, as seen in (25) & (26). Under these conditions
Il-E must apply on the lower cycle, preventing the later raising of the sentential subject, thus explaining the ungrammaticality of (23). The Il is what gets raised. Only if the Il-E applies on the lower cycle is (22) derivable. Therefore Il-E must be cyclic.

1.3 Third argument.

(27) Il est considéré nécessaire que tu viennes.
    (It is considered necessary that you come)

(28) *Il est considéré que tu as tort.
    (It is considered that you are wrong.)

For the present argument assume that the structure of the above sentence (27) is identical to that of the English sentence "It is considered to be necessary that you come." Also assume the rules of raising into object position and être deletion. I will justify these assumptions in 1.4.

If Il-E were postcyclic, (27) would have to be derived by extraposing the que-clause from the subject position, over the verb être considéré. However (28) shows that this is impossible, for that verb does not allow Il-E. We are forced to conclude that Il-E cannot be postcyclic.

Cyclic application of Il-E will give (27), since the embedded sentence contains the predicate (être)nécessaire which allows Il-E.

1.4 Arguments for Raising into Object position (RO)
The existence of RO in French has been contested. The alternative would be for the base to generate sentences of the form:

(29) NP V NP AP

where V belongs to a small class of verbs such as considérer, croire, trouver, etc... I will argue for RO against the alternative solution (29).

1.4.1 Distribution of infinitival vs que clauses after considérer, trouver etc.
The argument is that we find that there are restrictions on what kind of sentential object can occur with which AP and that these restrictions turn out to be the same as those required in a simplex sentence of the form NP beAP. Consider:

(30) Je considère stupide de se tuer sans raison.
    (I consider stupid to kill oneself w/o reason.)

(31) *Je considère peu probable de se tuer sans raison.
    (I consider it unlikely to kill...)

(32) Je considère peu probable que quelqu'un se tue sans raison.

(30), (31), (32) correlate with (33), (34) & (35):

(33) Se tuer sans raison est stupide
(§3) Il est stupide de se tuer sans raison.
(34) *Se tuer sans raison est peu probable.
   *Il est peu probable de se tuer sans raison.
(35) Que quelqu'un se tue sans raison est peu probable.
   Il est peu probable que quelqu'un se tue sans raison.

Être stupide allows an infinitival subject, but être probable does not. If there is no RO, we must state these restrictions twice; once for subjects of être AP, and once for objects of verbs like considérer. But the occurrence of infinitival objects of considérer follows with only one set of restrictions, if there is a simple S embedded under considérer, and if there is a rule of RO.

1.4.2 With a sentential object the AP has to be an Il-Extrapolation predicate.

Another ad-hoc feature of a base like (29) is that sentential objects are only allowed when the AP is an Il-E predicate in the simplex sentence NP be AP. Consider the following examples: (36) & (37) show that the predicate une bonne idée (a good idea) does not allow Il-

(36) Chatouiller un gorille n'est pas une bonne idée
   (Tickling a gorilla is not a good idea.)
(37) *Il n'est pas une bonne idée de chatouiller un gorille.
(38) Elle trouve ce voyage une très bonne idée.
   She finds this trip a very good idea.)
(39) *Elle ne trouve pas une bonne idée de chatouiller le gorille.
   She doesn't find it a good idea to tickle the gorilla.)

Although the predicate une bonne idée is fine as an AP after trouver when the object is a noun (as in (38)), the sentence is no good with a sentential object. Under the analysis that assumes the deep-structure (29), only an ad hoc restriction can account for this.

However the ungrammaticality of (39) will follow from that of (37) in the following way if we assume that there is RO: If RO exists, the DS of (39) must be like (40):

```
(40) NP → S → VP
    V → NP → S → VP
    S₀ → être une bonne idée
```
Sentence (37) shows that in S1, Il-E cannot apply. Therefore when RO applies it raises the sentential subject S0 into Object position giving (41)

(41) *Je ne trouve pas chatouiller le gorille une très bonne idée.

This is exactly the same judgement as for the English sentence (42):

(42) *I find to sit in the park very enjoyable.

The ungrammaticality of (41) and (42) is explained by the independently motivated Internal NP Clause Constraint (cf. Ross 1967, Kuno 1973 for discussion). This constant will rule out any sentence in which the embedded sentential subject is raised into object position. Therefore sentence like (41) can never arise.

The difference in grammaticality between (43) and (44)

(43) *Je ne trouve pas une bonne idée de chatouiller le gorille.
(44) Je trouve stupide de chatouiller un gorille.

is due to the fact that in (44), Il-E can optionally apply on the lower cycle, but that in (43) Il-E is impossible on the lower cycle (see (37)).

The contrast between (43) and (44) is in itself an argument for the cyclicity of Il-E, for if it could not apply on the lower cycle, sentence (44) would be ungrammatical. And in that case, even postulating a sentential-object shift rule to allow (45) to escape the Internal NP Clause Constraint will not solve the problem:

(45) *Je trouve chatouiller un gorille stupide. by shift rule

to give (44).

There would still be left to explain why this hypothetical shift rule cannot apply to sentences like (41) and give a grammatical sentence (note that the output of such a rule would be (43) which is ungrammatical).

1.4.3 Derivation of sentences with Il-E.

(46) Il est considéré absolument nécessaire que tu viennes.
     (It is considered absolutely necessary that you come.)

This sentence needs to be derived. As seen above (27), if there is an embedded sentence, RO, and if Il-E is cyclic, the grammaticality of it follows.
However with a deep structure like (29), no derivation of (46) seems possible. We cannot say that Passive applies and then the sentential subject is extraposed, because the verb être considéré does not allow II-E. There is one alternative left, which is to say that there exists a rule of object extraposition which has the following properties:

a. It applies only to the verbs that get into structures like (29): considerer, trouver, croire, etc...

b. It applies only to sentential objects: objects of verbs like considérer that are simple nouns cannot appear in final position, but must remain between the verb and the AP. Note that this property would follow automatically if sentences like (46) are derived by a rule of extraposition from subject position.

c. It applies only over II-E predicates.

d. It has the same restrictions with respect to these predicates as II-E does.

e. It is cyclic because it must precede Passive (in(46)).

This rule has the same properties as II-E and no properties of its own, which makes it very suspicious looking. Just like the shift rule mentioned above, this extraposition rule is an ad hoc device to recreate the effects of subject extraposition in an embedded S in the framework where neither an embedded S nor the rule of RO are believed to exist. Allowing such a solution means dividing a very simple general rule into independent processes, most of which are not otherwise motivated. Therefore postulating a rule of RO & an embedded S, which lets you keep a unified rule of Extraposition from Subject position, is a better alternative.

1.4.4 Conclusion to arguments for RO

Three arguments were presented that there is a rule of RO in French. I believe them to be convincing. However two points remain obscure:

- why is there no verb être after RO ?
- why does the raised II not appear in object position, but (re)appear in subject position after Passive has applied ?

The easiest solution to the first question is that there is a rule of être deletion (when the embedded S does not contain a deletable être, RO cannot apply).5

A possible answer to the second question is that languages differ as to where they must leave pronouns to mark a position in a sentence: English requires a pronoun both in subject position and in object position, French seems to do without the pronoun in object position, and I believe that Spanish for example, can do without either one. These questions need to be investigated.
2.0 Ça-Extrapolation is postcyclic.

We have seen that the construction that allows extraction out of the extrapoled sentential subject is derived by the cyclic rule of II-E. I will now show that the rule that derives sentences where extraction is impossible out of the extrapoled sentential subject cannot be cyclic but must be postcyclic.

2.1 There exist no arguments, to my knowledge, that Ça-Extrapolation is cyclic. This is the first out of three that it must be postcyclic:

The verb arriver (to happen) takes both II and Ça-Extrapolations, as in

\[\begin{align*}
(47) & \text{Il arrive que l'administration renvoie des employés illiciter.} \\
& \text{(It happens that the administration fires some employees illegally.)}
\end{align*}\]

\[\begin{align*}
(48) & \text{ Ça arrive que l'administration renvoie des employés illiciter.}
\end{align*}\]

But notice the contrast:

\[\begin{align*}
(49) & \text{Il a l'air de menacer d'arriver que l'administration renvoie des employés illiciter.}
\end{align*}\]

\[\begin{align*}
(50) & \text{ Ça a l'air de menacer d'arriver que l'administration renvoie des employés illiciter.}
\end{align*}\]

Since II-E is cyclic we can say that in (49), it has applied on the lowest cycle, followed by two applications of Raising with Menacer and avoir l'air. If Ça-E were cyclic we would expect exactly the same derivation to be possible: Ça-E over arriver and then Raising of the dummy subject, in this case ça. But as we see it is impossible (sentence (50)). Therefore Ça-E cannot be cyclic.

However there is a problem: Ça-E seems to apply regardless of what the verb is, which means that under a postcyclic analysis of Ça-E, we would expect it to apply over the verb complex a l'air de menacer d'arriver, giving (50). But notice the following:

\[\begin{align*}
(51) & \text{*Qu'on les renvoie a l'air de menacer d'arriver.} \\
& \text{(It looks like it threatens to take place that they be fired.)}
\end{align*}\]

\[\begin{align*}
(52) & \text{*Qu'ils renvoient des employés arrive.} \\
& \text{(That they fire employees happens.)}
\end{align*}\]

\[\begin{align*}
(53) & \text{Qu'ils renvoient des employés arrive très souvent.} \\
& \text{(That they fire employees happens very often.)}
\end{align*}\]

\[\begin{align*}
(54) & \text{Qu'ils renvoient des employés injustement a l'air.} \\
& \text{(That they fire employees unjustly seems to threaten to happen very often.)}
\end{align*}\]
The verb *arriver* cannot have only a sentential subject in (52). This does not mean that Ça-Ê is obligatory as can be seen in (53) and (54), but rather that there exists a constraint against leaving verbs like *arriver*, which do not have much semantic content at the end of their sentence; this I will call the No Dangling constraint. This predicts that we can improve (50) either by adding something after *arriver* or by changing the lowest verb to one not sensitive to the constraint.

(55) Ça a l'air de menacer *d'arriver* souvent qu'ils renvoient des employés injustement.
(56) Ça a l'air de commencer à lui sembler louche (It seems to be starting to look fishy to him que Marie rentre si tard. that M. come back so late.)

Therefore it seems true that (50) is out because the semantically 'light' verb is left dangling. We need to explain why *arriver* is not felt to be dangling in (48) and (57):

(48) Ça arrive que l'administration renvoie des employés illégalement.
(57) ?Ça menace *d'arriver* que des employés soient renvoyés illégalement.
(*Dislocated intonation.)

The more Raising verbs precede *arriver*, the more the extraposed sentential subject needs to be dislocated, with a pause before it and also the sentence intonation characteristic of dislocation (rise of level of pitch and drop on the sentential subject.). Sentences (55) and (56) are good only as dislocations. However (48) and (57) do not require a break or special intonation, and in fact (57) is out as a dislocation. In the cases of Extraposition without dislocation, the verb *arriver* is not in any sense dangling and the No Dangling constraint does not apply. But when only the dislocated reading is possible, as in the cases where the verb complex contains more than two verbs, the constraint rules out the sentences.

Therefore the explanation of the ungrammaticality of (50) under the theory that Ça-Ê is postcyclic is based on these two factors:
- the No Dangling constraint.
- Ça-Ê becomes like right dislocation when it applies over several verbs.

Notice that if Ça-Ê were assumed to be cyclic, it would be supposed to extrapose over one verb at a time, and there would be no way to explain the difference in intonation between (56) and (48).
2.2 An argument similar in nature to the first one can be given for sentence (58):

(58) *Ça a été trouvé nécessaire que tu fasses un effort.

(It was found necessary that you make an effort.

The cyclic analysis of Ça-£ cannot explain the ungrammaticality of (58) for it should be derivable by applying
1) Ça-£ -2)Raising into object position -3)Passive. It would then be parallel to (59), which is grammatical:

(59) Il a été trouvé nécessaire que tu fasses un effort.

The contrast between (58) and (59) shows that while on the one hand Il-£ is cyclic, Ça-£ has to be postcyclic.

2.3 The rule of Ça-£, unlike Il-£ which can apply freely, is discourse conditioned. It can apply only when all the information contained in the sentential subject is known from context or previous discourse.

The contrast can be seen in the following interchange:

-Qui va venir demain?
(60) -Il est probable que Jean viendra.
(61)? -C'est probable que Jean viendra.

(It is likely that Jean will come.)

In (60) and (61) Jean is the new information requested by the first speaker. It cannot be part of the extraposed clause in (61). However (63) is good because the extraposed clause contains no new information:

-Est-ce que Jean va venir demain?
(62) -Il est possible qu'il vienne.
(63) -C'est possible qu'il vienne.

It follows from this that Ça-£ sentences cannot be discourse initial (no information presupposed). Thus while (64) is well-formed as the first sentence of a conversation, (65) is not:

(64) Il est possible que je m'en aille en vacances.

(It is possible that I leave on vacation.)

(65) *C'est possible que je m'en aille en vacances.

It has been argued (Hankamer, 1973, 1974) that only non-cyclic rules are discourse sensitive, therefore we expect Ça-£ not to be cyclic, but postcyclic. The contrast with Il-£ supports this conclusion.
3. Conclusion.

We have seen that the two Extraposition constructions of French differ as to extractability:

- in Il-E construction extraction is possible out of the extraposed sentential subject.
- in Ça-E construction extraction is impossible out of the extraposed sentential subject.

This correlates with cyclicity: -Il-E is cyclic

Ça-E is postcyclic

How can we block extraction out of postcyclically extraposed S-subjects? The Extraposition constructions are identical except for the dummy subject. A possible solution would be to make extraction rules sensitive to the presence of Ça. However this will not do because there are many sentences containing que-clauses and Ça that have nothing to do with Ça-E constructions, and from those que-clauses extraction is allowed.

Another reason that such a solution must be wrong is that there exists a more general principle that can explain the facts: other pairs of rules have the same differences in behavior as Il/Ça-E, and only the cyclic one allows extraction out of what it moves (cf. Pinkham, 1975). One such pair is Extraposition from Subject position / Extraposition from Object position, which exhibit the following contrast in extractability:

(66) What do you expect it to be most likely that she will make for dessert?

(67) *What would you like it very much that she prepare for dinner?

The conclusion to this paper is therefore that the correlation between extractability and cyclicity demonstrated for the rules of Il-E and Ça-E is a reflection of the general principle:

Constituents moved by postcyclic movement rules become Islands.

1. Many thanks to Jorge Hankamer for his helpful suggestions, comments, and unlimited encouragement. Comments made to me by Michael Szamosi and Paul Neubauer were also very useful.

2. Several native speakers of French agree with me on their judgements. It should be emphasized that it is the contrast between sentences that I am interested in, not the absolute judgements of grammaticality.

3. Jacobson and Neubauer (1974) have presented arguments for the cyclicity of Extraposition in English, one of which is superficially similar to those presented here. Their argument, however, depends crucially on an assumption about the correct formulation of an exception feature for the
verb seem, an assumption that they do not justify. For this reason I do not think that the particular argument which they present can be considered convincing.

4. There is a rule of Right Dislocation (from object position) which leaves Ça: it operates on NP-clauses as well as regular NP's.

Je trouve ça laid, les gorilles.

Je trouve Ça stupide, de chatouiller un gorille.

5. Michael Számosi (in preparation) argues against RO in French and to be deletion (être deletion) in general.

6. Ça-E does not apply over être trouvé, probably because of the impersonal nature of the construction. The argument is not affected, since we also find the following contrast, which shows that Ça-E cannot be cyclic:

i) On a trouvé nécessaire que tes parents viennent.
   (It was found necessary that your parents come.)

ii) (?with pause) On a trouvé ça nécessaire qu'ils viennent
   (*without pause)

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THE ACOUSTICS OF TONOGENESIS

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University of Southern California

In recent years much interest has focused on the phonetic origins of tone systems. Cope (1970), Ladefoged (1971), and Louw (1971) among others have presented phonological evidence that certain consonants modify the realizations of tones in several African languages. Phonetic studies of English such as Lehiste and Peterson (1961) and Lea (1973) indicate that certain consonants affect the pitch of neighboring vowels; e.g. voiceless aspirated stops raise the pitch of the initial part of a following vowel, voiced stops lower the pitch of the initial part of a following vowel, and both lower the pitch of final part of a preceding vowel. Haudricourt (1972) presented data from Camuhi, which can be taken to indicate that the merger of fortis (aspirated?) stops with plain stops generated three tones: a high tone on following vowels, a low tone on preceding vowels, and a mid tone whenever the word contained no such consonant.

Data from Gill and Gleason (1963) on the correlation of Hindi breathy voiced stops with Panjabi tones have been cited by various phonologists as an instance of the consonantal origins of tone. Phoneticians have sighted the data in their listing of consonantal effects on fundamental frequency. Recently the Panjabi/Hindi data has been proposed as a model for the historical development of tone (Ohala, 1974). Purcell (1974) proposed a model for the development of tone in Balto-Slavic, very similar to the Panjabi/Hindi model.

Because no extensive phonetic study of this exemplary instance of tonogenesis had yet been reported, it was decided to investigate the acoustic manifestations of the 'before' and 'after' of tonogenesis on the Indian subcontinent. A list of Hindi and Panjabi cognates was prepared: five words exemplifying the correspondence CV ~ CV, five words exemplifying the correspondence VC ~ VC, five words exemplifying the correspondence Vh ~ Vv, and five words exemplifying the correspondence CV ~ CV. Test words were presented in isolation and in the middle of a frame sentence.

Panjabi  [kɪtəb dəwɪtʃ ɪə]  [abd he]
Hindi  [jeh ɪəbd ɪə kɪtəb me he]

Three linguistically naive native speakers of Hindi and three linguistically naive native speakers of Panjabi read the appropriate texts. Data were recorded on a Uher 4000-L and analyzed on a Kay Elemetrics spectrograph. Measurements of fundamental
frequency were made on narrow band expanded scale spectrograms at
the start, end, and fundamental frequency peak within vowels.
Segmentation procedures used were those set forth in Peterson and
Lehiste (1960) including the modifications proposed by Naeser

Figures one, two, and three present the data for similar
syllable structure, comparing Hindi consonantal effects to Panjabi
tone. A cursory examination of these plots should indicate that
the general slope and configuration of the Panjabi tones bear a
great resemblance to the Hindi consonantal effects. This resem-
bliance is not surprising in view of the widely held opinion that
Panjabi tone developed from the consonantal effects which are
still manifested in Hindi.

Figures four and five present the data for Panjabi and Hindi
separately, contrasting the Hindi consonantal environments and
the Panjabi tones. As shown on figure four (a), a postvocalic
breathy voiced consonant effects a higher fundamental frequency
within the preceding vowel, while a prevocalic breathy voiced
consonant effects a lower fundamental frequency on the following
vowel. Panjabi (figure four (b)) shows similar fundamental
frequency patterns without the conditioning consonants still
present in Hindi.

A careful examination of figure four will reveal that the
differences between the Panjabi high and low tones are greater
than the differences between the curves representing Hindi con-
sonantal effects. If we calculate the percentage increase from
the lower to the higher curve at the three respective points
(start, mid, and end) then the differences between the Panjabi
tones are on the average 4% greater than the corresponding
consonantal effects in Hindi. This greater difference between
the Panjabi tones than between the Hindi consonantal effects is
surely due to exaggeration. Panjabi has made the intertonal
differences greater than the original consonantal effects. Chen
(1970) presented data for the exaggeration of vowel len\thenni
before voiced consonants in English; here we present data for
the exaggeration of pitch.

Figure five presents averages for Hindi comparable consonantal
effects and averages for comparable Panjabi tones, in disyllabic
words. Since the Panjabi and Hindi cognates sometimes had differ-
ent vowels and since the comparable high and low environments in
either language show vowel quality differences which can be as-
sumed to affect actually measured fundamental frequency, it was
decided to factor out these intrinsic pitch effects. Therefore,
figure five presents averages adjusted to reduce the pitch effects
of vowel quality differences. By factoring out intrinsic pitch ef-
fec ts, we obtain a truer picture of the relative realizations of
Hindi consonantal effects and Panjabi tones. The data show that
Panjabi intertonal differences are on the average 2% greater than
the differences between the comparable Hindi consonantal effects,
when comparing equivalent points in the first syllable. Panjabi
differences in the second syllable are on the average 1% greater than the Hindi effects. Thus the exaggeration fades as we get further from the original consonantal location. This same trend holds in figure four. Our data show that Panjabi intonational differences are 10% greater than the Hindi consonantal effects at the start of the vowel, 5% greater at the mid, and 4% less at the end. From this we infer that the exaggeration fades as one moves further from the location of the original consonants.

Another very interesting phenomena appears in our data. As can be seen in figure five, the lower/higher effects of Hindi breathy voiced consonants appear in the second syllable, even though the consonants occur in the first. The same holds true for the Panjabi data: the high/low distinction appears in the second syllable; indeed the difference is greater in the second syllable than in the first. Even though the original prevocalic breathy voiced consonant in the first syllable was far from the vowel in the second syllable, an effect obtains. And even where our model would lend one to believe that the pitch should drop after a postvocalic breathy voiced consonant, when that consonant appears in the first syllable, the pitch remains high. In short the higher/lower effects obtain in both languages, in both syllables. In fact, the percentage differences between high and low environments are much greater in the second syllable than in the first; in spite of the fact that both languages consider the first syllable to be prominent. There is little in current models of phonetics or phonology that would explain the appearance of these differences in the second syllable. It is very interesting to note than studies of Serbo-Croatian word-tone (Lehiste and Ivč 1963) and Purcell (1973) have noted exactly the same phenomena: even though the first syllable is considered prominent, the second syllable tends to show differences in acoustic cues for tone which are greater than those which appear on the first. In our view the similarity between Panjabi/Hindi and Serbo-Croatian phenomena is more than accidental. Purcell (1974) hypothesized that Balto-Slavic tone originated from the sorts of consonantal effects and losses exhibited by Hindi and Panjabi.

Figure 6 displays the data for breathy voice effects in Hindi and corresponding Panjabi tones, for monosyllabic words; with the addition of the data for postvocalic [h]. As can be seen, there is a great degree of resemblance of the curves for postvocalic [h] and the curves for Hindi postvocalic breathy voice and Panjabi high tone. These data showing that [h]raises pitch on a preceding vowel match Wayne Lea's (1973) data for American English.

Time does not permit us to present the data for Panjabi mid tone and Hindi 'other' environments, but briefly we can say that they do not pattern neatly, and they do not fit published impressionistic descriptions of Panjabi tone. Figure 7 shows data for a minimal triplet for the three Hindi consonantal environments and the three Panjabi tones. Note that the Hindi environments pattern as expected, as do the Panjabi high and low tones,
while the Panjabi mid tone is not as expected.

In summary, we have presented data on tonal patterns in Panjabi and corresponding consonantal environments in Hindi. We take this data, along with comparative etymological evidence as proof that Panjabi tone originated from the loss of previously existing consonants. We have shown that there is a good match between Panjabi tonal contours and Hindi consonantal effects. We have shown that Panjabi has exaggerated the consonantal effects visible in Hindi. We have shown that the exaggeration tends to fade as one moves along the time dimension further from the affecting environment. We have shown that the pitch effects of Hindi consonants obtain in the second syllable, even though the consonants occur in the first. Similarly, Panjabi tonal contours exhibit the greater effects in the second syllable, even though the first syllable is judged prominent and the Panjabi tone is ascribed to this first syllable. We have pointed out that similar phenomena occur in Serbo-Croatian, where phonological tone, ascribed to the first syllable, actually shows greater acoustic differentiation in the second. We feel that this similarity is hardly accidental, that it is attributable to the Indo-European system of word prominence and similarities of historical development. And finally, we have presented the data for original postvocalic [h] and cited the resemblance between the effects of this consonant and postvocalic breathy voiced consonants.

**Notes**

1. Our test words were:

<table>
<thead>
<tr>
<th>Panjabi</th>
<th>Hindi</th>
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<tbody>
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<td>CV(C)</td>
<td>ČVC</td>
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</table>
Grateful thanks to Saeed Ali for his help in arranging the test corpus.

2. Our method of computation was the following: if the value at the start of the lower curve was 100 Hz and the value of the upper curve was 110 Hz, then the upper curve represented a 10% increase over the lower at the start, and there was a 10% difference between the two curves at this point.

3. Our reanalysis of the data for vowel intrinsic pitch was based on Lehiste and Peterson's (1961) data. First, we summed the intrinsic pitch values for all vowels in disyllabic words, in each syllable, for each consonantal environment. Next we calculated the mean intrinsic pitch value for all vowels in all syllables, for all environments (168 Hz). Next we compared the calculated intrinsic pitch for each syllable and each consonantal environment with the grand mean. If they differed, we adjusted our actually measured values accordingly. For instance, the second syllables of our Hindi CVC disyllabic words contained the vowels /a/, /i/, and /u/. Using Lehiste and Peterson's data, the mean intrinsic pitch for the three vowels would be 176 Hz. Our grand mean for all vowels in all environments was 168 Hz. Therefore, the means of our actually measured values for the second syllable of Hindi CVC words were lowered by 8 Hz. We lower the means for the start, peak, and end of the vowel, since the words in our corpus contained vowels which could be expected to artificially raise the relevant f0 values.

