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Proceedings of the

Ninth Annual Meeting of the

Berkeley Linguistics Society

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Ninth Annual Meeting of the
Berkeley Linguistics Society

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A METRICAL ANALYSIS OF ENGLISH PREFIXES
Dawn E. Bates
University of Washington

Despite the advances toward explanatory adequacy made by metrical theory in the area of stress rules, English prefixes have been given idiosyncratic analyses even in the most recent of theories. My proposal is that with the addition of a single rule, prefixes become vastly more regular under the assumptions of metrical theory. I will briefly review some of the most prominent accounts of prefixes to show the arbitrary status given prefixal stress. When presenting my ideas later in the paper, except where noted, I will accept the proposals of Hayes (1981). The main idea is that Hayes' theory already accounts for the behavior of prefixes in English.

Stated informally, the problem with prefixes is that some seem to carry stress of their own, some don't seem to affect stress placement, and some act both ways, depending on the situation. This perplexing behavior has compelled researchers to add special provisions to their theories of stress placement in order to allow them to account for prefixes within their frameworks. The treatment of prefixal stress in SPE led to a three-way distinction in the types of boundaries which separate prefixes from their stems. Two of these boundaries are still in use; Aronoff (1976), Siegel (1974) and Strauss (1979) follow SPE closely in stating that there is a class of affixes attached with the + boundary. These are precyclic affixes, have no stress conditions associated with them, and may attach to stems which are not words. Strauss argues that the second class of affixes, those traditionally (since SPE) attached with the # boundary, are actually elements of compounds and therefore the boundary distinction is unnecessary, since the speaker would already need the information that these affixes are stressed by the rules which govern compound stress. The third type of prefix proposed by SPE, attached with the = boundary, was shown by Siegel to be an arbitrary division of what were analyzeably all # affixes.

Hayes draws on Siegel and Strauss and stipulates that the Greek-derived prefixes form compounds with the stems they attach to. He cites Siegel's argument that these prefixes can undergo conjunction like compounds: "hyper- and hypothyroid" like "apple and cherry pie". However, this conjunction type is in no way general to either prefixes or compounds, as can be seen from the following:

*penta- and hexagonal	*lamb and pork chop
*hetero- and homonym	*deep-sea and skin diver
*photo- and electrograph	*water and snow ski
*tachy- and bradycardia	*motor and life boat
*bi- and unilateral	*lighthouse and zoo keeper

This data suggests that although extraction and conjunction of

particular items may be indicators of the word status of those
forms, they should not be considered infallible tests.
　　　More important, however, is the fact that the compound stress
analysis produces the wrong results. Since Hayes never objects
to or reformulates Liberman and Prince's (1977, henceforth LP)
Compound Stress Rule (CSR), we must conclude that he expects the
Greek-derived "compounds" (p. 187, Hayes) to be stressed by the
LP rule (p. 257 LP):

　　　CSR: In a configuration of the form [$_C$ A B $_C$]:
　　　　　　if C is a lexical category, B is strong if it branches.

Hayes cites LP's main examples:

law degree language requirement　　　law degree requirement changes
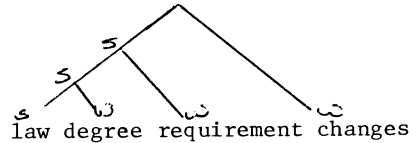
Notice that the Greek-derived prefixes listed by Hayes do not show
the effects of this rule:

*hypothyroid　*pseudo neo facist　*homosexual　*hyperextend

Compare the examples cited by Hayes in the first column below to
the prefix+stem forms in the second column, which by his
predictions should behave identically.

heteronym　　　　　　　　　*heterosexual

electrograph　　　　　　　　*electrobiology

laryngoscope　　　　　　　　*laryngopharynx

protolanguage　　　　　　　*proto Indo-European
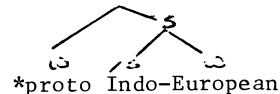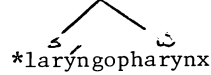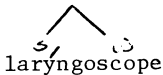
Even though many linguists accept that a certain prefix may
attach with both classes of boundaries, I contend that the
contrasts above have more to do with the monosyllabicity of
the stems chosen by Hayes than with the nature of the prefixes
themselves. Compare the examples in the first column above to
the following Greek-derived prefixes plus monomorphemic, but

polysyllabic stems:

    heterozýgote   pseudo-Tatamagóuchi

Main stress only falls on the prefix when the stem is mono-
syllabic. This is the only time when it appears that the CSR is
in effect.
    Having shown that prefixes are not best accounted for with
a compound stress analysis, we are again presented with the
problem of how to deal with them. Since metrical theory has
proved so successful in the area of stress, and Hayes' is a
well-articulated, well-motivated metrical analysis, it would be
optimal, in terms of general phonological theory, for prefixes
to fit into his system. However, it is clear that the ESR and
Strong Retraction, when applied to prefixed words as though they
were monomorphemic, produce incorrect results:
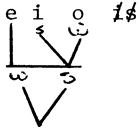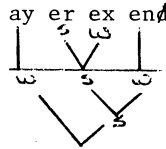
                                         Rime Projection
                                         C-ex, N-ex
                                       ESR, Strong Retraction
                                         Word Tree Construction
                                         Prestress Destressing

These trees give the incorrect *epíglottis and *hypérextend.
    I propose to account for the correct stressing of prefixed
words by a single rule coupled with the assumption that prefixes
cycle like stems do. Since this is a controversial proposal, I
will make some effort to justify it before formulating my rule.
    All of the previous analyses point to the central peculiarity
of prefixes: in very informal terms, their "almost but not quite
word" status. Even though we showed them to be less than
completely general, the arguments of Siegel, Strauss and Hayes
about the conjunction of prefixes give credence to the idea
that there is some "psychological integrity" given to prefixes
that is not ascribed, for example, to suffixes. To draw on our
earlier discussion of conjunction in prefixes, even though the
first column below might be strange, the second is unimaginable.

        ?pro- and anti-abortion   *happier and -est
        ?poly- and monosyllabic   *chlorine and -ide
        ?hyper- and hypothyroid   *phonemic and -etic
        ?over- and understated   *schizophrenic and -oid

This "almost word" status can be explained if we propose that
prefixes cycle like stems do. This explanation is considerably
less arbitrary than SPE's = distinction or Hayes' notion that
Greek prefixes equal nouns. In the other analyses, prefixes
are accidentally idiosyncratic, each one coming from the lexicon
with stress diacritics attached to it, unrelated to other
prefixes. My theory captures the generalization that in

isolation, polysyllabic prefixes are stressed according to the
ESR when the rule to be introduced shortly is recognized. Here
we have a principled set of exceptions being explained by one
rule, instead of with marking from the lexicon on individual forms.
In general phonological theory, a rule solution, if justified, is
to be preferred over lists of diacritics. It is obvious that a
rule solution is more plausible in terms of language acquisition;
the choice is between either postulating that the child formulates
one rule, or that she learns arbitrary lists of classified morphemes.
The productivity and generality of prefixation lends evidence to
the first choice.

All of these considerations would seem to argue for word
status for prefixes. But in fact prefixes are not treated, in
general, like words. Extraction from their stem-attached position
is impossible for most prefixes; consider the following
prefixes in isolation:

   *idio   *dys   *mis   *con   *ab   *hetero   *re

Even when some prefixes may be extracted, the circumstances must
be quite contrived: "Are you pro or anti Initiative 12?" "Pro."
To what, then, do we attribute the ambiguous nature of prefixes?
I propose that this status is the result of the cyclic behavior
of the following rule:

   Prefix Extrametricality:
      Rime $\longrightarrow$ +ex/_____] prefix

There is nothing extraordinary about this rule; it is formulated
in exactly the same way as Hayes' extrametricality rules. In fact,
Hayes notes that this rule is independently necessary to insure
the correct stressing of the Greek prefixes; they are stressed
like nouns (i.e. hétero, not *hetéro). I merely eliminate the
source language restriction, making the rule completely general.
The only peculiar thing about this rule is that I must say that
the final rime of the prefix stays +ex through the word formation
cycle, i.e., the rule is in effect one cycle longer than the other
extrametricality rules are. This could be what is strange about
prefixes, making them hard to extract. In trying to extract
them before word formation, they would be "in the middle of a
derivation", and it would be impossible to do this, just like it
is impossible for any form to appear without having all of the
obligatory processes of the phonology which are appropriate to
that environment already applied to it. From this we derive the
ambiguous nature of prefixes. On the one hand, they cycle,
giving them word-like status. This is countered, on the other
hand, by the fact that Prefix-ex is in effect through the word
formation cycle, making extraction of prefixes (extraction out of
the middle of a derivation) ungrammatical, giving the non-word
status of prefixes. I am assuming, I think reasonably, that
extraction normally occurs after all word formation rules.

Having justified the assumption that prefixes cycle, I turn to exemplification of my analysis.

Derivations that go through the word formation cycle show the interaction of Prefix-ex with other rules of cyclic stressing:
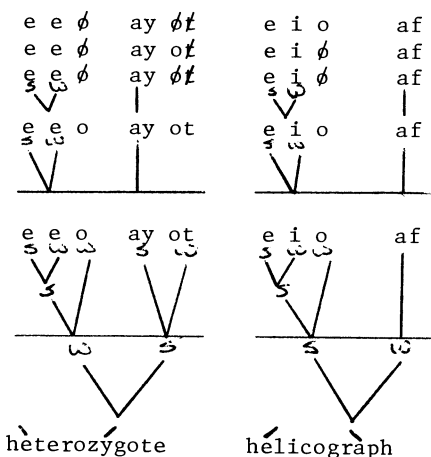
hetero	zygote	helico	graph	
				Pre Word Formation Cycle
e e ∅	ay ∅t	e i o	af	Rime Projection
e e ∅	ay ot	e i ∅	af	Prefix-ex, N-ex
e e ∅	ay ∅t	e i ∅	af	ESR
				W.F. Cycle
e e o	ay ot	e i o	af	Rime projection with previous structure attached
				C-ex, N-ex
				ESR (vacuous)
e e o	ay ot	e i o	af	Stray syllable Adjunction

Word Tree Construction

hèterozýgote   hèlicográph
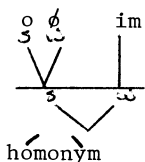
homo	nym	
		Pre W.F. Cycle
o o	im	Rime Projection
o ∅	im	Prefix-ex, C-ex
		ESR
		W.F. Cycle
o ∅	im	Rime Projection with previous structure attached
		N-ex
		Word Tree Construction
		Stray Syllable Adjunction
		Stray Foot Adjunction

hómonỳm

What is most important to notice at this point is that these derivations are completely in accord with Hayes' theory. The ESR applies to the prefix before word formation can take place, then on the cycle which includes prefix+stem, the ESR can erase structure created on earlier cycles (see the derivation of <u>helicography</u>, below), but Strong Retraction cannot erase structure. Furthermore, the ESR effects the minimal change possible on the pre-existing structure, in accordance with the theory of Kiparsky (1979).

The fact that the final rimes of prefixes stay extrametrical through the word formation cycle should be viewed as a principled oddity about that rule, and not an important exception to Hayes' Peripherality Condition (which states (p. 195) that extrametrical elements lose their extrametricality when not at the edge of the

domain of the stress rules). This is indicated by the fact that in all later cycles, the extrametrical syllables become -ex, as a result of the Condition. Compare the derivation of <u>helicograph</u>, above, to the later derivation of <u>helicography</u>:

Rime Projection with previous structure attached

N-ex
ESR (erases only what it has to)

Strong Retraction cannot apply
Stray Syllable Adjunction
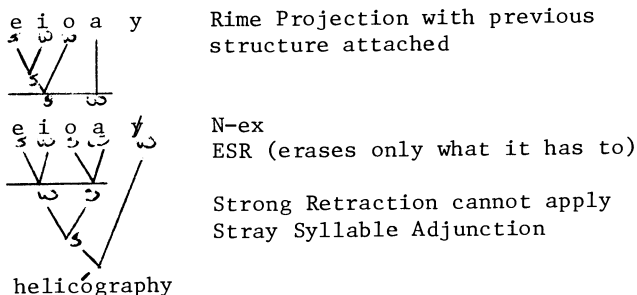
helicógraphy

Similarly, the derivation of <u>helicographic</u> shows the interaction of the principles being discussed. Hayes notes (p. 154) that <u>ic</u> is an exception to Adjective Extrametricality and is therefore metrical through this derivation.

Rime Projection with previous structure

C-ex
ESR effects minimal change

Word Tree Construction
The right node branches on the word tree now.

hèlicográphic
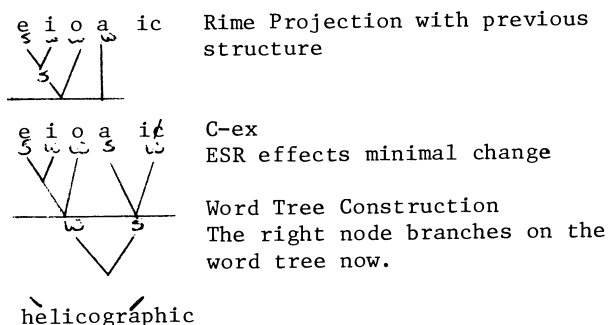
It is clear from the derivations presented so far that the rule of Prefix Extrametricality is well motivated for the analysis of polysyllabic prefixes. How this analysis applies to monosyllabic prefixes is less clear. One is at first tempted to state that Prefix-ex does not apply to monosyllabic prefixes just like Noun Extrametricality does not apply to monosyllabic nouns. However, the situation is more complicated than this. When attached to a monosyllabic stem to form a noun, a monosyllabic prefix will get main stress, showing clearly that it is not extrametrical at this time: $cómpress_N$, $\underline{prógram_N}$. The behavior of monosyllabic nouns and prefixes is apparently the result of a universal condition preventing a string that is entirely extrametrical, but how to formalize this condition is a complex matter. It seems that [+ex] rimes of nouns become [-ex] only if there is nothing else in the string at all. However, if a prefix is attached to a monosyllabic noun stem, Noun-ex is in effect. On the other hand, prefixes

stay extrametrical unless there is nothing else in the string
that can receive main stress. This can be seen in the fact that
monosyllabic prefixes are extrametrical when attached to verbal
and polysyllabic stems:

compress   befriend
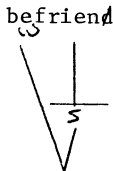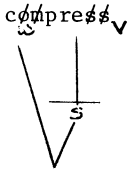
| | Word Formation Cycle
| | Prefix-ex, C-ex
| |
| | ESR
| |
| | Word Tree Formation
| | Stray Syllable Adjunction

If all monosyllabic prefixes were -ex, the ESR would have formed
a binary foot over <u>compress</u>, giving *cómpress. The ESR is prevented

from forming a binary foot in <u>immoral</u>, below, so it appears that
Prefix-ex is in effect here, too.
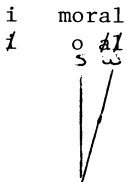
i   moral   <u>Pre W.F. Cycle</u>
i   o al   Rime Projection
                        C-ex
                        Adj-ex

                        ESR
                        Stray Syllable Adjunction
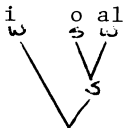
i   o al   <u>W.F. Cycle</u> with previous
                        structure

                        ESR, ADJ-ex apply vacuously

                        Stray Syllable Adjunction
immóral

The noun, verb pairs like $_{N,V}$[convict] $_{N,V}$[contrast] $_{N,V}$[pervert] and
$_{N,V}$[suspect] seem to confirm the observation that prefixes are
extrametrical except if everything else in the string is extra-
metrical; however, these results are confounded by the fact that
in the verbs, the prefixal syllable, even if it had been footed
earlier in the derivation, would undergo pre-Stress Destressing
because of the foot that the ESR would have made over the heavy
monosyllable in the verb. This is the reason I chose the verb
<u>compress</u> to illustrate my point; it is a member of such a noun,
verb pair, but has no extra consonant closing the stem syllable.
I will leave the two related generalizations unformalized, but
I will restate them here:

    Monosyllabic nouns are [-ex]only if they are the only
    elements in a string.

Monosyllabic prefixes are [-ex] only if everything else in the string is extrametrical.

Since this theory accounts for monosyllabic as well as polysyllabic prefixes, subject to possibly universal conditions on extrametricality, all of the traditional divisions of prefixes into different etymological or morphological classes can be eliminated.

There is one more traditional class of prefixes to account for. These are the "transparent" prefixes; under, over, out, in. Because they appear as separate words, these have been assumed to be introduced with the # boundary when they function as prefixes. These can be treated like any other prefixes under this theory, accounting regularly for the contrast between óverbite and overdevélopment. However, unlike with the other prefixes, the er in over and under never receives main stress in a fashion analogous to the o in homo: homógenized. It is possible that over and under are underlyingly monosyllabic, and the r syllabifies at surface structure for pronuciation purposes (SPE, Hayes and many others appeal to notions such as this in describing similar forms). Consideration of this type of phenomenon renders even these prefixes regular under the assumptions of this account.

In addition to complete generality and enhanced plausibility in terms of acquisition, my theory has the advantage of deriving as consequences two observations that must be stipulated by other researchers. The first is a condition on the word tree labeling process formulated in LP and cited by Hayes (p. 149):

In the configuration $N_1 N_2$, label N_2 as strong if...the tree dominates a verb and N_2 dominates a stem.

Since, in my analysis, the relevant syllables of the prefixes would always be extrametrical at the time this rule would apply, there is no need to make such a stipulation. The reader should notice that Hayes never reformulates this part of the LP rule. The second observation that follows automatically from my theory and not from other treatments is Kiparsky's comment that prefixes tend to form feet of their own. This theory derives this result from the fact that prefixes cycle.

In summary, I have justified the assumption that prefixes cycle and proposed a rule much like Hayes' other extrametricality rules which accounts for many of the apparently unrelated properties of different prefixes. I have shown this analysis to be empirically motivated and theoretically attractive. As a final note, I will admit, following Hayes, that some prefix plus stem combinations are stressed by the CSR; for example, if he is correct in reporting that he says bíochemistry and pséudointellectual, I need to allow for compound stressing of some prefixed forms. Though my dialect does not contain these stressings for these words, nevertheless, I do stress

prótolanguage and métalanguage on the first syllable, and this
shows CSR effects. It is not surprising that some forms with
prefixes might be lexicalized as compounds; they might start out
as compounds and become more regular through time, c.f. metaphýsics.
The overriding generalization is that my analysis describes the
typical, productive case.

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Social Organization and Referential Coherence in Classroom Discussions

James Collins
University of California/Berkeley

Introduction: In acquiring and mastering the skills of literacy young students must gain access to situations which allow them to learn and practice a variety of interpretive processes under the guidance of an adult. The following paper is concerned with how the language used by students and teachers influences the learning opportunities children encounter in formal classroom settings. More precisely, it is concerned with the way social organization interacts with question-answer strategies and topic-introducing strategies, to produce different learning environments for young students.

The way school children are organized into social groups and the linguistic means by which they communicate in those groups have an obvious bearing on the linguistic and cognitive skills they show as adults. This fact motivates the recurring concern in the United States with how social and linguistic factors influence achievement in a stratified educational system. In the past decade there has been a good deal of language-oriented research concerned with the effects of family background and teachers' expectations on educational achievement. Using a variety of coding schemes, discourse analysts and educational psychologists have found that teachers' elicitation strategies and students' response patterns vary with students' classification as high-ability or low-ability (Brophy & Good, 1974; Cherry-Wilkinson, 1978; Mosenthal and Jin-Ma, 1980). While rigorous in isolating linguistic and behavioral variables for analysis, this research suffers from two defects. First, it fails to situate ability-classification in an historical and institutional framework which would shed light on the observed patterns of behavior and achievement. Second, it concentrates on small discourse units (for example, question-answer pairs) and so fails to provide a processual account of the communication found in high-ranked and low-ranked classroom activities. What is needed is a precise account of the way the typical social organization of classrooms constrains the communicative options available to students and teachers, and does so in such a way as to produce the different patterns of linguistic behavior reported in the literature.

An important part of the organization of most early primary classrooms is the ability group. The ostensible justification for ability grouping is that it permits instruction to be tailored to student aptitude and that, being flexible, it can be adjusted to the given student population and to changes in that population. In practice it represents a very inflexible classifying procedure, permitting little movement into or out of groups, once

ability-status has been assigned. A recent review by Eder (1982) discusses the lack of fit between individual aptitude and ability grouping. She reports that variance in measurable aptitude accounts for less than one-fifth of the variance in ability group assignment. Variables such as socio-economic background are much more important in determining placement. Teachers and administrators are reported to believe strongly in the necessity and effectiveness of ability grouping, despite accumulating evidence to the contrary. In short, ability grouping represents a powerful a priori classification which restricts mobility, because groups are not added, deleted, or changed, despite initial or subsequent heterogeneity of student aptitude.

One result of such practices is that students who are perceived as less prepared or less attentive in early primary grades are grouped together as low-ability. But these decisions are made when children are five and six years old, an age when "ability" is very difficult to determine. The negative result is that once ranked, low-ability students are given different instruction than their high-ranked counterparts. The difference is due in part to teacher expectations, but also to the organization of activity. Micro-ethnographic studies of reading groups have shown that in low-ranked lessons there is more apparent inattention and distraction (both from inside and outside the groups), with the result that less time is spent actually reading. Thus, students most likely to have difficulty learning are assigned to groups where the social context is much less conducive to learning. There are clear and well-known effects on achievement.

The picture I have drawn so far is one in which an ideology regarding the classification of aptitude results in a social organization of classroom activity such that learning contexts differ radically for students classified in different ways. In particular, in low-ranked groups there seems to be less sustained attention to actual reading. It is well known that perception and learning require selective attention. Perhaps less well known is that attention in groups is a social accomplishment. Attention is signalled through verbal and non-verbal cues and maintained through sanctions. Additionally, the giving and receiving of sustained attention requires that participants in a communicative encounter be involved--that is, that they share some sense of the purpose of the communicative encounter, and further, that they pay one another sufficient heed for the exchange of relevant information (Goffman, 1972; Gumperz, 1982). However, as Goffman (1963) has pointed out, managing attention during an interaction is itself a form of inattention. It is a departure from the central purpose of an encounter, so as to create or maintain the necessary conditions for an encounter. Its relevance for the study of communication in educational settings is that time spent managing attention is time not spent teaching or learning.

Findings: Let us now look at the findings on all three
measures. There were two low-group and two high-group lessons,
from a second-grade and third-grade classroom, for a total of
eight lessons analyzed. This gave a corpus of approximately three
and a half hours of conversation and reading. In the tables,
numbers of instances, N, are listed in the left columns; mean
averages, expressed in events per minute or percentages, are in
the right columns. The rows key the groups, for class I (second
grade) and class II (third grade).

Comparing high with low groups for indices of inattention, in
the high lessons turn-violations occurred .45 times per minute,
while in the low lessons they occurred 1.44 times per minute--that
is, three times as frequently. Comparing groups for management
acts, in the high lessons there were .29 per minute, while in low
lessons there were .67 per minute--that is, they were twice as
frequent. These differences, summarized in table I, are
significant even without tests for inter-variable effects.

Table 1: Measures of Inattention.

	Turn-Violations		Management Acts	
	#/mins.	Per Min	#/mins.	Per Min
Class I				
High	16/39	.41	11/39	.28
Low	61/43	1.42	19/43	.44
Class II				
High	32/67	.48	20/67	.30
Low	70/48	1.46	42/48	.38
Total				
High	48/106	.45	31/106	.29
Low	131/91	1.44	61/91	.67

The findings on turn-violations indicate that low-ranked students
have less of an opportunity to engage in self-monitoring and
self-correction, and further, that some form of inattention or
disruption will occur twice as frequently during their reading.
In short, the act of reading is granted less dignity.

The measure of local uptake was whether questions
incorporated any part of an immediately preceding answer. This is
a simple formal measure of teacher-uptake, indicating where there
is at least referential continuity between a response and a
subsequent question. As is shown on table 2, in the high lessons
49% of all questions incorporated an immediately preceding answer,
while in the low lessons only 39% did so.

Table 2: Measures of Uptake.

	Uptake/No-Uptake	Percent Uptake
Class I		
High	51/99	52%
Low	55/127	42%
Class II		
High	120/249	48%
Low	66/177	37%
Total		
High	171/248	49%
Low	121/304	39%

Although I have not yet performed statistical tests for significance on this measure (the work is part of a larger dissertation-in-progress), the different amount of uptake seems important. Greater uptake of student responses is pedagogically beneficial,because it involves teacher-guided expansion of student contributions. It shows a fine-tuned adaptation of instruction to student performance.

Uptake included three related types of engagement: first, actual overt incorporation of some portion of a preceding utterance; second, elliptical incorporation, where the subsequent question unavoidably presupposed the preceding answer; third, paraphrase, where a word or phrase in a subsequent question provided a close restatement of a preceding answer. All three types are illustrated in examples one and two.

In example one the discussion concerns a passage in which street lights have been mentioned. I think it is clear that uptake, marked with a plus in parentheses, provokes an elaboration of the discussion and draws out the students' knowledge of the passage and the topic.

(1) Incorporation of answer into question (+).

```
1   T    Alright, what are they looking for?
2   C    Signals.
3   T    What signals? (+)
4   C1   Red.
5   C2   Red light and green.
6   C3   Three signals.
7   T    Alright, traffic signals.
8        Where do you find those? (+)
9   C    On the street.
10  T    Alright, where on the street? (+)
11  C1   Corners.
12  C2   Uh, corners.
13  T    The corner of the street...
14       At the corner of what kind of street? (+)
```

All of the uptakes involve overt incorporation, but the phrase

subsequent question overtly incorporates the paraphrase "Where do you find those?" (those=traffic signals=red light and green, etc.).

A second example is taken from a discussion which occurs after reading a passage from "Puss in Boots." The passage concerns a cat's plans to advance the social position of his master by fooling a king.

(2) Incorporation of answer into question (+).

```
1   T   What was the plan?
2   C   To make Jack_j play like he_j was the Duke of Willowonder.
3   T   Yes? (+)
4   C   To jump and then run into the river, but leave your_j clothes.
5   T   And pretend that he_j was doing what? (+)
```

The second question, line three, is simply "Yes?". It is an example of <u>elliptical</u> incorporation. Because it fully presupposes the preceding answer, it is like a "Why?" or "What else?" question in isolation; it merely signals "Go ahead." The student responds, in line four by reading a passage from the book, with slight modifications. The original text is "[I want you] to run and jump in the river, but leave your clothes under a stone." The teacher responds to the reading with a question, in line five, which incorporates an overt pronominal reference to the "your" of the preceding answer,and which requests a further specification of the protagonist's activities[3].

I would like to contrast the preceding with two cases of non-uptake. Examples three and four show an acknowledgement of answers, but no attempt to elaborate on the students' responses. Non-uptake is indicated by a minus in parentheses. In the examples the instructors seem to be covering a preset list of topics. In three the discussion concerns a passage, based on a variant of the Chicken Little story, in which a chicken and several animals are going to inform a king that the sky is falling.

(3) Non-incorporation of answer into question (-).

```
1    T    Why do you think they want to tell the king?
2    C    So they could get out of their city.
3    T    Okay, so maybe they can leave.
4         Do you think they think the king should know? (-)
5    C1   Yes.
6    C2   Cause he needta know too.
7    C3   Cause he the owner of the city.
8    T    Alright, so they think that he should know
9         that the sky is falling.
10        Alright, what does this fox tell them? (-)
```

Even though the students have given causal explanations in lines six "Cause he needta know too," and seven,"Cause he the owner of

the city," the teacher merely acknowledges the positive nature of the response, and then continues with a new topic.

In example four the discussion concerns a story about a rural Mexican family going to market.

(4) Non-incorporation of answer into question (-).

```
1   T   Okay, when we think of village, what do we think of?
2       Of village, we had that word once before.
3   C   A little town.
4   T   A small town, yes.
5       And, uh... the son's name is what? (-)
6   C   Raymon.
7   T   Raymon. And Raymon is going with his father and mother...
8       to the village market to sell their pottery.
9       Um, who had the reins? (-)
```

In this example we again see a perfunctory pattern of answers being acknowledged and then followed by questions which introduce different topics. In sequence the students are asked to define "village", the name of "the son" and finally, in line nine, "who had the reins?" (of the burro in the story).

As was noted above, and as should be clear from the examples, uptake is pedagogically beneficial. It draws students into whatever instructional dialogue the teacher is attempting to achieve, by use of their own answers. Some possible reasons why it occurs more frequently in high-ranked lessons are discussed below, but first let us turn to the last measure.

The analysis of referential <u>coherence</u> revealed that the typical way of establishing and maintaining question-topics was for the topic to be introduced with a lexical NP or name and subsequently referred to with a third person or zero-anaphor, during a series of question-answer-evaluation cycles. Topic-shifts were accomplished by introducing the new topic with a lexical NP or name.

This is a common pattern in English and can be seen in example five. The question-topic is introduced in line one, "Jenny," and thereafter referred to with a pronoun "she." When the question-topic is shifted in line nine then a lexical NP, "Her mother," is used.

(5) Typical NP distribution (NP_jlex - NP_jpro.../NP_ilex...)

```
1   T    Alright, what is Jenny_j doing?
2   C1   Runnin' out there like a stupid fool.
3   CC   Runnin' out in the crosswalk.
4   T    Alright, running out into the street.
5        Is she_j supposed to?
6   CC   No!
7   C2   She_j s'posed to wait for her mother_i.
8   T    She_j's supposed to wait.
9        Her mother_i is where?
```

```
10  C3  Over across the street.
11  T   Alright, on the other side of the street.
12      And what is she_i signalling to Jenny_j?
```

There is a notable departure from this pattern, however. Sometimes shifts in topic-perspective occur which are not accomplished with lexical NPs, but rather with potentially ambiguous pronouns. Such shifts are interesting for several reasons. For one, they throw into relief the kinds of contextual and semantic knowledge participants use to infer antecedents in a discourse. For another, conflicting inferences occur far more frequently in the low-ranked lessons, forming a complement to the heightened inattention and lessened uptake discussed above. Finally, in recounting and commenting upon the events contained in a text, students are practicing a variety of inferential strategies for determining antecedents in a discourse. It seems plausible that these are similar in kind to the strategies which skilled readers employ in comprehending text (Cf. also Gibson & Levin, 1975, and Webber, 1980).

The use of text knowledge, prior discourse, and semantic information to infer antecedents can be seen in examples six and seven. As an aid to the reader, in these examples the text which the discussion concerns is typed above the conversational excerpt.

(6) Successful resolution of ambiguous pronominal reference.

Text: The city mouse wanted to see the country mouse. "I know what I can do," he said. "I'm going to the country. I will surprise country mouse."

```
1   T   Alright, so what does the city mouse_i want to do?
2   CC  See the--go to the country.
3   T   Go to the country and visit who?
4   CC  The country--the country mouse_j.
5   T   The country mouse_j.
6       Alright, does the country mouse_j expect him_i?
7   CC  No.
8   T   No he_j doesn't, he_i's going to what?
9   CC  Surprise.
10  T   Surprise him_j, he_i's gonna surprise him_j.
```

In line eight there is a sudden shift in the reference of the pronouns: the first "he" refers to the country mouse, who does not expect the visitor; the second "he" refers to the city mouse, who is going to surprise his rural counterpart. In following the sudden, unmarked shift concerning who is in focus as the agent, the students had to apply their knowledge of the passage which had been read, as well as their knowledge of what aspects of the passage had been discussed thus far, in order to predict--as they successfully do--the likely antecedent of "he" in "He's going to what?"

A more elaborate example can be seen in seven. This is also taken from the lesson of "Puss in Boots." The passage has been read and partially discussed, a fragment is reread by a student, then the conversational excerpt occurs.

(7) Successful resolution of ambiguous pronominal reference.

Text: Two men were standing guard. The cat called to them. "The King is coming along the road. He will ask whose field this is. If you don't say it belongs to the Duke of Willowonder, I'll come back and chop you to bits."

```
1    T    So that's why he_i's gonna chop their heads off...
2         He_i threatened them_g, didn't he_i?
3    C1   Uh huh, so they_g would tell the king_k that
4         that's the Duke of Willowonder's_j.
5    T    Why'd he_g tell him_k that?
6    C1   So the King_k would know--So the King_k would realize that he_j
7         is the Duke of Willowonder_j.
8    T    That he_j had something, didn't he_j ?
```

In the question in line five, the teacher changes grammatical number from plural ("they") to singular ("he"). In order to infer that a guard is the antecedent of "he" in five, the students must have a clear idea of who was going to be speaking to whom and in what order. For example, they must infer that "why'd" equals "why would" rather than "why did," in order to get the correct sequencing of speech and speaker (cat tells guard, guard will tell king). The "he" of line six is the cat's master, Jack, who has not been mentioned in some time. Yet when Jack is re-introduced anaphorically, neither students nor teacher have trouble interpreting the reference of the pronoun (he=Duke of Willowonder=Jack), or understanding that Jack the Duke would be the one who "had something" in line eight.

But this sort of reference-maintenance occasionally breaks down. That is, situations occur in which contradictory interpretations are assigned to a pronoun, with the result that two or more topics are simultaneously on the floor. These situations occur much more frequently during low-ranked reading lessons. One result is that discussion time is given over to "repairing" reference, that is, to establishing just what is being talked about at a particular moment in the lesson. Examples eight and nine provide illustrations.

Eight is taken from the same lesson as example six. The discussion concerns the story of the city and country mouse, but occurs later, after the country mouse has invited the city visitor to dine with him.

(8) Referential misfire.

Text: City mouse wanted to eat. But he did not like the food.

eat with me?" "I can't," said city mouse. "I don't like
this food. Why do you eat it, country mouse?"

1	T	Who's not eating the corn?
2	CC	The city mouse$_i$!
3	T	Why not?
4	C1	He$_i$ don't like it.
5	CC	He$_i$ don't like it.
6	T	He$_i$ doesn't <u>like</u> it.
7		But does the country mouse$_j$ like it?
8	CC	Yeh!
9	T	Yes he$_j$ does--
10	C1	-He$_i$ like eat <u>insects</u>.
11	T	--he$_j$ likes that--
12	C1	-He$_i$ like eat insects.
13	T	Who?
14	C1	Uh...
15	T	Which one?
16	C1	The gray one$_i$.
17	T	Well, which one is he$_i$, the gray one$_i$?

Lines four through thirteen are most important. A student, C1,
refers to the city mouse as "he" in line four "He don't like it."
The teacher then talks about the country mouse in lines seven and
nine: "But does the country mouse like it? ... Yes he does."
In lines ten through thirteen we see the temporary breakdown which
ensues as the teacher tries to establish just which mouse is being
referred to by "he."

A second example can be seen in nine. The excerpt is taken
from a lesson dealing with the behavior of a young chameleon who
refuses to change his color, with the result that he is spotted
and nearly eaten by a passing crow. Prior to the conversational
excerpt a student reads a passage in which the crow has spotted
the chameleon and flown closer; whereupon, the chameleon, sensing
danger, darts under a leaf. There is a lengthy interlude, to
discuss a violated spelling rule, then the exchange in nine
occurs.

(9) Referential misfire.

Text: Christopher felt a shadow blocking out the sun.
And opened one bright eye and knew the time had come
to run! Beneath the leaf he darted, as fast as he
could go. And then he clung there hoping that he had
lost the crow. "I'm sure I spotted something," croaked
the crow from quite nearby. "It looked like a lizard
from up there in the sky."

1	T	Okay... Alright, after he$_i$'d run or he$_i$'d darted under
2		the leaf, he$_{i/j}$ started thinking about it, didn't he$_{i/j}$?
3	CC	Yeh.
4	T	What'd he$_{i/j}$ think?

```
5    CC   "I'_{i/j}m--"
6         "I_{i?}'m sorry."
7    C1   "I_j spotted something."
8    C2   "I_j'm sure I_j -spotted something."
9    T                    "-I_j spotted something."  Who said that?
```

In this passage, one possible topic-perspective, that presumably intended by the teacher, assumes coreference between the "he" of "dart" and the "he" of "think" in lines one and two. An alternate perspective assumes disjunctive reference, giving an interpretation something like the the following: "when he, the chameleon, darted under the leaf, what did he, the crow, think?" This alternative interpretation is made by some of the students, as is indicated by the speech which they quote. "I spotted something" is the speech of the crow.

Although such referential "misfires" are not pervasive, they do occur regularly in low-ranked lessons, usually one or two per lesson. When they occur, question-topics are typically abandoned, or returned to only after lengthy discussion aimed at clarifying who said or did what to whom. Although for this discussion I have selected examples which provoked probing by the teacher, answers which were referentially ambiguous were also likely to be ignored, whether correct or incorrect. Situations where contradictory inferences are drawn probably contribute to increased inattention and lessened uptake. But such reasoning should not proceed in an overly deterministic fashion.

<u>Discussion</u>: In thinking about causes for the above findings, the various measures should not be seen as simple cause and effect, but rather as different sides of a mutually reinforcing cycle. Lessened attention results in lessened uptake and referential coherence; these, in turn, lead to further inattention. All three measures are aspects of a synergistic process resulting in reduced communicative involvement.

The components of inattention are complex in themselves. "Calling out" may start as help--students providing the correct answer when someone hesitates or miscues. But it very quickly becomes a divisive form of rivalry, annoying each reader in turn, yet tolerated by the instructor. It prevents self-monitoring and self-correction and reinforces a fragmented oral reading style. By lessening the respect paid a turn at reading, and the intelligibility of reading and discussion during and after a turn, calling out lessens involvement in reading. This, in turn, provokes management acts, as teachers attempt to maintain attention by reprimanding inattention. The reprimands further disrupt the process of reading and discussion, contributing, albeit in a limited way, to lessened uptake.

If we ask why differential uptake occurs, several potential answers suggest themselves. One is that inattention directly reduces the possibility of uptake. If we focus on the difference between high-group and low-group management acts (N=31), we find that eleven of those thirty-one acts occurred during a question turn, when students had been asked a question, and had responded

with an answer which was correct, but provoked no elaboration. Assuming that a distraction during a question-answer-evaluation cycle will lessen the likelihood of uptake, then the intersection of the two categories of events provides an explanation for part (ca. 10%) of the lessened uptake in the low-ranked lessons. But this is only a partial explanation.

An alternative hypothesis is that differential uptake simply reflects either (a) the teacher's expectations about students and the pedagogical agenda thus employed, or (b) the inappropriateness of the students' responses. Both sides of this hypothesis are difficult to prove. If subsequent analysis were able to isolate a number of identical question and response patterns, which showed uptake with one group of students and lack of uptake with another, that would provide direct evidence of expectation effects, or a site for contrastive analysis of the appropriateness of student responses. It would leave still unanswered the question of what it is about the general communicative environment which maintains and reinforces the teachers' apparent predilections for differing uptake.

In the case of decreased referential coherence, the question is how contradictory, yet equally plausible interpretations of topic get established in the rapid back and forth of classroom discussion. It may be that low-ranked readers do not have as coherent a model of the text as their high-ranked counterparts, with the result that their anaphoric inferences are less constrained, more open-ended. But that is impossible to assess in this study. It may be that the oral discourse style of low-ranked readers places an additional interpretive burden on the teacher. These readers tend to use a fragment of reported speech when introducing a new topic, as if compensating for the shift in reference by clearly indexing the speech of the character (for example, in nine). This way of introducing characters may require additional inferential work by the teacher, to determine what the intended topic is. Last, and perhaps most important, in many of the referential misfires timing is important--for example, in nine, where discussion of a spelling rule had intruded between the reading of the text and discussion of the text. In several other cases, some sort of distraction occurred just prior to the misfire.

Conclusion: Whatever the ultimate causes of increased inattention and lessened uptake and coherence, it seems clear that the different ability groups encounter strikingly different contexts for learning. It is a sad irony that students perceived as low aptitude are grouped together so that--as a group--they encounter lessened communicative involvement in the task of reading. This practice--justified by a false and inflexible ideology regarding "ability"--merely reinforces existing inequalities of access to learning opportunities.

It should be noted, however, that the foregoing discussion has attempted to do more than document the effect of teacher expectations. Instead, I have discussed a few exploratory measures for assessing the overall coherence of communication

within learning micro-environments. The measures discussed do not assume taxonomic models of classroom discourse, but rather attempt to get at the preconditions for attentive, sustained involvement in the collaborative process of learning to read. These preconditions seem to include an orderly way of assigning turns, the right to a turn and to time for self-monitoring, a certain amount of teacher responsiveness to student contributions, and participants' ability to establish and maintain a shared sense of topic. Taken together, these preconditions indicate the complexity of the communicative events involved in acquiring the skills of literacy. Properly analyzed, they can enrich our understanding of the social, linguistic, and cognitive variables which play a role in those communicative events. Last, with regard to referential coherence, I hope to have suggested an area where there is a particularly interesting overlap between the strategies for interpreting talk and those for interpreting text.

Notes:

1. See Collins (1982) for additional description of the larger study and the individual groups.
2. The procedure is adapted from Marslen-Wilson, et. al. (1981), who use a similar distributional analysis to study reference maintenance in narrative.

3. Although it may seem more plausible that the "he" of line five refers to "Jack" or "he" in line two, uptake is restricted, for this analysis, to immediately adjacent speaker-turns; therefore, the uptake is assessed in terms of the coreference which exists between "your(=Jack)" in four and "he(=Jack)" in five.

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Quantifier Scope Ambiguity and Definiteness

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The sentence

(1) Every man loves some woman.

is well-known to be ambiguous. The ambiguity is traditionally ana-
lyzed as being a scope ambiguity, based on the order of the exis-
tential and universal quantifiers in the first-order predicate
calculus representation of the sentence:

(2a) ($\forall$: man x)($\exists$: woman y)(love x y)
(2b) ($\exists$: woman y)($\forall$: man x)(love x y)

Equally well-known is that there is a preference for the reading
in which the order of the surface quantifiers is the same as the
order of the underlying ones; the preferred reading of (1) is
(2a), while the preferred reading of (3) is (2b):

(3) Some woman is loved by every man.

While there is no doubt that the representations in logical
form in (2) correctly describe the facts about the ambiguity of
(1), it is in fact rather amazing from the linguistic point of
view, since the ambiguity appears to be solely a property of the
logical form, not the linguistic form. The treatment of quanti-
fiers as operators of some sort over an entire sentence is not
reflected in the surface syntax of English or any other natural
language to my knowledge. Instead, quantifiers are attached to
NP's, and in a few special instances to verbs. The scope ambiguity
of (2) is a feature of the syntax of the logical form that sen-
tences like (1) are usually translated into, not the syntax of the
natural language form: no surface syntactic scope ambiguity is
involved. Nevertheless, the ambiguity of (1) is quite clear. In
this sense, the representation of logical form fails to capture
why the surface form is ambiguous. In the surface-oriented analy-
ses that are current, various ad hoc devices such as Cooper
storage must be used in order to retain the logical-syntax scope
ambiguity explanation.
    If we disregard the representation of the ambiguity in logical
form for the time being and consider the usual linguistic solu-
tions for ambiguity, we have two options: ambiguity in surface
structure and polysemy in particular lexical items in the sentence.
The former is clearly not the case in ambiguities of quantifier
scope. However, the latter possibility offers an explanation for
the ambiguities of quantifier scope, the preference of one reading
over the other, and also certain other aspects of the behavior of

English quantifiers. Specifically, there is an ambiguity in the existential quantifiers some and a between a 'specific' and a 'nonspecific' reading which interacts with a distributive reading of the universal quantifier to yield the usual interpretations of sentences supposedly containing quantifier scope ambiguities.

The "wide existential scope" reading of (1), that is, (2b), is usually paraphrased as: "There is a woman such that every man loves her". In other words, the object denoted by the existentially quantified NP is definite, although not determinately identified in the discourse (its identity may be known to the speaker). This is the less preferred reading for (1), supposedly because of the surface quantifier order. The other reading is the so-called "narrow existential scope" reading, which is usually paraphrased as follows: "For every man there is some woman such that that man loves her". In other words, although the NP is singular for every man, there is not necessarily a single woman which satisfies the condition 'every man loves x'. However, the preference for one reading over the other, or even the requirement of one reading instead of the other, can be manipulated, and the ability to manipulate the preferences can be explained only by the fact that certain environments are more suitable for a specific or a nonspecific reading of the existential quantifier.

One can make the "wide scope" reading more and more preferred by modifying the existential NP with more and more definite descriptions[1]:

(4a) Every man in this room loves a woman in my phonology class.
(4b) Every man in this room loves a woman I met last night.
(4c) Every man in this room loves a woman I met at the Opera last night during intermission.

In (4), both readings are possible, as their representation in logical form would indicate; but (4b) and expecially (4c) are heavily weighted toward the wide scope reading. This fact cannot be predicted by the scope ambiguity analysis. However, if one treats a as ambiguous between a specific and a nonspecific reading, it is clear that the specific reading will be preferred by more definite NP's following the quantifier.

Another example of the preference for a specific reading is when the order of the quantifiers is reversed. This could be expected: normally, old information precedes new information in an utterance, and old information tends to be more definite by virtue of its being old information. In particular, subjects tend to be topics of the sentence and hence usually more definite:

(5) Some woman drank every bottle of liquor in the house last night.