References


Figure one. Mean fundamental frequency vs. duration plots for monosyllabic and disyllabic cognates; Hindi postvocalic breathy voiced consonants vs. Panjabi high tone.
Figure two. Mean fundamental frequency vs. duration plots for monosyllabic and disyllabic cognates; Hindi prevocalic breathy voiced consonants vs. Panjabi low tone.
Figure three. Mean fundamental frequency vs. duration plots for monosyllabic and disyllabic cognates: Hindi 'other' consonants vs. Panjabi mid tone.
Figure four. Mean fundamental frequency vs. duration plots for monosyllabic cognates; (A) Hindi prevocalic vs. postvocalic breathy voiced stops and (B) Panjabi high vs. low tones.
Figure five. Mean fundamental frequency vs. duration plots for disyllabic cognates; (A) Hindi prevocalic vs. postvocalic breathy voiced consonants and (B) Panjabi high vs. low tones.
Figure six. Mean fundamental frequency vs. duration plots for monosyllabic cognates; (A) Hindi postvocalic h (or reflex of a postvocalic h) vs. prevocalic and postvocalic breathy voiced consonants, and (B) Panjabi high and low tones from equivalent cognate consonants.
Figure seven. Mean fundamental frequency vs. duration plots for three cognates; Hindi consonantal effects vs. Panjabi tone.
ARTIFICIAL INTELLIGENCE AND LINGUISTICS:
A Brief History of a One-Way Relationship
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1. Introduction

For the past fifteen years there has been serious interest in the processing of natural language (English) by researchers in Artificial Intelligence (A.I.). This processing has included machine translation, question-answering (Q-A) systems, man-machine dialogue, and speech understanding. This keen interest has engendered an awareness of and a concern with ongoing activity in contemporary linguistics. Therefore, it may be of interest to linguists to discover what has seemed important for A.I. and how it has been adapted and used. Furthermore, I would suggest that current activity in A.I. should be of interest in its own right to linguists concerned with the whole range of problems involved in human language understanding.

An enduring definition of the goal of A.I. research is "to construct computer programs which exhibit behavior that we call 'intelligent behavior' when we observe it in human beings." (Feigenbaum and Feldman 1963, p.3) Winograd (1971), in discussing the goals of his research—which include the desire to have a usable language-understanding system and to gain a better understanding of what language is and how it is put together—states:

More generally, we want to understand what intelligence is and how it can be put into computers. Language is one of the most complex and unique of human activities, and understanding its structure may lead to a better theory of how our minds work.

These remarks suggest that there should be closer cooperation among A.I. researchers and linguists, although this clearly has not been the case, as Walker (1973) notes:

Linguists for the most part have not accepted the computer or even computation as an essential methodological component of their field. Moreover, many linguists have denied not only the relevance of the results of computational linguistics research for linguistics, but, more importantly, the possible relevance of such results.

I suspect this situation may derive from positions which can best be expressed by two hypothetical researchers:

A.I. Researcher: Linguists are too preoccupied with very small aspects of the total language problem. They need to look at the larger picture and they also need the realities of a computer program to constrain their imaginations in order to produce a more precise formulation of their theories.

Linguist: So far, A.I. researchers have trivialized the complexities of language understanding. They write large programs dealing with narrow domains involving relatively simple grammatical constructions. They just don't appreciate that we know so very little that a lot of
basic research is of the highest priority.

From the outset, the A.I. researcher has been compelled to deal with problems of syntax, semantics, context, pragmatics, and representation of knowledge for the design of even the simplest system. And although the treatment has often appeared superficial, recognition of the simplifying assumptions made and the difficulties of the problem has always been made. It is my task here to amplify this point of view.

The following sections will consist of a brief discussion of machine translation, a more extended treatment of Q-A systems, and finally a look at current research in speech understanding.

2. Machine Translation

Machine translation as a modern enterprise began in the late 1940's, and as of the present date it can be said that fully automatic high quality translation (FAHQT) has not been achieved nor are the prospects hopeful. The major effort in machine translation was a precursor to A.I. research, and its failures have been used to cast doubt upon the whole A.I. endeavour (see Dreyfus 1972). As early as 1959, Bar Hillel (1960), one of the earliest workers in this field and later its severest critic, argued that FAHQT was not a reasonable goal and was even an impossible one. Since much of the work up to that time had made use of rather simple grammatical notions together with large dictionaries and was basically word for word, his position was not altogether unreasonable.

But Bar Hillel did comment on the optimism aroused by the then recent achievements of Noam Chomsky (1957) and the hopes that his transformational model would lead to success where previous syntactical analysis had not. However, much of the work in mechanical translation continued to be based on the notion that words are the units of meaning and to this idea were added "notions of thesaurus classes of words, statistical associations, probabilities and superficial syntactic structures" (Simmons 1970). The final blow came with the issue of the ALPAC (1966) report, which signaled the effective end of large-scale support for machine translation.

One legacy of machine translation was a growing appreciation of the complexities of language and an awareness of the necessity to integrate various kinds of knowledge if language was to be dealt with in any meaningful manner. Recently there has been a revival of interest (see Wilks 1973a, 1973b). It should be mentioned that machine-aided translation involving either pre-editing or post-editing is an alternative but not a particularly attractive one to researchers in A.I.

3. Question-Answering Systems

For many of the early systems, the decision was made to avoid the necessity of dealing with the full complexity of natural language by the use of specially designed formats for both the input and the representation of knowledge within the system. Because the following treatment will of necessity be brief, the reader would be well advised to consult such surveys as Bobrow (1963), Simmons (1965,1970), Kuno (1967), Walker (1973a), and more detailed sources such as Feigenbaum
and Feldman (1963), Minsky (1968), Winograd (1972), Schank and Colby (1973), and Rustin (1973).

**BASEBALL** (Green et al 1961)

This computer program answers questions posed in ordinary English about wins and losses in American league baseball games. Typical questions are:

Where did the Red Sox play on July 7?
What teams won 10 games in June?
How many games did the Yankees play in May?

For our present purposes it is important to note that the linguistic routines involve a dictionary look-up for parts of speech and definitions, a syntactic analysis based on the work of Zelig Harris (1960) which is usually successful, in this case because of the restricted format for questions, and a semantic routine which uses the dictionary meanings and the results of the syntactic analysis to produce a specification list with which the fixed data structure can be searched. This program works well within its very limited domain, especially with such restrictions on the input sentences as single clauses, prohibition of "and", "or", and "not" as well as constructions involving relations such as "most" and "highest". But basically the level of language understanding is quite limited.

**SAD SAM** Sentence Appraiser and Diagrammer and **Semantic Analyzing Machine** (Lindsay 1963)

The basic semantic information of interest is family relationships, e.g. father, mother, brother, etc. There are eight such relationships in Basic English, a 1700-word subset of English which provides the acceptable vocabulary for the program. The task of the program is to construct family trees by extracting the kinship relationships contained in the input sentences. Sentences are analyzed by means of a context-free grammar, and the parse tree is used to extract the contained kinship relation. Since the semantics are limited to kinship terms, everything else in the parse is ignored, so that a sentence like

John visited his sister Mary in Chicago during the summer of 1967.

will result in a structure being built which represents only the information that John and Mary are siblings.

This program foreshadows the dominant linear processing paradigm of the 1960's: First parse to produce a representation of the input sentence, usually a tree; then apply "semantic" routines to produce a query language statement; finally, execute to retrieve information from the data base (see Kuno 1967). The use of grammars expressed as systems of rules is entirely due to Chomsky, and his influence is very pervasive in this period.
STUDENT (Bobrow 1964)

STUDENT is a program designed to accept an English language statement of a high school algebra problem, to convert it into a set of simultaneous linear equations, and finally to solve this set (if possible) and produce an answer. A typical problem is

If the number of customers Tom gets is twice the square of 20 per cent of the number of advertisements he runs, and the number of advertisements he runs is 45, what is the number of customers Tom gets?

Bobrow makes use of a notion of kernel sentences and transformations which he claims are different from Chomsky's; however, his idea is to assume an underlying structure which must be uncovered by a small set of sentence forms. In this restricted domain, meaning is expressible as a set of equations of the form P1=P2, where P1 and P2 may be strings of uninterpreted symbols. The program contains a number of procedures necessary to carry out simple pronoun reference, to match strings which are formally different, to relate the subject of one sentence to its immediate predecessor in the text, etc. Because of the restricted domain, a fairly small set of prestored sentence forms is sufficient to deal with a large number of apparently different sentences. A somewhat extended version of this notion is the basis for programs called by various names such as Doctor and ELIZA (Weizenbaum 1966).

Logic-Based Systems

In 1958, John McCarthy presented a paper titled "Programs with Common Sense" in which he proposed a program called Advice Taker, which would solve problems by manipulating sentences in formal languages. This motivated research in at least three directions: the development of adequate formal representations for natural language sentences, the investigation of problem solving in formal systems, and the study of methods for translating from natural language into formal languages.

Raphael (1964) developed a program called SIR (Semantic Information Retrieval) with the goal being to derive answers to simple questions expressed in natural language by the use of deductive procedures. As such, the class of input sentences was quite restricted, being limited to set relations, ownership, part-whole, number, and position. Employing a large number of special purpose routines and operating on a symbolic representation of the input information, the program was able to perform rather simple deductions.

In the mid-1960's, a powerful proof procedure for predicate calculus was developed by J.A. Robinson (1965). Thus, if problems could be formalized in predicate calculus, this procedure, called resolution, offered the possibility of an effective theorem prover as a problem solver. It should also be mentioned that during this period as well there was a considerable research effort devoted to robot systems which it was hoped could be commanded in English to carry out non-trivial tasks requiring some form of deduction. A new
approach thus emerged which required the translation of English into predicate calculus, the application of a theorem prover to the resulting formula, and, if successful, the return of a solution. For example, the input

Is there a green box under the table?

would be translated into something like

\[(\exists x, y) [\text{Box}(x) \land \text{Green}(x) \land \text{Table}(y) \land \text{Under}(x, y)]\]

The theorem prover must now determine if there are objects x and y which satisfy the above formula and if there are, the answer to the question will be yes with the specific objects named. Of course, this approach requires all information about the domain to be stored as predicate calculus formulae.

A problem requiring deduction for solution, for example, a version of the famous 'monkey and bananas' problem, will return the sequence of operations necessary for a solution (go to the box, push the box under the bananas, climb the box, get the bananas). The most important work done in this period was Green and Raphael (1968). Other interesting efforts using predicate calculus are Sandewall (1971) and Coles (1972). However, this overall approach has lost much favour, because for large data bases, the theorem prover, employing various refinements of the resolution principle, has turned out to be rather inefficient.

It is interesting that there has been a simultaneous revival of interest in the formal modeling of natural language in philosophy and linguistics as well as A.I. In linguistics the names of George Lakoff (1970) and James McCawley (1968, 1969, 1972) are particularly prominent. It must be noted however that on the whole their work has had little influence in A.I. Almost the only reference to it is to be found in Wilks (1972, 1973b). In the former he expresses strong disagreement with Lakoff's approach. In fact, much of the effort of the generative semanticists has gone virtually unnoticed in the A.I. community. Chomsky (1965) has continued to exert considerable influence even though his theories have come under strong and continual attack since 1966. This in itself is noteworthy, for with his emphasis on syntax to the exclusion of semantics, pragmatics, and much else, his point of view is in direct opposition to the requirements of Q-A systems. Nevertheless, there is a certain attraction to Chomsky's framework for syntax which lends itself to computer implementation. It is not surprising that the unsettled state of generative semantics should discourage the interest of researchers in A.I.

Augmented Transition Network Grammars

The names of W. Woods (1970, 1973) and of Woods and Kaplan (1971) are most prominently associated with this approach to natural language processing. Woods (1968) proposed the use of procedural semantics for a Q-A system. Assuming that the program had available a deep structure parse or phrase marker (in the Chomsky sense) of the
input question, he developed a system which attempted to match parts of the tree with a collection of prestored sub-trees. Each successful match would result in the incorporation of a piece of LISP code into a growing program. The execution of the completed program results in an answer to the original question. An example of a question and its semantic interpretation in terms of predicates and functions is

What is the departure time from Boston of every American Airlines flight that goes from Boston to Chicago?
(FOR EVERY X2/ FLIGHT: EQUAL (OWNER (X2), AMERICAN AIRLINES) AND CONNECT (X2,BOSTON,CHICAGO); (FOR THE X1/DTIME (X2,BOSTON): LIST (X1)))

The augmented transition network (ATN) grammar is a formulation of a transformational grammar which produces the (Chomsky-like) deep structure parse for input to the semantic processor. More detail is found in Woods (1970, 1973), but for the present it can be said that an ATN grammar is a collection of finite state graphs which allow recursive calls on the arcs and which has provision for storing partial parses. Some of its virtues are the clarity of the representation, the speed of processing, and the way in which regularities of language are captured by having a single piece of network serve multiple uses. The Woods system probably carries the linear paradigm for natural language processing as far as it can go.

Most linguists of a generative semantics persuasion would probably dismiss this whole effort as misguided and hopelessly inadequate as a model for language processing. But it does provide a fairly powerful system for certain practical situations. A prototype of Wood’s system was designed as a Q-A fact retrieval program to answer a geologist's questions about the reported chemical analyses of lunar rock samples brought back from Apollo missions. With a data base of approximately 13,000 individual chemical analysis measurements, the program performed remarkably well when run during a Lunar Science Conference in 1971. (See Woods 1973) Sample questions are

Do any breccias contain Aluminum?
What are they?
In how many breccias is the average concentration of Aluminum greater than 13 per cent?

It is also clear that the construction of such large systems does reveal many factors involved in the task of natural language processing and reveals them in a particularly immediate fashion.

SHRDLU (Winograd 1971, 1972, 1973)
This unlikely string of symbols is the name of Terry Winograd’s very important program for natural language understanding. A major departure is made by Winograd from the by now traditional generative grammar approach, to a view of language enunciated by M.A.K. Halliday (1970). There is a strong similarity between Halliday's position
and that of Fillmore (1968) to whom we shall return subsequently. Halliday's theory views meaning as having a central place in the way language is structured, and he proposes a number of "system networks" which describe how different features of a sentence interact and depend on one another. This is reflected in the following (Winograd 1971):

If we really want computers to understand us, we need to give them the ability to use more knowledge. In addition to a grammar of the language, they need to have all sorts of knowledge about the subject they are discussing, and they have to use all sorts of reasoning to combine facts in the right way to understand and respond to it. The process of understanding a sentence has to combine grammar, semantics, and reasoning in a very intimate way, calling on each part to help with the others.

It is this last sentence which expresses the crucial aspect of Winograd's contribution to natural language processing. Sharp distinctions between the various phases of processing--syntax, semantics, inference, context--are done away with, resulting in a system that is difficult to describe but powerful in its operation.

SHRDLU's domain of interest is a simulation of a hand-eye system resembling those that have been built at Stanford and M.I.T. For the simulation, we can visualize a table with blocks of various sizes, shapes, and colours, a box, and a mechanical arm able to move one block at a time. Thus the system can respond to commands ("Pick up a red block"), questions ("What does the box contain?"), and declaratives ("A 'steeple' is a stack which contains two green cubes and a pyramid."). The program requires the ability to perform inference when dealing with a command such as

Find a block which is taller than the one you are holding and put it into the box.

In addition, it must determine what is meant by "one" and "it". It can also deal with logical connectives, both in the grammar and semantics

Will you please stack up both of the red blocks and either a green cube or a pyramid.

as well as a wide range of grammatical constructions including passives, coordinates, and comparatives.

The inference powers of the system are lodged in programs written in the language MICROPLANNER which is a procedural language for doing various kinds of problem solving. What is interesting in Winograd's system is that inference plays an integral role in processing the natural language as well as in the associated problem solving. The role of the semantics component is to translate from grammatical structures into MICROPLANNER code. This process, however, is not done at the end of a grammatical phase but is carried out in tandem with
the grammatical analysis.
Since the program saves the ongoing dialogue, it is possible to
question it about past events, and the program itself can use the
dialogue to resolve questions of reference and context. In terms of
its scope both with respect to linguistic problems and problems of
representation of knowledge, linguists would be well-advised to
become familiar with Winograd's SHRDLU.

4. Other Systems
I want to mention two projects which do not fit under the
category Q-A systems. These are Simmons' (1973) work on semantic
networks and Schank's (1972, 1973a, 1973b) conceptual analysis models.
Simmons' model is based very strongly on Fillmore's (1968)
theory of deep case structures with further developments by Celce-
Murcia (1972) to provide a form for representing knowledge together
with an ATN grammar which is used to transform the input English into
these semantic structures. Fillmore has probably been the most influ-
tential linguist in A.I. over the past few years, with several systems
based on his theories. However, some of his recent writings (Fillmore
1971) which raise questions about case grammars do not seem to have
become well known in A.I. We might also mention Fillmore's influence
in cognitive psychology, especially in the memory models of Norman
(1973), Rumelhart et al (1972), and Kintsch (1972). The interaction
of A.I. researchers and cognitive psychologists in such shared inter-
est as memory, knowledge, semantics in particular and language
understanding and production in general, is an encouraging feature
of the past few years. The addition of linguists to this enter-
prise would certainly be a welcome event.

Simmons has also been noteworthy for his concern with sentence
generation and has developed an ATN for generating sentences (in
response to questions) using information from the appropriate part
of the semantic nets. An input question is processed by an ATN
grammar to produce a deep case structure parse. Search routines
attempt to match this structure with knowledge also prestored in
semantic networks of case structures. When matches are found, they
are input to the sentence generator component which produces an
appropriate response.

Schank and his students have been trying to produce a model of
human language understanding to serve as the basis for a computer
program and have indeed produced a prototype system. The program is
a kind of language understander which upon being presented with an
input will produce a series of paraphrases and will also draw some
simple inferences. The major stress here is on underlying meaning or
conceptualizations, with a forcefully expressed lack of interest in
syntactic constructions. Grammar is seen as a minor aid in determin-
ing meaning, although the system does have a conceptual parser as a
first phase. The aim is to produce a representation pregnant with
associations and potentialities for inference.

Schank uses the term ACT to represent the underlying actions
implied by the sentence surface structure. There is certainly a
similarity between his notion of a conceptual dependency network
centered around ACT's and Fillmore's case frames, although Schank argues that Fillmore's work is essentially syntactic. As an example of the underlying complexity of an apparently simple sentence, consider Schank's representation of

I walked to the cafeteria.

I will not attempt to explain the notation but basically the sense is that 'I completed a change in my position to the cafeteria by means of (instrument I) having moved my feet to the cafeteria.' Thus a valid inference which could be drawn here is that the object (I) is located at the direction (cafeteria). PTRANS and MOVE are two of fourteen primitive ACT's which Schank claims are necessary to represent all the actions underlying natural language. His system incorporates large numbers of rules which operate on the input sentence using a large and complex lexicon to produce conceptual structures. Schank will settle for nothing less than a model which explains how people understand natural language.

5. Speech Understanding Systems

At present there are three large-scale research efforts to achieve working speech systems by 1978: Reddy et al (1973), Woods and Makhoul (1973), and Walker (1973b, 1974). The basic specifications for these projects, all of which are supported by the Advanced Research Projects Agency, were drawn up in a report issued by Newell et al (1973). Some of these are that the system should accept continuous speech from many speakers of general American dialect, in a quiet room, over a good quality microphone, with a vocabulary of about 1000 words and a highly artificial syntax. The domain of discourse should be limited; the system, slightly tuned for each speaker, should operate in real time with less than 10% semantic error.

The development of such systems is a great challenge, since all the complexities of language understanding must be dealt with in relating acoustic signals to meaning. As such, researchers on these projects have, of necessity, to draw from the extensive phonological literature and integrate work in this area with current ideas from Q-A systems already in operation. But now the linear paradigm is totally inadequate because there is no way to convert the acoustic signal to a sequence of words without a whole range of syntactic, semantic, context and problem domain knowledge. It would seem that any single aspect of the language processing task requires informa-
tion and help from all other aspects.

Winograd (1974) has described two approaches to system organization for dealing with the apparent necessity of an all-at-once approach. The first he calls heterarchical. The project at Bolt Beranek and Newman (Woods and Makhoul 1973) demonstrates this approach. It involves a number of sub-components working together without a strict chain of command, i.e. any one can pass information to any other one directly whenever some particular knowledge is needed.

Heterarchical system organization (Winograd 1974)

Processing might proceed as follows (Winograd 1974):

The feature extractor looks at the incoming wave forms, and suggests possible phonetic features. These can be used by the lexical retrieval component to see what words are possibly there. The matching component takes a possible word and checks it against a piece of wave form to check the likelihood that it is actually there. The syntax component evaluates possible sequences of words...and a control box to decide what component will do what when.

The second system organization he calls pandemonium. It characterizes the work being done at Carnegie-Mellon University (Reddy et al, 1973):

Pandemonium organizational model (Winograd 1974)

He describes this approach:

It is as if we had a group of experts working on a common task, but no one of them knew anything about the others. Each expert might not even know how many others there were, or what kind of things they dealt with. Communication is managed by having each expert know how to propose sequences of words and assign a degree of confidence to them. Any individual component can look
at a current set of hypotheses (in the SCRATCHPAD), and either add new ones to it, or change the level of confidence in one of the old ones....

This domain certainly requires much cooperation between linguists and A.I. researchers if the effort is to achieve any kind of success.

6. Conclusions
While A.I. researchers have continually borrowed from linguistics, the converse has been rare. In their borrowings they have tended to ignore linguistic controversies and the unsettled state of theories. But they have also revealed some of the inadequacies, evasions, and errors in these theories by the practical necessity of programming a computer to implement them. If there are differences in opinion with respect to methodological principles, these should be overshadowed by the overriding similarity in goals—namely, to understand natural language.

NOTES
1. Work on machine translation using digital computers actually began in the late 1940's, but I am mainly concerned with the post-1960 period.
2. It is important to comment that models in A.I. need say nothing about human behaviour. Very often the concern with human activities is mainly as a source for ideas. There is no reason to expect that appropriate problem-solving techniques for computers will resemble in any way comparable human behaviour.

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ISOLATING SEMANTIC UNITS
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0. It has long been a commonplace of linguistics that the meanings of words can be analyzed into components. However, there seems to be little agreement as to what the components are. Little has been said about how to isolate units that are guaranteed to be of linguistic significance. It seems reasonable, however, to insist that the units play a systematic role in language rather than merely distinguish lexical entries. If no such requirement is imposed many ad hoc feature sets may be proposed. The situation is similar to that in phonology. If phonological features are not required to function systematically --appear in rules--nearly unlimited ad hoc ways of distinguishing lexical entries can be devised.

As a place to look for relevant semantic units, derivational and inflectional systems (including periphrastic ones) have desirable aspects. These morphological systems involve associating, by rule, given units of meaning and given forms. (For a formalization of the notion 'morphological rule', see Hoard and Sloat (1973).) These units of meaning may not be atomic but are units with a systematic role. Such units may be further decomposed on the basis of data from the same or a different language.

Another check on the creation of ad hoc feature sets is the requirement that the features be universal. A way to meet this requirement is to show that the features posited occur in the inflectional and derivational systems of unrelated languages.

Once features that meet the requirements above have been isolated, it is interesting to ask if they play a part in the definition of members of the open classes. In fact it is the case that the meanings expressed by the closed morphological classes--the inflectional and derivational affixes and the function words--in the languages of the world are important elements in lexical entries as well. These elements not only take part in the definition of an entry but in many cases also determine its syntax.

The elements that are identified here may not be universals, but it is likely that they are. Most of the components identified occur in the inflectional or derivation systems of both English and Coeur d'Alene. English and Coeur d'Alene are, of course, totally unrelated languages. A meaning component common to these languages has a good chance of being a universal.