This is true even when the subject is semantically a patient, as in passive sentences such as (3). It is in fact extremely difficult to get any reading other than the specific one in (3) and (5) (see

footnote 3).

   The possibility of a 'nonspecific' (i.e. "narrow scope") read-
ing of (1) depends on the ability to interpret the subject as
being applied distributively to the predicate, as well as on the
ability to interpret the existentially quantified NP indefinitely.
The English universal quantifier <u>all</u>, unlike <u>every</u> and <u>each</u>, can
also be interpreted as a collective application of the subject to
the predicate. Thus, its use tends to bias preferential readings
to the specific ("wide scope") reading, while <u>every</u> and particu-
larly <u>each</u> bias preferential readings to the nonspecific ("narrow
scope") reading[2,3]:

   (6a) All the men in this room love a woman in Linguistics 101.
   (6b) Every man in this room loves a woman in Linguistics 101.
   (6c) Each man in this room loves a woman in Linguistics 101.

   A more extreme case are the small number of predicates which
have only a collective reading, and consequently require the use
of <u>all</u> and a specific reading of the existential quantifier (cf.
Vendler 1967):

   (7a) All/*every one/*each of the blocks are similar in some way.
   (7b) All/*every one/*each of the corks in the bottle add up to
        some number.

In (7a) and (7b), the semantics of the predicate require a speci-
fic reading, even though the order of quantifiers would suggest
the opposite.
   In addition to the semantics of predicates and NP's, the prag-
matics of a given sentence can make an NP more definite or less
definite and thus create a bias towards the specific or nonspeci-
fic reading of the existential quantifier respectively. Consider:

   (8a) Gary put every apple in a cup.
   (8b) Gary put every apple in a barrel.

In (8a) the specific reading is extremely unlikely because most
cups will fit at most one apple. In (8b), the specific reading is
preferred because it is more likely pragmatically that every apple
was put into one barrel than each being put into its own barrel.
In (9), the nonspecific reading is preferred even though the
existentially quantified NP in question is the subject and pre-
cedes the universally quantified NP:

   (9) A bottle of perfume was given by every man to some woman.

The nonspecific reading is preferred because it is extremely
unlikely that every man gave the same specific bottle of perfume
to some woman, even if <u>some woman</u> is interpreted as a specific
woman. In this case the ambiguous quantifier approach successfully
predicts a nonspecific reading, since the pragmatics of the

sentence strongly imply that the NP <u>bottle of perfume</u> must be indefinite.

It should be expected that the specific, nonspecific, or both meanings of <u>some</u> and <u>a</u> should appear in contexts other than the multiple-quantifier context discussed above. Indeed, all the major uses of the existential quantifiers in other contexts are sensitive to the distinction, either displaying an ambiguity or requiring just one reading.

The other major context allowing a specific/nonspecific ambiguity is in wish or want contexts, such as the complements of <u>want</u>, or after possibility modals (including the future <u>will</u>).[4] Thus, sentence (10) is ambiguous:

(10) Freida wants to buy a Persian rug.

Sentence (10) can refer either to a specific rug which Freida is interested in buying, or it can express Freida's desire to buy some nonspecific Persian rug. As in the multiple-quantifier examples, the preferred reading can be changed by substituting a more definite description than the NP <u>Persian rug</u>:

(11) Freida wants to buy a Persian rug she saw in a shop in San Francisco last Friday.

Sentence (11) is much more likely to be interpreted as referring to a specific rug rather than referring to any one of the many Persian rugs Freida presumably saw in the shop in San Francisco last Friday.

A related context, imperatives, requires a nonspecific reading:

(12) Go buy me a Persian rug for the bathroom.

This sentence can only be interpreted as not referring to a specific Persian rug. This is to be expected, because it would be extremely infelicitous to command someone to perform some action on a specific item which you are nevertheless treating as indefinite, as the strangeness of (13) indicates:

(13) Go buy me a Persian rug I saw in a shop in San Francisco last Friday.

In (13), it appears that the speaker is holding back information that is required for the hearer to successfully carry out the command, since the definiteness of the NP virtually requires a specific reading of <u>a</u>.

The above contexts, which allow or require a nonspecific reading of the existential quantifiers, are all <u>modus irrealis</u> contexts, which can again be expected since a nonspecific reading requires a domain of possibilities for it to range over. It should also be expected that <u>modus realis</u> contexts such as the past, the nongeneric present, or the present progressive will require a

specific reading since that reading requires a real, although not fully specified, object as a referent. This is indeed the case in the context of reporting an event in which a participant is specific but not known to the speaker:

(14a) John is in the corner talking to a/some woman from IBM.
(14b) John was in the hallway talking to a/some woman from IBM.

The specific reading is also required in the so-called 'presentational' use, where a introduces an entity known to the speaker but not necessarily to the hearer:

(15) I met a woman I knew from my University of Chicago days.

It is a convention in English to introduce a new topic or referent into the discourse with a, but that convention fits the specific reading of a since the new referent is specific but not yet further identified in the discourse context.

The final use of some and a which must be covered is in the antecedents of conditionals. There a nonspecific reading is required, as one might guess from the modus irrealis context:

(16) If someone knocks on the door, ask him the password.
(17) If a Republican knocks on the door, don't let him in.

What is interesting and problematic about the conditional contexts is that someone in the antecedent of (16) can be replaced by anyone, and the a in (17) is likely to be read as "generic" a, which could be replaced by any; in neither case is the meaning appreciably altered:

(18) If anyone knocks on the door, ask him the password.
(19) If any Republican knocks on the door, don't let him in.

In fact, I perceive a slight difference between (17) and (19), where (17), although nonspecific, seems to refer to a "particular" possible event of some Republican knocking on the door, while (19) refers to any arbitrary possible event of a Republican knocking on the door. However, since in both cases only one "thing" is being talked about, whether that thing is arbitrary or just indefinite does not seem to matter.

The distinction between nonspecific some and any is much clearer in the following sentences (brought to my attention by Bob Moore):

(20) John wants to marry a Norwegian.
(21) John wants to marry any Norwegian.

Sentence (20) is ambiguous between a specific reading of a, where there is a person I know or can identify as a Norwegian who John wants to marry, and a nonspecific or nonreferential reading of a,

where there is no specific Norwegian in mind. However, the non-specific reading of (20) does allow John to meet a number of Norwegian women and not want to marry every one of them; in other words, an arbitrary pairing of John with a Norwegian woman will not imply that John wants to marry that woman. This is not the case in (21), where John is sufficiently enamored of Norwegian women that for an arbitrary individual extracted from the domain of Norwegian women, John wants to marry that individual.

A similar case can be made for commands. If I say to you

(22) Go out and buy me a newspaper from the newsstand.

it is possible for you to return with <u>USA Today</u> and for me to say something like "not that stupid rag; <u>I</u> want a <u>real</u> newspaper!", and reject it. However, if I had said

(23) Go out and buy me any newspaper from the newsstand.

it would be extremely infelicitous for me to reject whatever you brought me, since in the latter case I have indicated that an arbitrary item from the newsstand would satisfy my request as long as it was a newspaper.

Still another example of what appears to be a three-way distinction is the original sentence (1), with <u>a</u> replacing <u>some</u>:

(24) Every man loves a woman.

Besides having the specific and nonspecific senses, (25) also has the so-called "generic" sense, where <u>a woman</u> means any woman. (Note that <u>some</u> cannot have this generic meaning.)

The above interpretations of <u>some</u> and <u>any</u> imply that perhaps the correct analysis of <u>any</u> is as the ultimate indefinite existential quantifier: although the above sentences appear to be referring to a single unspecified entity, that entity is an arbitrary choice from the domain of the NP. A full argument for this analysis of <u>any</u> cannot be presented in this paper; however, an analysis of <u>any</u> as a special kind of existential quantifier has been made by Alice Davison (Davison 1980), where she argues that the defective distribution and lack of existential presupposition of the "universal" <u>any</u> (not shared by the true universal quantifiers but, suspiciously, shared by the nonspecific <u>some</u> and <u>a</u>), can be explained by treating <u>any</u> as an existential quantifier without the implicature of "not all" (whereas I would argue for a higher degree of indefiniteness). In the remainder of this paper I will assume this controversial interpretation of <u>any</u> in order to present additional parallels, with the hope that a fuller justification of this position can be presented in the near future.

If I am correct in arguing that the ambiguity of sentences like (1) should be attributed to polysemy in the English forms of the existential quantifier, namely, <u>some</u> and <u>a</u>, then it should be expected that other languages would have two explicit surface

forms corresponding to the two distinct meanings of the English
existential quantifiers. Russian provides an interesting example.
Russian has a rich set of indefinite quantifiers whose intricacies
in use go considerably beyond the scope of this paper; however,
there do exist a number of morphologically distinct indefinite
pronouns which can serve to make the same distinction as the
specific vs. nonspecific some and a.[5]

The nonspecific reading of (1) only is allowed with an indefi-
nite formed by suffixing the clitic-like -nibud':

  (25) Každyj čelovek l'ubit kakuju-nibud' ženščinu.
      Each   man   loves some-NONSPEC   woman
      'Every man loves some woman [nonspecific].'

Because of the distributive každyj 'each, every' the phrase odnu i
tu že 'one and the same' must be used in place of kakuju-nibud' in
order to obtain the specific reading. In general, though, the
characterization of the specific indefinite is a more complex
matter, because Russian makes additional distinctions which
English does not. In particular, there is a distinction between
the presentational use and the specific (but not known to speaker)
use. In ordinary contexts, the presentational indefinite is repre-
sented by odin 'one', or (less frequently) by the bare NP. The
non-presentational specific indefinite is represented by suffixing
-to instead of -nibud'. The suffix -to emphasizes that the speaker
does not know the identity of the entity denoted by the NP, but
again the bare NP may be used if the entity is not very relevant
to the conversation:

  (26a) Ivan   govoril   s   kakim-to antropologom.
       John talk-PAST with some-SPEC anthropologist
       'John was talking to some anthropologist.'
  (26b) Ivan   govoril   s   antropologom.
       John talk-PAST with anthropologist
       'John was talking to an anthropologist.'

The distinction between (26a) and (26b) in Russian is roughly
equivalent to the subtle distinction of emphasis on the anthro-
pologist between the English glosses with some and a.

In the other direction, the affix -nibud' can also have the
arbitrary choice meaning of l'uboj 'any'. However, l'uboj cannot
have the nonspecific-but-particular meaning; it is prohibited in
the antecedent of a conditional. Likewise, -nibud' cannot be used
in generic any contexts (in this respect, it functions like
English nonspecific some).

The full range of the Russian indefinites described above[6] can
be exhibited in (27):

  (27a) Ivan xočet ženit's'a na odnoj   ukrainke.
  (27b)   kakoj-to
  (27c)   kakoj-nibud'

(27d) Ivan xočet ženit's'a na l'uboj ukrainke.
(27e)   Ø

(27a) is a presentational sentence, announcing that John wants to
marry a Ukrainian; the conversation will presumably proceed with a
discussion of the Ukrainian. (27b) reports John's desire, and indi-
cates that John has a specific Ukrainian in mind, but that the
speaker does not know who she is. (27c) expresses John's desire to
marry someone Ukrainian; it is also ambiguous with the meaning of
(27d), which emphasizes that John will marry any Ukrainian he
finds. Finally, (27e) can have any of the above meanings in the
appropriate context (as well as a definite meaning if the Ukrain-
ian is already part of the discourse context), although it is a
more neutral way of expressing each meaning than its nonzero coun-
terparts.

   One can see that the different kinds of indefinite (and defi-
nite) quantifiers distinguished in Russian and English can be
organized into a set of right-branching distinctions, which are
displayed in the figure below (I have included demonstratives at
the top, although they really differ from definite articles only
in that they "point" in different "directions"):

	English	Russian
I. Deictic	this, that	to, eto
II. Non-deictic		
A. Definite	the	Ø, eto
B. Indefinite		
1. Known to speaker	a, some	odin, Ø
2. Not known to speaker		
a. Specific	some, a	-to, Ø
b. Nonspecific		
(1) Particular	a, some	-nibud', Ø
(2) Arbitrary	a, any	l'uboj, -nibud', Ø

   The polysemous quantifier analysis of sentences containing both
existential and universal quantifiers has considerable advantages
over the traditional scope ambiguity analysis. It does not depend
on a structural phenomenon not related to the surface structure of
English (or virtually any other natural language). It reveals the
ambiguity of the English sentences in an expected place, viz. the
polysemy of a lexical item. It corresponds to the surface behavior
of quantifiers, namely that they attach to NP's, not S's. It makes
predictions about preferred readings of various sentences based on
the semantically and pragmatically defined definiteness of the
existentially quantified NP, whereas the scope ambiguity analysis
can only make the weak statement that the preferred reading is
usually the one where the order of the underlying quantifiers is
the same as the order of the surface quantifiers. It also makes
predictions about possible morphological distinctions in other
languages which are borne out in at least one case, and is able to
accommodate additional distinctions in meaning which the scope

ambiguity analysis cannot.

Needless to say, the polysemous quantifier analysis implies a completely different view of quantification that that offered by the first-order predicate calculus approach. The analysis proposed in this paper is actually the first and most interesting one so far derived from the hypothesis that quantifiers are a type of indexical or referring expression which picks out entities in a domain according to certain principles of selection, and then applies the selected entities to a predicate in accordance with certain principles of application. The principles of selection which are used in English are those of quantity and definiteness, and the principles of application are those of distributiveness and perhaps sequentialness (in order to account for the every/each distinction). Traditional quantification and certain extensions of it to numerals and words like several and many (e.g., McCawley 1981) have concerned themselves chiefly with the principle of quantity. Little work has been done to my knowledge on the operation of the principles of distributiveness or definiteness; this paper is an attempt to cast light into the latter. As with demonstratives, the definite article and personal pronouns, the actual entities selected and the manner in which they are applied to the predicate are partly a function of the pragmatics of indexical expressions; I have nothing say about how the entities are actually selected, only what the appropriate quantifier tells us to begin with. Thus, when I say that the specific a is used to select an entity which is unknown to the speaker but which is definite, as in the situation when a loud crash is heard in the hallway and someone utters (28):

(28) Something just fell in the hallway.

I am not saying how the speaker (and hearer) understand that something refers to whatever made the crash, any more than saying the definite article the in (28) is used when the object is known to both speaker and hearer indicates how both speaker and hearer know that the hallway is that hallway where the crash occurred and not some other hallway; the meanings of something and the describe the way each NP "points" to an entity or group of entities, not how the hearer gets from the end of the metaphorical finger to the thing itself.

The reason for this brief recapitulation of the nature of indexicals is that the distinction between meaning and use is crucial for indefinite quantifiers, since unlike demonstratives and definite articles their meaning does not immediately establish an identifiable real world referent. Nevertheless, they do indeed point: the indefiniteness is in getting to the real world entities, not in the assertion itself. In fact, when indefinite quantifiers (singular or of higher number) are used, they establish discourse referents which can then be referred back to using personal pronouns or definite NP's, and it is understood that those pronouns and definite NP's are referring to the same entity or class of

entities as the original expression does, even though the original expression points to that entity or class of entities in an indefinite manner. (The traditional scope approach to quantifiers fails in this regard, since the coreferential expressions can occur later in the discourse in positions which have to be outside the scope of the original quantifier. Hence the paradoxes of the donkey sentences and coreference in world-creating predicates; I am indebted to James McCawley's solution to the latter problem (McCawley 1978, 1979) which inspired the approach to quantifiers embodied in this paper.)

Having distinguished the use of indefinites in discourse as establishers of discourse referents from the use of indefinites as indexicals pointing at real world entities, it remains to define the distinctions in meaning between the different indefinite quantifiers in English and Russian more precisely. In order to do so, I will introduce two notions from the current debate on naming and reference in the philosophy of language: numerical identity and specific identity. Numerical identity is the identity of a thing with itself; in English this usually referred to by the locution same x, as in (29):

(29) This is the same pie you tried to make me eat last night.

In (29) the same pie means that tonight's pie is the same real world entity as last night's pie; i.e. they are numerically identical. Specific identity, on the other hand, is the identity of two or more entities as belonging to the same class or species; it is usually represented by the locution same or same kind:

(30) This pie is the same/same kind as you gave me last night.

In (30), it is not being asserted that tonight's pie is numerically identical to last night's pie, only that it is another, numerically distinct object of the same class, namely, pie. Thus specific identity defines the class membership of a thing, while numerical identity defines the unique thisness of the thing itself.

The various degrees of definiteness of quantifiers can be defined in terms of the status of the numerical identity of the thing (or things) referred to in the minds of the speaker and the hearer. Beginning with the most definite, i.e. the definite article (the demonstratives differ from the definite article only in that they point in different "directions"), we have:

a. Definite. The numerical as well as specific identity of the thing referred to is known to the speaker and the hearer.

b. Presentational indefinite. The numerical identity is known to the speaker but not necessarily to the hearer.

c. Specific indefinite. The numerical identity of the thing is known to the speaker, but cannot be more specifically described

except by some vaguer specific identity, viz. the following NP.

d. Nonspecific indefinite. The numerical identity of the entity referred to is not known and thus is not necessarily fully determinate (or even existent); only its specific identity is known. Use of the nonspecific indefinite can only be made in contexts where there is a range of possibilities with respect to the real world; e.g. in the normal use of commands, non-referential readings of wish predicates or modals, the antecedents of conditionals, or distributive readings of predicates.

e. Arbitrary indefinite. The numerical identity of the entity to which the predicate is applied is arbitrary, as long as it is a member of the class described by the NP. That is, an arbitrary entity from the domain will satisfy the predicate. Again, a domain of possibilities over which the quantifier can range is required, i.e. _modus irrealis_, object of negation, etc.

These definitions may require modification, particularly when other languages are brought into the analysis. Here I have only attempted to show that a different approach to quantifiers can prevent us from having to attribute a linguistic ambiguity to a structural distinction not present in surface structure. It is hoped that a similar analysis will dispose of the other major scope ambiguity not exhibited in surface structure, namely negation.

Footnotes

1. I have also made the universally quantified NP more specific in order to make interpretations of the sentences in (5) more plausible. It must be noted that this also biases the sentences more towards a "wide scope" reading by making the whole situation of the sentences more definite and less vague.

2. All in its generic use--that is, without a following the--does not carry this collective bias; compare (i) to (ii), and also compare the poorness of (iii), where the generic all conflicts with the relatively specific context:

  (i) All the men love some woman.
  (ii) All men love some woman.
  (iii) All men in this room love a woman in my phonology class.

3. A description of the distributive/collective distinction of the different universal quantifiers as a principle of application to the predicate also accounts for the use of all and each as adverbs, with the corresponding preferences in readings (for some reason, every cannot be so used):

(i) The men all loved a woman in my phonology class.

(ii) The men each loved a woman in my phonology class.

Note also that in general the distributive reading can be obtained only when _every_ or _each_ modify the subject NP or the passive agentive _by_-phrase; thus (5) cannot be interpreted distributively.

4. Belief contexts appear not to affect the variability in acceptability of specific and nonspecific readings except when the belief concerns a nonexistent entity. McCawley (1978) notes this and also presents other distinctions between belief and wish contexts.

5. The details on Russian quantifiers presented here were provided by Boris Gasparov (Dept. of Slavic Languages and Literatures, Stanford University).

6. I have left out of this account certain other Russian indefinites which are not used as frequently and which appear to differ from the more common ones by nuances of meaning not having to do with degree of indefiniteness. For example, the prefix _koe-_ functions in a presentational context like _odin_, but in addition conveys the special importance of the information to the hearer, and thereby control of the situation by the speaker over the hearer.

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Agent-Patient Languages and Split Case Marking Systems[*]
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This paper addresses two points regarding the agent-patient case marking system and split systems of case marking. [1] First of all, the agent-patient system has often been mistaken for "split ergativity", which is a split between the ergative system and the accusative system of case marking. It will be shown that the agent-patient system and ergative/accusative splits are, in fact, distinct phenomena. Second, languages where case marking is split between the agent-patient system and another system will be examined, and compared to the ergative/accusative splits.

Before beginning this discussion, though, a couple preliminary points must be made. It is important to keep in mind that reflexes of a language's case marking system may include not only affixes attached to nouns, as in Latin or Russian, but also forms of person markers on the verb, other types of verb agreement, word order patterns, clitics on nouns or verbs -- or any other means a language may have to indicate the relationship a nominal argument bears to its verb. Also, note that this paper follows Dixon's (1979) terminology for the core arguments of verbs. The schema in (1) displays the three labels used, where S refers to the subject of an intransitive verb, A to the subject of a transitive verb, and O to the object of a transitive verb. Accusative case marking, as in English, is defined as A and S patterning together to form one category, often unmarked, opposed to O, which is distinguished by special marking. The union of A and S is called "nominative"; the marked case of O is "accusative". Ergative case marking, represented in (3), is defined by O and S patterning together ("absolutive" case), leaving A as the marked, "ergative" case.

(1)   intrans: | S | S = subject of intransitive
       trans: | A | O | A = subject of transitive
                            O = object of transitive

(2) Accusative   (3) Ergative   (4) Agent-patient
       system   system   system

intrans: | NOM | | ABS | | AG | PAT |
  trans: | ACC | | ERG |

1. The topic of this paper is a third system of casemarking, the agent-patient system, represented in (4). This system has also been called stative-active, active, or split-S. If (4) is compared to (2) and (3), it can be seen that the accusative and ergative systems are alike in one respect: all subjects of intransitive verbs get marked in the same way. In contrast, in the agent-patient system, two classes of intransitive verbs may be distinguished by their case marking pattern. Furthermore, one

class of intransitive verbs marks its subject identically to the
subject of transitive verbs, while the other marks its subject
identically to the object of transitive verbs. The names of the
two cases in this system are "agent" and "patient".

Examination of the two classes of intransitive verbs often
reveals a semantic motivation for this pattern. Intransitive
verbs that take the agent case are typically active ones such as
'jump' and 'dance', while the intransitive verbs that take the
patient case include statives such as 'be big' and 'feel pain',
as well as change of state verbs such as 'die' and 'grow'. [2]

(5), taken from Heath (1977), shows the agent-patient system
which operates in the bound person markers of Choctaw verbs. In
the first sentence, the A is 1st person and is marked by the
agent case, while the O is 2nd person and is marked by the
patient case. The second sentence contains a stative intran-
sitive verb, so the subject of this verb is marked by the patient
case. The third sentence contains an active intransitive, the
subject of which is in the agent case.

(5a)   či - pĩ:sa - li - h   'I see you'
       2PAT   see   1AG   present
   (b)   či - (y)abi:ka - h   'you are sick'
         2PAT   be sick   pres
   (c)   iš - iya - h   'you are going'
         2AG   go   pres

From a slightly different standpoint, the three types of
case marking systems may be considered in the following way.
Languages have two parameters which they may make use of in
assigning surface case to core arguments of verbs. One parameter
is valence of the verb: intransitive verbs may be treated in a
different manner than transitive verbs are. This distinction is
made by the accusative and ergative systems, which code all
arguments of intransitive verbs in a single way. Once the
transitive verbs have been distinguished from the intransitives,
accusative and ergative languages make a further distinction
between A and O, the two arguments of the transitive verb.

The second parameter, the one that distinguishes A from O,
is based upon semantic notions. Although there is no single
semantic property (e.g., animacy, agency, etc.) true of all NP's
appearing as A, one may say that A is much more likely than O is
to be an agent, or to be animate. A cluster of such properties
will characterize the clear cases where A is to be distinguished
from O; individual languages may differ in their treatment of the
less clear cases.

While the accusative and ergative systems make use of both
of these parameters in assigning nominative and accusative, or
ergative and absolutive case, the agent-patient system only
employs the second, semantically-based, parameter. NP arguments
are divided into two classes, one more agent-like than the other.
This semantically-based distinction is made regardless of the

transitivity of the verb.

The Choctaw sentences in (5) illustrate the prototypical agent-patient case marking system. Some other languages, however, deviate somewhat from this prototypical pattern. A few languages with agent-patient case marking seem to allow a conscious manipulation of the semantic difference between the agent case and the patient case. This is the phenomenon of "fluid-S" verbs that has been reported in the Caucasian language of Batsbi, and in the Pomo languages of Northern California. [3] (6), from McLendon (1978), illustrates an Eastern Pomo fluid-S verb:

(6a)   wí   c'e:xelka   'I'm slipping'
      I-PAT   slip

(b)   há: c'e:xelka   'I'm sliding'
      I-AG   slip

In these languages, there is a restricted class of intransitive verbs that may take either agent marking or patient marking. The gloss of the verb will vary according to which marking is chosen for the subject of the verb. Though this is probably a special sub-case of agent-patient marking, it is not typical of agent-patient languages in general.

Another pattern probably related to agent-patient case marking can be seen in languages like Lotha, a Tibeto-Burman language spoken in India. Here, at least in the perfective aspect, there seems to be an opposition of clitics: one marking subjects of active verbs, the other marking subjects of stative verbs. These clitics are illustrated below in (7), from Teixeira (1982). However, notice that the 0 of the transitive verb in (7a) is not marked by a clitic. Since the marker of the stative verb is not identical to that of 0, it does not correspond exactly to the prototypical pattern of agent-patient case marking.

(7a)   John   <u>nà</u>   fìró   cì   ekhó   chò
           subj   dog   det   hit   perf
     'John hit the dog'

(b)   mpó   <u>nà</u>   okí   nà   hapoi   cí   yi   chò
    he   subj   house   from   outside   det   go   perf
   'He went outside (from the house)'

(c)   nkóló   có   a   wopan   cìaŋ   <u>có</u>   Wokka-e   van   chò
     long   ago   I   family   det   subj   W.   loc   live   perf
   'Long ago my family lived in Wokka'

2. Recently, much attention has been paid to those languages characterized as "split ergative" -- that is, where part of the language operates on the ergative case marking system, while another part operates on the accusative system. Typically, such splits occur between person categories, between tense or aspect oppositions, or between main and subordinate clauses. (8), taken

from Comrie (1981), illustrates this sort of split in Dyirbal, where 1st and 2nd person display an accusative pattern, while 3rd person is ergative.

(8a) ŋadya   bayi yaṛa   balgan   'I hit the man'
      I NOM   man ABS   hit

 (b) ŋayguna   baŋgul yaṛangu balgan   'the man hit me'
      I ACC   man ERG   hit

 (c) ŋadya   baninyu   'I came here'
      I NOM   come here

 (d) bayi yaṛa baninyu   'the man came here'
      man ABS   come here

The agent-patient case marking system has sometimes been mistaken for an ergative/accusative split. It is easy to see how this might happen: if one's theory admits only two possible case marking systems -- ergative and accusative -- then the procedure for classifying a new language is simply to look at the case marking of S (subject of intransitive verb), and determine whether S is identical to A, or to O. If the language in question, however, is one where some S's look like A, while other S's look like O, the temptation is to call it a split system. This is the motivation behind the use of the term, "split-S". In fact, the agent-patient system is not split between ergative and accusative, but is actually a coherent system in its own right.

The true ergative/accusative splits vary the case marking of NP's within the paradigm of any given verb. As can be seen in the Dyirbal example, the subject of 'hit' is ergative if it happens to be in 3rd person, but nominative if it happens to be 1st person. Or, in a language that splits ergative and accusative along tense/aspect lines, the subject of 'hit' would be marked ergative if the verb is in past tense or perfective aspect, but if the verb is in present tense or imperfective aspect, the subject of 'hit' would be marked nominative. Thus, for any given verb, either transitive or intransitive, there will be case marking variation, and the choice of surface case is conditioned by factors other than the verb itself.

The agent-patient system differs in several respects from the pattern of split ergativity. First of all, there is never any variation of case marking for the transitive verbs (unless the language itself is split between the agent-patient system and another case marking system). Subjects of all transitive verbs are always marked as agents, and objects of all transitive verbs are always marked as patients. Although there is no uniform representation of S, the selection of case for the subject of an intransitive is determined by lexical properties of the verb itself, not by additional properties such as aspect or tense, nor by any features of the NP argument such as person. An individual intransitive verb, in a language which is consistently agent-patient, will never exhibit variation in case marking. [4]

Looking further at the intransitive verbs, another difference between the agent-patient system and actual instances of split ergativity may be seen. In the latter, S is always a unified category, either absolutive or nominative, depending upon the conditioning factor of tense, aspect, person, etc. The agent-patient system, on the other hand, never displays a unified category of S.

A final point arguing against analyzing agent-patient case marking as a type of ergative/accusative split, is that the agent-patient system may participate in split case marking systems itself -- as will be seen below.

3. A well-known analysis of split ergative/accusative languages is that in Silverstein (1976), which examines ergative/accusative splits sensitive to features of person, number, and animacy, showing that they pattern in a non-random fashion. More recently, DeLancey (1981) has provided a slightly different motivation in terms of viewpoint and attention flow, for not only the person/number type of split, but also tense/aspect splits, and fluid-S marking.

To summarize Silverstein's argument briefly, he sets up a hierarchy of NP's, where 1st or 2nd person is at the top, being most animate, followed by 3rd person [+human], then [-human, +animate], and so on. His claim is that accusative case marking is favored by NP's at the top of the hierarchy, while ergativity is more likely at the bottom of the hierarchy. Thus in (8), Dyirbal 1st and 2nd persons are accusative because they are most animate, and 3rd person is ergative because it is less animate. The "semantic naturalness" of various NP's to be agents or patients is the motivation for this hierarchy.

Although Silverstein does not consider the agent-patient type of case marking system, reflexes of his nominal hierarchy -- or something like it -- may be observed in agent-patient languages.

Lakhota. Lakhota is an example of a language whose case marking system is only partially agent-patient. Nouns in Lakhota are not marked for case, but person markers affixed to the verb reveal a split system, sensitive to features of person/number and animacy. This results in three types of case oppositions.

The person markers have the following case oppositions: 2nd person, both singular and plural, and 1st person singular display an agent-patient system, as can be seen in the examples in (9).

(9a)   wa - lowã   'I sing'
        1sg.AG   sing
  (b)   ma - hãska   'I'm tall'
        1sg.PAT   be tall
  (c)   ma - ya - gnayã - pi   'you pl. tricked me'
        1sg.PAT 2AG trick   pl

There is also a nominative-accusative opposition, which is found only in 3rd person animate plurals. As can be seen in (10d), the prefix wičha- marks O, with A and S marked by Ø-
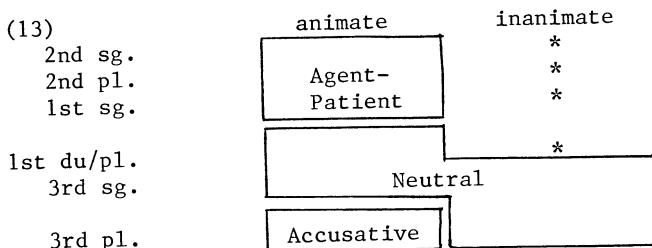
prefix, and by the plural suffix -pi. 3rd person animate plural O's are further marked by the fact that they are the only animate plural that will not trigger the suffix -pi upon a verb.

(10a) lowã - pi   'they sing'
       sing   pl
   (b) hãska - pi   'they (anim.) are tall'
       be tall pl
   (c)   ma - gnayã - pi   'they tricked me'
       1sg.PAT trick   pl
   (d)   wicha - wa - gnayã   'I tricked them'
       anim.3pl.ACC 1sgAG trick

The remaining person/number categories in Lakhota -- 1st person dual and plural, 3rd person singular, and 3rd person inanimate plural -- have what can only be called neutral case marking. Examples are shown in (11) and (12). In a neutral system, there is no formal distinction between A, O, or S. Both of the 3rd person categories have Ø- in all cases, while 1st person dual and plural is ũ- for all cases.

(11a) ũ - lowã - pi   'we sing'
   (b) ũ - hãska - pi   'we're tall'
   (c) ũ - gnayã - pi   'we tricked him'
                              OR 'he tricked us'
                              OR 'they tricked us'
(12a) lowã   'he sings'
   (b) hãska   'he's tall'
   (c) gnayã   'he tricked him'

The chart in (13) provides a schematic representation of the three types of case marking in Lakhota. The neutral system seems to be a gap between agent-patient and accusative. Note especially that the accusative system -- which Silverstein claims is always at the top of the NP hierarchy -- is here outranked by the agent-patient system.

(13)   animate   inanimate
    2nd sg.   *
    2nd pl.   Agent-   *
    1st sg.   Patient   *

    1st du/pl.   *
    3rd sg.   Neutral

    3rd pl.   Accusative

Wichita. Another example of a language with a split case marking system is Wichita. Like Lakhota, it has agent-patient case marking in 1st and 2nd person, as can be seen in (14). [5]

```
(14a)   ta - s - ki? - ?i::s        'you sg. saw me'
        indic 2AG 1PAT    see       [taski?i::s]
   (b)  ta - t - á: - ?i::s         'I saw you sg.'
        ind  1AG  2PAT  see         [tatá:?i::s]
   (c)  ta - t - hisha              'I went'
        ind  1AG  go                [tachish]
   (d)  ta - ki? - hiya:s           'I'm hungry'
        ind  1PAT   be hungry       [takihiya:s]
```

In 3rd person, the agent-patient opposition is neutralized:

```
(15a)   ta - t - Ø - ?i::s          'I saw him'
        ind  1AG  3    see          [tac?i::s]
   (b)  ta - Ø - ki? -?i::s         'he saw me'
        ind  3   1PAT   see         [taki?i::s]
```

The only overt marking of 3rd person is that some of the tense/mood prefixes have a special form if there is no 1st or 2nd person argument. So, for intransitives with 3rd person subjects, or transitives where both arguments are 3rd person, the indicative marker ta- becomes ti-.

```
(16a)   ti - Ø - Ø - ?i::s          'he saw him'
        ind  3   3    see           [ti?i::s]
   (b)  ti - Ø - hisha              'he went'
        ind  3   go                 [tihish]
   (c)  ti - Ø - he:c?i             'he's fat'
        ind  3   be fat             [tihe:c?]
```

However, there is also evidence for ergativity in 3rd person, especially in the marking of non-singular. (Here, it should be noted that another difference between 1st and 2nd persons and 3rd person in Wichita is the number of number categories that are marked for each on the verb. 1st and 2nd persons distinguish three numbers: singular, dual, and plural. For 3rd person arguments, however, only two categories are distinguished: singular and non-singular. In this discussion of Wichita, "non-singular" will mean "two or more", as distinct from "plural", which is "three or more".)

The non-singular category of 3rd person is marked ergatively: the morpheme -?ak- (and other allomorphs) marks a 3rd person O or S as being non-singular. 3rd person A can only be marked non-singular by using the prefix hi?- and leaving the person marker slot empty.

```
(17a)  hiʔ - ta - ∅ - kiʔ - ʔiːːs          'they saw me'
       pl A   ind  3  1PAT      see          [hitakiʔiːːs]
  (b)  ta - t - ʔak - ʔiːːs                 'I saw them'
       ind  1AG 3plABS  see                  [tacʔakʔiːːs]
  (c)  ti - ʔak - hisha                     'they went'
       ind  3plABS   go                      [taʔakhish]
  (d)  néʔaʔ aki - uc - ʔak - hahris         'they were angry'
       bad   aor        3plABS be in mood    [néʔaʔ akíːcʔakháris]
```

In contrast, the morpheme -ra:k- marks plurality of either
1st or 2nd persons, and either agent or patient.

```
(18a)  ta - s - ra:k - ʔiːːs               'you all saw him'
       ind  2AG  1,2pl   see                [tasáːkʔiːːs]
  (b)  ta - kiʔ - ra:k - ʔiːːs             'he saw us all'
       ind  1PAT  1,2pl   see               [takiráːkʔiːːs]
```

There are other reflexes of ergativity that extend
throughout 3rd person, including the pattern of noun
incorporation, which occurs frequently in Wichita. The objects
of transitive verbs, and subjects of both active and stative
intransitives, may be incorporated within the verbal complex.
Here, the ergativity is not restricted to 3rd person non-
singular, but operates in the singular as well.

```
(19a)  ti - wi:c - ʔiːːs  ka:hi:kʔa     'the woman saw the man'
       ind   man     see    woman       [tiwi:cʔiːːs ka:hi:kʔa]
  (b)  ti - wi:c - hisha                 'the man went'
       ind   man    go                   [tiwi:chish]
  (c)  néʔaʔ ta - kiʔ - uR - uR - ta:ras - uʔakhirʔis
       bad   ind  1PAT  poss       horse      feel
       'my horse is not feeling well'
       [néʔaʔ  taki:ríːta:rasiʔakhirʔiːs]
```

Thus, there are several manifestations in Wichita of 3rd
person behaving distinctly from 1st and 2nd persons. The one of
most interest in this discussion of case marking systems is that
the agent-patient system shows up in 1st and 2nd persons, but not
in 3rd. 3rd person, instead, has ∅-marking for person, and an
ergative means of marking non-singular.
     4. One of the most intriguing similarities between Lakhota
and Wichita, as well as several other languages with case marking
splits involving the agent-patient system, is that 3rd person
displays a neutral system of case marking. Most Siouan
languages, for example, have no distinction between A, O, or S
in 3rd person. In Choctaw, a Muskogean language, 3rd person is
neutralized not only in terms of case, but also lacks the number
distinctions found in 1st and 2nd person. It is puzzling why 3rd
person should show a preference for neutral case marking, and why
this should correlate with agent-patient marking in 1st and 2nd
person. However, these splits provide evidence for DeLancey's

(1981) claim that 1st and 2nd person, as Speech Act Participants, enjoy special grammatical status not extended to 3rd person.

I suggest that Silverstein's hierarchy applies to agent-patient languages in the following way: if the agent-patient system is used in a language, then it will always outrank any other case marking system, be it accusative, ergative, or neutral. From that, the implication follows that if a language has agent-patient case marking in 3rd person (as Iroquoian and Paraguayan Guarani do), it will also have agent-patient marking in 1st and 2nd persons.

There are other types of agent-patient splits that deserve attention. Tupinamba, a 16th century Tupi-Guarani language of Brazil, displays a particularly interesting type of split. [6] Although there are several other complicating factors in Tupinamba, there is a clear difference in case marking between indicative mood and subjunctive mood. Indicative verbs follow the agent-patient pattern, while subjunctive verbs are ergative. DeLancey (to appear) reports another type of split in Lhasa Tibetan: perfective aspect displays an agent-patient pattern, with what DeLancey calls the ergative case obligatory for subjects of active intransitives and subjects of transitives. In the imperfective aspect and future tense, however, the ergative case is optional: here the pattern seems to be "fluid-S", with semantic information conveyed by choice of case.

It seems that there are at least as many possibilities for split case marking involving the agent-patient system, as have been observed for splits between the ergative and accusative systems.

Footnotes

[1] The case marking typology discussed in this paper is one of morphological case, not syntactic case.

[2] The inclusion of a specific lexical item, such as 'die' in the stative/change-of-state class of intransitive verbs, should not be considered a necessary criterion for calling a particular case marking system "agent-patient". Rather, the claim is that most of the verbs in one class will be active intransitives, while most of the verbs in the other class will have non-agent subjects.

[3] Scott DeLancey (to appear) has reported "fluid-S" verbs in Tibeto-Burman languages, as well. See the discussion in section 4. of this paper.

[4] Unless the language is one of the few agent-patient
languages that allows "fluid-S" marking, discussed above.
[5] All the Wichita examples are from Rood (1971, 1976), and
are given in the form used there. The strings on the left
represent underlying forms of morphemes, with the phonemic
representation given in the square brackets.
[6] I am indebted to Aryon Rodrigues for providing the
Tupinamba data.

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Samaná English: a dialect that time forgot

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Samaná English is spoken by descendants of "free" black
Americans who settled over 150 years ago in what is now the
Dominican Republic. The fact that the dialect has survived until
now in a virtually monolingual Spanish-speaking nation is
attributed mainly to the isolation of most remaining speakers on
the sparsely-populated Samaná peninsula. The assumption that
language change is retarded by isolation invites speculation that
Samaná English is representative of the speech of "free" Afro-
Americans around 1824, when the first American immigrants set foot
on Hispaniola.

1. Background of the present study.

Published information on the English-speaking black Domini-
cans is hard to find. General texts on the Dominican Republic
offer a paragraph at most, confirming the group's existence and its
location on the Samaná peninsula, but giving few additional de-
tails. The most thorough existing study is an article by H.
Hoetink entitled "Americans in Samaná" based on data he collected
during a 1962 visit to the Dominican Republic. It incorporates the
findings of a Commission of Inquiry from the United States which
visited Samaná in 1870.

Hoetink's article was published at a time when I was
serving as a Peace Corps Volunteer in the Dominican Republic, and
I first heard of the English-speaking colony through lectures by
Hoetink on Dominican history and culture, as part of my Peace
Corps training at the Institute of Caribbean Studies, University
of Puerto Rico. During my Peace Corps service I met several
English-speaking black Dominicans and made one unsuccessful
attempt to reach Samaná by driving eastward from Santo Domingo to
Sabana del Mar, on the southern shore of the Bay of Samaná; but
failing an attempt to cross the bay on a fishing boat.

In 1979, enroute to a Conference on Theoretical Orienta-
tions in Creole Studies in Saint Thomas I stopped briefly in the
Dominican Republic and succeeded in reaching Samaná by land:
driving north to Puerto Plata and eastward along the northern
coast onto the peninsula.

Once in Samaná, I had no difficulty finding English speakers,
but Spanish appeared to be the usual language of public interac-
tion. I spoke at length with two adult males (ages 70, 40) who
supplied most of the data upon which the following analysis is
based.

2. Historical background.

The Haitian Revolution played a key role in the events which resulted in the emigration from North America of the group from which the present speakers of Samaná English are descended. On the one hand it produced a favorable political situation for the settlement of former American slaves on Hispaniola; and on the other hand it served as a beacon of hope and political rallying point for the American freedmen, many of whom identified strongly with Africa and saw eventual repatriation to their lost homeland (or settlement in a newly-independent country such as Haiti) as preferable to permanent residence in North America.

A vivid synopsis of the situation confronted by "free" Africans during the early years of the Republic is given by John Henrik Clark (1974). Tracing the recurrence of the "back to Africa" theme in Afro-American history, he observes that

> "During the eighteenth century there was strong agitation among certain groups of Black people in America for a return to Africa. This agitation was found mainly among groups of 'free Negroes' because of the uncertainty of their position as freed men in a slave holding society . . . (xvii)"

As early as 1788 a proposal for a "general exodus to Africa on the part of at least free Negroes (DuBois, 1940)" was transmitted by the Negro Union of Newport, Rhode Island to the Free African Society of Philadelphia.

At roughly the same time, the western third of the island of Hispaniola was about to embark upon the world shaking series of events summarized as follows by C. L. R. James (1963):

> "In August 1791, after two years of the French Revolution and its repercussions in San Domingo, the slaves revolted. The struggle lasted for 12 years. The slaves defeated in turn the local whites and the soldiers of the French monarchy, a Spanish invasion, a British expedition of some 60,000 men, and a French expedition of similar size under Bonaparte's brother-in-law. The defeat of Bonaparte's expedition in 1803 resulted in the establishment of the Negro state of Haiti which has lasted to this day (ix)."

One measure of the impact of the Haitian revolution upon the lives of "free" North American blacks is the fact that advocates of black repatriation sent agents to South America and Haiti, as well as Africa (Clark op. cit. xxiv); and that a "Maryland Haytian Society" was formed in 1821 by freedmen from that state to promote the resettlement of black Americans in Haiti (Hoetink p. 6).

By 1801, Haitian forces controlled the entire island of Hispaniola but they were repelled from the Spanish side in 1909 by Spanish loyalists aided by the English fleet. In 1822, however, the Haitians, under General Jean Pierre Boyer, once again occupied Santo Domingo and ruled the entire island until 1844 when the Dominican Republic attained national independence.

The migration of large numbers of American freedmen to Hispaniola is thought to be the result of efforts of one Johnathan Granville who was sent to New York in 1824 as an agent of the Boyer government, authorized to promise prospective settlers free passage, four months of support, and thirty six acres of land to every twelve workers (Hoetink, op. cit.).

3. The original settlers.

My research has produced no better estimate of the numbers and time of arrival of the original settlers than Hoetink's guess of six to thirteen thousand, the first boatload of whom arrived in Santo Domingo City on November 29, 1824. Hoetink's estimation of the number of settlers is based in part on documents showing that the agent Granville was authorized to bring 6,000 immigrants to "Spanish Haiti" of whom 200 were to be placed in Samaná "to produce various fruits" (ibid.).

The 1870 Commission of Inquiry found a thriving colony of five to six hundred souls, engaged mainly in farming. Most families were producing a surplus on a small fraction of sixteen acres of land that each immigrant reportedly got from the Boyer government.

The original settlers were reportedly from Philadelphia, or a nearby location such as Baltimore or New Jersey. Hoetink's informants "without exception" claimed Philadelphia as the place of origin of the first settlers. My 70-year-old informant, a lifelong Samaná resident whose parents and grandparents were born there, counts his great-grandfather among the original settlers but cannot specify the time of their arrival. He specifies their American places of origin as "Philadelphia" and "Delaware."

"Some come from Philadelphia, some . . . from Delaware, and so we here is descendant of them."