1. The means which are available for expressing inflection and derivation are listed in I.

   I. prefixing
      suffixing
      infixing
      reduplication
      symbolism
periphrasis
prosodic contrast
In II I have given a sample of meaning components (semantic
features) along with an indication of the means by which they
are expressed in the inflectional and derivational systems of
English and Coeur d'Alene.¹ The sample is representative but
by no means exhaustive. Many more features with similar
properties can be adduced. The feature names are enclosed in
angle brackets.
II. (1) < causative>
   English
   suffixes  -en, lighten; -ize, regularize
   symbolism raise (cf. rise)
   Coeur d'Alene
   suffix  -m, ?icxésmºncut
           'he is reforming': ?ic-
           'continuative', xes 'good',
           -on 'instrumental', -cut
           'reflexive' (3rd person singular is
           unmarked)
   symbolism miy 'clarify' (cf. mey 'evident'
           and  cid 'shade(v.)',  cëd 'shady')
   (2) < reciprocal>
   English
   periphrasis each other
   Coeur d'Alene
   suffix  -tweº, tapºntweº 'there was war':
           tapº 'shoot(at)', -on 'instrumental'
   (3) < iterative>
   English
   suffix  -er, glimmer, flicker, chatter
   Coeur d'Alene
   reduplication táxºtºµncut
   and symbolism 'he was hesitant': táxº
   'be at rest'
   (4) < mutative>
   English
   periphrasis get, get wet,
           turn, turn green
           grow, grow old
   Coeur d'Alene
   infix  -?-µ naºs 'it got wet':
           nasº 'it was wet'
   reduplication ?i:ºèlºº 'he is standing up':
           ?i:- 'progressive',
           èlº 'be in standing position'
   (5) < resultative>
   English
   suffixes  -ment, arrangement; -t, complaint
   prosodic contrast  insult (cf. insult)
Coeur d'Alene

prefix \(\text{-s}, \text{scsqwelt 'sweat(n)'}, \text{čsqwelt 'sweat(v)'\}}

(6) < passive>

English

suffixes -able, portable

-ee, employee

symbolism bound (cf. bind), a bound volume

periphrasis be-(e)d

Coeur-d'Alene

prefix \(\text{ʻu-, ʻu: tiš 'sweetened'}

(cf. tiš 'sweet, sugar')

suffixes secondary vs. primary conjugation

\(\text{gwič-tom 'he was seen' vs. } \text{gwič-c 'he saw him': gwič 'see'}\)

Some of the items of II need comment. Uncharacteristically, English and Coeur d'Alene use the same means to express < causative>. As we see in II (1), both languages use a suffix or vowel symbolism.

The form \(\text{lápontweč 'there was war'}\) (in 2) illustrates not only the reciprocal but also a typical use of the Coeur d'Alene instrumental aspect. The form literally means 'they used each other to shoot at'. The instrumental derives many forms that English speakers regard as transitives (The Coeur d'Alene form in (3) is a further example of this.).

In (3) the form \(\text{táp-táxʷmačuc 'he hesitated'}\) has undergone iterativization, a Coeur d'Alene process involving suffixal reduplication of the root and glottalization of all the resonant consonants in the root and suffixes. This form should be contrasted with \(\text{táp-táxʷmačuc 'he stopped'}\), which lacks the reduplication and glottalization.

The English forms stand up and sit down are ambiguous. Both forms have mutative and non-mutative senses. To stand up, for example, can mean either 'to come to an erect position' (mutative) or 'to maintain an erect position' (non-mutative). The Coeur d'Alene form \(\text{ʔiːcčel in (4) conveys the mutative sense of stand up}, \) but not the non-mutative. The form, however, is not directional in the same way as stand up. It also means 'stand down' (from, say, a horse).

The only method of forming resultives in Coeur d'Alene—by prefixing an s—is illustrated in (5). In English there are many ways of forming resultatives. Only a few are pointed out in (5).

In Coeur d'Alene < passive > is expressed by the prefix \(\text{ʔu-} \) with adjectives. This is shown in (6). Contrasting sets of inflectional endings distinguish passive from active with transitive verbs. In English < passive > is expressed by several means including suffixing and vowel symbolism. However, the suffixes mentioned in (6), -able and -ee express meanings in addition to < passive > ; -ee, for example, also expresses 'one who'. When < passive > occurs in English as a verbal voice, it is of course rendered periphrastically by \text{be} and the past participle.
2. Each of the meaning components identified above as common to the inflectional and derivational systems of Coeur d'Alene and English is also lexicalized in English. By lexicalized I mean that the feature takes part in the definition of members of the open classes. To be considered lexicalized the unit in question may either be listed as an idiosyncratic property of a word or be added to definitions by a redundancy rule. The component <causative> for instance, distinguishes the senses of got in E1 and E2.

E1. She got her foot caught. (non-causative)
E2. She got her nose fixed. (causative)

Got in E3 is ambiguously causative/non-causative. (Actually

E3. She got her hair wet.

both E1 and E2 are ambiguous in the same way. It is just that
the alternative interpretation for each is not salient.)
It is clear that the causative and non-causative senses of
got must be distinguished lexically. No generalization follows
from the fact that non-causative got occurs in, say, E4.

E4. Her foot got caught.

The other copular structures do not parallel those with get. We
have E5 but not E6. Even the closely related have does not

E5. Her foot was caught.
E6. *She was her foot caught.

parallel get in this regard. Compare E7 and E8. In E7 had is

E7. She had her foot caught (for hours).
E8. Her foot had caught (*for hours).

a copula. In E8 had is the perfect auxillery. Thus if we tried
to account for the ambiguity of E3 by deriving the non-causative
sense from E4, the transformation would have to specify get. Such an outcome is certainly not preferable to (and is probably
not really different from) recording both the causative and non-
causative senses of got in the lexicon.

3. The feature <reciprocal> plays a role in the definition of a
word such as meet in He and she met, where it relates two
arguments as does Coeur d'Alene -tweč 'each other'. The
presence of the feature <reciprocal> in a verb makes it
incompatible with the reflexive themselves, as is shown by E9
and E10.

E9. *He and she met themselves.
E10. *They met themselves.
Notice that a reflexivization rule for English will have to be sensitive to features of the verb as well as to features of the subject.

The feature <iterative> distinguishes senses of English keep. In E11 keep is iterative; in E12, non-iterative. The position of

E11. They keep running the water.
E12. They keep the water running.

the participle depends on the presence or absence of <iterative>.

4. As was mentioned above <mutative> is lexicalized in one of the senses of stand. It is also, of course, in the corresponding sense of sit. The core meaning of get is <mutative>. 3 Mutatives seem to be incompatible with durational adverbs, as in E13 and E14. In E14 the ambiguous stand up is of course

E13. *He got angry for hours.
E14. He stood up for hours.

disambiguated. Only the non-mutative sense is conveyed.

5. Although suffixing -ment is a very common way of expressing <resultative>, so-called functional shift is also common, as in deal(v), deal(n); sweat(v), sweat(n). Functional shift is, of course, a species of lexicalization. Not every verb has a corresponding resultative noun with identical shape, e.g. ask(v), *ask(n); hear(v), *hear(n), etc. Thus it will be necessary to append a note to each verb in the lexicon which does have a phonologically identical resultative. (Prosodic contrast is not a very common means of deriving resultatives (see Sloat 1974).)

6. The lexicalization of <passive> in English takes more than one form. Certain nouns and adjectives have <passive> as an essential part of their definitions, for example, reject 'thing thrown aside' and obvious 'very easily perceived'. A number of verbs also have lexicalized passive senses, e.g. shock in E15.

E15. Mary shocks easily.

For a list of many other such verbs, see Jespersen (1927:347-52). 4 An interesting set of facts is associated with these passive verbs. First of all, as John Anderson (1968:17ff) has demonstrated, they do not derive from more basic active senses but, quite the contrary, underlie more marked causative senses with ergative subjects as in E16.


E15 conveys that something is easy to 'do to' Mary. It conveys this because shock has the feature <passive> in its
definition. Other intransitives that underlie ergative senses do not necessarily have the passive interpretation: His shoes shine, He shined his shoes.

The passive intransitives such as shock require adverbial modification (Anderson 1968:12). For example, we have E15 but not E17. The non-passive intransitives have no such

E17. *Mary shocks.
requirements, as can be seen in E18.

E18. He drowned.

Not only is it the case that adverbial modification is required with passive intransitive, it is also true that the adverb easily is incompatible with the corresponding causative when certain ergatives are present. Compare E19 and E20.


The use of easily with the causative requires an ergative subject that acts deliberately. Compare E21 and E22.

E22. *John easily shocked Mary by accident.

Salacious remarks, of course, don't act deliberately.

In E19 the adverb measures the difficulty that the deliberate agent encounters in working his will. The role of easily seems quite different in E15 and E23. A by + NP phrase can be appended

E23. Mary is easily shocked.
to E23 even if the NP is not capable of deliberate action. The by + NP phrase in E24 seems not to be the expression of the

E24. Mary is easily shocked by salacious remarks.
E25. Mary is allergic to eggs.

ergative nor does the verb seem to be causative. Rather, the by + NP phrase seems to be a specifier like to eggs in E25. The verb phrase as a whole in E24 is descriptive of an attribute, as in Mary shocks easily (see Jespersen 1927:350-1).

All other things being equal, then, there should be a subtle ambiguity in E26. And there is. One interpretation is

E26. Mary is easily shocked by John.

especially equivalent to that of E27, where John is a deliberate agent. The other interpretation is parallel to that of E24.
E27. John easily shocks Mary.

By John is the specifier of the passive sense of shock. Under this interpretation the sentence ascribes to Mary a certain sensitivity to John whether John is acting deliberately or not.

7. The feature <vocative> is not in the list above. It is, of course, expressed in the pronominal system of all languages, where at least one form is used to address one's interlocutor.

The vocative is usually regarded as a 'case', a modification of nouns not verbs. However, it seems to be a verb feature in direct questions. The verb was in E28 is vocative (addresses

E28. Was he gone?
E29. Tell me whether he was gone.

interlocutor) while in E29 it is non-vocative. The vocative in E29 is tell. The distinction vocative/non-vocative then can be generalized so as to obviate the distinction direct/indirect.

Actually, the distinction vocative/non-vocative extends even further. Part of the difference between the interrogative adverb in E30 and the relative adverb in E31 is that the interrogative is vocative and the relative is not.

E30. Where is he going?
E31. Tell me where he is going.

Thorne (1966) recognized the important role of <vocative> in imperatives. Anderson (1968:19) wrote the following rule for the introduction of the feature <imperative> into verb phrases.7

R1. VP ➔ [+ imperative]/ [+ vocative]

Though R1 reflects some facts about imperatives, a more explanatory account is possible if R1 is replaced by R2, a lexical redundancy rule, which can be applied optionally. A fully grammatical imperative verb must be volitional. There may be other restrictions as well (see Anderson 1968:21-2).

R2. < Volitional> ➔ <imperative, vocative>

The effect of this rule is to add to the set of features of a volitional verb the additional components imperative and vocative.8 A verb so marked will not require a subject. In Go home, go itself is vocative, i.e. used to address the interlocutor. Recognizing this allows us to dispense with the awkward business of setting up you as a subject just to knock it down.

To account for the fact that everybody in E32 and E34
E32. Everybody go home.
E33. Help yourself.
E34. Everybody help yourself.

is vocative requires us to recognize that <vocative> is a transfer feature in the sense of Weinreich (1966:429ff.). It is transferred from the verb to the subject and/or reflexive. (We noted above, in the discussion of <reciprocal> that a reflexivization rule would have to be sensitive to features of the verb.) Certain facts about imperatives call for a slight extension and modification of Weinreich's theory of transfer features. I said above that the effect of R2 was to add <vocative> and <imperative> to the set of features that define volitional verbs. Actually the feature(s) mentioned on the left side of a redundancy rule are also added. Usually this is without effect, simply duplicating features already present. However, if the rule is misapplied the added feature will set up a feature conflict.

Feature conflicts underlie many figurative uses of words. For example, a feature conflict accounts for the 'metaphorical' interpretation of grief in just a grief ago, where non-temporal grief is interpreted as a space of time because a temporal feature is transferred to it from ago. The superimposition of <volitional> on a non-volitional verb by misapplication of R2 accounts for such imperatives as get lost, be smart, etc. where a non-volitional state is treated as if it were volitional.

Questions and imperatives are obviously closely related. E28 and E30 come very close to expressing what is expressed by E29 and E31, respectively. Vocative is an important feature in both sentence types.

To summarize: I have tried to develop a way to isolate semantic units that are not ad hoc. The ones illustrated can, with some justification be regarded as semantic universals. Further, these features not only play a role in closed morphological classes—derivational and inflection affixes, function words, etc.—they are also components of the lexical entries of the morphemes in the open classes (or are added to such entries by redundancy rules). I have demonstrated that these units are relevant to linguistic descriptions. As components of lexical entries they function to determine the syntax of the entries as well as to characterize their meanings.

It is in order to ask whether semantic analysis is too severely constrained if it employs only features that meet the stringent tests of both occurring in morphological rules and being universal. I think not, if suitable attention is given to metaphor or 'conventional' interpretation.

In looking at ordinary dictionaries, I am impressed with how often non-geometric notions such as that conveyed by the verb state are given partially spatial definitions, such as 'set forth in words'. The 'set forth' part of the definition can be rendered with <causative> and the feature (or features) associated with the English preposition before and the Finnish postposition edessä.
The 'in words' part of the definition is referential and designates the non-geometric field to which the spatial metaphor 'set forth' is to apply.

It is well known that languages give time a geometric frame. Minutes, like milestones, pass. Periods of time are long, short, etc. Other semantic fields apparently get treated in a similar way. The languages of the world distinguish many locational features in their closed classes. This gives us reason to hope that definition by spatial metaphor can be carried out to a high degree of delicacy in areas where it is applicable.

It may be the case that some parts of some definitions cannot be accounted for even by pushing the kind of analysis proposed here to its limits. However we are far from knowing that now. For the present it seems reasonable to define as subtly as we can in terms of units known to have a systematic role in language, i.e. to keep our definitions linguistic.

In any event, if supplementary methods of demonstrating systematic relevance are not forthcoming, we can consider the linguistic analysis of meaning complete when we have reduced it to units that meet the requirements specified here. This is true because the components of putative definitions that cannot be tied to features of language are denotive. They are data to be accounted for by a theory of reference, not a theory of meaning.

Determining which features are relevant to linguistic description is very important. Objects in the real world have many properties that do not necessarily correspond to components of the meaning of the words that refer to them. Weinreich's definition of chair is interesting in this respect, 'furniture to sit on' (1966:419). Why did he assume that a chair is to be defined in terms of its function? Why didn't Weinreich choose to define in terms of a back, a seat, legs, etc. His questions about the kind of logical system needed to convey the definition 'furniture to sit on' is quite premature. One can enquire fruitfully into the relations among semantic units only when one knows what the semantic units are and what their nature is. If Weinreich had chosen 'has a back, a seat, legs, etc,' his definition would not have required the transitive relation because the definition would have constituted a cluster, i.e. a set of attributes. I see no reason to delve into logical systems that attempt to explicate meanings without respect to the features of language that compose them.

1: All Coeur d'Alene forms and their analyses are based on Reichard 1938. The forms and analyses may be changed slightly to accord with information gained in my own field work with the language. The emendations, however, are all inconsequential with respect to the points at issue in this paper.

I have systematically made the following changes in Reichard's orthography: c, s, c, g', e, o replace her ts, c, tc, gw, a, o,
respectively. I use $\delta$ where Reichard may have used E, u, or $\psi$.

2: It is tempting to call verbs such as meet mediopassives. The middle voice contrasts with passive in English in, for example, the two interpretations of It's easy to adjust. And the presence of the middle voice has a syntactic effect. The expletive, but not the personal pronoun, it is compatible with the middle voice in sentences such as It's easy to adjust.

3: This is demonstrated in Taylor (1975).

4: Anderson (1968:32) suggests that $\langle$passive$\rangle$ (actually $\langle$active$\rangle$ in his terms) may be added to the entries by means of a kind of redundancy rule. It is not crucial to the point being made here whether he is correct or not in his particular view of what is determining and what is redundant.

5: A few, which do not have experiencer subjects, do not require the modification: The horse won't sell.

6: Informants who accept E20 as grammatical seem to regard salacious remarks as a kind of instrumental. These informants seem to read a deliberate agent into the sentence, because remarks imply someone to make them. I could find no takers for *Bad soil easily shocks Mary, where there is no reasonable chance of reading a deliberate agent into the situation.

7: The feature $\langle$imperative$\rangle$ is an inflectional feature of many, if not all, languages. In Coeur d'Alene, $\delta$ is suffixed for singular imperative: $\text{hóycan}$ 'shut up' (hoy 'stop', $\text{can}$ 'mouth'). In English, the imperative feature limits the application of the inflectional rule that adds $-s$ to singular verbs: cf. Everybody go home (imperative), Everybody goes home (non-imperative).

8: Coeur d'Alene seems to have a volitional suffix $-im\omega$ (Reichard 1938:601). The English modal would seems to have $\langle$volitional$\rangle$ thoroughly intermixed with $\langle$conditional$\rangle$: I would if I could.

A Quantitative Analysis of Variation: i in Tok Pisin
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The principal goal of this paper is to describe the variation in the distribution of the verbal particle i in Tok Pisin and to present grammatical evidence that, in the course of its assimilation from substrate Austronesian languages, i has changed from a subject agreement particle to something more akin to a complementizer, and that part of the surface variation in its occurrence is due to phonological conditioning of a non-deterministic nature.

The corpus for this study is primarily based on tape-recorded data gathered by Gillian Sankoff and Suzanne Laberge in New Guinea during the summer of 1971, and consists of roughly 1,350 sentence tokens drawn from the speech of four fluent speakers of Tok Pisin.

Tok Pisin, or Neo-Melanesian, is a pidgin language spoken in the Trust Territory of New Guinea, parts of Papua New Guinea, the British Solomon Islands, and the New Hebrides. According to Wolfers (1968), its speakers number about 530,000, and the roughly 10,000 native speakers among these testify to its current creolization.

The principal lexical source languages of Tok Pisin are English, which accounts for the bulk of the lexicon, and Kuanua, an Austronesian language spoken in the Gazelle peninsula of New Britain.

The verbal particle in question is exemplified in (1)-(3):

(1) Em i no giaman. "He wasn't lying."
(2) Oloitim mitupela i stap long kunai. "All the time we've been living in the grasses."
(3) Man i klostu i laik i lapun. "The man was getting old."

As these examples illustrate, i occurs in pre-verbal position. (1) exemplifies the negative no, one of the few elements which can intervene between i and the verb, and (3) shows that i marks embedded verbs as well, as both klostu and laik are auxiliaries. Turning to (4), we can see that i has an analogue in Kuanua, the Tok Pisin substrate language mentioned above. It is quite clear that i is not, as was previously thought by some early grammarians, derived from the third singular subject pronoun in English.

(4) KUANUA: a tuntana i takpa ra vudu. "The man takes the banana."

The example from Motu in (5), an SOV language with the analogue e, exemplifies the particle's transcendence of the SVO/SOV factionalism that exists among New Guinea Austronesian languages. Note that it still occurs pre-verbally, however.

(5) MOTU: mero ese hudi e vaia "The boy takes the banana."

Some other Austronesian languages with i-analogues are illustrated in (6) and (7).
(6) WEDAU: wei numa-na i gaegae "This house is large."
(7) KIRIWINAN: bua bogwa i yagi "The betel has now ripened."

The distribution of the i-analogues in those Austronesian languages in which it occurs seems to be controlled by the subject. Although these languages vary in morphological complexity, and some of them incorporate tense markers into their i-analogues, subject agreement, with different forms of the particle occurring depending on the person and number of the subject, is the norm. An example of a conjugation from Petats is given in (8) for the verb la 'go'.

(8) PETATS: elia a la nauk "I am going."
elo o la nom "You are going."
eiau e la no "He/she is going."
eri e la ro "We (incl.) are going."
elam e la nam "We (excl.) are going."
emi e la nomi "You (pl.) are going."
eru e la er "They are going."

In Petats, as (8) shows, only the first and second singular have special forms, with e marking the remainder of the pronouns. In general, the e or i form is associated with majority representation and usually includes third person, but the subgrouping of forms among remaining pronouns varies from language to language.

What distinguishes these languages from Tok Pisin, first of all, is the categorical nature of the subject agreement constraints. In Tok Pisin, like in other pidgin languages, there is a great deal of variation, and the distribution of i is no exception. Consequently, the constraints are more difficult to determine. Table I lists the percentage occurrence of i in simple sentences in my corpus, correlated with the various pronominal forms, in rank order.

<table>
<thead>
<tr>
<th>Person/number</th>
<th>Percentage</th>
<th>Tok Pisin form</th>
</tr>
</thead>
<tbody>
<tr>
<td>3rd/sing.</td>
<td>83</td>
<td>em</td>
</tr>
<tr>
<td>3rd/plur.</td>
<td>80</td>
<td>ol</td>
</tr>
<tr>
<td>3rd/plur.</td>
<td>60</td>
<td>ologeta</td>
</tr>
<tr>
<td>1st/plur.(excl.)</td>
<td>55</td>
<td>mipela</td>
</tr>
<tr>
<td>2nd/plur.</td>
<td>48</td>
<td>yupela</td>
</tr>
<tr>
<td>2nd/sing.</td>
<td>15</td>
<td>yu</td>
</tr>
<tr>
<td>1st/plur.(incl.)</td>
<td>6</td>
<td>yumi</td>
</tr>
<tr>
<td>1st/sing.</td>
<td>5</td>
<td>mi</td>
</tr>
</tbody>
</table>
This table indicates, among other things, that for no pronoun is the presence of \( \_1 \) correlated in a truly categorical way with its person and number. Moreover, if one examines more closely the distribution of these percentages, the fact emerges that they form a descending hierarchy with respect to the final segment of the pronoun. Those forms with which \( \_1 \) occurs most frequently end in a consonant, those of intermediate frequency end with a low vowel, and those of lowest frequency end with a high vowel. In addition, the figure for ologeta, which is also a third plural pronoun, but ends in a low vowel rather than a consonant, shows a considerably lower frequency of occurrence with \( \_1 \) than ol does.

To further test the hypothesis that the variation in occurrence of \( \_1 \) was at least partially determined by phonological constraints, all of the data in the corpus were lumped together, that is to say both sentences with pronominal subjects and sentences with NP subjects, and the occurrence/non-occurrence of \( \_1 \) was correlated with the nature of the segment immediately preceding the space where \( \_1 \) occurred or should have occurred, regardless of the nature of the word in which the segment was located. The Sankoff-Cedergren program was used to determine multiplicative probabilities for the factors preceding high vowel, preceding low vowel, and preceding consonant, among others. In coding the data, reduced occurrences were counted as full occurrences, and dubious cases were thrown out. The results, shown in Table II, are similar enough to the percentages in Table I to suggest that phonological conditioning may have something to do with the pattern of variation associated with person and number of subject. Under this hypothesis, \( \_1 \) would be deleted by a phonological rule with roughly the form given in (9):

| TABLE II Probability of non-application of \_1-deletion as a function of preceding environment. |
|---|---|---|---|
| \( P \) | \([-\text{voc}]\) | \(0.5\) | \(+\text{voc}\) | \(0.0\) | \(+\text{voc}\) | \(+\text{high}\) | \(-\text{high}\)|
| \([-\text{voc}]\) | \(+\text{high}\)
| \(+\text{voc}\) | \(+\text{high}\) |
If i is not functioning as a subject agreement particle, then what is its function in Tok Pisin? I would suggest that i has become a kind of complementizer or sentence marker, that it is still sensitive to the presence, but not the nature, of subjects, and marks S-nodes in constituent structure.

Various patterns of variability in i-occurrence with respect to selected syntactic processes of Tok Pisin suggest that a cyclic rule of i-insertion operates to place the particle pre-verbally, and distributional facts suggest that this rule, like the phonological deletion rule, must be equipped with variable constraints.

What little data there is concerning syntactic processes in Austronesian languages with i-analogues does not appear to display any of the characteristics to be discussed below, but a detailed comparison is unfortunately impeded by the lack of reliable grammatical descriptions of these languages.

First, i-insertion is variably verb class governed. The rule is controlled by both the matrix and embedded verbs, bearing in mind that since i can complementize root sentences, the term 'embedded verb' has been extended to include the verb in simple clauses, embedded in a deleted performative if you will.

The verb class restrictions are of three types. Embedded verbs of saying and thinking, and perhaps all epistemic non-factives, exemplified in (10) and (11), highly favor non-application of the rule:

(10) em tok ɶ em i tok "He talks"
(11) man i larim ol tingting "The man let them think"

Motion verbs like go, kam, and stap, which are used aspectually in Tok Pisin, have the opposite effect: they strongly favor insertion, and are exemplified in (12) and (13)

(12) man i mekim dok i go outsait "The man made the dog go outside."
(13) man i mekim yu i kam kisim "The man made you come get

The third type of constraint is proposed tentatively, due to undex
exemplification. According to Wurm (1971), the auxiliaries and modals can be divided into two groups, depending on whether \( i \) insertion occurs in the cycle below them or not (this is my interpretation of Wurm's data). In other words, this is a question of matrix, rather than embedded, verb conditioning. (14) and (15) exemplify inhibitory aux's, (16) and (17) excitatory ones.

(14) mipela i mas kaikai "We must eat."
(15) em i tra'im lulkul long mipela "He tried to look at us."
(16) man i klostu i laik i lapun "The man is getting old."
(17) em i laik i brukim paiauw "He wants to chop firewood."