The younger informant, when asked about the group's origins, replied:

"In 1892 (sic.) we had a big revolution over here . . . These is slaves, you know, the Englishes brung in to fight gints the Haitians . . . and then they stayed here . . ."

The English did in fact seize Samaná in 1809, and hoisted

the Spanish flag there, in a move that forced the retreat of
occupying Haitian forces to the western sector. The possible role
of English speaking slaves in that struggle, some of whom may
have remained as permanent Samaná residents, needs to be explored
further since it is consistent with my informant's statement.
If corroborated, it means that the Americans from Philadelphia
and thereabout, were not the first, or only, black, English-
speaking settlers on the Samaná peninsula.

4.   Samaná English and Black English.

Samaná English resembles modern BE in terms of such features
as absence of post-vocalic /r/; "simplification" of final conson-
ant clusters; nonstandard usage of the verb suffix /z/; nonstandard
usage of the copula/auxiliary be; and non-inversion of the subject
NP and AUX in questions. Certain features, however, which occur
variably in modern BE, in alternation with standard or mesolectal
forms, seem to occur categorically, or nearly so, in Samaná English.

Non-realization of post-vocalic /r/, for example, is cate-
gorical in the speech of one informant (Table IB); and practically
so in the other (IA); the only exception being his pronunciation
of girls in a manner that seems strongly influenced by orthography.

TABLE I: r-less forms in Samaná English

Informant A

/hi/ ~ /hiyə/ ~ /heyə/   'here'
/boɔn/   'born'
/la:nz/   'learn'
/ye:z/   'years'
/θʌd/   'third'
/ðeyə/ /ðæ/   'there'
/fa:m/   'farm'
/owvə/   'over'
/fʌs/   'first'
/ha:d/   'hard'
/sɔ:vis/   'service'
/wʌkin/   'working'
/mʌðə/   'mother'
/hʌ/   'her'
/girlz/   'girls'

Informant B

/ye/ ~ /hye/ ~ /hi/   'here'
/bo:n/   'born'
/lʌnz/   'learn'
/ye:z/   'years'
/θʌdiyeyt/   'thirty-eight'
/ðey/   'their'
/fa:mz/   'farms'
/owvə/   'over'
/yandə/   'yonder'
/skye:s/   'scarce'
/yʌnstəz/   'youngsters'
/ča:ǰ/   'charge'
/brʌðæz/   'brothers'
/ʌndə/   'under'
/kyælifonyə/   'California'

The pronunciation of 'gift' and 'host' in

Big gift shops charge a whole host of money . . . . (40)

and the selection of the /iz/ allomorph of the plural suffix in
the production of 'tourists' by the same informant, indicates that
simplification of final consonant clusters is also a categorical

feature of his dialect. The other informant also consistently produces simplified clusters in such forms as <u>priest</u>, <u>island</u>, <u>just</u>, etc.

Neither informant shows any instances of NP-AUX inversion, and typically produce such questions as:

<u>This is the first time you come here Santo Domingo?</u> (70)
<u>You is just givin a little walk?</u> (70)
<u>When you will be coming back?</u> (40)

In modern BE, by comparison, noninversion of NP-AUX is a variable feature.

Both informants exhibit more frequent "hypercorrect" usage of the verb suffix <u>-s</u> than is characteristic of modern BE, eg:

<u>Plenty people goes to the country.</u> (40)
<u>When we gits 'em we sell 'em to the touris'es.</u> (40)
<u>We speaks it bad.</u> (70)

Both informants produce "zero" copula constructions; and both use <u>ain't</u> as a negator.

<u>They scarce.</u> (40)
<u>We at your service.</u> (70)
<u>Y'all ain't going to find it.</u> (70)
<u>. . . when we ain't sellin' to the touris'es.</u> (40)

Both exhibit full forms of <u>is</u>, <u>am</u>, and <u>are</u>, though not always with standard subject-verb agreement.

<u>She said you is quite beautiful . . .</u> (70)
<u>They are lookin' now for teachers.</u> (70)
<u>The Chinos they is uptown.</u> (70)
<u>That is the name of a Indian.</u> (40)
<u>I am of them.</u> (40)

Contractions of <u>are</u> do not occur in the data, and contracted <u>is</u> occurs only with the demonstrative <u>that</u>:

<u>That's a island.</u>   (70)
<u>That's the (hotel) Cayacoa.</u> (40)

One instance of uninflected <u>be</u> occurs with apparent habitual meaning. The seventy year old, when asked if he had any relatives in the United States, replied:

<u>By the radio I bees hearin' nowadays plenty family I have out from here . . . but not because I know that . . .</u>

In all of the above characteristics of the copula system, Samaná English and BE are quite similar. The younger informant produces what appears to be the contraction I'm before the past participle been, in a manner which suggests that in his idiolect /ʌm/ is classified as a lexical variant of the subject pronoun I. In answer to the question Have you ever been to the States? he replied:

> No. I'm never been. I'm been in Porto Rico, I'm been Miami, I'm been Spain because I belong to the Navy, you know, Dominican Navy, and, I'm been in the Guantanamo base, but, uh, I'm got some brothers over there New York . . .

Previous suggestions that /a/ and /ʌm/ function in BE as variants of the first person singular pronoun (Stewart 1966; Dillard 1972) have been met with scepticism; the best known example being Labov's (1972) review of arguments for and against an underlying copula in the structure of BE sentences. The case for lexical variation rests primarily upon the fact that speakers who frequently produce "zero" variants of is and are after other pronouns normally produce I'm /ʌm/, That's /ðæs/, it's /is/ and what's /hwʌs/ before predicate noun phrases, adjectives, locatives, Ving constructions, etc. It receives additional support from occasional observations of North American children using /ʌ m/ as a subject pronoun, e.g.: /ʌm iz ə kawboy/ 'I'm a cowboy.' Similar usage has been attested by adult speakers, i.e.: /iz ðæs ə fak/ 'Is that a fact?' uttered by a gentleman in his fifties or sixties in Berkeley, California in 1981. While such observations may be dismissed as typical or marginal behavior limited to a few among the very young and very old, the Samaná data provide the most solid evidence yet of the lexical alternation between I and I'm as a firmly established dialect feature.

The classification of I'm, that's, it's and what's as subject pronouns not only facilitates the description of BE as a dialect in which the copula is normally realized as "zero" after pronoun subjects: it also is supported by the intuitions of native speakers of BE. In a paper now in progress on basilectal BE and the creolist hypothesis of the origin of BE, I discuss the question of the nature of BE subject pronouns at length, taking into account the Samaná data.

The main purpose of the present paper has been to demonstrate that Samaná English resembles modern BE but appears archaic in comparison. The inference that an archaic variety of BE was spoken by the original settlers, and by their counterparts whom they left behind in Philadelphia, Baltimore and thereabouts, is consistent with the hypothesis developed in DeBose in progress; i.e.: that BE is the result of continuing decreolization of a North American basilect distinct from Gullah or "Plantation Creole" (Dillard op. cit.) which has always been highly decreolized in comparison to the English-derived creoles of West Africa and the Caribbean.

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Agentivity and Causation: Data from Newari

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This paper represents part of an investigation into
the nature of agentivity and its status as a universal
syntactic and semantic category (see also DeLancey 1982a,
b, to appear a,b). Evidence has been accumulating for
some time now that the term agent, as generally used in
linguistics, applies to a multifactoral concept, and that
in the real world which language must report on the
various factors can occur independently (see e.g. Cruse
1973, Lakoff 1977, Hopper and Thompson 1980). I will be
reporting here on three morphosyntactic constructions in
Newari[1], a Tibeto-Burman language of Nepal, each of which
describes some sort of causal relationship between an
entity and an event. Our interest is in identifying the
distinctions among causal events which are sufficiently
salient to merit differential morphosyntactic coding.
The results of investigations such as this will be the
material from which we will eventually construct a gen-
eral cross-linguistic classification of types of caus-
ation and their relationship to the category of agentiv-
ity.
    The most obvious construction of the sort described
above is the simple transitive clause. Newari is an[2]
unusually consistent ergative language; transitive sub-
jects are marked with a postposition which occurs as -ṍ
following a stem-final consonant, and -nṍ (in normal
speech usually reduced to nasalization on the preceding
vowel) after final vowels. Nearly all verbs are lexic-
ally transitive or intransitive, though I have found one
labile verb (tɔja-e-gu 'break (tr. or intr.)'). Nearly
any simple transitive clause can be contrasted with a
corresponding clause with an intransitive verb and the
'agent' marked by both ergative case and a following
-yana, as

    1) harsa-nṍ wo misa-yatɔ siat-ɔ
        H. -ERG   the woman-DAT kill-PERF
        'Harsha killed the woman.'

    2) harsa-nṍ-yana wo misa   sit-ɔ[3]
                                 die-PERF
        'Because of Harsha the woman died.'

The glosses, which are my informant's, suggest the nature
of the semantic distinction coded here, but before we
attempt to explicate it we will need to examine some
contextual data. My informant suggested as contexts for

(2) situations in which the woman is clearly dangerously
ill and Harsha delays calling a doctor until too late, or
in which the woman had had a weak heart and Harsha brought
her news which affected her sufficiently so as to precip-
itate a fatal heart attack. These have in common an inter-
mediate event in the chain of causation leading from some
act of Harsha's to the death of the woman, and this I will
suggest is the central meaning of the -yana construction.
(1) is impossible in either of these contexts, which sug-
gests that part of what is involved in agentivity is dir-
ectness of causation (cf. Givón 1975). Inevitably, how-
ever, the matter is more complicated. (1) is a possible
description of an event in which a physical assault by
Harsha precipitates a fatal heart attack in the victim.
We still have a case of mediated causation, but we have
added an element of intention on Harsha's part. That
this qualifies the event for coding in a transitive clause
is consistent with the widely-noted relationship between
intention and agentivity. (This recalls the legal prin-
ciple described by Fillmore (1977) according to which one
who intends to commit murder and accidentally kills the
wrong victim is nonetheless guilty of murder. Note that
Harsha need no have intended to kill the woman in order
for the event to be describable by (1) -- it is enough
that he intentionally inflicted harm which eventuated in
her death).

        To complicate matters further, we can show that
intention is not a necessary prerequisite for (1). Con-
sider the class of events in which Harsha is driving a
car which strikes the woman and kills her. If the accid-
ent is entirely the woman's fault, then neither (1) nor
(2) is appropriate. If the accident required negligence
on the part of both Harsha and the victim, (2) but not (1)
is possible. However, if the accident is clearly Harsha's
responsibility -- suppose that he was drunk and driving
fast and erratically down a crowded street -- then (1)
is a more appropriate description. The distinction here
is between shared and sole responsibility -- Harsha is
more of an agent if he alone caused the event, even if he
had no intention of doing so.

        The above data constitute evidence for the proto-
type approach to transitivity and agentivity suggested by
Lakoff (1977) and implied in Hopper and Thompson 1980.
The prototypical transitive event has as agent an inten-
tionally acting entity whose act is the sole and immediate
cause of the event. Deviations from this prototype with
respect to any of these parameters remain legitimate, if
less than perfect, transitive clauses up to a certain
(obviously not clearly determinate) point. However, too
much deviation from the prototype, in the form of inter-
mediate causes, shared responsibility, lack of intention,

or some combination of these, disqualify an event for the transitive category, and require a different coding.

There are two structural differences between the simple transitive and the -yana constructions -- the transitivity of the verb, and the presence of extra morphological material, which we have so far treated as a postpositional NP marker. Since the presence of this extra material correlates with the presence of extra 'distance' between cause and effect, we have here a degree of iconicity of meaning-form mapping of the sort discussed in e.g. Givón 1979. In this context we can adduce further information about the nature of -yana which will constitute an interesting study in grammaticalization. -yana is transparently (to the informant as well as the investigator) a participial form of the verb yat- 'do'. The -yana sentences can thus be analyzed as structurally isomorphous with the very common participial embedding construction exemplified in (3):[4]

3) bə̃duk-ya goli   la-na   wo misa sit-ɔ
   gun-GEN bullet get-PART the woman die-PERF
   'Having gotten (hit by) a bullet from the
   gun, the woman died.'

This construction is frequently used when a causal relationship is being attributed to the two clauses, as in (3), but the existence of such a relationship is a matter of pragmatic inference and not a part of the essential meaning of the participial chain. All that is asserted is temporal succession,[5] as can be seen from an example like (4):

4) misa   la na-ya   won-ɔ
   woman meat eat-PART go-PERF
   'The woman, having eaten the meat, left.'

where there is no asserted or implied causal relation between the two clauses.

This suggests an analysis of a sentence like (2) as meaning something like 'The man having done something, the woman died'. (Note that we now have at least a historical explanation for the ergative case marking on -yana-marked NPs). This would provide a neat account of the 'mediated causation' sense of this construction, which could now be seen as even more clearly isomorphic with the structure. We could then describe a semantic continuum coded by sentences like (5-7):

5) kica-nõ wo misa-yatɔ siat-ɔ
   dog-ERG the woman-DAT kill-PERF
   'The dog killed the woman.'

6) kica-nɔ̃-yana wo misa sit-ɔ
   dog-ERG-YANA the woman die-PERF
   'Because of the dog the woman died.'

7) kica-nɔ̃ ɲa-na wo misa sit-ɔ
           bite-PART
   'The dog having bitten her, the woman died.'

(5) makes no mention of any intermediate proposition, treating the overall sequence of events which begins with the dog's biting and ends with the woman's death as a single unitary event. (7) traces the sequence of events in some detail, explicitly mentioning the bite ---- die sequence. (6) is intermediate between the two, in that it makes explicit reference to an intermediate event, but doesn't overburden the discourse by describing it. The biting, which is definitely backgrounded in (7), is suppressed even more. Thus the sentence focusses on the woman's death and the dog's responsibility for it, minimizing the salience of the intermediate stage without completely ignoring it.

There are, however, reasons to believe that this account is too simple and straightforward to correspond to the actual facts of the grammar. To begin with, the -yana clauses which we have looked at so far would in fact be ungrammatical if they were interpreted as containing ya-na 'having done.' yat- is a transitive verb, and must have either an explicit or an anaphorically recoverable object. We can have a main clause with finite yat- and no explicit object, as in (8):

8) misa-nɔ̃ yat-ɔ   'The woman did it.'
   woman-ERG do-PERF

But this is grammatical only as an answer to a question like 'Who finally fixed it?' We can also have true participial constructions with yat-, as in (9):

9) wo misa jya ya-na chẽ-ẽ won-ɔ
   the woman work do-PART house-to go-PERF
   'The woman, having done the work, went home.'

However, in the -yana sentences of the type we have been examining, there is no explicit or recoverable object for -ya-na if we choose to interpret it as yat-. Thus, if we choose this analysis of -yana, we will need to mention in the grammar that in this use it has idiosyncratic syntactic behavior -- i.e. we will have to note that -yana in this construction has been at least partly grammatical-ized.

More significant and interesting evidence for the same con-clusion is to be found in the semantics of -yana. Note that while the participial analysis neatly predicts the 'mediated causation' sense of the construction, which is its sense in most of the sen-tence-context pairs spontaneously supplied by my informant, it does

directly predict its appropriateness for the other contexts in
which we have seen that it can be used. In fact, we can find
examples of -yana where there is no plausible interpretation which
accords with the literal sense of 'do', such as (10):

> 10) dhewa-nɔ̃-yana wo misa sit-ɔ
>   money
>   'Because of money the woman died.'

The appropriate context for this sentence is one in which the woman
was killed in a quarrel over money. Now, in this case the -yana
still has a version of the mediated causation sense -- the money is
the ultimate cause of death, but the effect is brought about by an
action performed by someone else. However, we cannot give -yana
here the literal sense of 'do', for the money in fact does nothing.
     Thus we find that, while the origin of the -yana construction
remains synchronically transparent, it has acquired both syntactic
and semantic idiosyncrasies which require special description -- i.
e. it is no longer describable as a straightforward case of the
participial construction. (It is worth noting that my informant,
an intelligent and linguistically sophisticated individual who early
in our work developed a keen interest in and talent for explicating
Newari syntactic structure for me, prefers to translate -yana as
'because of', while with ordinary examples of the participial con-
struction he much prefers English translations with participial
subordinate clauses.) Thus we have here an example of the diachr-
onic process of grammaticalization in its early stages. A construc-
tion which originally (presumably) made explicit (but **not** specific)
reference to an intermediate event in a causal chain is being
bleached of concrete reference and becoming an abstract marker of
reduced transitivity.
     I should reiterate here that, even if we were to analyze -yana
as a monomorphemic postposition synchronically unrelated to the verb
yat-, there is still some degree of iconicity in the mapping between
meaning and form, in that deviation from the prototypical transitive
schema is reflected in deviation from normal transitive syntax.
There is, however, no need to make so drastic a decision; there can
be no doubt that speakers of language are capable of recognizing
both the similarities and differences between -yana and ya-na.
     In this connection it is worth a detour to consider another
construction based on the verb yat-. Newari, like many languages,
has a contrast between a morphological and a periphrastic causative
construction, the latter constructed with the verb yat-.[6] This can
be seen in examples like (11-12):

> 11) wo misa-nɔ̃ məca-yatɔ chahi-kal-ɔ
>   the woman-ERG child-DAT walk-CAUS-PERF
>   'The woman walked the child.'

12) wo misa-nõ   məca-yatɔ chahi yat-ɔ
                              walk   do-PERF
     'The woman made the child walk.'

The semantic difference, which is imperfectly captured by my English
glosses, is again direct versus mediated causation. In (12) the
woman orders the child to walk and he does so. Notice that the
child retains some control; the woman's act, a command, must induce
in him a disposition to walk, which in turn is the cause of his
doing so. (11), in contrast, describes unmediated causation; the
woman is dragging the child down the street, or perhaps manipulating
his legs for him. This is, of course, a special case of the same
distinction which we have already attributed to the alternation
between -yana-marked and ordinary transitive agents. I do not see
any point in trying to link these two constructions (i.e. the yat-
causative and the -yana 'agent') syntactically (though it would not
take too much ingenuity) but we may remark, first that the
semantic values of the two causative constructions are predictable
according to the same line of argument which I have suggested in
connection with the iconicity of the -yana construction (this is
the typical semantic contrast associated with this morphosyntactic
contrast; cf. Givón 1979), and further that the existence of this
alternation of causative constructions and the nature of the seman-
tic contrast coded by it no doubt reinforce the semantic value of
-yana. (Arguments of this type have been convincingly developed
within Langacker's Space Grammar framework; see e.g. Tuggy 1982).
Indeed, it may be that the existence of the yat- causative helps
to keep alive in the grammar the association between -yana and yat-,
and thus retards the process of grammaticalization.
    There is another construction which we need to consider here,
one which poses somewhat stickier problems than does -yana. To
set the stage let us note that in Newari, as in many other languages
of the area, ergative and instrumental case are marked identically:

13)   misa-nõ bə̃duk-ɔ̃ kica siat-ɔ
      woman-ERG gun-INSTR dog kill-PERF
      'The woman killed the dog with a gun.'

They may be distinguished by eligibility for transitive subject,
which is restricted to ergative NPs:

14)   *bə̃duk-ɔ̃ kica siat-ɔ
      gun-INSTR dog kill-PERF

(Note that (14) is grammatical if interpreted as having an anaphoric
zero agent, '(S/he) killed the dog with a gun'. It would also, of
course, be grammatical in a fantasy about a magical gun possessed
of volition). Instruments are also ineligible for the -yana con-
struction:

15) *bə̃duk-ɔ̃-yana kica sit-ɔ
    gun   dog   die-PERF

(But cf. ex. (10); with a human patient (15) would be acceptable in
the sense of 'killed over a gun'). This restriction is consistent
with the mediated causation interpretation of -yana, since a true
instrument is by definition the most immediate cause of the effect
on the patient; what qualifies it as an instrument is that it is
not also the ultimate cause.
    There is a class of sentences in Newari in which this ergative/
instrumental case form occurs with an intransitive verb:

16) bikh-ɔ̃   wo misa sit-ɔ ⁷
    poison-ERG   woman   die-PERF
    'The woman died from poison.'

All of the examples which I have found of this can also be put into
ordinary transitive form with no apparent change of meaning:⁹

17) bikh-ɔ̃   wo misa-yatɔ siat-ɔ
    poison-ERG   woman-DAT   kill-PERF
    'Poison killed the woman.'

(It is clearly relevant, though somewhat difficult to interpret,
that in many cases my informant describes a sentence like (17) as
somehow 'personifying' the poison, though not so much so as to make
the sentence inappropriate for ordinary discourse). Most of them
can also be recast as -yana sentences, again without apparent seman-
tic effect:

18) bikh-ɔ̃-yana wo misa sit-ɔ
                        die
    'Because of poison the woman died.'

There is an apparently systematic set of exceptions to this last
statement, however; disease and diseases are eligible for both the
ordinary transitive and the ergative intransitive constructions,
but not for -yana:

19) jɔr-ɔ̃   wo misa-yatɔ siat-ɔ
    fever-ERG   woman-DAT   kill-PERF
    'Fever killed the woman.'

20) jɔr-ɔ̃   wo misa   sit-ɔ
    fever-ERG   die-PERF
    'The woman died of fever.'

21) *jɔr-ɔ̃-yana wo misa sit-ɔ

As these examples suggest, this construction is limited to inanimate causal entities, which bring about their effect without acting. More typical agents cannot occur in this construction:

22) *kica-nɔ̃ wo misa sit-ɔ
        dog

Nor can instruments:

23) *bə̃duk-ɔ̃ wo misa sit-ɔ
        gun

The latter restriction shows that it is not animacy per se which determines eligibility for the ergative intransitive construction, but the distinction between active (prototypically, moving) partic- ipants in the event and inactive entities which somehow produce their effect simply by being in the right place at the right time. This is reminiscent of the role of 'money' in ex. (10), but 'money' cannot occur in the ergative intransitive:

10) dhewa-nɔ̃-yana wo misa sit-ɔ
        money   the woman die-PERF
        'The woman died because of money.'