Woolford (1975) distinguishes another verb-class, the set of verbs which potentially take the transitive suffix -im, and presents figures which indicate that this class also has an inhibitory effect. It may be that this constraint can accommodate the variation found in the auxiliaries. In Table III there is some data summarizing the effect of the above classes, with other factors held as constant as possible.

<table>
<thead>
<tr>
<th>Verb Class</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aspectuals</td>
<td>90</td>
</tr>
<tr>
<td>Aux's</td>
<td>i.d.</td>
</tr>
<tr>
<td>Saying or thinking</td>
<td>10</td>
</tr>
</tbody>
</table>

A second feature of the variation involves the presence or absence of subjects. According to Wurm, when an embedded NP has adjuncts it may optionally be postposed, and when it has an embedded relative, postposition is obligatory. Furthermore, the operation of this process of 'Heavy NP Postposition', again according to Wurm, has a categorical blocking effect on the insertion of \( i \). So one finds the normal ordering as in (18), the optional variants (19) and (20), and the obligatory postposing of (21):

(18) larim dok i kæm insait "Let the dog come inside."
(19) larim tispels tupela man i kaikai "Let these two men eat."
(20) larim kaikai tispela tupela man "Let these two men eat."
(21) larim kaikai man i sndaum i stap lohap "Let the man who is sitting over there eat."

Note that \( i \) is not found when postposing has occurred. Although there are no examples of this type in my corpus, it may be that it is the subject NP's presence or absence which is crucial here, and that \( i \)-insertion is inhibited when subject NP's are ripped out or deleted. Some evidence that does bear on this question
is the patterning of \( i \) with respect to conjunction reduction. When the second of two identical NP's in subject position is deleted in conjoined sentences, there is a strong tendency for \( i \) to occur less often in the second conjunct, approximately 30% of the time in my data, compared with simple sentences restricted to third person pronominal subjects.

A summary of these constraints is given in the formulation of \( i \)-insertion in (22). Although I have presented some order of magnitude values for the constraints in the body of this paper, because of the relatively small size of the corpus, speakerwise and tokenwise, because of the fact that all variables must be analyzed simultaneously for correct interpretation, and because of the fact that the 'correct' choice of independent variables is largely a matter of judgement, exact specification of coefficients must await a more comprehensive analysis. In particular, when the correct set of determining variables is isolated, the patterning of \( i \) may turn out to be more categorical than here indicated.

(22) Cyclic \( i \)-insertion

\[
\emptyset \rightarrow \langle i \rangle / \langle a_{S_1} \rangle [\langle b_{V_1} \rangle \langle c_{NP} \rangle \rightarrow \langle d_{V_2} \rangle \langle a_{S_1} \rangle ]_{S_2}
\]

where \( a, b, c, d \) are coefficients representing contributions towards probability of rule application, and \( a \) depends on presence or absence of \( S_1 \), \( b \) depends on verb class of \( V_1 \), \( c \) depends on presence or absence of NP, and \( d \) depends on verb class of \( V_2 \).

To summarize then, I have tried to show that while \( i \)-analogs in many Austronesian languages are deterministically constrained subject agreement particles, the patterns of variation in the data tentatively suggest a more attenuated role for Tok Pisin \( i \) as a stochastically constrained sentence marker dependent on such things as verb class, presence or absence of subject NP, occurrence in subordinate or superordinate position, and phonological environment.

1. I would like to thank Gillian Sankoff and William Labov for much valuable assistance, and the participants in Gillian Sankoff's Tok Pisin seminar at the 1973 Linguistics Institute for transcriptions. Responsibility for the above ideas rests with me.
2. Examples (4)-(8) are from A. Capell.
3. The assumption of non-interaction of the independent variables is made, but possibly not warranted.
4. See for example Codrington (1885).

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THE 'ACUTE' PROBLEM OF TONOGENESIS

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This study was designed to determine whether there is a linguistically significant variation in the fundamental frequency (\(f_0\)) of syllable nuclei when surrounded by grave versus acute consonants. Jakobson, Fant and Halle (1952) described the feature grave as being the predominance of energy at either half of the significant speech spectrum. Chomsky and Halle (1968) abandoned all acoustic features in establishing a strictly articulatory feature system. Although one must concede the possibility of acoustic correlates of articulatory features, there still have been attempts (Hyman 1973) to phonologically motivate a reinstatement of the feature grave.

The crucial datum given by Hyman is in high vowel reduplication in Petit Diboum villages of Fe?fe?-Bamileke, spoken in the Cameroons of Western Africa. Hyman claims there is an acoustic assimilation that cannot be handled by the Chomsky-Halle feature coronal. Remember that the feature +/-grave is not mutually substitutable for the feature +/-coronal. The reduplicated vowel in Fe?fe? patterns with the graveness of the preceding consonant. The feature coronal does not group together palatal stop [c] with palatal glide [y] which is necessary to handle the reduplication datum. On this basis Hyman argues for a reinstatement of the acoustic feature grave.

The brunt of any proof for a reinstatement of the feature grave will fall on whether there is phenomenological (here I mean acoustic and/or articulatory) evidence for acoustic assimilations. Evidence has been presented showing the effects of phonetic context on the pitch of syllable nuclei, and their significance in tonogenesis. Here I am talking about segmental effects.

The following are factors that have been demonstrated about phonetic context. The phonetic environment of voiceless obstruents has the effect of raising \(f_0\) of syllable nuclei, as compared to voiced obstruents (Peterson and Lehiste 1961 and Lea 1972). In general, voiceless environments have higher pitch. Lea (1973) also observed \(f_0\) contour dips after voiced obstruents, and monotonic \(f_0\) increases in \(f_0\) after sonorants. Excluding consonantal environments, every vowel has its intrinsic pitch: higher vowels tend to have higher \(f_0\) (Lea 1973, Lehiste 1970 and Peterson and Lehiste 1961). Voice-
less stops in English tend to increase $f_0$ in following vowels (Lea 1972 and Peterson and Lehiste 1961), and there is diacronic evidence (Haudricourt 1972) that voiceless aspirated stops in Camuhi motivated the development of high tones: note that English stops are aspirated.

This has lead to speculation that aspiration is the motivating phonetic parameter for the development of high tones. I have observed that the acoustic property of aspiration is high frequency energy between the closure of the stop and the onset of phonation. If such high frequency noise serves as one perceptual motivation for the occurrence of high tones, then this means that other segments which do have this acoustic property of high frequency noise might produce the same effects.

Segments classified acute (versus grave) also have this above described energy at higher frequencies, and these consonants were examined to see if there was a transferring of this high frequency energy from the consonantal environment to the $f_0$ of the syllable nuclei. It was decided to examine fricatives in English to test the high frequency energy factor, and to get datum free of the aspiration factor. There is evidence (Stevens 1960) that this datum could be generalized to some voiceless stops, because the aspiration should be acoustically similar to a homorganic fricative.

Four vowels ([i], [ɛ], [ɔ] and [ʊ]) were chosen to get a representative sampling of the vowel quadrilateral i.e., four corners. The vowels [i] and [ʊ] were chosen because they have similar (relatively high) intrinsic pitches, and the vowels [ɛ] and [ɔ] were chosen because they have similar (relatively low) intrinsic pitches. The fricatives [s] and [f] and their voiced counterparts [z] and [v] were chosen because they, too, in syllable initial position have very close effects on $f_0$ of syllable nuclei (Peterson and Lehiste 1961). Although the major effect was thought to be from the syllable initial consonant onto the vowel, it was decided to use a syllable of the type $C_1VC_1$ in order to magnify the effects of the environment. The fricatives used in this study will be crucial items of acoustic effects because there can be no articulatory explanation such as variations in larynx height for $f_0$ fluctuations.

Except for one, the subjects were all native speakers of the Southern California dialect of Standard American English. There were eight subjects between the ages of twenty-three and forty years old. They were all
males and were not members of any minority group nor did they speak nonstandard dialects. They were not involved in linguistics, nor were familiar with any tone languages. The one exception to being a speaker of the Southern California dialect learned English as his native language in India from people with a British cultural heritage. This compensated for missing observations of the vowel [ɔ] which gave a relatively more balanced number of scores on each item, for each subject. The subjects were told they were in an experiment to demonstrate the difficulties of the English spelling system. They were told that it was their task to memorize a list of nonsense words. They were given a pronunciation key (in the form of sample English words) for each symbol that appeared in every nonsense word i.e., it was demonstrated to the subjects that every symbol was phonemic.

The sentence frame"Read the form many times" was used for two major reasons: first, it puts sentence medial stress and intonation on the syllable; second, it surrounds the test item with the bilabial nasal [m]. This consonant was ideal because it was easy to articulate in the context of the fricatives (no tongue twisters), and it has a neutral pitch. Each item occurred twice in random order. With the four vowels and four consonants, this yielded a matrix of sixteen different items (therefore, thirty-two for each subject) and a matrix of forty-eight scores for each item. With the above-mentioned syllable structure, each item had three "phonemes." Broad and narrow band sonagrams, were made. The syllables were segmented, and the syllable nucleus was separated out using Naesar's (1970) criteria for segmentation—minus glottal transitions. The nuclei were then halved and the midpoint located. Measurements of f were taken at three points: syllable onset, medial and final. All measurements were done in one-fortieth of an inch scale.

The experimental design was set up to be a four factorial Analysis of Variance, with the factor grave at two levels, and with the factor voice and two levels, the factor vowel quality at four levels, and finally the factor syllable point at three levels. This yields forty-eight combinations of levels. The factor grave was assigned the first major break while the factors voice, vowel quality, and syllable point were assigned the second, third and fourth, respectively. With eight subjects, this should yield a total of seven-hundred-sixty-eight observations; however, one-hundred-eighty-three were missing, leaving five-hundred-eighty-five scores. All of the missing observations, however, were statistically compensated for by the computer program.
The above described ANOVA was carried out by the UCLA Health Sciences computer program BMD X64. The program uses a general linear hypothesis, and computes regression coefficients, gives descriptive statistical data, predicted values for cells, and it generates variables for missing observations. Also it gives a complete ANOVA table. The factors grave and voice had one degree of freedom each, while the factor vowel quality had three degrees of freedom, and the factor syllable point had two degrees of freedom. This program does all of these analyses between cells to show interactions. With the dependent variable being held constant for each cell, this will leave an error score of five-hundred-thirty-seven.

<table>
<thead>
<tr>
<th>FACTOR</th>
<th>SIGNIFICANCE LEVEL</th>
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</thead>
<tbody>
<tr>
<td>voicing</td>
<td>.01</td>
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<tr>
<td>vowels</td>
<td>.01</td>
</tr>
<tr>
<td>syllable point</td>
<td>.01</td>
</tr>
<tr>
<td>interaction of grave and syllable point</td>
<td>.05</td>
</tr>
</tbody>
</table>

Having examined how the experimental design was carried out, it should be time to examine the results. Statistically (see table 1) the ANOVA showed significant differences in levels of the factor voicing, the factor vowel quality, and the factor syllable point. They were significant at the .01 level. The only interaction that turned out to be significant (at the .05 level) was between the factor grave and syllable point. As can be seen from figure 1, there was a significant difference between grave versus acute consonantal environments at the point of syllable onset. This was the environment most crucial to my hypothesis. The variance at syllable medial and final points were also above the difference limen for \( f_0 \) (0.7 per cent, Flanagan and
FIGURE 1: Mean fundamental frequency values for grave versus acute consonantal environments.
FIGURE 2: (A) Mean fundamental frequency values for grave voiced versus voiceless consonantal environments. (B) Mean fundamental frequency values for acute voiced versus voiceless consonantal environments.
FIGURE 3: (a) Mean fundamental frequency for the four vowels in voiced grave versus acute consonant environments. (b) Mean fundamental frequency for the four vowels in unvoiced grave versus acute consonant environments.
Saslow 1958), but were relatively insignificant compared to the syllable initial point.

The following generally known facts about the effects of phonetic context on the $f_0$ of syllable nuclei (mentioned above) were replicated by this study, and therefore, give some indication of this study's validity. As figure 2 shows there are significant differences between voiced and voiceless environments. Part A compared to part B show the grave versus $f_0$ contours hold for both voiced and voiceless environments. This study did demonstrate the intrinsic pitch of vowels as shown on figure 3. [i] had a higher pitch than [ɛ] for both voiced and voiceless environments (figure 3, parts A and B, respectively). However, the difference between [u] and [o] was insignificant: one hertz for the mean of combined voiced and voiceless environments. Notice on figure 3 that $f_0$ contours for grave versus acute consonantal environments hold for all voiced environments. Here note that I am talking about the shape of the contour, not its pitch. It also held for the voiceless environments except for the vowel [u] where the contour is concave rather than convex relative to higher $f_0$.

One very crucial point to my hypothesis was that at syllable onset point, every acute consonantal environment had higher pitch.

Although this datum might be considered in conflict with Lehiste (1970) relative to the effects of [s] and [f], it should be noted that Lehiste was not looking specifically at these segments, nor does she give datum on syllable onset position.

Even though this datum is new and perhaps surprising, it does match previously examined factors like voicing and vowel quality. As expected there were significant differences for all items, dependent upon the point the syllable was measured. However, this and/or similar experiments should be conducted to determine this study's reliability. The task of the phonologist now becomes to look for languages which do (or have) made use of these observed pitch differences, because this will be the ultimate test of this study's validity. I would expect to see one of two possibilities: first, after acute consonants, the development of a high tone; second, the development of a falling tone after acute consonants.

Finally, it becomes clear that here is a case of phenomenological—more phonetic than phonological—evidence for acoustic assimilations.
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FIGURE AND GROUND IN COMPLEX SENTENCES

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FIGURE and GROUND

We begin by noticing a certain pair of cognitive-semantic categories. Their relevance shows up, in the first instance, in relation to a semantic event of motion or location, i.e., one considered to signify

one physical object moving or located with respect to another.

Here, each object is taken as bearing to the whole event a significant and distinct relation, respectively that of FIGURE and that of GROUND, by term. The following sentences can serve for immediate exemplification of these categories:

the pen lay on the table
the pen fell off the table;

in both, the pen specifies the object which functions as FIGURE, and the table the object which functions as GROUND. The terms have been taken from Gestalt psychology, but are here given the following particular characterization for use in linguistic semantics:

The FIGURE object is a moving or conceptually movable point whose path or site is conceived as a variable the particular value of which is the salient issue.

The GROUND object is a reference-point, having a stationary setting within a reference-frame, with respect to which the FIGURE's path or site receives characterization.

While these categories are clearly assignable within a motion event where one object is moving and the other is stationary, they might there be thought to be merely a restatement of the fact of movement vs. locatedness rather than independent notions in their own right. The existence of these categories in semantics can be demonstrated, therefore, in a locational event where both objects are stationary. Thus, whereas one might expect two sentences like
(a) the bike is near the house
(b) the house is near the bike

to be synonymous on the grounds that they simply
represent the two inverse forms of a symmetric rela-
tion, they in fact do not mean the same thing. They
would be synonymous if they specified only this sym-
metric relation—i.e., here, the quantity of dis-
tance between two objects. But in addition to this
(a) makes the non-symmetric specifications that, of
the two objects, one (the house) has a set location
within a framework (here, implicitly, the neighbor-
hood, world, etc.) and is to be used as a referenc-
point by which to characterize the other object's
(the bike's) location, understood as a variable
(realistically so in this instance, since the bike
will be in different locations on different occa-
sions) whose particular value is the salient issue;
whereas (b) makes all the reverse specifications—
one which do not happen to conform with the exi-
gencies of the familiar world, and hence more
clearly flag the sentence as different from (a).
The non-synonymy of the two sentences is thus due
to the differentiality with which their nominals
specify the semantic functions of FIGURE and GROUND
(realized at the surface, for the examples above,
by the nominals' order), as can be indicated by
parenthesized function markings abbreviated sym-
bolized as 'F' and 'G':

(a) the bike (F) is near the house (G)
(b) the house (F) is near the bike (G).

Consideration here of some additional locative
and locative-like situations will clarify further
the semantic characteristics of the FIGURE and
GROUND categories:
The subject matter of a locative question must
be the FIGURE there and in the answer. This is
shown here both for a symmetric relation, be next
to, and for an 'inverse-pair' of asymmetric rela-
tions, be over/be under:

(a) (Where's the light?)

The light (F) is next to the chair (G).
*The chair (F) is next to the light (G).

The light (F) is over the chair (G).
*The chair (F) is under the light (G).

(b) (Where's the chair?)
*The light (F) is next to the chair (G).
The chair (F) is next to the light (G).

*The light (F) is over the chair (G).
The chair (F) is under the light (G).

The sentences marked as unacceptable are so because of having the wrong assignment of FIGURE and GROUND. Since these categories are indicated in ENGLISH both by order and by prosody, those assignments become righted—and the sentences acceptable—either with the reverse order of nominals (as in the accompanying partner sentences) or with a special intonation pattern, viz., emphatic stress on the first nominal and low stress and (almost 'under one's breath') tone on the rest:

Where's the light?

The chair (G) is next to it (F).

The latter sentence-type must be resorted to where there is lacking any surface verbal expression which would specify the relation for a FIGURE-first GROUND-second sentence. Thus, to the question 'where's the pen?', while RUSSIAN, for one, can answer in the preferred FIGURE-GROUND order:

Ivana's

the pen (is) 'by': in-the-possession-of John

ENGLISH must resort to the specially-intoned reverse-order type of sentence:

John has the pen.

The same considerations which have applied in the preceding physical situations hold as well for homologous non-physical situations. Thus, the sentence

(a) she resembles him,

which might be thought to derive from something like

she is near him in appearance

or  her appearance is near his appearance,

is not understood in the same sense as

(b) he resembles her

for all the reasons given above: that not merely quantity of resemblance is being specified, but, additionally, that one of the objects is taken as a reference-point and the other object is taken to
have a variability whose particular value is at issue. These additional understandings are brought into relief when, beside the above locative-like sentences, we place the motion-like sentence

(a) she grew to resemble him,

which would never be claimed to be equivalent to

(b) he grew to resemble her.

An 'equational' sentence, whose very name implies an assumption of its invertible equivalence, actually shows the same difference between its nominals as to variable vs. reference-point function as was seen above for the spatial sentences. This can be seen upon semantic inspection of an inverse-pair of sentences such as below in an example drawn from comicdom, where it is known that the 'real' identity of the man from Krypton is 'Superman' and his identity of disguise is 'Clark Kent'. It is thus appropriate to treat the former identity as a fixed reference point and the latter identity as displaced therefrom, and inappropriate to treat them in the reverse way, hence the difference in acceptability between the otherwise equivalent inverse sentences (with a superscript eks marking marginal acceptability)

Clark Kent is Superman.

$^x$Superman is Clark Kent.$^2$

So, far from any aptness in characterizing 'equational' sentences like the preceding on the model of mathematics, quite the reverse is the case. For, in the standard form of equations, like

$$y = 3x^2 + 1,$$

$y$, FIGURE-like, is considered a 'dependent variable' and appears alone on the left, while $x$, GROUND-like, is considered an 'independent variable', appears on the right, and is there grouped together with all operators and modifiers. This arrangement has no purely mathematical significance but rather derives from the same cognitive-semantic processes which determine the form of sentences like

The bike is to the left of the house

Clark Kent is really Superman in disguise.

Complex Sentences

Now, what the categories FIGURE and GROUND
pertain to can be generalized from the relative location of objects in space to the relative location of events in time—spatio-temporal homologies such as are illustrated by the following sentence pairs:

the fly was located (at a point) along the branch
the explosion took place (at a point) during the performance
flies were located all along the branch explosions took place all during the performance
this road goes (extends) for three miles/to the next town
the performance went on (lasted) for three hours/until 11 o'clock.

Paralleling that given earlier for spatial objects, the categories can be given the following more precise characterization for temporal events:

The temporal site of the FIGURE event is considered as a variable whose particular value receives characterization with respect to a GROUND event, considered as a reference-point set in a temporal reference-frame (usually, the one-dimensional time-line).

'FIGURE' and 'GROUND,' applied to events, are very near, if not the same as, 'asserted' and 'presupposed,' and constitute a generalization of these notions because of their application to physical objects as well.

The applicability of the semantic categories to temporal structures can be seen in a complex sentence like

he exploded after he touched the button,

which seems to assign a 'GROUND' interpretation to the button-touching event—setting it up as a fixed, known reference-point—and seems to assign a 'FIGURE' interpretation to the explosion event—establishing the location in time of this more salient occurrence with respect to the other. That such assignments have really taken place is perhaps demonstrated simply by noting that the inverse sentence

he touched the button before he exploded

is different in meaning: to this speaker, in fact,
it sounds comical, acquiring a becoming seriousness only after the imagining of such special circumstances as an official search into the possible causes of a known death.5

Since either asymmetric relation in an 'inverse-pair' equally well specifies the same relational information, the advantage to a language in having lexification for both—as ENGLISH has in before/after—is precisely that either of the related events can be specified as functioning as the FIGURE. In any language, however, there are inverse-pairs for which simple means of expression exist for only one of the relations (and it may be deemed that the language's expressive range suffers for the lack of the other). Such is the case in ENGLISH, e.g. for the inverse-pair expressing 'temporal-inclusion' between a 'point event' and an 'extent event'. When it is the point event that is relatively less known and is to be temporally located—as 'included within'—with respect to the better known extent event, the relation has simple lexical representation, as in

Shāt Māt of Persia was assassinated during Caesar's reign,

whereas where it is the extent event that is relatively less known and is to be temporally located—as 'including'—with respect to the better known point event, there is no simple apt lexical representation, as seen in

Shāh Rūkh ruled Persia \{ \text{around} \} \{ \text{through} \} \{ \text{before and after} \}

Christ's crucifixion.

While the above case is taken from ENGLISH, there is immediately noticeable across languages a bias perhaps within each inverse-pair for the same asymmetric relation. In fact, probably for every inverse-relation-pair, there holds one of two universal statements, an implicational one:

(a) only where a language has some, or simple, or simpler lexical means for the specification of an asymmetric relation R (of a complex situation) does it also have means for the specification of the inverse relation $R^{-1}$,

or an absolute one:
(b) whereas a language may have lexical means for the specification of the asymmetric relation \( R \) (of a complex situation), it never has such for the inverse relation \( R^{-1} \).

One example of a relation to which the first universal statement seems to apply is 'after'. English, of course, has the presence of lexical means, and equally simple such, for the specification both of this relation and of its inverse in the words after and before. ATSUGEWI for one, however, expresses the notion 'after' simply and directly with a verb suffix (akin in function to Russian's 'past gerundive' ending), as in

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having-eaten, we left,
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whereas it expresses the notion 'before' in a more complex and indirect way (by the addition of two independent words to the 'after' verb form), as in

```
still not having-left, we ate.
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Universal (a), if it is true for 'after' vs. 'before', thus implies that a language may, like English, have means for expressing 'before' equally simple as for 'after', or may, like ATSUGEWI, have less direct means for expressing 'before' than 'after', but that no language will have simpler and more direct means for expressing 'before' than 'after'.

An example of a relation to which the second universal statement seems to apply is 'all-during', as expressed at the surface, e.g. in English by all during, the whole time that, and while, etc. Since this relation may at first seem symmetric (aside from issues of FIGURE and GROUND), it first behooves us to show that it is not. This can be done by demonstrating a difference in the characteristics of the first and of the second events which may comprise the terms of the relation; if they are different, it follows that they cannot be reversed without changing the meaning in at least some cases. The sentences below reveal that for the second event in the relation, the extent of time occupied is necessarily bounded at both ends, for a second-position clause which specifies an inherently unbounded (at either end) event, such as the state of being dead, creates an unacceptable sentence:
she was studying in an American college the whole time that her father in Iran was ill; her father in Iran was dead.

On the other hand, the first event in the relation is not necessarily bounded at both ends, as is shown by putting into first-position the same clause specifying an inherently unbounded event and this time getting an acceptable sentence:

her father in Iran was dead the whole time that she was studying in an American college (but she didn't know it).

The difference between the first and second events as to the necessity of the temporal boundedness is schematized in the following diagram:

```
\[ \text{her father in Iran was dead} \text{while she was studying in an American college (but she didn't know it)}, \]

none will have the means for expressing

\[ *\text{she was studying in an American college while-}^{\downarrow} \text{her father in Iran was dead.} \]

For a second demonstration of the asymmetry of 'all-during', it is to be noticed of the two events comprising the terms of this relation that if the possibility of occurrence of one event is contingent on the occurrence of the other, it is only the former which can function as the relation's first term. For example, since the act of dreaming is contingent on the state of being asleep, a clause specifying the former can acceptably appear only in first-position in a sentence which specifies the occurrence, extensionality, and contemporaneousness of the two events:

\[ \text{he dreamt while he slept; } *\text{he slept while he dreamt.} \]

The second universal's holding for this redemonstratedly asymmetric relation 'all-during' would
imply that no language has a lexical equivalent for *he slept while he dreamed,* and indeed, in at least the several languages I have asked for such a form in, none exists.