24) *dhewa-nɔ̃ wo misa sit-ɔ

This suggests that the ergative intransitive construction asserts direct rather than mediated causation. This conclusion may be further indicated by the fact, noted above, that when the -yana construction equivalent to an ergative intransitive sentence is possible, it is difficult (indeed, so far impossible) to find a con- text in which one but not all of the three alternatives is acceptable. I suspect that this is because the chain of causality connecting something like poison with its effect is completely imperceptible, making it pragmatically impossible to identify it as direct or med- iated causation, and thus neutralizing the distinction.[8]
        While I have referred to the construction exemplified in (16) and (20) as "ergative intransitive", thus suggesting the identific- ation of the NPs in question as ergative, this analysis is obviously problematic. I have made the identification on the grounds that 'poison' and 'disease' play the same role in the events described by the ergative intransitive sentences, (16) and (20), and the ordinary transitives (17) and (19), and because ordinary instruments cannot occur in the ergative intransitive construction. There is, of course, something odd about identifying an argument in a formally intransitive clause as ergative. It is likely that this too is a false issue, and that we should regard the ergative/instrumental morpheme as three ways polysemous rather than two.[9]
        While the morphosyntactic analysis of this construction is difficult, its functional analysis is clear. Those events which are eligible for the ergative intransitive construction deviate

from prototypical transitivity in lacking an **an agent**
which **performs** a perceptible action. This is a different
kind of deviation from the prototype from that typically
associated with the -yana construction, but it is in its
own way a significant enough deviation to merit distinct
morphosyntactic coding.[10] (It follows from this that the
fact that the -yana construction can be used in some sit-
uations which normally call for the ergative intransitive
is further evidence that -yana is generalizing from its
original restricted sense into a marker of reduced trans-
itivity of whatever kind.)

Notes

1)   My informant for Newari is Mr. Harsha Dhaubhadel,
who grew up in a small Newari-speaking settlement in a
primarily Maithili-speaking area near the Indian border.
His speech belongs to the Katmandu dialect group.
2)   In this informant's speech transitive subject lacks
ergative case in some progressive and some first person
subject clauses. The conditions which determine case
marking in these situations are not yet clear.
3)   For purposes of clarity I write ergative case every-
where as -nõ, although when followed by -yana it would al-
most always be realized as nasalization on the stem-final
vowel.
4)   Sentence (3) was volunteered by the informant in the
course of the investigation described in the preceding
pages. I asked him whether exx. (1) or (2) would be ap-
propriate in describing a situation in which Harsha is
firing a gun and the woman walked into his line of fire.
Neither (1) nor (2) is possible in that context, which
requires fuller explanation. Thus we can see that major
deviations from prototype cannot be described by reference
to the prototype, but require explicit description.
5)   There is another participle form, identical to this
but with a nasalized final vowel, which indicates that
the two clauses take place simultaneously.
6)   There is at least one additional causative construc-
tion which I have yet to investigate.
7)   As with all of the examples in this paper, the order
of the NPs is pragmatically determined and is irrelevant
to our present discussion.
8)   I have a persistent feeling that the fact that dis-
eases occur in the ergative intransitive but not the -yana
construction is relevant here, but as yet I cannot say
how.
9)   I use the term 'polysemous', and refer to 'the'
ergative/instrumental morpheme, advisedly, for reasons
which should be obvious (see DeLancey 1981). Space lim-
itations prevent me from developing the argument here.

10)   As pointed out to me by F.W. Householder, a similar situation obtains in English, where one ordinarily <u>dies</u> <u>of</u> disease, hunger, etc., rather than being killed <u>by</u> them.

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On Drawing the Right Conclusions in Psycholinguistics:
Some Critical Areas of Control in Experimental and Analytical
Practice

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For the past decade or so I have been involved in a research program concerned with the experimental testing of linguistic rules, particularly in phonology (and morphophonology). Two quite distinct interpretations of the notion "linguistic rule" should perhaps be distinguished at the outset. The first, oldest and most familiar one views a rule as any *regularity* which can either be extracted from or imposed upon a sample of transcribed utterances in some language. This is the notion of rule with which linguists have been, until relatively recently, at least, almost exclusively preoccupied. The second interpretation of the notion of "rule" in linguistics refers to some generalization internalized by speakers in the process of language acquisition and which plays some role in the speaker's use of the language, i.e., in the production and comprehension of at least some of his utterances. It is important to understand that a relation of very uncertain relevance exists between these two fundamentally different notions of "rule," and the reason for this seems obvious enough: the mere discovery (or invention) of a regularity by the linguist with respect to some particular linguistic corpus provides no guarantee whatever that this same regularity has been similarly identified, extracted or invented by the ordinary· language learner (i.e., as Kiparsky has put it, "the fact that a generalization can be stated [is not] enough to show that it is psychologically real" (1968, p. 172).[1] Moreover, the proliferation of linguistic "schools" over the past several years, even within the basic framework of generative grammar, has made it increasingly obvious that any linguistic phenomenon can be quite correctly described (i.e., in a way that is in complete accord with the available linguistic facts) in a dizzying multitude of ways, depending on the initial theoretical assumptions of the particular fellow who happens to be doing the describing. Thus, as Y.-R. Chao put it so well nearly 50 years ago, "[D]ifferent [linguistic] systems or solutions are not simply correct or incorrect, but may be regarded only as being good or bad for various purposes" (1934, p. 363). One cannot therefore in any sense "demonstrate" knowledge of any particular formulation of a rule simply by analyzing adult speech data in terms of that rule, and "maximum generality" is no less arbitrary a decision criterion than any number of others that might be formulated and employed (cf. Derwing, 1974). In this context, therefore, the appropriate course of action seems clear enough, if linguistics is really serious about extending its efforts beyond the bounds of an arbitrary descriptive science and if linguistic theorizing is to develop on the basis of a firm empirical underpinning: we must *first* seek out means of discovering which rules/regularities have *in fact* been learned or acquired by speakers and proceed from there.

What, then, might be a suitable test for knowledge of a linguistic rule? One promising line of inquiry has been suggested by the long tradition of association in linguistics (from Humboldt to Paul to Jespersen to Bloomfield to Chomsky; see Derwing *et al.*, 1979, pp. 4–5) between the concept of rule and the phenomenon of linguistic "creativity." The suggestion is, of course, that it is precisely the knowledge of a rule that enables a speaker to deal appropriately with new or novel linguistic forms. To *test* for knowledge of a particular linguistic rule, therefore, we need to look at more than familiar speech forms as used under ordinary circumstances of language use, as any number of these might just as well have been learned by rote. What is required is supplementary information about how the language user manipulates *unfamiliar* forms

that are *novel* to his linguistic experience.[2]

An early and familiar experimental advocate of this particular route to the testing of rule knowledge was Jean Berko (1958). Her approach was to assure novelty by inventing a variety of nonsense words, which were subsequently administered to a sample of child and adult subjects in syntactic frames deemed appropriate to elicit such inflected forms as the plural and possessive of nouns, the past tense of verbs, etc. Her findings are well known: she demonstrated quite conclusively that her subjects could properly and consistently inflect forms that they had surely never been exposed to previously, and on this basis it seemed quite safe to conclude that *some* kind of rule(s) had been learned which enabled them to do this.

Berko also, however, left quite a number of interesting questions unanswered by her study. What *particular* rule had been learned, for example (see Derwing 1979 for a discussion of a number of alternative hypotheses all fully consistent with Berko's findings), and how did this rule develop in the child over time? Not only did Berko deal with a very narrow age range in her child subjects, she also employed only a tiny sampling of the possible range of nonsense stem-types. (On the plurals test, her largest, for example, only about a third of the possible stem-final phonemes were represented, and no stem-final consonant clusters were included at all). Subsequent researchers also contributed relatively little towards the filling of either of these gaps. The very first experimental project I became involved in, therefore, was intended to supply answers to such questions as these. (Small portions of this research have now been reported in the literature and we now know at least some of the answers. For details, see especially Derwing & Baker 1979.)

The most ambitious study connected with this project was one that I began as long ago as 1972. In that study I constructed two sets of 100 mixed nonsense and real stems to be used with each of five different inflections and administered them to 112 subjects evenly distributed over the age range from 3 to 9 years. Ignoring ancillary IQ and other control data also collected, this eventually yielded a total of 56,000 verbal responses, and it took more than a year simply to collect them. For the remainder of this paper, I want to try to explain the main reasons why, even after all this time, the bulk of these data still lie undisturbed in a departmental computer file.

From almost the beginning, this line of research was beset by a number of major challenges, both externally and internally generated. In the former category we encounter first the "logical loophole" argument. This amounts essentially to a claim that behavioral, experimental evidence cannot in principle be used in order to falsify any particular linguistic hypothesis. The argument rests on the distinction (made most forcefully by Chomsky 1965) between language knowledge ("competence"), which is the subject of the linguist's main concern, and the psychological mechanism(s) by which this knowledge might be said to be put to use in actual linguistic behavior ("performance"). The form of the argument goes something like this: if C represents some claim about "competence" and P is a hypothesis (usually unspecified) about "performance," then B, any particular instance of elicited linguistic behavior, is a prediction based on the conjunction of C and P. It is thus only this *conjunction* that may be confirmed or disconfirmed by means of any experimental test (i.e., to put it directly, C can always be preserved in the face of negative evidence by attributing the difficulties to P). On this basis Kiparsky has argued that psycholinguistic experiments can "constitute evidence in one direction only – a positive result will confirm the psychological reality of the tested grammatical rule, but a negative one does not disconfirm it" (1975, p. 203).[3]

Clearly, however, this line of argument is an indictment not of

experimental psycholinguistics, but rather of the kind of linguistic theorizing that is permitted to invoke it. One simple illustration (originally suggested by D. Broadbent, I am told) should suffice to demonstrate this. Suppose we find some child who is quite adept at basic arithmetic. One possible hypothesis about the "competence" thought to underlie this skill might be to attribute the child not with something so mundane as a learned, laborious, step-by-step procedure for carrying out simple arithmetic operations, but rather with knowledge of number theory. And what if experimental results are found that seem to fly in the face of this hypothesis? Just chalk them up as "performance errors" and the well-favored theory remains inviolate. To invoke this kind of "loophole," therefore, is quite obviously to do something that does not at all enhance the scientific status of the discipline. It is simply not credible to insulate serious scientific hypotheses in this way from the possibility of empirical disproof, and what is required, clearly, is an integrated performance model whose components are all equally susceptible to the same prospect of being demonstrably wrong.

The problem has arisen historically, of course, because linguists have traditionally been concerned primarily with describing the form of utterances, especially (in more recent years) in terms of the possible relationships between one set of linguistic objects or structures and other such sets. This kind of purely descriptive work was being carried out long before any serious concerns were raised about such matters as the "psychological reality" of the formulations, much less questions about how such "purely syntactic" descriptions might be integrated into any kind of workable performance model. When the basic (generative) theoretical and descriptive framework was originally conceived, therefore, not only were such obviously critical factors as discourse context and pragmatics excluded from consideration, but even meaning in general (cf. Chomsky 1957). It comes hardly as a great surprise, therefore, that, as Bresnan has recently observed, "Despite many...expressions of hope by linguists, and despite intensive efforts by psycholinguists, it remains true that generative-transformational grammars have not yet been successfully incorporated in psychlogically realistic models of language use" (1982, p. xvii). A model that is originally designed to do one thing is not likely to prove very effective for doing something radically different, and any attempts to force-fit the model to a task for which it is not well suited are bound to be strained, artificial, and, in the end, unsuccessful (see Derwing & Baker 1978 for further discussion).

Leaving behind this ultimately self-defeating mode of objection to experimental work, therefore, let us now move on to a more substantive and important line of criticism. From the standpoint of methodology, for example, the most serious and potentially most devastating of these challenges is certainly the one that bears on the question of the *validity* of the results that have been obtained using the particular experimental technique under discussion here. In 1977, for example, Kiparsky & Menn raised the issue of what they called the "strangeness effect" (p. 64). This criticism was based on research conducted by Haber (1975), which yielded a rather bewildering array of bizarre responses by adults in a Berko-type pluralization task (e.g., the plural of [blif] is [blayvz], etc.), clearly errors of a type which do not commonly occur in spontaneous speech. I replicated Haber's study with a sample of 48 adults from a variety of different educational backgrounds and it was quite true: though the vast majority of the adult subjects performed as expected, the adult responses overall were nonetheless decidedly inferior to those obtained from my older child subjects, who generally performed in almost exact conformity with the supposed "adult rule" (i.e., /-Iz/ after stem-final sibilants, /-s/ after voiceless non-sibilants, and /-z/ elsewhere). The main thrust of of the Kiparsky-Menn challenge was, of course, the suggestion that the experimental situation itself (involving the rapid-fire eliciting of

responses to a long list of pictures of "nonsense" creatures and actions) was producing results which did not accurately reflect the (adult) subjects' true capacities.

I had, however, noticed very little evidence of this "strangeness effect" with any of my child subjects, and there was, after all, a perfectly straightforward potential explanation for this. Surely the adults knew perfectly well that the pictures and verbal stimuli that they were being asked to respond to were all nonsense, so perhaps, for at least some of these subjects, a nonsensical response seemed called for. For the children, on the other hand, who were quite accustomed to encountering new and unfamiliar words on a regular basis, a corresponding reaction was to be much less expected. But it was important to know for sure, since it was a validity issue that was at stake here. Moreover, it was indeed hard to quarrel with Kiparsky on the point that "the more the experimental design is made to approximate normal linguistic behavior, the more reliably can the results be used for linguistic conclusions" (1975, p. 202).[4]

At my urging, therefore, a "validity check" was subsequently conducted by Rollins (1980), in which results in a Berko-type task were compared with those obtained in a naturalistic "free play" situation. Twenty subjects were involved in this study, ranging in age from 3 to 6 years, with five children in each age group, and three separate testing situations were utilized. The first and third tests were both Berko-type tasks, in which the subjects were individually presented with pictures of six real and eight imaginary animals, the latter consisting of a set of nonsense stem-types that the prior studies had shown to be representative. The second test, conducted a week after the first and one day before the third, involved the introduction of the subjects to a set of large, brightly colored stuffed animals which corresponded to the pictures seen in the first Berko test. The examiner pointed out the names of each of the animals and continued to play with the children (two at a time) until she was confident that the children knew all of the names and could pronounce them correctly. At this point the children were allowed to play with the animals on their own and as they saw fit (some version of a "race game" turned out to be the most popular kind of activity), while the examiner merely stood by and recorded the plural forms as they were spontaneously produced. (Prompting occurred only rarely, as when some particular pair of animals had been ignored and the game seemed to be winding down. At this point the examiner would say something on the order of "What about these animals. Don't you want to play with them, too? They're all alone." With this approach, plural forms for all of the items were quite readily obtained.)

The results of Rollins' study are summarized in Table 1 below, which shows the number of "correct" responses by each subject in each of the three tasks. An analysis of these results showed that only the factor of age was significant, as expected; scores were not significantly different as a function of the test type (i.e., Berko vs. the "free play" situation). For children, therefore, the results were solid, and non-validity arguments based on the "strangeness effect" seemed to be quite without force in this instance.

Or so, at least, I might thus far have led you to believe. In fact (as perhaps some of you may have already noticed), I have engaged in a sort of statistical sleight of hand of a kind not uncommonly employed in psycholinguistic research generally, and especially in developmental work. Specifically, I have employed the logical fallacy of using *between-subject* data to draw *within-subject* conclusions. Suppose, for example, that two subjects were to take the same ten-item True/False test and that the first scored correctly on items #1-6, the second on items #5-10. Both subjects would score 60% overall and might on that basis - following quite standard contemporary analytical practice - be grouped together as

"equivalent" performers, despite the fact that that they scored the same on only 20% of the items, viz., items #5-6!) It is important to notice that a version of this same perverse logic has already been presented to you in the form of Table 1. Look again at the results for Subject #1 on the three tests: 6/14 correct on Berko-1, 7/14 on the Free Play task, and 6/14 correct on Berko-2. Is this really "equivalent" performance on the three tests? Perhaps, but *only* if the correct responses were generally obtained on the *same items*. If it were the case instead that this subject performed correctly on items #1-6 on the two Berko tests, say, but on items #8-14 in the free play situation, he would be performing about as *differently* in the two situations as was theoretically possible to do, and just the *opposite* conclusion would have to be drawn to the one I drew in the immediately preceding paragraph. Fortunately for my argument, when we look at Rollins' results on an item-by-item basis, we actually do find the pattern I earlier tried to convince you (on the basis of quite insufficient evidence) was in fact the case, namely, that 6 of the 7 correct responses in the free play task were obtained on the same 6 items that were performed correctly on both Berko tests; in fact, even the *incorrect* responses by this subject proved to involve errors of precisely the same kind across the board. So this subject in fact performed identically on 13 of the 14 items on all three tests, and quite similar patterns held for the other subjects, as well. I may then stick to my original conclusions, after all, though not on the basis of the data as originally presented. By the same token, however, Berko's "% correct" figures don't really tell us very much, when we get right down to it, and she gives us no further details about how consistently her subjects performed across her items. Hence the replications.

There still remains a rather disturbing fly in the ointment, however. What, for example, is meant by the term "correct" response in the discussion above? For Berko, who used an "adult standard" (1958, p. 158), this meant "responding as some adult did." But we already know that adults perform quite erratically on this type of task (and Berko, too, found some measure of variation in her adults' responses). With this approach, what is "correct" will therefore vary as a function of the particular adults tested, yielding in the end a "standard" which is far too unstable to be considered a reasonable standard at all. In my own early work, therefore, I adopted an admittedly arbitrary standard along the following lines: for the nonsense (and real, regular) items, "correct" was taken to mean "in accord with the general rule" (as described a few paragraphs above), while real, irregular forms were evaluated according to the preferences indicated in some standard dictionary (see Derwing & Baker 1977 for details). This fixes the standard, at least, but is it a *good* standard? There are some compelling reasons to suspect not. If we seek to understand the course of rule-learning in the child's development and the various factors that bear upon it, presumably we are not so much interested in comparing the child's performance at each stage with the adult's rule (even if we knew for certain what the form of that rule was), but we are rather more concerned with the question of how the child interprets the data for *himself*. That is, since learning a rule presumably involves making a generalization that cuts across an entire class of forms, it is critical that we discover which forms the child groups together (i.e., which ones he treats *in the same way*) at each stage, whether these groupings conform to the (presumed) "terminal state" or not. In short, it is the *within-child* patterns of responses, once again, that are of crucial relevance here.

A third problem, finally, associated with the analysis of developmental data is the real cruncher: how are subjects to be partitioned into homogeneous "strategy groups" in the first place? This problem has plagued language acquisition studies from the beginning and falls under the general rubric of the "stage" controversy. How does one define a

discrete "stage" and how many of the beasts are there? How different (similar) does the performance of two subjects have to be in order for us to say that they are operating at a different (the same) level? And how can the investigator ever hope to identify the critical patterns or strategy differences (similarities) among his subjects unless he first has some reliable basis for grouping these subjects for purposes of comparison? It was largely this problem, therefore, that was responsible for the long delay in doing anything with the massive corpus of data described early on in this paper, for there was simply no way to deal with it all unless some non-arbitrary means could be found to tell us which subjects belonged with which. Berko herself avoided the problem by simply pooling all of her data and trying to draw some general conclusions about "children" (despite her finding that there were significant differences between her pre-school and first-grade subjects). The standard approach of course, has been to group subjects according to age categories, though it is well known that this is a generally quite unreliable procedure. Age is invariably correlated significantly with performance in developmental work, of course, but the magnitude of the correlation is often far too low to yield much clear evidence of the operative developmental trends (see Innes 1974 for a good illustration of this.) And we have no reason at all to believe, of course, that developmental stages click off in precise synchrony with the motion of the earth around the sun. The currently popular adoption of the "MLU" standard, finally, while it indicates some improvement over the age criterion, at least, is certainly also far from satisfactory. (Does "Stage III" *really* begin at MLU=2.75 and end at MLU=3.50, as suggested by Brown [1973, p. 56]? The arbitrariness is apparent. See also the extended critique of this measure by Crystal 1974.)

To make a long story short, a significant breakthrough may at long last be at hand. Thanks largely to the inventive genius of my psychologist colleague, William J. Baker, we now have available at Alberta an analytical technique that seems to circumvent all three classes of difficulties just outlined above. The procedure focuses on within-subject response patterns (specifically, on co-occurrences of responses among test items) and it utilizes an original development of the statistical technique known as "hierarchical cluster analysis" to establish both the grouping of the subjects and (within bounds) the number of groups involved. (It can also be utilized to effect an analogous grouping of the test items, once the subject-groups are first identified.) This work has already been described in some detail elsewhere (see Baker & Derwing 1982ab) and time does not permit even a cursory explication here. Suffice it to say for the moment that after a careful evaluation of the technique as applied to familiar data, we now feel justified in extending it to the data from the large inflections study already alluded to, and Figure 1 below represents the first analysis to come out of that effort. It shows the partitioning of the 112 subjects into five distinct strategy groups, based on their performance in a Berko-type pluralization task involving 59 common nonsense stems. Both an age trend and a correlated systematic "honing" of the relevant stem-classes (clusters and all) as subjects progress up the scale provide strong presumptive evidence that a "stage" interpretation is appropriate for these groups. The results so far, in fact, could hardly be more encouraging. (But that, too, must be the topic of another paper.)

Footnotes

[1]Contrast Valian, who blandly asserts that the grammar (whose? which one?) is psychologically real "by definition" (1976, p. 64)!

[2]Kiparsky, by invoking the competence/performance distinction to be discussed further below, makes the claim that, for some rules, at least,

"not even partial productivity would be a necessary criterion for psychological reality" (1975, p. 198). If so, the problem still remains to find some (other) test to determine whether or not any particular formulation of such rules is in fact part of a speaker's knowledge. Claims about knowledge are empirical, psychological claims and for them to be of any scientific interest whatever, they have to be tested (and hence testable). So if Berko's test won't do, then other tests have to be devised.

[3]In my original abstract I also promised to discuss here an experiment which could beat this logic on its own terms, namely, by testing two rules of the same putative type in precisely the same experimental situation. Since the same performance factors must presumably be operative for both rules in such a case, if one of the rules is borne out in the experiment and the other is not, the particular "loophole" described cannot be invoked to save it. Such an experiment has, in fact, been performed with results as indicated, but the work was conducted by someone else and the data, thanks to the combined efforts of the flu virus and the postal service, were not forthcoming in time for this meeting. I am loath to get into so potentially controversial a study without having the full facts at hand, so I have decided simply to expand somewhat the other portions of my paper and to leave this particular experiment for some other occasion.

[4]Syntacticians, too, might usefully take note of this particular admonition, given the prevailing mode of argumentation built mainly on a base of "grammaticality" judgments – largely self-generated – with respect to sentences totally isolated from any kind of discourse or human interactive contexts. A good, consistent set of such judgments from linguistically untrained subjects has yet to be seen.

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TABLE 1.	Proportions of "Correct" Responses on Three Experimental Tasks (Rollins 1980)		
Subject	Berko-1	Free Play	Berko-2
1	6/14	7/14	6/14
2	11/14	9/14	11/14
3	8/14	9/14	8/14
4	2/14	2/14	2/14
5	2/14	2/14	2/14
6	10/14	11/14	12/14
7	9/14	10/14	11/14
8	12/14	12/14	13/14
9	11/14	11/14	11/14
10	5/14	7/14	8/14
11	14/14	14/14	14/14
12	8/14	9/14	8/14
13	11/14	12/14	12/14
14	11/14	11/14	11/14
15	12/14	14/14	14/14
16	12/14	13/14	13/14
17	12/14	12/14	12/14
18	11/14	12/14	12/14
19	10/14	10/14	10/14
20	12/14	14/14	14/14

FIGURE 1. Subject Groups on the Plurals Task (n=112)

Yakut Assimilation and the Strength/Sonority Hierarchy
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In this paper I want to review some claims about the strength/ sonority hierarchy, (henceforth, only "strength hierarchy"), primarily as made by Hankamer and Aissen in their 1974 paper entitled 'The Sonority Hierarchy', in light of data from Yakut, a Siberian Turkic language.

First, I want to demonstrate that an analysis of Yakut phonotactics can be made using a strength hierarchy formulation. Then I want to look at assimilatory phenomena in Yakut and show that they can also be described—in the formal sense—with the strength hierarchy. Finally, I want to comment on two major claims made by H&A—which are also implicit or overt in other literature on the subject of strength/sonority hierarchies, first that the existence of a strength hierarchy means that major class features are, in H&A's words, "artifacts of the binary feature system" and secondly, that the strength hierarchy is to be considered a phonological prime —by which I take it to mean that it is an element in phonological theory which is irreduceable and universal.

In recent years, a number of proposals have been made to incorporate the notion of a hierarchy of sonority or strength into phonological description as a prime. As all of these proposals have noted, it is a time-honored observation that segments interact with each other in a manner which can often be described in terms of inherent sonority or strength.[1]

Hankamer and Aissen (H&A) show that in Pali, various progressive and regressive assimilations can be accounted for in a unitary fashion by the incorporation of the notion of a sonority hierarchy into phonological description. Thus, assimilations such as r+y → yy, s+y → ss, r+s → ss, s+t → tt^h, l+m → mm, l+b → bb etc., are neatly accounted for by assuming a sonority hierarchy for Pali as follows: r → y → l → N → s → t, where sonority diminishes from left to right. Exceptional instances of assimilation aside, assimilating is determined by relative sonority, less sonorous elements assimilating to more sonorous ones. (H&A, 132-134).

Other proposals are numerous. Kim 1972 bases a phonological hierarchy proposal on aperture. Hooper proposes a strength hierarchy for optimal syllable structure. Venneman 1972 deals with Icelandic assimilation with a strength hierarchy. Hooper 1976 proposes a universal hierarchy (p. 206) and also a language specific one for American Spanish. Foley 1977 analyses historical change with strength hierarchies; Stampe 1979 deals with h and glide deletion in English with a strength hierarchy. There is a great deal of accord on the general internal structure of these hierarchies. All of them range segments along a scale which, it is claimed, is generally correlated to a large extent with inherent acoustic sonority which, it is also claimed, can in turn be correlated with the general degree of closure in the vocal tract (including the nasal passages) and among the obstruents with voicing. All proposals expect a certain degree of language-specific variability to occur in the hierarchies, due to the non-categorial nature of sonority differences among closely related classes of sounds. H&A (p. 138) particularly

emphasize this.

Allowing, then, for language specific variation in certain areas, the six proposed hierarchies I have examined generally agree that the strength scale will follow along these lines:

1. (vowels) → glides → liquids → Nasals → vced stop → fric → vcles stop → (?) affricate

where the long arrows indicate language specific possibilities for re-alignment. Thus, for Icelandic, Venneman groups f and the voiced stops; for Pali, Hankamer and Aissen propose that the glides are stronger than r, and l is stronger than the glides; for Hungarian they propose that r and y are stronger than l; Hooper's universal strength hierarchy suggests that the relation between voiceless continuants and voiced stops may be entirely non-significant or language specific; her hierarchy for American Spanish ranks trilled r higher than the fricatives.

It is possible to make use of a strength hierarchy to describe phonotactic and assimilatory phenomena in Yakut. It does not appear, however, that a language-specific hierarchy for Yakut will correspond to the proposals cited above in at least one major way, namely, the strength hierarchy approach to Yakut will require us to rank nasals as weaker than liquids and glides. I suggest that the following strength hierarchy will be necessary to describe Yakut assimilation and phonotactics:

2. h → nasals → l → glides → voiced obstruents → voiceless continuants → voiceless obstruents → geminates

Before examining the data which lead to the proposal of this hier-archy, consider first the original Yakut phonological hierarchy proposal. In a remarkable work published in 1851, Otto Böhtlingk proposed a set of relations among the 21 consonantal 'simple elements' (einfache Elemente) in Yakut.[2] He groups the consonants into classes according to sonority, 'concreteness', and quantity, as well as what we might now call their phonological relation to each other on the basis of distribution and assimilation patterns. He further ranks each set of consonants hierarchically with other sets based on the same criteria. In presenting his arrangement, I translate his terms harte, weiche, flüssige and schwache as 'hard', 'soft', 'labile', and 'weak', and his cover terms starre and starke as 'fixed' and 'strong'.

3.

```
                ┌─hard      k   x   t   č s   p
        ┌─fixed─┤
strong─┤        └─soft      g   ɣ   d   ǰ     b
        │
        └──────────labile       ŋ   n   ń   m
                                     l   r
           weak                      ÿ  yh    (ÿ = nasal palatal glide)
```

I have made adjustments in the alignment of the elements to correspond with Böhtlingk's interpretation of their relations in his explanatory text; I have also substituted the transcription used in this paper for his adapted Cyrillic. Böhtlingk makes some very clear statements about the relation of classes to each other within his hierarchy. Hard and soft consonants alternate; in some cases, both hard and soft consonants alternate with the labile set: through the use of the class feature 'fixed' l

can speak of these alternations generally. He can also effectively
sum up phonotactic relations:

> 3. "...among the _fixed_ consonants, only _hard_ consonants appear
> in final position and only _hard_ consonants can follow each
> other; the _labile_ consonants can succeed each other and
> also appear before (but not after) _fixed_ consonants;
> finally, the _weak_ consonants are only found inter-
> vocalically. [italics are B's; footnotes ommitted][3]

With Böhtlingk's analysis in mind, consider now unit morpheme and
syllable structure in Yakut.

All consonants occur in word-initial position except p̱, x̱, n̄,
r̲, y̲ (except in one word) and y̱ (h̲ is purely allophonic, a fact
also recognized by Böhtlingk; it occurs intervocalically as an
allophone of s̲). In final position, devoicing precludes the
appearance of voiced obstruents; none of the phonetic palatals
(č̲,ǰ̲,ń) appear here either, though the phonologically palatal s̲
and r̲ do. Unit-morpheme internally, contrastive gemination is
found, but not among s̲,ń̲,r̲,y̲ and y̱,which (along with certain
alternations presented below) leads me to consider this group
phonologically palatal. We need, then, to set up groups that
include s̲ and r̲ versus ǰ̲,ń̲,y̲ and y̱—the latter being the true phonetic
palatals that are excluded from appearing in final position; and
we want to set up the group s̲,ń̲,r̲,y̲, and y̱ which do not geminate
internally in unit morphemes—the phonological palatals. This
is no problem within the feature system, using the features
[coronal], [high], and (possibly redundantly) [anterior]. A
strength scale can be employed in describing consonant clusters, how-
ever. Consider first those found unit morpheme internally.

Table 1 shows possible sequences of consonants in unit
morpheme internal position (where the term unit morpheme excludes
sequences across morpheme boundaries; I will henceforth simply
refer to these as 'morpheme-internal' sequences). This table
is based on Kreuger's and Böhtlingk's glossaries, and Böhtlingk's
'Consonant Index, pp. 203 ff. (Note that the table suggests
that p̱ and k̲ are weaker than t̲ in clusters.) From this material
emerges the operative principle in morpheme-internal consonant
sequences: the first consonant must be less than or equal to the
second in strength. I will refer to this as the Weak-Strong
condition (W-S). This is nothing more than the requirement that
(stated in major class terms) sonorants always preceed non-sonorants
and not vice-versa. Note, however, that there is a constraint on
gemination: the phonological palatal continuants do not geminate.
This can also be captured using a strength hierarchy notation. It
seems equally possible, however, to make the same statements using
major class features in the form of a negative constraint. In both
cases, we must assume a voicing rider that states where two non-
sonorant (or two segments above n strength) are found in succession,
their voicing must agree.

Table 1. Unit morpheme internal -CC- sequences in Yakut

	k	t	p	č	ǰ	s	x	b	d	g	ɣ	m	n	ń	ŋ	l	r
r	x	x	x	x	x		x	x	x	x	x						o
l	x	x		x	x		x	x	x	x	x					●	
ŋ	x			x	x			x							●		
ń								x						o			
n	x	x		x			x	x	x	x			●				
m		x	x	x	x	x		x				●					
ɣ								x									
g								x									
d																	
b								x									
x		x		x		x	●										
s	x	x	x			o	x										
ǰ																	
č				●													
p	x	x	●	x		x	x										
t		●															
k	●	x			x (rare)												

> x indicates a sequence
> rk rt rp etc.
> ● indicates attested gemination

4. W-S Condition:

$$C \qquad\qquad C \qquad ==? \qquad {}^{*}C \qquad\qquad C$$
$$[p\ \text{strength}] \quad [q\ \text{strength}] \qquad [-\text{son}][+\text{son}]$$
$$\text{where } p\langle q \text{ and } p \neq q \qquad\qquad \&\ {}^{*}C'C'$$

$$\begin{bmatrix} +\text{palatal} \\ +\text{continuant} \end{bmatrix} \qquad\qquad \begin{bmatrix} +\text{palatal} \\ +\text{continuant} \end{bmatrix}$$

Loanwords that violate the W-S condition are adjusted through epenthesis: Russ: titratka 'notebook' > tetereet (the final ka sequence may have been analyzed as the dative case and eliminated through back formation); daklat 'report' > dokɨlaat; saxmat 'chess' > saaxɨmat.

Native and loanword evidence supports the analysis of l as stronger than r. In native words, only l is found word initially (labaa 'branch', lökö 'stout'), and only l is found geminate morpheme internally. But recall that no phonologically palatal continuants are found geminate, so this latter distribution is not decisive. Note that this relative strength relation only holds in word initial position (cf. Table

5. Russ. luk 'onion' > luuk, Russ. lafka '(a) shop' > laappɨ. Borrowings with initial r, however, epenthesize:

Russ. rajon 'region' > oroyon; repa 'turnip' > erieppe.

This suggests that l̲ is 'strong' enough to form a syllable margin, but r̲ must be reinforced by the epenthetic vowel.

Cross-boundary assimilation in inflection: progressive. Like the other Turkic languages, Yakut is agglutinating and exclusively suffixing. There is considerable assimilation across morpheme boundaries. Progressive assimilation can be described in terms of a strength hierarchy more effectively than by using traditional feature values here. I shall call the general operative principle in Yakut progressive assimilation 'strength matching'. It operates under these conditions:

6.   i. the articulatory integrity of the assimilating segment is maintained

    ii. the assimilating segment matches the strength of the preceding segment in conformity with the W-S condition and other phonotactic constraints.

Table 2 shows the distribution of suffix-initial segments after stem finals. I shall assume that the underlying representation of the suffix-initial segments is that which appears after stem-final vowels (there are some indicated instances of voicing and articulatory adjustment). If we assume the strength hierarchy as proposed above, it is clear how the assimilating segments, t̲,l̲,p̲,č̲, and k̲ pattern according to the strength matching conditions. The alternation of underlying tt with t can also be interpreted as a strength adjustment; here the 'stronger' geminate is reduced to its weaker counterpart. The deletion of t̲ and n̲ in the 3POSS and accusative is paradigmatically conditioned.

Table 2. Distribution of suffix-initial segments in Yakut

	/-tt	-t	-t	-l	-n	-p	-č	-k
C [-vce]		-t		-t		-p t→p/_+p	-č,-s	-k t→ k /_+k k→ x /x+_
r,y	-t	-d	-ø	-d	-ø	-b	-ǰ	-g
l		-l		-l				
N		-n		-n		-m n→ m/_+m	-ń	-ŋ n→ ŋ /_+ŋ
V	-tt	-t	-t	-l	-n	-b	-h	-g /Vhi__ - ɣ /Vlo__

ABL	PART	3POSS	PL ACC	1PLPOSS	AGT	DAT
-ttan	-ta ADV -tik PAST -ti CAUS -tar COMP	-ta	-lar -ni SOCs -liin -lari -laan ADV -lii DnmVB -laa	1SVB NEG -pat PrDUR -bin PstPT -bit	-sit	-ka 2PSVP -gin 2PLVB, 2PLPOSS -git

These adjustments call for a problematic rearrangement of the hierarchy compared to its apparent alignment in word-initial position. The glides, for example, are weak in word initial position and never appear there. It has also been shown that r is weaker than l in word initial position. Yet, if we are going to accept that voiced stops are stronger than liquids, apparently an uncontroversial placement in all other published hierarchies, then to maintain the claim that there is a strength hierarchy involved in these Yakut assimilations we will have to rank r and y higher than l. It is possible that what is being manifested here is the phonological role of r and y as palatal continuants, that is, voiced fricatives. They are simply excluded from word initial position and from gemination for reasons that have nothing to do with strength.

I have ranked nasals lower than r,y, and l on the assumption (as above) that d is stronger than l and n. Note that in the p,č, and k alternations, r,y, and l group as a conditioning class; to rank the nasals above this class in strength seems natural enough, but it then requires us to rank the (suffix initial) voiced stops as lower in strength than the nasals.

But are we dealing with a strength hierarchy here at all, or are we facing a condition that in effect maintains part (i) of the strength matching requirement and simply matches segments by class, voiceless stop with voiceless stop, lateral with lateral, nasal with nasal, and voiced with the only remaining voiced consonantal segments? (Note that when p,č, and k assimilate, r,y, and l form a class because there is no labial lateral available for assimilation.) This class-matching strategy seems to me to be more likely an explanation than any strength matching one, but it leads to another problem—there is no convenient and explanatory way to describe it within current phonological theory.

Consider a formulation of these assimilation rules in feature terms. While strictly possible, they have an air of spuriousness about them. For the suffixes which begin with underlying l we require the rule below:

7.
$$[\text{+lateral}] \rightarrow \begin{bmatrix} \text{C} \\ -\text{lateral} \\ \alpha \text{ nasal} \\ \beta \text{ voice} \end{bmatrix} / \begin{bmatrix} \text{C} \\ -\text{lateral} \\ \alpha \text{ nasal} \\ \beta \text{ voice} \end{bmatrix} + \underline{\hspace{2cm}}$$

We require a somewhat different and more complex formulation for the segments in underlying initial t.

8.
$$\begin{bmatrix} \text{C} \\ +\text{anterior} \\ +\text{coronal} \\ -\text{continuant} \\ -\text{vce} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{vce} \\ \alpha \text{ nasal} \\ -\alpha \text{ lat} \end{bmatrix} / \begin{bmatrix} \text{C} \\ +\text{vce} \\ \alpha \text{ nasal} \\ -\alpha \text{ lateral} \end{bmatrix} + \underline{\hspace{2cm}}$$

If we attempt to write a general rule for all the assimilating segments based on an archiphonemic underlying segment, the situation becomes still more unsatisfactory.

9.

$$
C_{[-cont]} \rightarrow \begin{bmatrix} \alpha\ \text{vce} \\ \beta\ \text{nas} \\ -\beta\ \text{lat} \\ \gamma\ \text{ant} \\ \gamma\ \text{cor} \end{bmatrix} \Bigg/ \begin{bmatrix} \alpha\ \text{vce} \\ \beta\ \text{nas} \\ -\beta\ \text{lat} \end{bmatrix} + \underline{\hspace{3cm}}
$$

(This rule of course is to be followed by the other indicated
adjustments whose formulation is straightforward).

Yet there remains an obvious generality about these assimilations
that we find difficult to express with equal generality in the
notation.[4] Strength-hierarchy notation, however, provides us with a
straightforward unitary description of the phenomena whatever its
ultimate explanatory value may turn out to be.

<u>Cross-boundary assimilation in inflection: regressive.</u>
Regressive assimilation is less frequent than progressive assimilation
in Yakut, and it is generally lexically conditioned. I will not dwell
on it here, except to point out that even the lexically conditioned
cases show their phonologically motivated origin, as the W-S
condition operative regressively across suffix boundaries here as
well. For example, the initial <u>l</u> of the plural suffix -<u>lar</u>, when
employed as a marker in verb paradigms, does not alternate with <u>d</u>
after stem final <u>r</u> as shown in Table 2; rather, the stem final
<u>r</u> assimilates to suffix-initial <u>l</u>:

10. ahïr   'take, 3 p.s.'   ahïl+lar   '3 p. pl.'
     oloror   'sit, 3 p.s.'   olorol+lor '3 p. pl.'
     baar   'there is'   baal+lar   'there are'

It is not the case, however, that regressive assimilation operates
unexceptionally in the verb paradigm. Only dentals assimilate
regressively, whether they are obstruents or nasals:

11. sot-   'wipe'   sop+pot   '3 p.s. neg.'
     suun-   'wash'   suum+mat   '3 p.s. neg.'
but
     tik-   'sew'   tik+pet   '3 p.s. neg.'
     ton-   'freeze'   ton+mot   '3 p.s. neg.'

Note, however, that <u>r</u> and <u>l</u> do not alternate with a voiced obstruent
in this position, indicating a <u>lower</u> rank than the nasals on a Yakut
strength scale, which suggests that a notation involving major class
feature appears to be required for the progressive assimilations.

12. kel-   'come'   kel+bet   '3 p.s. neg.'
     onor-   'make'   onor+bot   '3 p.s. neg.'

This data has been presented to show that the (perhaps some-
what procrustean) application of a phonological strength hierarchy
can provide a unitary description of some rather complex
assimilations. It is still an open question, however, as to whether
the application of the strength hierarchy is truly explanatory or

truly phonological. It seems to me that the Yakut assimilations would be best notated and explained by a theory which allowed a straightforward 'class-matching' generalization to be made at some level of the analysis. The use of such a generalization would also capture the kind of assimilations H&A present from Pali.[5] Many of the other general statements made by strength hierarchy notation would then have to be viewed again in terms of major class features. But recall that H&A in particular have made the strong claim that major class features are "artifacts of the binary feature system".

Or are they? Consider that on every strength hierarchy proposed in the literature referred to here, the only variation among members of the hierarchy occurs on either side of the [+sonorant]/[-sonorant] division. The only exception to this is Hooper's claim that trilled r in Spanish patterns along with the nonsonorants. But recall that there are two phonological r's in Spanish, one tapped and one trilled. This high-strength placement of trilled r could mean nothing more than that it is phonologically a fricative in Spanish, and thus patterns along with the other fricatives. This has nothing to do with inherent strength. In this respect, note that the Yakut r is described by Böhtlingk and Kreuger as 'strongly trilled', and yet there is no indication that it functions as a high-strength consonant. In other words, strength-ranking may be as much a function of the total phonological pattern of the language as of any inherent phonetic or phonological qualities of segments.

I would prefer to hypothesize that 'strength' is an as yet ill-understood phonetic aspect of language, possible a complex of a number of traditionally cited features such as 'aperture' and force of articulation, which are acted upon and shaped language-specifically by syllable structure conditions, the language-specific influences such as available segment inventory, direction of assimilation, and paradigmatic conditioning of rules.

Whatever "strength" or "sonority" is, it does not appear to be a categorically defineable phonetic prime that has a whole history of evolutionary refinement for its linguistic (i.e. phonological) employment behind it as does, say, voicing, tongue-tip control, velum control, or even tongue-flange control—this latter despite H&A's claim that 'the features distinguishing r from l are ad hoc' (p. 140). Yet it would be a mistake, I believe, to dismiss the strength hierarchy literature. There is too much evidence in it that some type of highly general linguistic operations are occurring.

Afternote: In a very stimulating intervention at the conference, John Ohala suggested that recourse to a strength scale in phonological description was merely a non-explanatory and reductionist rearrangement of the data, and that a more legitimate way to deal with phenomena of the sort discussed in the hierarchy literature is to be found in reference to mulitvalued phonetic primitives. ("...the inherent constitution of speech sounds, i.e., how they are made and how they sound, have as much or more importance than system-internal relations, in determining the behavior of speech sounds." Ohala, 1979,46). As I noted in reply, I agree to a great extent with this position.

Yet, I do not believe that reference to more and more refined levels of phonetic description—i.e., a kind of phonetic determinism—is by definition explanatory. To capture the general phonetic/phonological functions of a number of disparate phonetic elements, by the use of a unitary concept is, it seems to me, highly explanatory—if we use the right concept. And if the concept is _functionally_ irreduceable and universal, then I would say it is legitimate to call it a prime (at its level of description). This is not to say that I want to ascribe an "occult specific quality" (the phrase is Newton's) to a term like "strength". It was to avoid any premature assignment of phonetic properties to the phenomenon that I have preferred the term "strength" over the more specific term "sonority". But there are legitimate precedents in the history of science for the postulating of as yet unverified primitives to account for observed (but not yet fully understood) phenomena. The term "force" in physics is one of these. This, of course, does not rule out the possibility of what Ohala suggests, namely that the 'strength hierarchy' is only the _appearance_ of unity—but this position, too, awaits testing and verification.

Footnotes

*Thanks are very much due to William O'Grady, Peter MacKinnon, and Aleksandra Steinbergs for discussion and criticism of this paper; they are, of course, not responsible for any errors of fact or emphasis.

1. DeSaussure, Fouché, Grammont, among others cited in H&A and Kim.

2. Böhtlingk was a noted Indologist who published this detailed and exhaustive study of Yakut as, in effect, a favor for a friend. See Kreuger's introduction to Böhtlingk 1851 (1964).

3. Böhtlingk makes no distinction between morpheme-internal and cross-boundary sequences of segments in his analysis.

4. I am obviously rejecting here a 'solution' which claims that there are merely lists of forms that speakers memorize. Even if we consider, as does Venneman 1972, that rules are in effect well-formedness conditions, it would be difficult to accept the idea that Yakut speakers use hundreds of independently varying variants of inflexional and derivational endings—thanks to the interaction of vowel harmony and consonant assimilation—without recourse to some kind of productive generalizations.

5. Note also, that while a class-matching generalization can _describe_ the Pali assimilations presented in H&A, it does not suggest an explanatory account of the assimilations as does the strength-hierarchy approach.