It can be clear only after an extensive survey of languages whether there exists any universal bias towards one as against the other relation of asymmetric inverse-pairs like those above as well as others—including non-temporal ones—and then whether such bias is total or is proportional, involving relative simplicity of expression. But it is tentatively suggested that such a survey will reveal that sentences like the upper ones of the following pairs (merely an illustrative selection) represent the favored, or unmarked, relations of inverse-pairs, and that sentences like the lower ones represent relations—the corresponding inverses—which either are never or are not more simply expressed—and which in most cases here can in fact be indicated only by devised English phrases:

(a) she departed after his arrival (...after he arrived) he arrived before her departure

(b) he had two affairs during his marriage (...while he was married) he was married through-a-period-containing two affairs of his

(c) she rested until his arrival (...until he arrived) he arrived at-the-end-of her rest (-period)

(d) we stayed home because of the rain (... because it was raining) it was raining to-the-point-of-occasioning our staying home

(e) we went out despite the rain (...even though it was raining) it was raining in-futile-oppositiveness-to our going out.
FOOTNOTES:

1 One can see with the aid of the diagram below—schematizing as an example, a pen falling off a table—that for there to be any notion of the motion of an object (i.e., the FIGURE), there must also be present both a reference-point (the GROUND) and a reference-frame.

For, as illustrated in (a), if an observer (or conceiving) has in sight (or mind) only the FIGURE object, he can know only that the object exists, but nothing of change of position. Even when, as in (b), the observer sees both FIGURE and GROUND objects—still without any reference-frame, however—he can additionally know only that there is a change from the two objects' being together to their being apart, but could not know which object (or if both) moved nor whether there is any further motion once the two objects are apart, since there is no way to determine (change of) distance. Only when the observer sees both objects within a framework, as in (c), can he know which object is stationary, which object moves, by how much, and along what path. The notion of the motion of an object also crucially depends on the correlation of the spatial points of its path with points of the temporal continuum, but this will be taken up in detail in a subsequent study on space and time in language.

2 So semantically parallel are 'equational' sentences to locative sentences that I would even propose including in their underlying structures a deep preposition homologous with at, as if one could say at the surface, e.g.

Clark Kent is at Superman.

There is in fact syntactic evidence for something of this sort in ENGLISH with the preposition as, at least for copula sentences where the second nominal
expresses the role or function of the first:

Jim is on the throne in the play ⇒
the play has Jim on the throne (in it)

Jim is (as) the king in the play ⇒
the play has Jim as the king (in it)

Some languages do have a pre-/postposition at the surface beside the 'predicate nominal' of a copula sentence, SAMOAN overtly so with its 'o preposition, as in

'o se atua ia
"(as) a god he"
'he was a god'

'o le agasala 'ea le tulafono
"(as) the sin (interrogative) the law"
'is the law sin?'

and JAPANESE, somewhat disguisedly, in its desu verb, in

kore wa pen desu
"this (subject-marker) pen is"
'this is a pen'

This latter in some of its paradigmatic forms clearly breaks up into a postpositional particle de plus the verb aru (otherwise the 'be-located' verb for inanimate objects); its coalesced form follows the only postpositionless nouns in JAPANESE. The particle coalesced in desu may be identified with the elsewhere-appearing postposition de, having instrumental 'with' meaning, making the whole JAPANESE copula construction with desu parallel to that of RUSSIAN where the 'predicate nominal' is in the instrumental case, as in

on byl doktorom (instr)
"he was (as) a doctor"

The form of the complex sentences cited here—i.e., consisting of a main and a dependent clause with subordinating conjunction—derives, as I will develop the matter in a subsequent paper, from a syntactically deeper structure of a different form. A sentence more closely reflecting the latter at the surface consists of two nominalized clauses, a relational verb, and a 'subordinating preposition', as in the following analogs of the cited sentences:
his exploding occurred after his touching the button
his touching the button occurred before his exploding,
a form homologous with that of a locative sentence. The statements about FIGURE and GROUND in complex sentences are unaffected by the difference.

4 The remarks which follow about particular relations exemplifying these universals are not based on a survey of many languages but rather on a spot check, and are accordingly to be considered heuristic, pointing out a direction for investigation.

5 Not to be confused with this apparently universally lacking form is a form present in many languages, including ENGLISH, which arises secondarily by a derivational process dealt with in a later paper under the term 'copy-clefting':

he slept, \{ and he dreamt the while \} \Rightarrow dreaming (the while) \}. 
FUNCTIONAL CONSTRAINTS ON DELETION
OF WORD FINAL /S/ IN CUBAN SPANISH

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The term "functionalsim" in diacronic theory was used extensively by the French linguist, Martinet in his development of theories of phonological change. Kiparsky (1972) in his discussion of "explanation" in phonological theory defines functionalism in terms of speech performance, that is, perception, production, and acquisition. Specifically he proposes (p.195) that linguistic metatheory must, in addition to the paradigm of formal explanation, include "substantive conditions which pertain not to the form of grammars... but to their output." Two are suggested: leveling conditions "which state that allomorphy in paradigms tends to get eliminated" and distinctness conditions "which...state that there is a tendency for semantically relevant information to be retained in surface structure." Of course we are a long way from a characterization of the phrase "semantically relevant information." One way of investigating this problem is to examine the operation of deletion rules in phonology. Labov (1971:472), in this context suggested the following statement as a candidate for a linguistic universal constraint on language change:

"...whenever a final consonant is variably deleted the rule will operate more often...if it is an integral part of the word and not a separate morpheme... (P.472)."

This condition has, of course, a direct functionalist interpretation: grammatical morphemes may carry essential information, whereas more often than not the deletion of a final consonant from a monomorphic word would not hinder greatly its recognition.

If, then, in the study of variable phonological rules we find morphological conditioning, it may be the case that distinctness is operating as a functional constraint. However, Kiparsky warns, "The mere existence of such morphological conditioning in rules is not of course in itself enough to prove that functional factors are at work."

The purpose of this paper is to examine the operation of deletion rules in Spanish as evidence to support this functionalist hypothesis. In addition certain refinements in the formulation of functionalist condition will be suggested.

In extensive investigations of Cuban Spanish (for a complete discussion see Terrell, December 1974), it was found that
the weakening of syllable and word final /S/ was governed by two phonological processes, intrinsically ordered: Aspiration (S→h) and Deletion (h→∅). Aspiration was shown to be basically phonologically controlled. Deletion, on the other hand showed strong grammatical correlations in addition to the phonological correlations. The relationship of the phonological constraints for both processes (N=7149) is shown on the following graph:

GRAPH I Aspiration and Deletion

![Graph showing the relationship between aspiration and deletion](image)

Preconsonantal  Prevocalic  Prepausal

Aspiration = all h plus ∅
Deletion = ratio of ∅ to h plus ∅
Word final /S/ in Spanish may be an intrinsic part of a monomorphemic word such as mes 'month', lápiz 'pencil', entonces 'then', it may be a part of a morpheme, such as in the first person plural verb marker /mos/, hablamos, 'we speak', and it serves as a plural marker for determiners, adjectives, pronouns, and nouns, mesa, mesas 'table, tables.' Since there is adjective-noun agreement in Spanish, plural markers may be redundant: las muchachas bonitas 'the pretty girls.'

In order to test the possibility for gross morphemic correlations, I compared the index of Deletion of /S/ in monomorphemic words to the index of Deletion of /S/ in grammatical morphemes. The results are given in Table 1.

**TABLE I**

<table>
<thead>
<tr>
<th>Deletion</th>
<th>C (%)</th>
<th>V (%)</th>
<th>// (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Inflectional</strong></td>
<td>20% 447/2203</td>
<td>39% 263/673</td>
<td>69% 296/428</td>
</tr>
<tr>
<td><strong>Monomorphemic</strong></td>
<td>29% 156/535</td>
<td>45% 89/199</td>
<td>44% 43/98</td>
</tr>
<tr>
<td><strong>All Categories</strong></td>
<td>23% 672/2890</td>
<td>40% 390/957</td>
<td>65% 355/549</td>
</tr>
</tbody>
</table>

It is impossible to draw clear cut conclusion from the data. If functional constraints are at work, they are certainly not reducible to terms of grammatical vs lexical morphemes.

An examination of the index of Deletion of the /S/ of the verbal suffix /mos/ is quite interesting, however:

**TABLE II**

<table>
<thead>
<tr>
<th></th>
<th>C (%)</th>
<th>V (%)</th>
<th>// (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>/mos/</td>
<td>72% 110/153</td>
<td>76% 83/107</td>
<td>57% 13/23</td>
</tr>
<tr>
<td>Norm</td>
<td>23%</td>
<td>40%</td>
<td>65%</td>
</tr>
</tbody>
</table>
The deletion rate is quite high (except in prepausal position where a low N makes the index unreliable). This /S/, on the other hand serves no contrastive function; there is no possibility of confusion if the morpheme is reduced to /mo/ (as in Italian). These data suggest that a gross grammatical/lexical distinction is incorrect. The original functionalist hypothesis was formulated in terms of information retention. Therefore I decided to reconsider the data in terms of grammatical redundancy.

There are data from previous studies which support this position. For example, Cedergren (1973) argued against Labov's proposal for a universal constraint on deletion rules which favored deletion in monomorphemic words by showing that the deletion rate for inflectional /r/ was consistently higher than the deletion of non-inflectional /r/.

**TABLE III: /r/ Deletion**

<table>
<thead>
<tr>
<th>Syllable Final</th>
<th>Monomorphemic</th>
<th>Infinitive</th>
</tr>
</thead>
<tbody>
<tr>
<td>15% 580/3949</td>
<td>13% 223/165</td>
<td>34% 114/3240</td>
</tr>
</tbody>
</table>

The infinitive /r/ is totally redundant, both phonologically and syntactically; the infinitive is always phonologically identifiable by its thematic vowel and stress pattern: 1st conjugation habla(r) 'to speak', 2nd conjugation come(r) 'to eat', 3rd conjugation vivi(r) 'to live'. (Only in the case of the 3rd conjugation forms is there homophony, in this case with the 1st person singular Past (Preterite) forms: vivi 'I lived'. However, since the two forms never occur in the same syntactic (or semantic) context, the meaning is entirely predictable.)

Another example of this type is the deletion of intervocalic /d/. This particular process is "stigmatized" in most dialects and therefore highly stratified socially. However, again it is clear that the deletion occurs much more frequently in the participial morpheme /do/. The following data are also from Cedergren's Panamanian study:

**TABLE IV: D - deletion**

<table>
<thead>
<tr>
<th>Non-grammatical</th>
<th>Grammatical</th>
</tr>
</thead>
<tbody>
<tr>
<td>21% 931/4434</td>
<td>34% 377/1110</td>
</tr>
</tbody>
</table>
The most crucial test of any redundancy hypothesis will occur within the noun phrase. Kiparsky (p. 206) claims that "number" is a strong category, i.e., highly resistant to loss. However, since all words of the simple noun phrase are marked for plurality, if redundancy is indeed a constraint on deletion, the morphological correlations with Deletion should be clear.

Originally, I had planned to divide nouns into three groups: (1) number indicated by a preceding modifier; (2) number indicated by a following modifier; and (3) unmodified noun. Examples would be:

1. Los niños ya llegaron....'the children already arrived'
2. Un lugar de arboles grandes 'A place with big trees'
3. Un grupo de plantas 'A group of plants'

The number of occurrences of case (2) was so limited, however, that I will only report the data of the polar cases:

<table>
<thead>
<tr>
<th></th>
<th><em>C</em></th>
<th><em>V</em></th>
<th><em>///</em></th>
</tr>
</thead>
<tbody>
<tr>
<td>Modified previously</td>
<td>45%</td>
<td>63%</td>
<td>78%</td>
</tr>
<tr>
<td>Norm</td>
<td>23%</td>
<td>40%</td>
<td>65%</td>
</tr>
<tr>
<td>Unmodified</td>
<td>11%</td>
<td>20%</td>
<td>38%</td>
</tr>
</tbody>
</table>

The results are quite clear---the frequency of deletion is very high if the noun is preceded by a modifier which causes a plural indicator. If the noun is unmodified, the deletion rate is very low. Note, however, these correlations will be explanatory only if the deletion rate of preceding modifiers is very low. In order to ascertain if this is the case, I compared the deletion rate for the first adjective in the noun phrase (for the most part determiners) with the rate for all other adjectives, with presumably redundant plural markers.
TABLE VI: Modifiers

<table>
<thead>
<tr>
<th>Adjectives</th>
<th><em>C</em></th>
<th><em>V</em></th>
<th>_//</th>
</tr>
</thead>
<tbody>
<tr>
<td>1st</td>
<td>3%</td>
<td>2%</td>
<td>5%</td>
</tr>
<tr>
<td>Norm</td>
<td>23%</td>
<td>40%</td>
<td>65%</td>
</tr>
<tr>
<td>Others</td>
<td>39%</td>
<td>51%</td>
<td>71%</td>
</tr>
</tbody>
</table>

The data are clear; the /S/ which is preserved is the first plural marker encountered in surface structure.

The data from Spanish strongly support Kiparsky's proposal for distinctness conditions in functionalist terms. In particular, I propose that distinctness conditions are not correlated with morpheme functions, but rather with redundancy in surface structure. An exact characterization of redundancy will not be easy. It is well known that a certain amount of redundancy is necessary for ease of communication. For example, number in the noun phrase is almost always redundant if other factors such as discourse contexts, subject-verb agreement, certain morphological changes in determiners, etc., are taken into consideration. However, the interesting fact is that in spite of this Spanish speakers consistently avoid suppressing all traces of a morphological indication of number. Even in dialects (Andalucian) in which Deletion is almost categorical, it is claimed that a system of vowel contrasts and harmony preserves the morphological distinction (see Hooper, 1973 for discussion). More detailed studies of constraints on deletion rules will further refine our notion of redundancy as a factor in language change.

BIBLIOGRAPHY


Speaker Alignment and Embedded Performatives

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Numerous papers have appeared challenging Ross's formulation (Ross 1970) of the performative analysis, e.g. (Anderson 1969), (Fraser 1969), and (Sadock 1974). One of the most frequently attacked parts of this formulation has been the performative-as-highest-verb hypothesis. Although many counter-examples have been given, we know of no attempt to characterize the conditions under which performatives may embed while retaining their performative force. We believe that such conditions exist, and in this paper, we would like to examine one such condition on a class of embedding constructions in terms of the function of that embedding.

Jerry Morgan has discussed the utility of such a "functional" approach to grammar, that is, an examination of grammatical elements in terms of their function in a discourse (Morgan 1973). Explicit performatives, by their nature, do something rather than be something, and thus are uniquely suited to such a functional investigation.

The class of performative embeddings we are considering are those in which the performative is embedded in an expression of emotional state. Perhaps the most famous example of this class is given in (1):

(1) I regret to inform you that my cobra ate your chihuahua.

A more productive form of this class is that of infinitival complementation under adjectives, exemplified by (2) - (4).

(2) I'm pleased to announce the ascension of my only begotten son.
(3) I'm proud to nominate Homer T. Pettybone to be the next chairman of the Fraternal Order of Mastodons.
(4) I'm sorry to find you guilty of conspiracy to over-populate, Mr. Rabbit, but the law leaves me no choice.

Although this construction is highly productive, it is constrained in a number of interesting ways. Thus, many emotional state adjectives are not suitable for embedding performatives, as in (5), and other emotional state adjectives, when embedding performative verbs, remove those verbs' performative force, as in (6).
(5) *The Chair is surprised to recognize the delegate from Pretoria. confused
disconcerted

(6) I am reluctant to find you guilty, but I may have to if your attorney doesn't stop making obscene gestures at opposing counsel.

After eliminating such cases, we are left with a group of emotional state adjectives that occur frequently with performatives while retaining performative force, a representative sampling of which we list below:

<table>
<thead>
<tr>
<th>Positive</th>
<th>Negative</th>
</tr>
</thead>
<tbody>
<tr>
<td>I am happy to</td>
<td>I am sad to</td>
</tr>
<tr>
<td>glad</td>
<td>sorry</td>
</tr>
<tr>
<td>pleased</td>
<td>distressed</td>
</tr>
<tr>
<td>proud</td>
<td>embarrassed</td>
</tr>
<tr>
<td>relieved</td>
<td>(I regret to)</td>
</tr>
<tr>
<td>delighted</td>
<td></td>
</tr>
</tbody>
</table>

The positive adjectives above are part of a natural class which serves to report a positive emotional attitude on the part of a speaker towards some act or event. It follows that embedding a performative under one of these adjectives reports the speaker's attitude toward the speech act performed. In the case of the positive adjectives, this aligns the speaker favourably towards his performance of that act, while in the case of the negative adjectives, which are part of a natural class reporting negative emotional attitude, the alignment is unfavourable.

At the same time, many performative verbs have alignments of their own, so that one would expect that if the alignment implicit in a given performative is contradictory to that of the adjective under which it is embedded, the resulting sentence will be anomalous. This is indeed the case, as seen in (7) and (8).

(7) I am *sorry to congratulate on winning a year's glad supply of Cupid's Quiver, the raspberry flavoured douche.

(8) Your Honor, I am sorry that I must object to *glad opposing counsel's outrageous behaviour.

In order to account for for these grammaticality judgments in accordance with our comments above we propose the following principle:
The Speaker Alignment Principle (SAP)

A performative utterance may be embedded under a member of one of the classes of adjectives listed above only if that adjective functions to align the speaker in a direction consistent with the alignment implicit in context of that utterance.

The SAP predicts the grammaticality judgements in (7) and (8). The unfavourable alignment associated with sorry is inconsistent with the notion of favourable reaction which congratulate functions to communicate, while that of glad is consistent; whereas in (8) a favourable alignment towards the act of objecting, which is culturally marked as rude and impolite, is clearly inappropriate.

"Implicit in context" is necessarily vague, but involves such factors as speaker-based felicity conditions on the performative act, prevailing social attitudes, and the degree of speaker control over the action itself. There is, in addition, the speaker's anticipation of the addressee's reaction. It is the diverse character of this phenomenon that motivates the appellation of "functional", because it cuts across the traditional boundaries of syntax, semantics, and pragmatics in seeking an explanation for an overtly syntactic constraint, as such a condition on the permissibility of embedding might once have been considered.

On the basis of this concept of alignment in context, it is possible to distinguish four classes of performative verbs with respect to favourable and unfavourable alignments: those which are neutral, embedding freely under either type of adjective; those which strongly involve a favourable alignment, thus embedding only under positive adjectives; those which tend to involve a favourable attitude, but which may also embed under negative adjectives in contexts where the speaker is not a free agent; and finally a small class which strongly involve an unfavourable alignment, embedding only under negative adjectives. A representative sampling of each class is given below:

I. Neutral: announce, inform, report, say, tell (In McCawley's expansion of Austin's classifications (McCawley 1973), these are all Expositives.)

II. Favourable Alignment: apologize, baptize, bequeath, congratulate, nominate, promise, pronounce, swear, undertake, vow (McCawley - Behabitives, Operatives, Commissives)
III. **Favourable Alignment** But (allowing unfavourable alignment when performed under duress): allow, recognize, resign, permit, propose, vote, declare, find

IV. **Unfavourable Alignment**: complain, object, protest

(McCawley - Dehabitves, Expositives)

Verbs in Category I are characterized by a lack of implicit alignment, stemming from their use as emotionally neutral, informative verbs. Note that they all belong to the class of Expositives. Because of this lack of alignment a speaker is free to embed a category I verb under either a positive or a negative adjective, thereby aligning himself either favourably or unfavourably towards his speech act, as we see in (9).

(9) I am pleased to announce that Nelson Rockefeller's sorry appointment has just been confirmed.

Verbs in Category II are characterized in normal circumstances by a felicity condition which requires that the speaker perform the act voluntarily. Searle has discussed this in some detail with respect to the verb *promise* (Searle 1969), and the rest of the verbs in this category seem to share a similar felicity condition. This in turn implies a favourable alignment on the part of the speaker, permitting embedding under positive adjectives, but precluding it under negative adjectives, as can be seen in (7) above.

Verbs in Category III differ from those in Category II in that when a speaker performs a speech act involving one of them he does not necessarily do so voluntarily. For instance, one may resign voluntarily or under pressure. Embedding these verbs under a negative adjective is thus possible only in contexts involving coercion or duress, since one does not voluntarily do what one is opposed to. The pattern for verbs of this category is exemplified in (10) – (12).

(10) I am pleased to cast my vote for Phineas T. Phogbound.

(11) *I am sorry to cast my vote for Manfred Malaprop, considering that he is the perfect man for the job.*

(12) I am sorry to cast my vote for Manfred Malaprop, but he owns the mortgage on my house.

Verbs in Category IV appear to be socially marked as non-cooperative, thereby implicating that the
speaker is obliged to do as he does by force of circumstances and precluding a favourable alignment. Example (8) above belongs in this class. This class appears to be extremely limited in membership, and it is not clear to us that it does, in fact, constitute a separate class.

This classification of performatives is in terms of characteristics orthogonal to those used in previous taxonomies, and so there is no simple relationship apparent between the position of a verb in any of those taxonomies (we have examined those of Searle, Austin, Fraser, and McCawley), and its category as defined above. It does seem reasonable however that all Category I verbs will be Expositives in the Austin-McCawley sense, although the converse is probably not true. We think the phenomenon discussed in this paper exemplifies the kind of explicit metric needed to adequately classify performative verbs, where previous investigators have largely used their intuition (pace Searle's word to world/world to word work).

Fraser has shown (Fraser 1973) that it is possible to embed performatives under certain modals (e.g. must, have to, can, be able to) while retaining the performative force of the utterance. The SAP correctly predicts both which modals will combine with which verbs to create hedged performatives, and when such hedged performatives will embed under what adjectives. First we note that for have to and must the lack of voluntariness they convey implies a negative alignment, while the voluntariness associated with can and be able to implies a positive alignment. The SAP claims then that we will be able to find can and be able to forming hedged performatives with verbs from categories I, II, and III but not from IV. Likewise it predicts that have to and must will combine with verbs from categories I, III, and IV. Examples (13) - (20) below demonstrate the correctness of this claim.

(13) I am now able to report that we can see the light at the end of the tunnel.
(14) I am now able to bequeath the Hope diamond to you, my only remaining relative.
(15) After considerable effort, I am now able to find in favour of the defendant.
(16) ??After spending two nights in this dump, I am able to complain that the food is terrible.3
(17) I have to report that the tunnel just collapsed.
(18) *I have to swear to love, honour and obey.3
(19) Mr. Nice, I have to declare you guilty, despite my personal feelings in the case.
(20) Waiter, I have to complain about the cockroaches in my soup.

Hedged performatives formed like those above adopt the alignment of the participating modal. Thus hedged performatives containing can and be able to function as performatives in category II, that is, they embed only under positive adjectives, while those containing must and have to behave like those in category IV, embedding only under negative adjectives. This pattern is exemplified by examples (21) and (22) below, where we see a verb from Category I which normally has no restrictions on embedding. When it forms a hedged performative with be able to, it behaves like a category II verb, but when it combines with have to, it behaves like a category IV verb.

(21) I am happy to be able to tell you the results of the last race.
(22) I am *happy to have to tell you the results of the last race.

The SAP attempts to account for a single condition on a particular class of embedded performative constructions. It is difficult to imagine a way to account for the facts presented in other than functional terms. Other conditions on this and other similar classes of embedding constructions are certainly needed, and they also will require statement in functional terms. (23) – (26) below give some examples of such other constructions which seem to invite a functional explanation.

(23) It will come as no/a surprise to you, my friends, when I declare my opposition to the measure at hand.
(24) The man who I hereby nominate for this office is Homer T. Pettibone.
(25) You two, who I now pronounce man and wife, may now embrace.
(26) I choose to resign.

In conclusion, we hope we have demonstrated in this paper the utility of investigating grammatical phenomena in a frame of reference which, although not formal, is nevertheless explicit, and that this small demonstration will encourage the use of such a functional approach in other investigations of similar recalcitrant problems which, like this one, cut across traditional boundaries.
Footnotes

1. We owe a debt of gratitude to George Lakoff, for many helpful suggestions and examples, to friends too many to mention for many hours of discussion, and to Robin Lakoff for championing the "explicit but not formal" approach. Needless to say, all errors of both omission and commission are none's responsibility but our own.

2. Due to Robin Lakoff

3. The grammaticality judgements for these sentences are for the performative reading.

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N+V COMPOUND NOUNS IN THAI*

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Compound words in Thai, as noted by Haas (1966), are of various types, e.g. N+N, N+V, V+N, and V+V, etc. In this paper I will discuss whether N+V compound nouns
(a) are formed by transformational rules,
(b) are formed by lexical rules, or
(c) are lexical items.
I will argue that compounds of this type should be lexical items. In addition, I will discuss the cases in which some N+V compound nouns may have a phrasal origin (i.e. N + verbal modifier). Finally, I will show how the native speaker knows that the compound is related to its components, and how this knowledge should be captured in a linguistic theory.

1. N+V compound nouns

N+V compound nouns and phrases which look like compound nouns are exemplified below.

(1.1) a. khraān-bin (machine+to fly) 'airplane'
    naām-tōk (water+to fall) 'waterfall'
    bay-phāt (blade+to spin) 'propeller'
    bandsy-lāān (stair+to move) 'escalator'
    b. yaa-salōp (medicine+to faint) 'anesthetic'
yaa-thāāy (medicine+to have a bowel movement) 'laxative, purga-

    tive'
    c. kāy-chon (chicken+to fight) 'fighting cock'
    plaa-kāt (fish+to bite) 'fighting fish'
    huu-rūnt (ear+to slide) 'zipper tab'
    māāy-thāāw (wood+to lean on) 'walking stick'
    e. nāāsā-khām (book+to print) 'newspaper'
    thuā-tāt (bean+to cut) 'peanut brittle'
    sāy-krāk (intestine+to fill) 'sausage'
    naalikāa-phōk (watch+to carry (in a pocket, etc.)) 'pocket watch'
    naalikāa-plūk (watch+to wake) 'alarm clock'
    f. māāy-khīt (wood+to scratch) 'matches'
    g. bay-sēt (sheet+to finish) 'receipt'
    bay-kāāt (sheet+to be born) 'birth certificate'
    (1.2) a. manaaw dōōą (lime+to pickle) 'pickled lime'
    b. krāthiām dōōą (garlic+to pickle) 'pickled garlic'
    c. khaāw phāt (rice+to fry) 'fried rice'
    d. khaāw tōm (rice+to boil) 'boiled, watery rice'

This is a revised version of what appears in the UCLA Papers in Syntax, No. 6, 1974. Edited by Sandra A. Thompson.
The grouping of the above items is done for expository and reference purposes. Most of them are grouped according to the meaning or the paraphrased meaning of the phrases and compounds. For example, (1.1a) indicates the capability of the noun-component, (1.1b) has a causative meaning, etc. Some of them can be interpreted in more than one way. For example, (1.1c) can be understood as having either purpose or capability (of the noun-component) reading. The status of the items in (1.2), i.e., whether they are compounds or phrases, calls for discussion. The forms in (1.3a-b) consist of a noun and a so-called descriptive verb. (1.4a) contains forms which may be regarded by some speakers as compounds, but by others as polysyllabic words. The (metaphorical) meaning of these 'compounds' can, given some extra-linguistic knowledge, e.g., a particular Thai legend, or imagination, etc., indirectly be related to the componential parts of the 'compounds'. For example, naañ-kwàk is a plant whose curl leaves look like the shape of a lady's hand when it is waved palm downward. (1.4b) contains forms which may not be regarded by anybody as compounds, because their (metaphorical) meaning is too far fetched from the componential parts. I have tried to illustrate variety of N+V, but I do not claim to have shown them all.

Before moving on to discuss the mechanism which accounts for the compound nouns of the form N+V, we have to show that this kind of compound exists. To do this, let's consider the difference between a compound, e.g., banday-làan 'escalator (stair+to slide or move)' and tô làan 'table' 'to move', which is a non-compound structure. (1.5), (1.6), and (1.7) show that banday-làan , like
a noun, can take a demonstrative, a numeral, and a modifier, but
tó lânan cannot.

(1.5) a. banday-lânan nán
    stair move that
    'that escalator'
b. *tô lânan nán
    table move that
    '*that moving table'

(1.6) a. banday-lânan 2 an/khrâng
    Classifier
    '2 escalators'
b. *tô lânan 2 tua
    Classifier
    '*2 moving table'

(1.7) a. banday-lânan sîi dam
    color black
    'black escalator'
b. *tô lânan sîi dam
    '*black moving table'

(1.5) - (1.7) indicate that banday-lânan does not function
syntactically as a phrase consisting of a noun and a verb. How-
ever, they do not rule out the possibility that banday-lânan is
a noun phrase, i.e. a noun plus a modifier. The following will
show that this is not the case. If lânan can function as a
modifier, it should be able to modify a noun in a liberal way,
provided no selectional restrictions are violated and the contexts
are appropriate. The fact that lânan cannot be used to modify
nouns such as 'table' and 'chair', even in the following situa-
tion, indicates that it cannot function as a modifier. Suppose
there is an earth tremor and some tables and chairs are moving.
Upon seeing the moving tables and chairs one cannot say,

(1.8) *duu tô/kâwiî lânan tua nán sî
    look table/chair move Classifier that Particle
    '*Look at that moving table/chair.'

In addition, after the tremor, one cannot say (1.8) to mean
'Look at that moved table/chair' either. Since lânan in
banday-lânan can function neither as a verb nor a modifier, it
must be a part of a compound. This means that compound nouns
of the form N+V exist.

2. Transformational rules, lexical rules, or compound lexical
   items

   Earlier linguists, for example Warotamasikkhadit (1972) and
   Fasold (1969), derive the compounds in question by transforma-
tional rules. In addition, some authors of Thai textbooks, e.g.
Yates and Tryon (1970), imply that these compounds are derived
transformationally.

   Warotamasikkhadit (1972), using a generalized transformation
framework, derives a compound from two independent sentences.
Without elaboration, he posits, for example, rules GT.26 and GT.
32 to derive sentences containing the compounds nám-khâng 'dew
(water + stay, or get stuck on or in something)' and taw-riît
'an iron (stove+to iron)', as illustrated in (2.1) and (2.2).
(Note: K = kh, and P = ph)
(2.1) GT.26 \( X + N_1^{1}(\text{Det}^1)Y \) \( \rightarrow \) \( X + N_1^{1} + V_1(\text{Det}^1)Y \)
\( \text{N}_2^{2}(\text{Det}^2)V_1 \)

where \( N_1 = N_2 \)

\( \text{na'am + kɔʔ + bay + yàâ} \)
water hold leaf grass \( \rightarrow \text{na'am+kàaŋ+kɔʔ+bay+yàâ} \)
dew stays on leaves of grass

\( \text{na'am + kàaŋ} \)
water stay

(2.2) GT.32 \( X + N_1^{1}(\text{Det}^1)Y \)
\( \text{N}_2^{2}(\text{Det}^2)VB \{ \text{duay} \} \text{N}_3^{3}(\text{Det}^3) \)

where \( N_1 = N_3 \), \( VB = V_1 \), \( V_{am}, V_t + \text{Nom}, V_m + \text{Nom}, \text{pen} + \text{Nom} \)

\( \text{kàw + mii + taw} \)
he have stove \( \rightarrow \text{kàw+mii+taw+riiṭ} \)
'he has an iron'

\( \text{raw + riṭi + Paâ + duay + taw} \)
we iron cloth with stove

The shortcomings of this analysis are:
(a) Constraint: There doesn't seem to be a non-ad hoc way to prevent his rules from deriving unacceptable strings. For example, (2.3), which has exactly the same structure as (2.2), will undergo rule GT.32, when it should not.

(2.3) \( \text{kàw + mii + màây} \)
he have wood \( \rightarrow \) *kàw+mii+màây+tii²*

\( \text{raw + tii + dék + duay + màây} \)
we beat child with wood

\( \rightarrow \) *'he has a switch (used for whipping)*

The problem with rule GT.26 can be shown as follows: \( N_1 \) in the first string in GT.26 must be allowed to be either a subject (so that sentences like (2.1) can be derived) or an object (so that a sentence like kàw hén na'am-kàaŋ 'he saw dew' can be derived). A problem arises when we try to derive sentences like (2.4).

(2.4) \( \text{na'am bon bay bua nán kɔʔ tɔaak na'am-kàaŋ thì ruamtua kan} \)
water on leaf lotus Dem born from dew Rel combine together

The water on that lotus leaf came from dew which combined together.

According to GT.26, the second string doesn't allow anything before the noun and after the verb. Therefore, the underlying structure of (2.4) will have to be (2.5) (disregard irrelevant transformations which may have applied).
(2.5) **nââm bon bay bua nán kòet caâk naâm thî ruamtua kan**

\[
\begin{align*}
N_x & \quad P & \quad N & \quad N & \quad \text{Dem} & \quad V & \quad P & \quad N_y & \quad \text{Rel} & \quad V & \quad \text{Adv} \\
\text{water} & \quad \text{on} & \quad \text{leaf} & \quad \text{lotus} & \quad \text{That} & \quad \text{born} & \quad \text{from} & \quad \text{water} & \quad \text{which} & \quad \text{comb.} & \quad \text{to.}
\end{align*}
\]

**nââm khaâŋ**

\[
N_z & \quad V_1
\]

**water stay**

First, the first string in (2.5) is semantically odd. Second, since both \(N_x\) and \(N_y\) are identical to \(N_z\), rule GT.26 can attach \(V_1\) to either \(N_x\) or \(N_y\). In the second case, we have (2.4). In the first, we have the semantically anomalous string (2.6):

(2.6) **nââm-khaâŋ bon bay bua nán kòet caâk naâm thî ruamtua kan**

\[
\begin{align*}
N_x & \quad V_1 & \quad N_y \\
\text{dew} & \quad \text{on} & \quad \text{leaf} & \quad \text{lotus} & \quad \text{that} & \quad \text{born} & \quad \text{from} & \quad \text{water} & \quad \text{which} & \quad \text{comb.} & \quad \text{to.}
\end{align*}
\]

The dew on that lotus leaf came from water which combined together.

(b) Criteria: There are no criteria for what can appear as an input to the rules. For example, instead of formalizing rule GT.32 to allow the second string in the rule to be `raw riit Pâ duay taw "we iron cloth with 'stove'", we can formalize it in such a way that the second string can be `raw/khâw/khon raw chây taw riit phaâ/sâaphâa "we/he/human beings use 'stove' to iron cloth/clothing", or taw chây sâmrap riit phaâ/sâaphâa "a 'stove' is used for ironing cloth/clothing".

(c) Deletion: Deletion of meaningful items doesn't seem to be well motivated or to have independent justification. For example, all the following items in the second string of (2.2) -- the subject, the object, and the adverbial -- are subject to deletion. This kind of deletion is highly questionable.

(d) Meaning: It is not clear how the meaning of the compounds can be arrived at.

Like Warotamasikkhadit (1972), Fasold (1969) divides compounds of the type \(N+V\) into several groups. Unlike Warotamasikkhadit, however, he demonstrates how a compound itself is derived. Instead of deriving a compound from two independent sentences, he derives it from an NP containing a relative clause, according to the paraphrased meaning of the compound. For example, râa-bin (boat+ to fly) 'seaplane, airplane' and naaâlîkâa-plûk (clock+to wake) 'alarm clock' are derived as in (2.7) and (2.9).

(2.7) **BASE:** \(<\text{râa} \quad \text{râa} \quad \text{bin} \quad S \quad NP>\)

boat boat fly

(For the benefit of the reader, I illustrate the intermediate derivations (as would be derived by Fasold himself) as follows:)

T. Rel Insertion

\(<\text{thîi} \quad \text{râa} \quad \text{bin} \quad S \quad NP>\)
After that a general noun compound (GNC) rule, illustrated below, will turn the last structure above into a compound, i.e. ráa-bin.

(2.8) \[
\text{GNC} \rightarrow ^{\text{N}} \left\{ \begin{array}{c}
\text{N} \\
\text{V}
\end{array} \right\} \rightarrow ^{\text{NP}} \\
1 \quad 2
\]

(2.9) \[
\text{BASE: } \langle\text{naalíkaa} \quad \text{khon} \quad \text{cháy} \quad \text{sámráp} \quad \text{kaan} \quad \text{kaan} \\
\text{pen} \quad \langle\text{naalíkaa} \quad \text{plúk} \quad \text{khon} \quad \rightarrow ^{\text{NP}} \\
\text{be} \quad \text{clock} \quad \text{arouse} \quad \text{person} \quad \rightarrow ^{\text{S}} \\
\rightarrow ^{\text{NP}} \quad \text{naalíkaa} \quad \rightarrow ^{\text{S}} \\
\text{clock} \quad \rightarrow ^{\text{NP}} \]

After a series of Relativization and Deletion rules, which need not be elaborated here, have applied to this string, the structure will be

\[
\langle\text{naalíkaa} \quad \langle\text{plúk} \quad \text{khon} \quad \rightarrow ^{\text{NP}} \\
\text{clock} \quad \text{arouse} \quad \text{person} \quad \rightarrow ^{\text{N}}
\]

(Actually the position occupied by N should be VP. It might be a misprint in his manuscript.)

Then Fasold (1969) states (p.116):

GNC applies to this structure to form naalíkaaplúkkhon , a non-occurring compound. In order to generate naalíkaaplúk , we will have to add a rule which applies to structures generated by GNC to delete unwanted members in cases like this. The rule is of this form:

\[
\text{Noun Compound Deletion 1. } \langle\text{N} \quad \text{V} \quad \left[ \begin{array}{c}
\text{+N} \\
\text{+Pro}
\end{array} \right] \rightarrow ^{\text{N}} \\
1 \quad 2 \quad 3 \\
\rightarrow ^{\text{} 1} \quad 2 \quad \emptyset
\]

This optional rule deletes the third member of a noun compound of the form Noun-Verb-Noun if the third member is a Pro-noun like khon 'person'.

The problems encountered by Fasold's model include the following:

(a) Constraint: There does not seem to be a non-ad hoc way
of blocking the transformational rules from deriving unacceptable strings. For example, a rule which derives the acceptable compounds in (2.10a) will also derive the unacceptable ones in (2.10b), because both of them have exactly the same underlying structure.

\[
\begin{align*}
(2.10) \quad (a) \quad \{ \text{klûay} \} & \quad \{ \text{khâaw} \} \\
& \quad \{ \text{mapraaw} \} \\
& \quad \{ \text{lîncii} \} \\
& \quad \{ \text{kûn} \} \\
& \quad \{ \text{plaa} \} \\

& \quad \text{sûn} \quad \text{tàak} \quad \text{hûn} \\

(b) \quad \text{plaa} \\
\end{align*}
\]

Another example is that the Noun Compound Deletion Rule Fasold posited will also delete an item when it is not supposed to. According to this rule, the second noun of the structure Noun-Verb-Noun will be deleted if it "is a Pro-noun like khon 'person'". Unfortunately, there are compounds of the form N-V-N in which the second noun is khon and it cannot be deleted, e.g. mânût-kin-khon (man+to eat+person) 'man-eating person', and tômâay-kin-khon (plant+to eat+person) 'man-eating plant'.

(b) Meaning - Deletion: A transformational approach which derives a compound from a structure which is based on the paraphrased meaning of the compound to be derived will encounter a problem with compounds like:

\[
\begin{align*}
(2.12) \quad & \quad \text{a. tûa-camnam} \quad \text{(ticket+to pawn)} \quad \\
& \quad \text{b. bay-laaw} \quad \text{(sheet+to take leave)} \quad \\
& \quad \text{c. sànaam-bin} \quad \text{(field+to fly)} \\
& \quad \text{'receipt listing pawned items'} \quad \\
& \quad \text{'application for a leave of absence'} \quad \\
& \quad \text{'airport'} \\
\end{align*}
\]

For example, the meaning of (2.12b-c) is (2.12b'-c'):

\[
\begin{align*}
(2.12) \quad & \quad \text{b'. bay sàn khôn yàm phàa khûn laaw nàn} \\
& \quad \text{sheet rel person submit for request take work leave} \\
& \quad \text{c'. sànaam sàn khriàan-bin bin khán (bin) lon} \\
& \quad \text{field rel airplane fly up fly down}
\end{align*}
\]
If we capture the meaning of a compound in the deep structure, (2.12b-c) will have the deep structure (2.12b'-c') respectively. This means that all the underlined items in (2.12b'-c') have to be deleted. This kind of deletion should be objected because several meaningful, unrecoverable items are deleted. On the other hand, if we disregard the meaning in the deep structure and derive, for example, (2.12b) from

(2.12) b*: bay  săn  laa
     sheet Rel  to take leave

we have difficulty explaining how the actual meaning of the compounds is arrived at.

Yates and Tryon (1970) will not be discussed because they merely imply that the compounds are derived transformationally.

What is left to be decided is whether the compounds we are discussing are best described as single lexical items or as derived by lexical rules. The decision will be based on (i) the productivity of the rule which conjoins a noun with a verb, and (ii) the predictability of the meaning of the compounds.

(i) **Productivity**

If the rule which combines a noun with a verb is synchronically productive, the compound should be derived by a rule, in order to capture the correct generalizations and to keep the complexity in the lexicon from increasing. On the other hand, if the rule is not productive, or is what Cram (1972) calls 'semi-productive', the compounds should be listed in the lexicon, because there is no good way to allow the rule to apply to some cases and not to the others when in both cases the structural description of the rule is met. In addition, no generalization can be made.

(2.13) a. nsalîka-phûk  (watch+to carry)  'pocket watch'
mît-phûk  (knife+to carry)  'pocket knife'
pâîn-phûk  (gun+to carry)  'pistol'
b. *witthayû-phûk  (radio+to carry)  'pocket radio'
  *nânsîi-phûk  (book+to carry)  'pocket book'
c. witthayû-krapaw  (radio+pocket)  'pocket radio'

(2.14) a. sât-îian  (animal+to raise)  'domestic animal'
b. *maa-îian  (dog+to raise)  'domestic dog'
  *kay-îian  (chicken+to raise)  'domestic chicken'
c. maa-bâan  (dog+house)  'domestic dog'
  kây-bâan  (chicken+house)  'domestic chicken'

(2.15) a. *maay-khiip  (wood+to grip)  'chopsticks'
b. takiap  ---

(2.16) a. din-duut  (earth+to pull)  'quicksand'
b. *lek/mîk-dûut  (iron/ore+to pull)  'magnet'
c. mîk-îek  (mother+iron)  'magnet'

These examples indicate that the compound rule can be only semi-productive. In some cases the verbal component of a compound
has 'competition'. For example, phók 'to carry in a pocket' can be combined with 'watch', 'knife', and 'gun', but not with 'radio' and 'book', as illustrated in (2.13a-b). The second element in the compound 'pocket radio' is kràpaw 'pocket', as shown in (2.13c). (As for 'pocket book', I would like to predict that nānsā chàbàp kràpaw (book Classifier pocket) 'pocket edition book' or nānsā lêm lék lék (book Class. small small) 'small book' will be used, instead of nānsā-phók. ) likewise, the second element in 'domestic dog' and 'domestic chicken' is baan 'house' when it should have been lián 'to raise'.

Finally, semantically, it is very appropriate to call 'chopsticks' and 'magnet' *màay-khiph (wood+to grip or take up (with forceps, chopsticks, or pliers)) and *lèk/rx-x-dúut (iron/ore+to pull) respectively. But as seen in (2.15) and (2.16), these two compounds are unacceptable. Note that what makes it even more appropriate to call 'magnet' *lèk/rx-x-dúut is the existence of the compound din-dúut 'quicksand' (shown in (2.16a)). The objection to deriving the compounds by lexical rules is, therefore, that adequate criteria for the application of the rules do not seem possible.

What seems to be in favor of a lexical rule hypothesis is that there are verbs such as dòng 'to pickle', thòt 'to fry', and tôm 'to boil', etc., examples of which are given in (1.2), which can occur with nouns liberally. Therefore, it seems that the compound rules are productive here. However, there is one question that has to be answered: Is a noun plus one of these verbs a compound or, as Marchand (1966) calls it, a syntactic group?

It is certainly very difficult to distinguish between a compound and a syntactic group. For example, European and American grammarians and linguists have long been trying to establish adequate criteria to distinguish between the two. Some of them resort to stress, others to spelling, etc. If the situation is bad in English, it is worse in Thai. Stress is non-phonemic in Thai. To seek morphophonemic stress patterns for compounds is, therefore, out of the question. Spelling offers no help because words are written next to one another without spacing. A syntactic group of two words and a compound consisting of two words look exactly the same. However, I find the following guidelines helpful.

(i) If the second element in the form N+V can't occur with nouns in a liberal way, e.g. if it can occur with only one or a limited number of nouns, it is likely that it is functioning as a componental part of a compound. For example, the verb län 'to move', which can be a componental part of a compound but not a modifier, can occur with banday 'stair', as in (1.5), but not with tó 'table' and kàwi 'chair', as exemplified in (1.8).

(ii) If the form N+V is not understood as the sum of the two constituent elements, it must be a compound. For example, there is a conceptual difference between the compound (2.17a) and the non-compound (2.17b).
(2.17) a. phăa-lăn | (cloth + be yellow)
b. phăa lăn | (cloth + be yellow)

The former is conceptualized as something worn by a Buddhist monk, i.e. 'Buddhist robe'. The latter is understood as a piece of cloth with a certain shade of color (i.e. yellow), i.e. 'yellow cloth'. This conceptual distinction can be substantiated in many cases. For example, the predicate sîi sôm 'to be orange' constitutes a contra-
diction if its subject is the non-compound (2.17b), but not if its subject is the compound (2.17a), as illustrated by (2.18a and b).

(2.18) a. phăa-lăn phăn nîi sîi sôm
   'This yellow cloth (=Buddhist robe) has an orange color.'
b. *phăa lăn phăn nîi sîi sôm
   *'This yellow cloth has an orange color.'

Returning to the question whether a combination of a noun plus a verb, such as dGov 'to pickle' and thêt 'to fry', etc., is a compound, my contention is that it is not. The conclusion is based on the following observations:

(i) Syntactically, the second element (i.e. the verb) behaves like a modifying noun, e.g. mày 'wood' and lèk 'steel', etc., in that it can occur with any noun provided that no selectional re-
strictions are violated and/or the context is appropriate. Several of these verbs (the second elements) can occur in hypothetical forms, i.e. forms which are normally regarded as semantically weird, e.g. rGovthâaw tôm 'boiled shoes' (in the expression 'During the war some soldiers had to eat boiled shoes.'). Another example: Suppose that while a cook is frying something some toothpicks happen to fall into the pan; the cook may ask his friend,

(2.19) khun yàak kin mày cómfan thêt yàak
     you want eat toothpick fry Question
     'Would you like to eat some fried toothpicks?'

(ii) Conceptually, a noun plus one of these verbs is understood the same way as a noun plus a modifying noun or a so-called descriptive verb, in that the meaning of the form is the sum of the two constituent elements.

(iii) As mentioned earlier the verbs in question can occur liberally with a noun. If a noun plus one of these verbs is a com-
   pound, there must be a very large number of this kind of compound. However, the Thai Dictionary (1950) has not listed a single N+V in
   which the V is the verb in question. This certainly casts doubt on the assumption that this kind of N+V is a compound, because normal
   dictionaries usually list a large number of compounds.

(ii) Predictability
   It is desirable to derive the compound by a rule if the meaning of
   the compound is always predictable on the basis of its components. In other words, if it is the case that upon knowing the meaning of
the noun and the verb which make up the compound, the speaker will always know the meaning of the compound, then the compound should be derived by a rule, in order to capture the fact that the speaker knows that the compound is related to its components, i.e. the noun and the verb. The following examples show that the meaning is not always predictable.

(2.20) a. rót-thaña (vehicle+to plow) 'tractor (used for plowing/farming')
   b. rót-khên (vehicle+to push) 'pushcart'

(2.21) a. roon-camnam (building+to pawn) 'pawnshop'
   b. tūa-camnam (ticket+to pawn) 'receipt listing pawned items'
   c. khvñ-camnam (thing+to pawn) 'pawned items'

rót-thaña (vehicle+to plow) is, as described by the verbal component, 'a vehicle which is used for, or is capable of, plowing', but (normally) rót-khên (vehicle+to push) is not 'a vehicle which is used for, or is capable of, pushing'. How can, for example, the semantic rule be prevented from assigning a 'purpose' or 'capability' meaning to rót-khên ? Likewise, the second component in the compounds in (2.21a-c) is the same, i.e. camnam, but each of these compounds has a different type of meaning.

Since the compounds may have various types of meaning, unless we can come up with some criteria as to how a semantic rule assigns the correct meaning to the compounds, a lexical rule approach is undesirable.

In short, the evidence concerning the productivity and the predictability argue against deriving the compounds in question by lexical rules.

Based on the two guidelines for distinguishing between a compound and a syntactic group, and my own intuition, the items in (1.1a-g) and (1.3a) are classified as compounds, and (1.2) and (1.3b) are non-compounds. The status of (1.4a-b) have already been discussed earlier.

It should be pointed out that saying that the rule is not productive does not mean that it cannot be used to derive new forms. Since new words are sometimes formed and added to the language, it is possible that any inactive morphological rules may become generative again, at least temporarily. In other words, new compounds of the type N+V may be created on the basis of the already existing forms.

Before discussing the treatment of these compounds in a grammar of Thai, I would like to mention that there seem to be cases of expressions which are in the process of becoming compound
lexical items. One such form is หมูย่าง 'roast pork', which is coming to be used to refer to a special kind of roast pork, i.e. the kind that has crisp skin and some layers of fat. In such a case, the phrase หมูย่าง 'roast pork' and the compound หมู-ย่าง referring to this particular kind of roast pork are not as easily distinguished as ผ้า-ล้าน 'a cloth worn by a Buddhist monk' and ผ้า-ล้าน 'a yellow cloth'. However, intuitively, the relationship seems to be similar in the two cases. We might predict that the compound หมู-ย่าง will take on more idiosyncratic properties which will distinguish it from the phrase หมูย่าง. Other phrases which are candidates for compound lexical items are ข้าวผัด 'fried rice' ((1.2c)), ข้าวต้ม 'boiled, watery rice' ((1.2d)), and น้ำยอด 'roast chicken' ((1.2h)).

3. Lexical relation

How does a Thai speaker know that the compound of the type N+V is related to the independent noun and verb which make up the compound?

First of all the speaker has an internalized knowledge of the lexical structure. In our case, he knows the internal structure of the compound. For example, given the word ญวน-bin 'airplane', he knows that it may be broken down into two possible words, or in other words, that it consists of ญวน 'machine' and bin 'to fly'. This fact may be represented as (3.1), which is an abbreviated form of a lexical redundancy rule.

(3.1)

\[ N_x \rightarrow N_y \rightarrow V \]

Second, he knows whether the meaning of the compound is related, either directly or indirectly (i.e. metaphorically), to the meaning of the two componential parts. If it is, he assumes that the compound is related to the independent words which form the compound. If not, he will not associate the compound with the independent words. The items in (1.4b) are examples of the latter case. Those in (1.4a) are on the borderline. That is, they may be regarded as compounds by some speakers, but as polysyllabic words by others, depending on extra-linguistic facts (e.g. being told, or from an imagination, etc.), and/or the particular linguistic knowledge of each individual.

Using Jackendoff's (1974) framework, I propose that the meaning relation between, for example, ญวน-bin 'airplane', ญวน 'machine, engine', and bin 'to fly' may be exemplified by (3.2).

(3.2)

\[
\begin{align*}
+\text{N} \\
\text{VEHICLE WHICH HAS} \\
\text{W AND CAN Z} \\
\text{(or W WHICH CAN Z)} \\
\end{align*}
\quad \rightarrow \quad
\begin{align*}
+\text{N} \\
\text{W} \\
\text{Z} \\
\end{align*}
\]
Concerning lexical representation, every compound of the type discussed will have a full lexical entry. For example, khrān-bin 'airplane' will have the lexical entry (3.3):

\[
(3.3) \quad \begin{array}{l}
/khr\text{ān} + \text{bin/} \\
+N \\
\text{Syntactic information} \\
\text{Semantic information}
\end{array}
\]

Lexical redundancy rules, both morphological and semantic, will designate the information in the lexical entry which is predictable by the existence of the related lexical items as redundant. The predictable information designated by a lexical redundancy rule is not new information, but is the information the speaker already knows. In other words, not all the information in the lexical entry of, in this case, the compound is new information. Therefore (Jackendoff 1974, p.8):

In knowing two related lexical items one then knows less than when one knows two unrelated items of commensurate complexity.

It is claimed that this hypothesis of the compound of the type N+V, which lists the compound as a full entry, and which incorporates Jackendoff's framework of lexical redundancy rules, captures the speaker's competence concerning the relation between the compound and its components.

4. Conclusion

I have given examples of N+V compound nouns of various semantic types and argued that they should be listed in the lexicon, instead of being derived by transformational or lexical rules. Arguments against the transformational approach arise from (a) constraint on the transformational rules, (b) the criteria for the structural description of the transformational rules, (c) the deletion of meaningful items, and (d) how to arrive at the meaning of the compounds. A lexical rule approach is rejected because the rule which one may want to posit to derive the compounds can only be semi-productive. In addition, the meaning of the compounds is not always predictable from the components. I also pointed out that N+V are of two types, compound and non-compound. Two guidelines were provided for the distinction between a compound and a syntactic group of the type N+V. Furthermore, I hypothesized that some syntactic phrases may have been, or are being, lexicalized. Finally, I discussed how a Thai speaker knows that a N+V compound noun is related to the independent noun and verb which form the compound, and how this knowledge should be captured or represented in a linguistic theory.
NOTES

1. For work of other linguists and grammarians, see, e.g. Gedney (1947), Haas (1966), Noss (1964), and those referred to by Fasold (1969).

2. Semantically, *pəm-pən (wood+to beat) 'a switch used for whipping/beating' should have been a possible compound, because, at least previously, teachers and some parents in Thailand sometimes whipped their students/children with a switch. (The compound for 'a switch used for whipping/beating' is pəm-pən (wood+to be tapering).)

3. The correct forms of these compounds are:

(2.11)  
məpəm-pən (coconut+to be dry) 'copra'  
lən-nən (litchi+to be dry) 'dried litchi'  
kən-nən (shrimp+to be dry) 'dried shrimp'  
plən-nən (fish+to be dry) 'dried fish'

4. For an interesting presentation of lexical rules, see Thompson (1973), in which lexical rules deriving resultative verb compounds in Mandarin Chinese are discussed.


6. Although stress doesn't always work to distinguish between a compound and a syntactic group in English, it sometimes serves as a clue, e.g. blackboard vs. black board. In Thai, no such clue can be found in the stress.

7. Further discussion concerning the status of some of these forms is to follow.


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Morphology and the Rule Ordering Controversy

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The rule ordering controversy has been with us a long time. This is not at all surprising, since the notion of the "rule" is crucial to generative grammar, and consequently its behavior (and constraints on it) are of vital concern. The ordering of some rules with respect to each other is easily demonstrable with small sets of data, but more and more complex sets of data are needed to go beyond the immediately obvious orderings. This inevitably leads to controversy: how the ordering is established, or the constraints which are postulated for rules, depends critically on which complex sets of data are being explored, and how the linguist chooses to interpret the results. Each successive solution accounts for either a different or larger set of data than the last, no solution presumably ever the one final one. Working within such a framework, it becomes necessary to step back occasionally and examine the data base, the models, the claims, the adequacy and accuracy, the function, and the explanatory power of suggested proposals. The preferred solution is the one which can handle the greatest amount of data, capture the significant generalizations, make the correct claims about observable behavior and appropriate predictions with respect to new forms and future changes, account for speaker behavior so far as possible, all without violating the linguist's intuitive judgments as to how things ought to work. The controversy over rule ordering with the many approaches which have been suggested may be seen as steps in the evolution of a solution which works, explains, and satisfies.

Attempts to provide language-independent principles for guiding the possible behavior of rules with respect to each other have resulted in several proposals which have concentrated more on the function of the rule rather than its formal characteristics (Kiparsky, 1971; Kisseberth, 1973; Wilbur, 1973a; 1974). This in turn has led to a need for more explicit recognition of the fact that some rules which look like phonological rules are not phonological rules at all, but morphological rules which do not require phonological conditioning to trigger their application, but rather depend on syntactic and semantic factors to condition their application. Pre-generative grammarians considered morphology to be closely aligned with syntax. Except in those cases where a phonological alternation was restricted to a particular morphological
category, the phonological component and the morphological component were generally kept distinct. Until recently, generative phonologists have not explicitly dealt with morphology. The assumption was generally made that the morphological component was somehow responsible for the phonological representation to which phonological rules apply, and that, therefore the morphological component precedes the phonological component (Kiefer 1973).

Several models have evolved, some of which do not hold to the principle that the morphological component comes first. Chomsky (1965) does not have a separate morphological component at all. In Sound Pattern of English, Chomsky and Halle (1968) divide grammar into semantics, syntax, and phonology. They have "two concepts of surface structure: input to the phonological component and output of the syntactic component." Where the two do not coincide, it is assumed that readjustment rules in the phonological component would make the necessary corrections, but some of the functions now attributed to the morphological component were still retained in the syntactic component. Models that have evolved with explicit recognition of morphological rules have differed on the representation of these rules. Kiefer (1973), Wilbur (1973a; 1974) and Cearley (1974) hold to a model where the morphological rules are contained in separate unified component which precedes the phonological rules. Kiefer outlines an internal structure for such a component, with the derivational rules operating before the inflectional rules. On the other hand, Anderson (1974) argues that morphological, phonological, and phonetic rules may be freely intermixed, although the preferred ordering is morphological, phonological, phonetic. A compromise position is put forth in Aronoff (1974) where it is suggested that morphological rules may precede phonological rules, but that there are also "breaks" in the phonological component where morphological rules may intervene. Such "breaks" occur between phonological cycles, after all cycles, and possibly after all word-level rules. It is at these points, and these points only, that morphological rules may come after (or between) phonological rules.

Here then is the intersection of morphology and the rule ordering controversy. The question centers on the status to be afforded to morphological rules and the manner in which it is to be incorporated into the grammar. A direct comparison between the three positions is possible because all three attempt to deal with the same set
of data, namely the exceptional behavior of reduplicated forms. It will be argued in this paper that 1) the Anderson model fails to achieve descriptive adequacy with respect to the data from Luiseño, and 2) variable ordering approaches (Anderson and Aronoff) and rule rebuilding approaches (Aronoff) are missing a significant generalization about the behavior of reduplicated forms as well as denying the differences in function which exist between morphological and phonological rules. I will present the data first and then the suggested solutions with their relative problems and merits.

In Tagalog, the future tenses are formed by copying the C₁V₁ of the base of the verb stem. The present is distinguished from the future by the infix -um- as well as reduplication. The form without reduplication but with the infix is referred to as the modal form.

<table>
<thead>
<tr>
<th>Modal</th>
<th>Future</th>
<th>Present</th>
<th>Gloss</th>
</tr>
</thead>
<tbody>
<tr>
<td>s-um-úlat</td>
<td>su-súlat</td>
<td>s-um-u-súlat</td>
<td>write</td>
</tr>
<tr>
<td>um-íbig</td>
<td>i-íbig</td>
<td>um-i-íbig</td>
<td>wish⁵</td>
</tr>
</tbody>
</table>

Some verbs appear instead with prefixes which end in /ŋ/. This final nasal then undergoes a Prefix Assimilation rule, in which initial /b/, /p/, /t/, /d/, and /s/ of the verb stem are often assimilated to the corresponding homorganic nasal, while /k/ is always changed.

/p/  panálo? (paŋ-pálo?) but paŋ-parikit
/b/  pamílimit (paŋ-bílimit) but paŋ-bambo
/t/  panáli? (paŋ-táli?) but paŋ-takto
/d/  panaláŋin (paŋ-daláŋin) but paŋ-diliŋ
/s/  nanaríwa? (naŋ-saríwa?) but paŋ-sakay
/k/  naŋapása? (naŋ-kápasa?)

Those forms which do not undergo the complete Prefix Assimilation process do still undergo the regressive nasal assimilation, suggesting two rules, one for homorganic nasal assimilation and the other for subsequent consonant deletion. However, regressive nasal assimilation is not required for the forms on the right above, except that it usually occurs before dentals. The final nasal of the prefix is lost before nasal initial stems.

/d/ mandurúkit ~ maŋdudúkit (d→r / v___v)
/m/ namahála? (naŋ-mahála?)

In reduplicated forms, the assimilated nasal appears on
both the copy and the original, giving the appearance that Prefix Assimilation has applied where its environment has not been met.

/bigáy/ mamigáy mamimigáy namimigáy give
(maŋ-bigáy) (maŋ-bi-bigáy) (naŋ-bi-bigáy)
/sumpá/ manumpá manunumpá nanunumpá curse
(maŋ-sumpá) (maŋ-su-sumpá) (naŋ-su-sumpá)
/kúha/ maŋuña maŋuñúha naŋuñúha take
(maŋ-kúha) (maŋ-ku-kúha) (naŋ-ku-kúha)
/isdâ/ maŋisdâ maŋiñisdâ naŋiñisdâ fish
(maŋ-isdâ) (maŋ-i-isdâ) (naŋ-i-isdâ)
/libák/ manlibák manlibák nanlibák scoff
(maŋ-libák) (maŋ-li-libák) (naŋ-li-libák)

In Luiséno, adjectives derived from verbs by reduplication are exceptions to a general rule which changes /č/ to [ʃ] before a consonant or word boundary. The output of the Reduplication rule is C₁V₁C₂V₂+C₁V₁C₂V₂+i+č, where C₁V₁C₂V₂ is a verb root, i is a nominalizer, and č is an absolutive ending. There are several rules which are relevant.

1. Stress Placement -- under normal conditions, stress is root-initial, except if the root contains a long vowel, in which case, the long vowel gets the stress

2. Stress Retraction -- shifts the stress to the last vowel of the root when a particular class of verb augments is added to the stem. The ad hoc feature [+R] is used to mark this class.

3. Vowel Deletion -- when a two vowel sequence arises (only across morpheme boundaries) the second of the two vowels regularly deletes.

4. Vowel Syncope -- V→Ø / ̃C ______ CV

5. č to š -- /č/→[ʃ] / ̃

6. Vowel Raising -- /e/ and /o/ are raised to [i] and [u] respectively in unstressed position.

The order of the relevant rules as established by Munro and Benson (1973) is:

Reduplication Vowel Deletion
Stress Assignment Syncope
Stress Retraction č to š
As a result of these rules, the deverbal adjectives derived by reduplication usually have the shape C₁V₁ C₂V₂C₁C₂V₂s. However, when C₁ is /č/ and the derivation brings it into contact with C₂, the /č/ to [š] rule fails to apply.

Čara -- "to tear" Čaráčraš "torn" *Čarasraš
Čoka -- "to be limp" Čukáčkaš "limping" *Čukaškaš

Munro and Benson (1973) demonstrate that it is not the stem itself which is exceptional, that the ad hoc feature [+R] which triggers Stress Retraction cannot be used to characterize this class as exceptions, that the regular notion of exception feature does not suffice either, and that within a traditional framework of sequential ordering, rule ordering cannot be used.

Both Anderson and Aronoff attempt to deal explicitly with the data from these two languages. They do so however without considering these forms in context with data from other languages. In fact, languages which contain reduplicated forms which are exceptional in some respect to a phonological process can be grouped into two groups, those in which a particular phonological rule fails to apply where it should (like in Luseno) and those in which a rule appears to apply where it should not ("overapplication" as in Tagalog).6

1. Rule Failure
   Madurese (regressive nasal assimilation)
   Akan (palatalization)
   Luiseno
   Japanese (Intervocalic consonant nasalization)
   Palauan (consonant deletion)
   Dakota (Vowel combination)

2. Rule Overapplication
   Tagalog, Javanese, Sundanese, Madurese, Agta, Malay, Palauan (regressive nasal assimilation or replacement)
   Chumash (aspiration, glottalization)
   Dakota (palatalization)

From the list, it can be seen that the particular type of exceptional behavior is not a feature of certain phonological processes (i.e. palatalization fails in Akan but overapplies in Dakota), nor is it a feature of the language itself (Dakota, Madurese, Palauan
appear in both lists). In Squamish, from the same underlying representation, two alternating surface forms are derived, one which involves failure, the other overapplication.

I have argued elsewhere (Wilbur 1973a,b,c) that when reduplicated forms are exceptions to phonological rules (both failure and overapplication), the end result is always to keep the original and the copy as identical as possible. Presumably this is to keep phonological processes from obscuring morphological processes, although clearly some opacity is tolerated, as evidenced by reduplicated forms which undergo any phonological process which can apply to them. In comparing the three models, it is of interest to note how, if at all, this generalization can be incorporated into the grammar.

Anderson's proposal to handle the Tagalog data is simply to order Prefix Assimilation before Reduplication. This will combine the final nasal of the prefix with the initial consonant of the stem of the verb root into one nasal consonant which will then be copied by Reduplication.

/pan / + bilm / +Redup
Prefix Assim. pa m ilmit
Redup pa mi mi lmit

In Anderson's framework, a phonological rule may precede a morphological rule. Anderson does not attempt to account for those forms where Prefix Assimilation does not occur, but notes that "other languages of the family show the operation of the same rules in the opposite order" (Anderson 1974:4).

Aronoff starts by treating Prefix Assimilation as two rules, nasal assimilation and consonant deletion. He then gives the following as the regular order for Tagalog.

1. Prefixing (Morphological) /pan/, /man/, /nap/
   - [coronal] / anterior
3. Consonant Deletion [+cons] → Ø / [+nasal] +
4. Flap rule /d/ → [r] / V V
5. Reduplication (Morphological)
The sequence 234 defines a phonological cycle in the sense that if 2 is the first phonological rule of the grammar, and 5 is the only morphological rule which occurs in this particular break, then the next phonological rule to apply after 5 must be 2. No morphological rule may intervene between 2 and 3, or 3 and 4. The forms which do not undergo Prefix Assimilation (paŋbambo) are "assigned" the ordering 23415 so that Prefixing does not create the environment to which rules 2 and 3 must apply. Forms like mandurukit require Reduplication to feed the Flap rule, so therefore they must have the ordering 15234. Aronoff notes that forms with the ordering 15234 also have alternate forms with the ordering 23415.

There are some technical problems with the Aronoff solution. One is that forms which do not undergo Prefix Assimilation as a whole may be seen to be exceptions to Consonant Deletion (rule 3) but may nonetheless undergo Nasal Assimilation (rule 2). No ordering of the morphological rules 1 and 5 with respect to the phonological rules 2, 3, and 4 can account for what is clearly an internal phonological matter. The forms must be allowed to undergo rule 2 but not rule 3. In connection with this, the ordering assigned to forms like mandurukit 15234 also fails, since mandurukit undergoes 1, 5, 2, and 4 but not 3. Again, no ordering of the morphological rules with respect to the phonological rules as a group can handle these forms.

For the Luiseño data, Anderson suggests that the /č/ to [š] rule be ordered before Adjective Reduplication. In this way, /č/ to [š] would apply to the other forms before Reduplication creates the adjectives to which it does not apply. Anderson notes "there are important problems with the ordering relations among the rules discussed here in Luiseño, as Munro and Benson note. They can be resolved easily within the framework of local ordering (see Anderson 1969; 1974b)" (Anderson 1974a:11). One of the important problems is that Stress Assignment, Stress Retraction, and Syncope, which all apply regularly to the adjective forms derived by Adjective Reduplication, need Reduplication to apply first so that they can apply properly. The ordering

\[ ø \) to ø
Reduplication
Stress Assignment
Stress Retraction
Syncope
or a possible alternative

Reduplication
Stress Assignment
Stress Retraction
Č to Š
Syncope

can be demonstrated as inadequate by derivations in which the adjectives in question have several more suffixes. Syncope also applies when the stress on the preceding vowel is only secondary stress, so in some forms Syncope may apply twice. For example,

\[ \text{čukačkašmi} \quad \text{from} \quad \text{čoka i č um i} \]
\[ /\text{čoka i č um i}/ +\text{Redup} \]

Reduplication \text{čoka čoka i č um i}
Stress Assignment \text{čoka čoka i č um i}
Stress Retraction \text{čoká čoka i č um i}
Vowel Deletion \text{čoká čoka č um i}
Syncope \text{čoká ř ka č um i}
Č to Š \text{čoká ř ka ř m i}
Raising \text{čuká č ka š m i} \ast \text{čukačkašmi}

Aronoff attempts to avoid the ordering problem by building Syncope into the Reduplication rule. Aronoff’s (1974:163) revised rule is:

\[
\begin{array}{cccccc}
C & V & C & V & X^# \\
1 & 2 & 3 & 4 & 5
\end{array} \quad \Rightarrow \quad \begin{array}{cccccc}
1 & 2 & 3 & 4 & 1 & 3 & 4 & 5
\end{array}
\]

There is however already a productive rule of Syncope in Luiseño which the revised Adjective Reduplication rule will duplicate. Aronoff recognizes this, but says only that sometimes generalizations have to be given up. In Wilbur (1974), I argued that when a generalization must be given up, there must be good reason for it. If giving up a language-specific generalization allows us to maintain a more universal language-independent generalization, that might be considered a good reason. By giving up the generalization in Luiseño provided for by a single Syncope rule and a general Reduplication rule, we have not gained anything except a workable solution for one set of forms. And in fact it can be demonstrated that the solution is not all that workable. Only those adjectives which are derived from verb roots marked with the feature [+]R actually undergo Stress Retraction and Syncope. Other adjectives derived from verbs by the same Reduplication process and having similar meaning do not
undergo Syncope because the verb roots are not marked [+R] and do not trigger Stress Retraction which is necessary for Syncope to apply. So we find forms like pelavela from the verb pela- "dance" which may surface as either pelavela or pelavela, but not with Syncope. Thus Syncope is not itself an inherent part of the Adjective Reduplication rule as Aronoff suggests. The only argument for a separate Reduplication rule for the adjectives is that this particular group has a meaning of de-intensification, rather than intensification as other reduplicated forms do, but a separate reduplication rule could still not incorporate Syncope, so there would be no actual advantage to writing such a rule, as the problems inherent in Anderson's solution would still remain.

The solution suggested in Wilbur (1973a) for all languages which contain exceptional reduplicated forms argued as follows:

Reduplication is not a phonological rule, but rather a morphological rule. It seems appropriate for morphological rules to precede phonological rules, since they create the environment to which phonological rules apply. The following solution does not require morphological rules to precede all phonological rules, but it does provide a framework in which such an ordering can be easily accommodated. Separating morphological rules from phonological rules is one way in which the difference in function can be acknowledged. (Cearley (1974) lists some putative distinctions between morphological and phonological rules. Cearley claims that by definition morphological rules precede phonological rules.)

To the extent that it can be argued that a notion such as the preservation of underlying semantic, syntactic, or morphological information should be incorporated into the phonological component, it can also be argued that the appropriate way to accomplish this incorporation would be if all the cases in all the languages were handled similarly, by a single notational device. In Wilbur (1973a,b) it was argued that no one single device currently available is strong enough to handle all the data and also capture the generalizations about the behavior of reduplicated forms, with the exception of global rules. It was argued that global rules would 1) allow all morphological rules to precede all phonological rules and 2) handle all languages the same, and presumably thereby capture the generalizations that the end result of the exceptional behavior in all the languages is to preserve as much of
the identity of the original and its copy as possible, or in other words, to maintain the transparency of the reduplication process. At the same time, the derivational information required for a global rule is exactly that two units (segments, syllables, morphemes, whatever) are related by a copying rule, without having to build the copying rule into the phonological rule. It is, after all, crucial to all of the examples given, and others in Wilbur (1973a), that the unit (in these cases, segment) which is exceptionally affected is related to another such unit by a copying rule. Other units which are identical (i.e. the suffix -o in Luiseno) but which lack a correspondence with some other identical unit by a copying rule are not adversely affected. Such phonological coreference is as difficult to build in to the phonological component as specifying certain types of pronominal coreference in the syntactic component.

The formulation of the global rules needed can be made independently of any particular language. For rules which overapply, a global rule would look something like

\[ X \ (\text{and} \ X') \rightarrow Y \ i f \ AXB \]

where \( X' \) is defined as the "mate" of \( X \) by a copying rule (Reduplication, Vowel Copy, etc.) and interpreted as

\[ X \ \text{and its mate (if there is one) become} \ Y \ i f \ X \ \text{but not necessarily} \ X' \ i s \ i n \ the \ environment \ A____B. \]

Thus both parts undergo the rule although only one is actually in the proper environment. For rule failure, a global rule would be formulated as

\[ X \ (\text{and} \ X') \rightarrow Y \ i f \ X \ (\text{and} \ X') / A____B \]

which is interpreted as 1) \( X \) becomes \( Y \) if \( AXB \) and 2) \( X \) and \( X' \) become \( Y \) if both of them are in the environment \( A____B \), otherwise neither of them become \( Y \).

This solution essentially claims that it is a property of a particular rule in a particular language that it will behave exceptionally with respect to reduplicated forms, and that the exceptional behavior (failure or overapplication) is also a rule/language-specific feature, but that languages and rules which
have this feature have only two options in terms of behavior, and that the relevant function in each language is to treat the copy and the original in the same way. It may be seen as a drawback of both Anderson's and Aronoff's proposals that they require different analyses for each language and fail to incorporate any portion of the universality of this behavior.

Further Remarks on Global Rules

Global rules have been shunned by many phonologists as being entirely too powerful and essentially unconstrained. Part of the problem has been that global rules have not actually been formalized when they have been proposed. In this case, however, I think the use of global rules is well-justified -- they capture exactly and only the behavior which is observed, they formalize in a reasonably satisfying notation, they eliminate the barbarous abuses of other notational devices -- exception features, special reduplication boundaries, etc. (see Wilbur 1973a) -- to handle reduplicated forms, and they do not require overhauling the phonological analysis of a particular language at all. They also eliminate the need to make arbitrary decisions between two alternate possible analyses of the same data. For example, the failure of Regressive Nasal Assimilation in Madurese (/ban/ * bambaq) may be treated by either 1) ordering Reduplication after Regressive Nasal Assimilation or 2) having the output of the Reduplication rule marked by a special reduplication boundary across which Regressive Nasal Assimilation cannot apply. Both solutions capture the fact that only reduplicated forms do not undergo Regressive Nasal Assimilation. The difference in traditional terms is a matter of esthetics, especially if there is no evidence to indicate that the rule ordering solution will not work. The function of the rules and the similarity across languages are ignored by language-specific solutions of the type proposed by Anderson and Aronoff.

Remarks on Variable Ordering

So far I have constrained my comments on Anderson's and Aronoff's approach to the data at hand. There are some theoretical complaints which can be lodged against a theory which incorporates local or variable ordering. When the order in which rules apply is not fixed within a particular language, a derivation has no fixed beginning, middle, and end. In order to know how a parti-
cular word is derived, the surface form must be looked at to determine what the end result will be, from which the ordering of the relevant rules can be deduced. At first glance, this might not seem to be a serious problem -- the surface form, after all, is only actual occurring form that we have, it is the observable data. Within such a framework, however, there is no predictive power whatsoever. It is not possible to predict what new forms will look like, unless there is some morphological semantic conditioning for the different orderings. In addition, the traditional notion of a derivation is destroyed, since rules do not generate forms when they have to look ahead to figure out what order to apply in. That is, if the surface form must be used to determine the order of two rules, then it can no longer be said that the rules produce the form. The rules then become a means of capturing generalizations about the relationships between the surface and underlying forms, in which case there is no need to order them, since they do not apply, they only describe. It is not at all clear that this is a totally undesirable situation, however, in that attempts to derive all the forms of a language that can possibly be squeezed into derivational form seem to be misguided, and serious reconsideration of the purpose and function of rules seems to point to a need to overhaul much of the work currently being done within the generative framework at all (Wilbur and Menn, 1974).

Remarks on Functionalism and Formalism

Part of the overhaul has already begun in the form of an increasing number of functional solutions which have been proposed as explanations for several different phenomena (Kisseberth and Abasheikh 1974a,b,c,; O'Bryan 1974; Flora 1974). These solutions deal with trans-derivational processes (Nessly 1974) but no attempt at formalizing them is made. In the case of the data discussed here, the notion of a global rule captures derivational information; the fact that only two kinds of global rules are involved captures some cross-language information. The problem is that the global rule solution does not seem to be sufficient. It is not an appropriate characterization of transderivational information in the sense that it does not capture the idea that languages have strategies for preserving underlying information, for regularizing paradigms. In other words, explanations are being proposed for different phenomena which are not currently formalizable.
These explanations seem to require a place not just in the grammar, but also in the theory -- the difference being that global rules puts the identity tendency into the grammar, into the phonological component specifically. But the overall tendency, the so-called guiding principle behind why there should be global rules in the phonological component, is statable but not formalizable (yet). Functional solutions can be seen as extremely important since they provide explanations that are not apparent when simply working within the present set of notational devices like rule ordering, but if they are not formalized then I am afraid that the end result will be a descriptive set of statements which will have no significant impact on the way phonology is done. We are cycling back to statements of the kind that resembled the X becomes Y by analogy statements of pre-generative grammarians. The real advance comes when it is possible to formalize (or even define) analogy in such a way that it becomes useful in terms of analyzing language. Heading in this direction will, I believe, eliminate the rule ordering controversy.

Footnotes

1. See Wilbur (1973a) for a more detailed discussion of morphology in pregenerative and generative linguistics.

2. It is not necessary to hold to a sharp distinction between derivational and inflectional morphology in order to accept a separate morphological component.

3. This discussion will be confined to theories with sequential rule ordering. Clearly a theory with simultaneous rule ordering (Koutsoudas, Sanders, Noll) makes no distinction between morphological and phonological rules and, although interesting in terms of the rule ordering controversy itself, is basically irrelevant to the present question.

4. cf fn. 3

5. Vowel-initial stems are actually glottal-initial. So: ?-um-ibig, ?i?ìibig.


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Some Steps in the Acquisition of Factive and Implicative Sentences*
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There are many kinds of linguistic properties, both syntactic and semantic that play an important role in the interpretation of a sentence or a proper sub part of a sentence. Frequently such properties are not contained in the sentence or the relevant sub part. For example the linguistic properties of a matrix verb or predicate often play a major role in determining how one interprets the predicate complement of a sentence. Consider the sentence pairs (1) and (2) and the effect that the matrix verb has on our interpretation of the predicate complement 'John came for dinner.:'

1a. John managed to come for dinner.
   b. John didn't manage to come for dinner.

2a. John asked to come for dinner.
   b. John didn't ask to come for dinner.

When the main verb belongs to the class of implicative verbs (Karttunen 1970), as for example manage in sentence (1a) we know that if the sentence is in fact true, then John came for dinner. However, if the verb is one like ask, as in sentence (2a), we know nothing about whether John came or will come to dinner, though presumably he wanted to. When implicative verbs like manage are negated, as in (1b), we learn that John did not come to dinner, though apparently he wanted to. But when verbs like ask are negated as in (2b), all we know is that John didn't ask. Additional examples of such complex relations are found in the well known sentence pairs given in (3) and (4):

3a. Archie knew that John came for dinner.
   b. Archie didn't know that John came for dinner.

4a. Archie thought that John came for dinner.
   b. Archie didn't think that John came for dinner.

When the matrix verb belongs to the class of factive verbs (Kiparsky and Kiparsky 1970), as know in (3a), we understand that the speaker of the sentence has presupposed that John came to dinner. This is not true when the matrix verb is one like think, as in sentence (4a). Here the speaker has not committed himself to any such presupposition, but is merely reporting Archie's positive thought. When factive verbs are negated as in (3b), the presupposition still holds. That is John came for dinner. However, when neg raising verbs (Lakoff 1970) like think are negated, as in (4b), the sentence can be used to report Archie's negative thought. That is, on one reading Archie believed that John didn't come for dinner. This is merely part of the complex system associated with matrix verbs which a child must learn in order to understand sentences with predicate complements.
In light of this diversity we can make two hypotheses about how the child goes about acquiring the relevant semantics of this system.

Hypothesis I: the child treats each predicate as a unique case; his performance with one member of a predicate class is independent of his performance with another member of that class.

Hypothesis II: the child acquires systematic control over certain semantic properties on a class basis.

If Hypothesis II is correct, we would expect it to be reflected in the child's superior comprehension of one class of predicates over another.

METHOD

Participants

The experiment which I will report on here is part of a larger investigation which attempts to address these hypotheses. Four separate tasks were utilized in this investigation. This paper will present one of these tasks in which 23 kindergarten children (mean age 6.1 years), 20 first graders (mean age 7.0), and 21 second graders (mean age 8.1) participated; a total of 64 children. Thirty one of these children were female, thirty three were male. Approximately half the children in each grade were male, half were female. The median family income of the census tract served by the school, which is in the Los Angeles area, is $17,500.

Materials

The three implicative verbs used in this study are careful, let, and get. The three factive predicates are know, happy, and find out. These items were selected on the basis of their widespread use in first grade reading texts. The assumption being that if these items presented no problem for first grade children in texts, even kindergarten children would have control of them in simple sentences. The materials consisted of 36 sentences: 12 implicative sentences and 24 factive sentences. The 12 implicative sentences consisted of the three verbs with two different subordinate clauses, with each one of these six sentences having a positive and negative version. The 24 factive sentences are divided into 3 groups. One group of 12, I will call major factives, the second group of 6 I call minor factive sentences, and the final group of 6, filler factives. The major factive sentences consisted of the 3 factive predicates with two different subordinate clauses. Each of these 6 sentences had a positive and negative version. Thus there were 12 of them. The minor factive sentences were the 6 factive sentences with a positive matrix and a negative subordinate clause. The 6 filler factives consisted of the 3 factive predicates with a new set of predicate complements.

Design

The sentences were presented to the child in the following format: If Jane knows that Flap ate the cake, did Flap eat the
cake?' The child answers Yes or No. The implicative and factive sentences were presented in separate sessions to the child on different days. Half of the children had the implicative sentences first, half the factives first. Each child had 6 training items before each task. A random sequence of the implicative and factive sentences were drawn up. It and its reverse were presented to approximately the same number of children in each grade.

RESULTS

The comparison of children's responses to the implicative and factive sentences supports the hypothesis that children gain control over the relevant semantic properties on a systematic basis as determined by predicate class membership. We will consider the results from the implicative sentences first. Out of a total of 768 items, there are 95 errors, or 12% of the responses were incorrect. More important, 36% of the children made no errors at all and 78% of the children had two or fewer errors as we can see from Table I.

<table>
<thead>
<tr>
<th>No. of errors</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>% of children making no more than x errors</td>
<td>36</td>
<td>55</td>
<td>77</td>
<td>91</td>
<td>97</td>
<td>97</td>
<td>100</td>
</tr>
</tbody>
</table>

Cumulative Implicative Error Distribution

I will refer to children who make 2 or less errors as children who have control over the implicative relation that is being tested. There appears to be some difference in ability between grade as presented in Table II but this is not statistically significant ($x^2=2.6689$, df=2).

<table>
<thead>
<tr>
<th>K</th>
<th>1</th>
<th>2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Children with 0-2 errors</td>
<td>15</td>
<td>17</td>
</tr>
<tr>
<td>Children with 3+ errors</td>
<td>8</td>
<td>3</td>
</tr>
</tbody>
</table>

Grade by Error Group Comparison of Implicative Sentences

Turning to the results from the factive sentences, let us consider the major factive group first. Out of a total of 768 items there were 198 errors or 26% of the responses were incorrect. Sixteen percent of the children made no errors at all and 47% had two or fewer errors.

<table>
<thead>
<tr>
<th>No. of errors</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>% of children making no more than x errors</td>
<td>16</td>
<td>33</td>
<td>47</td>
<td>66</td>
<td>70</td>
<td>80</td>
<td>91</td>
<td>94</td>
<td>95</td>
<td>100</td>
</tr>
</tbody>
</table>

Cumulative Major Factive Error Distribution
As with implicatives I refer to children who make two or less errors as children who have control over the factive relation that is being tested. Graph 1 compares the children's performance on implicative and major factive sentences. We see that for any maximum error level, up to the implicative ceiling of 6, the implicatives include a greater percent of children than the factives.

It is clear that as a group children do much better on the implicative sentences than on the major factive sentences. This difference cannot be attributed to one particular factive verb being unusually difficult, since no pair of implicative sentences has more errors than any factive pair. The number of errors for each of the implicative pairs are 24, 32, and 39. The errors for each of the factive pairs are 61, 65, and 69. As for the factive competence distribution by grade, it appears that there is even less of a between grade difference for the factive sentences than with the implicative sentences ($x^2=0.4237$, df=2).

<table>
<thead>
<tr>
<th>Table IV</th>
<th>K</th>
<th>1</th>
<th>2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Children with 0-2 errors</td>
<td>12</td>
<td>9</td>
<td>9</td>
</tr>
<tr>
<td>Children with 3+ errors</td>
<td>11</td>
<td>11</td>
<td>12</td>
</tr>
</tbody>
</table>
Turning to the minor factive sentences, out of a total of 384 items there were 175 errors or 46% of the responses were incorrect. Only 11% of the children make no errors and only 22% make zero or one error. Recall that there are only 6 items in this group. Table V presents the cumulative percentage breakdown for minor factives.

<table>
<thead>
<tr>
<th>No. of errors</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>% of children making no more than x errors out of maximum possible</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6 errors</td>
<td>11</td>
<td>22</td>
<td>39</td>
<td>69</td>
<td>89</td>
<td>97</td>
<td>100</td>
</tr>
</tbody>
</table>

Cumulative Minor Factive Error Distribution

It is clear that the children's performance on minor factive sentences is much worse than either implicative or major factive sentences. Let us now consider the filler factive sentences. Out of a total of 384 items there were 57 errors or 15% of the responses were incorrect. Fifty percent of the children made no errors and 77% made zero or 1 error. Only eight children had three or more errors. Recall that there are only 6 items in this group. Table VI presents the cumulative percentage breakdown for filler factives.

<table>
<thead>
<tr>
<th>No. of errors</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>% of children making no more than x errors out of 6 possible</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>errors</td>
<td>50</td>
<td>77</td>
<td>87</td>
<td>97</td>
<td>100</td>
</tr>
</tbody>
</table>

Cumulative Filler Factive Distribution

As we can see from Table VII, these are relatively easy, as compared to the major and minor factives.

<table>
<thead>
<tr>
<th>% of incorrect responses</th>
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</thead>
<tbody>
<tr>
<td>Implicatives</td>
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<tr>
<td>Major Factives</td>
</tr>
<tr>
<td>Minor Factives</td>
</tr>
<tr>
<td>Filler Factives</td>
</tr>
</tbody>
</table>

Comparison of % Incorrect Responses for the Four Sentence Classes

DISCUSSION

The basic results of this study support the hypothesis that in regards to sentential negation, children acquire systematic knowledge about the semantic properties of classes of predicates. It also seems to be the case that children through grades K to 2 have better control of the semantics of implicative sentences.
than factive sentences. However, neither the data for implicative sentences nor factive sentences demonstrate a significant increase in comprehension on the part of the older children. This is curious since one would normally expect at least a rough correlation between increased proficiency and higher grade level.

I will attempt to account for this apparent absence of learning through a more detailed investigation of the implicative and factive data. First the implicative sentences. There is some difference between the kindergarten and higher grade children (Table II) but this difference is not statistically significant. This difference shows up again in the percent of correct responses for each grade: K=65%, 1st=85%, 2nd=81%. A more interesting difference between kindergarten and the other grades occurs within the distribution of errors for positive and negative sentences with children who make three or more errors.

<table>
<thead>
<tr>
<th>Grade</th>
<th>Positive sentence errors</th>
<th>Negative sentence errors</th>
</tr>
</thead>
<tbody>
<tr>
<td>K</td>
<td>7</td>
<td>19</td>
</tr>
<tr>
<td>1</td>
<td>6</td>
<td>8</td>
</tr>
<tr>
<td>N=15</td>
<td>8</td>
<td>7</td>
</tr>
</tbody>
</table>

Grade by implicative sentence error distribution (children making 3 or more errors).

Even though these differences do not reach statistical significance ($x^2=2.9848$, df=2) they suggest that older children who have difficulty with the implicative sentences, do so on a random basis, in contrast to the kindergarten children who have much more difficulty with the negated implicative sentences. An analysis by child of the kindergarten group reveals that 12 of the negated sentence errors are made by four children who have no errors for the positive sentences and three errors with the negated sentences. None of the first and second graders demonstrate such a pattern, i.e., zero errors on the positive sentences and a high number of errors with the negated sentences. This suggests that an early approach for responding to implicative sentences is to adopt a strong YES strategy under which the response to a positive sentence is 'yes' and the response to a negative sentence also tends to be 'yes'. If these four children are removed from this group, the remaining three kindergarten children make a total of seven errors with the positive sentences and seven errors with the negated sentences. Thus we find the random pattern of errors that characterize the first and second grade children. What I am suggesting is that children develop through the following sequences in their responses to implicative sentences: first, a strong YES strategy which may be somewhat influenced by the value of the sentence; second, partial control over the semantics of implicative sentences with a random YES/NO response strategy when they are not sure of the meaning of the sentence; third, response on the basis of the meaning of the sentence. Under this hypothesis
the absence of a strong between grade difference is explained by
the following factors 1) the first stage in this sequence takes
place before kindergarten for most children, 2) some children may
be in the second stage at least as late as second grade.

The across grade pattern for the major factive sentences is
even more curious than that of the implicative sentences. There
is a slight indication of increased difficulty in the higher
grades (Table IV). This is consistent with the percent of incor-
rect responses by grade: K=25%, 1st=25%, 2nd=27%. A partial
explanation for this phenomenon may lie in the positive/negative
sentence error distribution with children who make three or more
errors. Even though it is not statistically significant
($x^2=1.6497$, df=2) the higher grade children make more errors on
the negative sentences than the kindergarten children.

<table>
<thead>
<tr>
<th>Table IX</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive sentence errors</td>
</tr>
<tr>
<td>Grade</td>
</tr>
<tr>
<td>K</td>
</tr>
<tr>
<td>1</td>
</tr>
<tr>
<td>2</td>
</tr>
</tbody>
</table>

Grade by major factive sentence error distribution. N=34
(children who make 3 or more errors)

This difference is clearer in Table X where it is presented in
terms of percentage of error with positive and negative sentences.

<table>
<thead>
<tr>
<th>Table X</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent of positive sentence errors</td>
</tr>
<tr>
<td>Grade</td>
</tr>
<tr>
<td>K</td>
</tr>
<tr>
<td>1</td>
</tr>
<tr>
<td>2</td>
</tr>
</tbody>
</table>

In contrast to Table X, we would have expected that the negative
sentences would be more difficult for the kindergarten children
than for the second graders. An explanation for this may be that
some kindergarten children are utilizing a YES strategy which
tends to ignore the value of the sentence. This would result in
many correct responses to the negative factive sentences. The
second graders could be taking more into account the value of the
sentence and therefore giving a higher proportion of incorrect
responses to the negative sentences. These strategy differences
would make it appear like there is no learning among the older
children. What may be happening is that there are three stages
in acquiring control over the major factive sentences: first, a
YES strategy which is sometimes influenced by the value of the
sentence; second, a response strategy largely influenced by the
value of the sentence (similar to the implicative strategy); third, response in terms of the semantics of the sentence.
Even if the above set of hypotheses in regards to strategy differences for implicative and factive sentences are correct, they do not in themselves provide a basis for explaining why over half the second graders have difficulty with the major factive sentences. The answer to this problem may lie in the general semantics of implicative and factive sentences. The activity of an implicative root verb or predicate directly effects the activity expressed by the subordinate clause. The relation between the implicative verb and subordinate clause is not accidental or peripheral, but causal. On the other hand, in factive sentences there is no such necessary effect between the activity or state of the factive predicate and the activity expressed in the subordinate clause. These two activities are independent. The child must establish through his understanding of the semantics of factive predicates the necessary presuppositions that hold between the main predicate and subordinate clause. This would appear to be a more difficult relation to master than the causal-like relation that holds between implicative verbs and their subordinate clauses. It would then follow that the older children continue to have difficulty with major factive sentences in part because they have not yet mastered the general semantics of factive predicates.

A crucial hypothesis which evolves out of the above discussion is the claim that children gain control over implicative sentences before major factive sentences. Do the data support this hypothesis? That is, are the children who do well on the major factive sentences a subset of the children who do well on the implicative sentences? There are 49 children who make two or less errors on the implicative sentences. There are 30 children who make two or less errors on the major factives sentences. These two sets have 23 children in common. This means that there are 7 children who appear to understand the factive relation but not the implicative relation and 8 children who seem to understand neither. This distribution is presented in Table XI

<table>
<thead>
<tr>
<th>Table XI</th>
</tr>
</thead>
<tbody>
<tr>
<td>Factives</td>
</tr>
<tr>
<td>Implicatives</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>-</td>
</tr>
</tbody>
</table>

Four way assignment of children to apparent comprehension states

The differences between these frequencies do not reach significance \( (x^2=0.0045, df=1) \). However, before we reject the hypothesis that children who understand the factive relation necessarily understand the implicative relation, it is worth undertaking a detailed examination of the responses of some of the children who make up this 7 member cell. Recall that the filler factive sentences proved to be quite easy for a majority of the children and that only 8 out of the 64 children tested made 3 or
more errors on these sentences. Five of these 8 children are in this crucial 7 member cell. The unique feature of the filler factive sentences is that the correct response to these sentences is "no" even though they contain no negative element. One hypothesis which would account for the performance of these 5 children on the filler factive sentences is that when unsure about how to respond to a question they function under a strong YES bias. If this hypothesis is correct, it would also account for the apparent understanding that these children show in their responses to the major factive sentences. The correct response to both the positive and negative major factive sentences is "yes". Therefore, their apparent comprehension of the major factive sentences may only be a function of the strong YES strategy which they employ.

Further support for this hypothesis comes from the fact that 3 out of these 5 children are the kindergarteners discussed earlier who seem to be using a YES strategy with the implicative sentences. They made three errors with the negated implicative sentences, but had no errors with the positive sentences. Thus they may be using the same strategy for both implicative and factive sentences. The remaining two children from this group of five are one first grader and one second grader. The first grader's 12 responses to the major factive sentences are all correct; but his six responses to the minor factive sentences were incorrect. That is, he consistently answered "yes" to these 18 sentences. This suggests that he may be utilizing a strong "yes" response bias for factive sentences. The second graders responses are not quite as uniform. He has only five incorrect responses to the minor factive sentences and an incorrect positive sentence response and an incorrect negative sentence response for the major factive sentences. This response pattern is also consistent with a strong "yes" response bias. If it is the case that at least four or five of the seven children who seem to understand major factive sentences before implicative sentences do so only because they utilize a strong "yes" response strategy, then these children do not constitute counter examples to the crucial acquisition order hypothesis. The remaining two or three children who are not consistent with this hypothesis are not sufficient grounds for necessarily rejecting it. Sixty one out of the 64 children fit the order acquisition hypothesis. There may be a satisfactory explanation for the two or three exceptions.

An unexpected result of this study is the fact that nearly all children have a great deal of difficulty with factive sentences that contain a negative subordinate clause. This raises the question of whether this phenomenon extends to sentences utilizing other classes of matrix verbs and other types of complex sentences. The children's performance with the sentences which I have called filler factives indicates that it is not the case that they ignore some of the content in the complement clause. One explanation for the children who know both major factives and
implicatives is that they have the strategy of processing negation in the complement as if its scope includes the matrix verb. However it is not possible to explore this hypothesis here.

MATERIALS

Implicative Sentences
Mike let Flap go in the house. Did Flap go in the house?
Mike did not let Flap go in the house. Did Flap go in the house?
Mike let Flap get the ball. Did Flap get the ball?
Mike did not let Flap get the ball. Did Flap get the ball?
Flap got the cat to run away. Did the cat run away?
Flap did not get the cat to run away. Did the cat run away?
Jane got Flap to sit up. Did Flap sit up?
Jane did not get flap to sit up. Did Flap sit up?
Mike was careful to color inside the lines. Did Mike color outside the lines?
Mike was not careful to color inside the lines. Did Mike color outside the lines?
Mike was careful to put the cup on the saucer. Was the cup on the saucer?
Mike was not careful to put the cup in the saucer. Was the cup in the saucer?

Major Factive Sentences
Jane found out that Flap ate the cake. Did Flap eat the cake?
Jane found out that Flap took the shoe. Did Flap take the shoe?
Mike is happy that Jane broke the piggy bank. Is the piggy bank broken?
Mike is happy that Jane threw the ball. Did Jane throw the ball?
Jane knows that Mike took the doll. Did Mike take the doll?
Jane knows that Flap buried the ball. Is the ball buried?

Minor Factive Sentences
Jane found out that Flap did not eat the cake. Did Flap eat the cake?
Jane found out that Flap did not take the shoe. Did Flap take the shoe?
Mike is happy that Jane did not break the piggy bank. Is the piggy bank broken?
Mike is happy that Jane did not throw the ball. Did Jane throw the ball?
Jane knows that Mike did not take the doll. Did Mike take the doll?
Jane knows that Flap did not bury the ball. Is the ball buried?

Filler Factive Sentences
Jane found out that Flap ate her cookie. Does Jane have the cookie?
Jane found out that the bird fell out of its nest. Is the bird in its nest?
Mike is happy that Flap dropped the bone. Does Flap have the bone?
Mike is happy that Jane hit the ball. Did Jane miss the ball?
Mike knows that Jane closed the door. Is the door open?  
Jane knows that Mike lost his money. Does Mike have his money?  
**Practice Sentences**  
Mike gives a bone to Flap. Does Flap have the bone?  
Mike does not give the bone to Flap. Does Flap have the bone?  
Mike sees the turtle. Does Mike know where the turtle is?  
Mike did not see the turtle. Does Mike know where the turtle is?  
Jane saw Mike shut the window. Is the window open?  
Mike heard Flap bark at the cat. Did Flap bark?  

**FOOTNOTES**  
*I would like to express my gratitude to Dave Bessemer with whom I discussed in detail many aspects of the design of this study. His criticisms are responsible for any merit which the design may contain. Thanks also to Robert Berdan and Stanley Legum for their comments and criticism and a note of appreciation to the students and staff of the Mesa Robles Elementary School in Hacienda Heights, California for their courtesy and cooperation. The work upon which this publication is based was performed pursuant to Contract NE-C-oo-3-0064 with the National Institute of Education, Department of Health, Education and Welfare.*  

1In kindergarten there were 12 females, 11 males; in first grade 9 females, 11 males; in second grade 10 females, 11 males.  
2A partial list of these sentences is included at the end of the text.  
3This seems to be the semantic basis for the claim that factive predicates presuppose their complements, but implicative verbs entail their complements.  

**REFERENCES**  
Steinberg and Jakobovits (eds.), Cambridge University Press.  
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