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*e before nasal clusters in Latin

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It is tiresome that our handbooks state so many facts of Latin
so badly.[1]

1. *e before m + labial stop

Leumann, Neuausgabe 1977, 45 §42c, lists good evidence to
support the view that *e became i before labial nasal + stop:
imber < *embris < *mbhris (: Osc. anafríss 'Imbribus'),
simplex (: semel) < *sm-, simpuvium 'a kind of dipper' (: Lith.
semiù sémti?), limbus 'ribbon, band' < lembus, nimbus (: nebula,
crossed with imber?). The exceptional Nouimber may be simply a
chance happening of unclear cause; the normal December etc. were
surely protected through their obvious relation to the numeral
simplexes. Leumann modifies his derivation with the vague specifi-
cation "sporadisch vor r l, vielleicht dialektisch."
R. Pfister, in his revision of Sommer's Handbuch (1977) 54 §53,
1,b, has the change e > i "in einigen Fällen". His examples are
imber, limbus (compared to Indic lámbate 'hängt herab', an inher-
ently weak comparison in view of the required IE *b), nimbus (but
Balūčĭ namb is a far-fetched comparandum), to which he adds simbella
'half pound' < *sĕmbella < sēmi(lī)bella. Pfister concludes that
since "nempe, tempus usw." fail to show the change the development
must have held true only before mb; he correctly notes that the
month names would have been protected by the simple numerals.
Pfister reports Hoffmann's and Niedermann's view that simplus and
simplex reflect the change in position before m + a cluster
whereas semper had only single p. While one obviously cannot reach
certainty on the point, it seems equally possible that semper re-
flects nothing more than a levelling of the vocalism from forms
such as semel; but see below for a more principled proposal.
Pfister speculates that stlembus is dialectal, while noting that
Leumann sees -imb- as reflecting dialect; in fact, stlembus shows
a unique initial, which renders the question undecidable.
For membrum Leumann observes that the br represents *sr and
that the e is to be derived from *ē, but he draws no formulated
conclusion. Pfister explicitly, and I think correctly, reasons
that the long *ē of *mēms- was shortened later than that of
simbella and too late to participate in the change *e > i.
Leumann reports the opinion that the p in exemplum < *ex-em-
lo-m was developed after the change *e > i. Pfister, while
recognizing the same pre-form, is far less explicit in his state-
ment of the development. The chronology must be as Leumann has
stated it for this word.
This brings us to templum, which Pfister regards as having an
old -mp- + l-, as does also Leumann 213 §216a. I simply do not
understand how these scholars can have persisted, in the face of

phonetic difficulty, in resisting the solution seen so clearly by Ernout and Meillet DELL (s.v.) and bequeathed to us by the ancients and in particular by Varro.[3]   When we relate templum to Greek τέμ-νω 'cut' we have *tem-l-o-m, with precisely the same phonetics and morphology that is generally agreed for exemplum, i.e. no original *p.

Turning now to nem-pe and sem-per, we find that these offer no basic difficulty whatever, since -pe and -per here are easily regarded as old enclitics. In other words we have the reflex either of an old morphological seam of a different order, or else a different chronology of concatenation.

Our one remaining problem is now tempus; for this there is no agreed etymology. However, the semantics seem to lead us to an association at bottom with templum. In contrast with other expressions, for 'time', tempus meant a delimited space or segment of time, 'epoch, season, time of day, etc.' A derivation once again from *tem- 'cut' seems, I submit, obvious; the semantic parallel to Greek καιρός (: κείρω 'cut') is exact. Thus one possible source of the p in tempus is an association with the earlier near-synonym templum, i.e. 'a cut, slice'. But another possibility is to be seen in the phonetic development expected in the ancient compounding or derivational form *tem-s- which must be supposed once to have existed. Thus *tems- > *temps- just as sūmō : sūmpsī, temnō[4] : tempsī etc. Finally, we may perhaps see our source verb concealed under the notorious polysemy of temptō.[5] It seems possible that at an early date by a convergence of phonetically developed p in formations with following l,s and t, the stem of *tem- may have been revised to temp- throughout; or at least a doublet of this configuration would help explain the resolution in the replacement by temptō. I propose therefore that tempus temporis was originally *temos temes-os (→*tem(p)-os-es[6]), with no *p.

The regular development of *e before original *mp and *mb, then, was Latin i. That is,

$$*e > i \ / \ \_\_ *m + \text{labial stop.}$$
The apparent counter-instances therefore vanish.

2. iuvencus

Leumann 1977, 45 §42a Note, reports "unerklärt ist das e statt i von iuvencus idg. *yuwn̥kos ...; vielleicht bleibt e nach v erhalten wie in iuvenis; unwahrscheinlich ist Entlehnung aus umbr. iuenga." R. Pfister, 4th revised edition of Sommer's Handbuch 1977, 54 §53, 1, a, has a different version: "behält e bei nach iuvenis, iuventus (Hoffmann, a.a.O.) oder ist Dialektform;" he then compares Umbrian and other matters. The first point that must be made is that it is regrettable at this late date that reports of our knowledge of Latin should be so much at variance.

In fact, Leumann 90 §94 suggests reasonably, in a paragraph of more general and more principled purport, that the more open vowel quality arises systematically after a glide. The mechanism here would be one of phonetic dissimilation.

We may in turn ask how the glide arose. It seems likely that in this word at an early time there was a hiatus, as I have described KZ 84, 1970, 1. The pre-form, then, was *H̥ iu-Hn-ko-.

With this form explained, we may now say on the basis of the remaining well known examples that without exception *e̲ became Latin i̲ before velar nasal; or

$$*e̲ > i̲ \ / \_ [ŋ].$$

A preceding glide either blocked this development or returned the vowel to a mid quality by dissimilation.

3. *e̲ before grave nasal clusters

It is well known that IE *e̲, and e̲ developed from syllabic nasal, remained Latin e̲ in position before n̲ + dental stop, with just two exceptions: the syllabic of the negative prefix in- and the forms of in 'in'. In these two instances it is easy to see that a special accent/stress condition obtained, since here the context is that of a pretonic.

We are now in a position to generalize our findings. IE *e̲, or more precisely Proto-Latin *e̲, was raised to i̲ before a clustered grave nasal. That is to say,

$$* \begin{bmatrix} -hi \end{bmatrix} > \begin{bmatrix} +hi \end{bmatrix} / \ (\begin{bmatrix} -glide \end{bmatrix}) \begin{bmatrix} V \\ -grv \end{bmatrix} \begin{bmatrix} +grv \\ +nas \end{bmatrix} \ C$$

We have, in short, an interplay between diffuseness and gravity. This seems clearly to be the case before the labial. If, however, the labial and velar contexts marked two separate prehistoric events, it is possible then that the velar had a palatalizing effect, such as we find for velars next to front vowel at other stages of Latin.

We note here, in any event, another instance of the usefulness of gravity in formulating a phonetic behaviour.

* * *

Finally, let us emphasize how much work remains to be done on rudimentary aspects of a language even so well known as Latin.

NOTES

[1]Cf. Romance Philology 36, 1982, 28-9 esp. fn. 2.

[2]This opinion is documented by Walde-Hofmann LEW₃ I (1938) 426 s.v. exemplum: "nicht jedoch templum." Under templum, ibid.II (1952) 660, we read "vlm. von *temp-lo- auszugehen."

[3]The notion of the templa of the heavens being originally augurs' "sections", or "cuttings", and of a templum in the forest being a "clearing" is productively elaborated by Maher 1978 (originally presented 1973) 476-7.

[4]LEW₃II(1952)657 rejects a relation with Greek τέμνω, preferring a dubious ancestry "wohl aus *temb-no̅"...< *ste(m)b/p-. This last would however violate our rule of *e̲ > i̲; I therefore reject this hypothesis without even investigating its dim sources among IE roots.

[5]The claimed derivation, $\underline{LEW}_3$ II (1952) 662, of $\underline{tempt\bar{o}}$ as being an iterative to $*\underline{temp(i)\bar{o}}$ fails on the same argument as that advanced in note 4.

[6]I have motivated Godel's account of $*-\underline{os}-$ in such forms (in place of the expected $*-\underline{es}- > -\underline{er}-$), $\underline{Incontri\ linguistici}$ in press.

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The English l/r Distinction for Native Japanese[1]
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1. Introduction

Sheldon (1979) at the Winter Meeting of LSA
(Linguistic Society of America) presented an inter-
esting study of native Japanese treatment of the
English l/r distinction, suggesting that production
may run ahead of perception. That is, Japanese
speakers of English as a second language consis-
tently appear to produce an l/r distinction before
they can consistently distinguish them in percep-
tion. This is contrary to the usual wisdom that by
stressing production skills, perception will take
care of itself.

We wish to question the findings from two view-
points: firstly, we have methological objections,
but secondly, assuming that the findings are at least
in part true, we wonder whether there is some prin-
cipled explanation and whether this might show us
something of phonological interest.

Sheldon had six native Japanese students read
through a randomly ordered list of l/r minimal pairs.
Four native English speakers listened to the tapes of
these items, and responded in a forced choice task,
identifying them as either /l/-words or /r/-words.
Conversely, the native English speakers read through
the minimal pair list, and the Japanese responded in
the forced choice task.

When the tapes made by the Japanese were heard
by the English, there were only 1% misidentifications,
and for no speaker were there more than 3% misidenti-
fications. Thus, it was judged that the Japanese
were good producers. However, native Japanese speak-
ers' errors in judging native English items, averaged
across all contexts, ran slightly over 10%. Thus the
conclusion that there were perception problems.

We should note, however, that the tapes were
made by having each speaker read through the list
twice; then the best token of each item was used.

[1] Earlier version of this paper was presented at
the Summer Meeting of the Linguistic Society of Amer-
ica, August 1980.

Although this procedure was intended to assure acoustically clear tokens, it creates an obvious bias. Production by non-native speakers is presumably more variable in quality than production by natives. The effect of the procedure, then, is to throw out production errors by the Japanese. Further, the laboratory situation in general may induce more careful speech, the words were read from lists, rather than from sentences, so the tokens presented for the Japanese may be unrepresentatively accurate. We will assume, though, that the magnitude of this effect is not great, and that without this bias there would still exist a discrepancy between production and perception scores.

We would like to suggest, however, that the simple fact that native Japanese speakers consistently produced distinguishable /l/ and /r/ tokens does not prove that they have mastered production. A comparison of the phonologies of English and Japanese may suggest other possibilities.

First, however, let us note the contexts in which perception errors were more frequent.

Table I
Sheldon's Results: % errors by 5 good Japanese

Initial Position

	% error		% error
read	0	lead	35
room	15	loom	15
road	0	load	20
right	0	light	0

Stop·Liquid

breed	35	bleed	10
broom	27.5	bloom	5
grow	32.5	glow	10
grass	10	glass	10

Intervocalic

mirror	10	miller	27.5
berry	12.5	belly	12.5
correct	17.5	collect	12.5
arrive	12.5	alive	2.5

Final

dear	0	deal	0
core	2.5	coal	7.5
war	2.5	wall	0
tire	2.5	tile	0

Table I shows perception errors by five good native Japanese speakers in distinguishing English /l/ and /r/ on four native English speakers. We note that /l/ is misperceived as /r/ in syllable-initial position, but that /r/ is misperceived as /l/ in clusters. Intervocalically, the error rate is high and possibly the direction depends upon stress. In final position, errors are few. Taken together, this suggests that different allophonic forms of /l/ and /r/ pose different problems.

2. Phonological Characteristics

McCawley (1968) claims that Japanese is a mora-counting syllable language. This means that rules such as accent insertion should assign an accent to a syllable and rules such as pitch realization should be applied to morae.

A mora can consist of a consonant plus a vowel (CV), a vowel alone (V), mora nasal (N), or a first obstruent of geminate consonants. A syllable consists of a consonant plus a vowel (CV), a vowel alone (V), a long vowel ((C)V:), a vowel plus a mora nasal ((C)VN), or a vowel plus a first obstruent of geminate consonants ((C)VC). For instance, kyooka 'subject' is a three mora word (kyo-o-ka) as well as a two syllable word (kyoo-ka). McCawley defines mora as a "unit of phonological distance" and syllable as a "prosodic unit."

The mora is a basic metrical unit. The Japanese syllabary is based upon morae: each kana character represents each mora. Thus, it is hard for the Japanese without linguistic training to perceive か as a sequence of k and a. In other words, the Japanese writing system is mora-based and the mora is the psychological minimum unit of sound.

Japanese is also a language with only one phonemic liquid, /r/, but that liquid varies in production from a retroflex flap, [ř], to a retroflexed lateral, [l], to a tap [ɾ]. Its allophones are in relatively free variation and within them it covers the space of both English /l/ and /r/. Japanese has only five phonemic vowels, but these undergo various contextually predictable variations in coloring.

The Japanese speaker learning English must find a strategy to distinguish /l/ and /r/, both in perception and production. They differ in at least three dimensions: (1) the articulatory position of the consonant, per se, (2) the duration of the con-

sonant, and (3) the effect on the surrounding vowels. The first is, of course, a phonemic distinction, but we should note that /r/ is generally longer than /l/, and that vowels before /r/ are quite colored, and that even when /r/ is initial, following vowels tend to be slightly centralized (e.g., "light", "right").

The "correct" answer for production is to make a consistent distinction between the two consonants, but in the absence of that, it is still possible to produce consistently different minimal pairs which will resemble the English forms, merely by manipulating vowel color and consonant length. We have in fact been told by some native Japanese that they cannot distinguish /l/ and /r/ well in isolation, but perceive and produce them distinctly in context through these cues.

It could be further argued that this is a natural naive approach for a Japanese speaker who, taught to read in a mora-based system, has never analyzed initial consonant and vowel as separate units, but always as a single whole. Therefore, the new distinction between /l/ and /r/ likewise is not treated in isolation. Syllable final liquids, on the other hand, are in a separate mora from the vowel, so the Japanese listener will tend to isolate them. What we interpret as r-coloring on the vowel, the Japanese listener may interprete as a short vowel and longer consonant.

Why should the weight of the distinction be shifted away from the consonant? Many studies have shown that categorical perception is less marked for vowels than for consonants. Categorical perception (usually a synthetically generated one), acoustically marginal between two phonemes, will judge it to be one or the other, not some mixture of the two. This holds especially for consonant transition curves. There is, to the ears of a speaker of English, no halfway between the /t/ and /k/ phonemes.

But categorical perception does not hold to nearly the same degree for vowels. Listeners can tell that two tokens of the same vowel are not identical, whereas they frequently cannot with stops. They can tell that two vowel stimuli, of the same phoneme, differ in a particular dimension, that one is higher, or more central or that one is nearer the boundary of some other phoneme category.

To our knowledge, categorical perception in second languages has not been much investigated. Despite the absence of experimental evidence, it

seems reasonable to assume that speakers will tend to perceive consonants in a second language according to the categorical boundaries of their native language, but will, at an earlier stage, be aware of variations in vowel quality.

The consequence of this, for a native Japanese learning English, is that there may not be a clear boundary between /l/ and /r/, perceptible to the listener, but that on being taught that certain pairs are distinctive, the student will be able to find a distinction in the surrounding vowels, and in the duration of the consonants. It is not clear how fluent a speaker must become before a new boundary is located, and the perceptual burden is shifted to that dimension. Even a fluent non-native is likely to be influenced substantially by non-distinctive cues in the environment.

3. The Results

Let us note the general patterns of the errors in Table I:

1) In initial position there is a bias toward /r/.
2) In post-stop position, there is a bias toward /l/.
3) There are fewer errors on low vowels.
4) The errors are mixed but frequent in intervocalic position.
5) There are very few errors in final position.

These effects are not necessarily statistically significant, but let us ask why they would occur. What kind of perceptual strategy would cause these errors? We tentatively offer this set of perceptual principles for native Japanese listening to English:

6) /l/ is perhaps more marked, or for some unidentified reason the direction of error favors /r/ in initial position.
7) Final /r/ spreads forward onto the vowel, therefore the consonant is interpreted as longer.
8) When the liquid is intervocalic, coloring of the prior vowel is not noted as it is in a separate mora. The length difference noted in #2 is compressed.
9) Initial /r/ alters vowels somewhat, especially low vowels.
10) A short liquid is more likely to be /l/.
    -- Postconsonantally, all liquids are shorter, causing a bias toward /l/.

The tendency to perceive short liquids as /l/ accounts for the results for clusters. Liquids in clusters will be shorter than those in isolation, so /r/ in a cluster may be mistaken for /l/. But a long initial /l/ may be mistaken for /r/.

There is some evidence that even native English speakers may be induced to misperceive /r/ as /l/ based on peripheral cues -- see for example Cutting and Day (1975), so it is not unreasonable for us to suppose that perception by Japanese may be swung one way or the other by such cues. Whether or not these specific principles are correct ones remains to be proven, but is not really crucial to a more general conclusion: it appears that the errors are systematic in direction, therefore that some set of cues, not phonemic in English, but usually predictable, is playing a role. The errors may occur especially when these secondary cues do not behave true to expected form.

4. Production vs. Perception

Is Sheldon justified in her conclusion that the Japanese in her study were better producers than perceivers? Not necessarily. It depends on what we mean by a "satisfactory" producer. The speakers are producing distinguishable /l/ and /r/ forms, but it is not clear that they are producing a good English /l/.

Her ultimate conclusion is that ESL teachers should perhaps pay special attention to perceptual skills, lest they discover that their apparently satisfactory students have a problem. We suspect, however, that any ESL teacher would quickly find that these students had not mastered the production of the l/r distinction even though, in a forced choice task, a sound might be recognizable as /l/ or /r/. It could very well be produced as an indeterminate liquid, surrounded by exaggerated contextual efforts. Only spectrographic or cineradiographic evidence would confirm this.

Assuming that native English speakers can make use of these contextual cues, there is no reason why they should not be able to distinguish "indeterminate" consonants. Their errors should occur only when the production of the consonant, as such, errs beyond indeterminacy toward the opposite end, that, when an /l/ is clearly /r/-like. Even if the tokens are produced by the wrong strategy, they will have to be

really bad before they are misunderstood. But the native Japanese, listening to English, will place greater weight on the surrounding context, and will misclassify if the context "errs," even if the consonant itself is perfectly well produced. The discrepancy between production and perception results does not show that the two are separate, that there is a strategy for production not available for perception.

Our moral, as phonologists, is that we simply do not know enough about the importance of context in classifying sounds. We assume that phonemes are distinctive on the basis of minimal distinctive features, and we tend to dismiss allophonic effects from the equation. There is good reason, even in a native language situation, to believe that this is an over-simplification, certainly in perception, quite likely in production as well. Sheldon's study suggests that in a second language situation, context may play an even greater role in perception and production.

Sheldon questions the traditional assumption that perception precedes production. We find the evidence for that inconclusive. However, we question an equally traditional assumption, that perception and production may be adequately described by neatly linear segmental models. We suggest that a better description of /r/ and /l/ must include features not usually considered distinctive, and not always confined to the temporal demain associated with the consonant.

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Symbolics and Symbolic Change
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This paper is an attempt to give the terms 'concept' and 'meaning' their distinct formal definitions. Based on the assumption that a constant concept can have various meaning manifestations across languages and through time, this paper also expounds the notions of symbolics and symbolic change.

Anyone who has an opportunity to perform translation from one language into another or compare equivalent expressions between two languages will inevitably notice the fact that apparently identical ideas sometimes are represented in remarkably different ways across languages. Of course, sometimes the contrast can be explained as a result of two languages having different syntaxes and morphologies. However, quite often the contrast has to do with idiomaticity and ceases to be a purely structural phenomenon. In other words, given a pair of equivalent sentences from two languages, its members are rarely the literal translations of each other, even after syntactic and morphological adjustments are made to enhance maximal grammaticality in translation. Language apparently defies literal translation. As a result, what is an idiomatic expression in one language, when literally translated sometimes becomes unidiomatic in another. Thus, for example, (1a) in English has its usual counterpart (1b) in Mandarin Chinese and (1c) in Korean. But if one literally translates (1b) and (1c) into (1b') and (1c'), one obtains inaccurate English translations:

    (1a)   I showed him the book.
    (1b)   Wǒ bǎ shū ná gěi tā kàn.
           'I-hold-book-take-give-him-see.'
    (1b') I took the book and gave it to him to see.
    (1c)   na nun ku chayk ul ku i eykey po-i-ess-ta.
           'I-topic m-that-book-acc. m-that-person-dat. m-see
              CAUSE PAST STATEMENT.'
    (1c') I let/caused him to see the book.

Sentence (1b') is inaccurate because it improperly emphasized the actions of 'taking' and 'giving' and (1c') is inaccurate because of its undue emphasis on the action of 'letting/causing.' To express whatever idea (1a) is supposed to have expressed, the English language has already made (1a) available. Therefore, (1b') and (1c') are poor substitutes, and in that sense they are unidiomatic expressions in English.

Let us call two sentences such as (1a) and (1b) or (1a) and (1c) a pair of translation-equivalent sentences or a translation-equivalent pair. Then the lack or loss of idiomaticity caused by the literal translation from (1b) or (1c) into (1b') or (1c')

raises the question of whether a pair of translation-equivalent sentences are merely two different expressions of the same thought (or idea) or whether they represent two different thoughts. The question as it is phrased is not precise enough to allow for a clear answer. For one thing, we have not yet adopted a theory of translation to serve as a context in which to answer the question. So, let us first propose a theory of translation and then try to answer the question in the context of such a theory.

Let us begin by assuming that an idea (or thought or notion) is comprised of a number of subideas, never mind for the moment what exactly an idea is. The relation between an idea and its subideas may be roughly equated with an unordered set and its elements, or with an ordered set (i.e., a sequence) and its components. Let $X = \{x_1, x_2, \ldots, x_n\}$ be a (unordered) set. Then $x_1, x_2, \ldots, x_n$ are the elements of X. That is, for each $i \leq n$, $x_i \in X$. Let $S = <s_1, s_2, \ldots, s_n>$ be a sequence, then $s_1, s_2, \ldots, s_n$ are components of S. An idea may be equated with X, and its subideas with $x_1, x_2, \ldots, x_n$ or an idea may be viewed as S, and its subideas as $s_1, s_2, \ldots, s_n$. In the former case, we say that the idea is a loose heap or that it is not structured or organized. In the latter case, we say that the idea is structured or organized. For the sake of simplicity, we will restrict ourselves to the discussion of ideas that are not structured, and thus treating the relation between an idea and its subideas as a relation that holds between a set and its elements.

Suppose that we are given two sets, namely, $X = \{x_1, x_2, \ldots, x_n\}$ and $Y = \{y_1, y_2, \ldots, y_m\}$. It can happen that X and Y do not share any element. Then we say that their intersection is empty, and we write $X \cap Y = \phi$. If X and Y share some but not necessarily all elements, we say that X and Y intersect, and in that case exactly one of the following four situations obtains: (i) X = Y, that is, X and Y are identical because they share every element; (ii) $X \subseteq Y$, that is, X is a subset of Y, meaning that every element in X is also an element in Y; (iii) $Y \subseteq X$, which is the reverse of $X \subseteq Y$; and (iv) $X \cap Y = Z$, for some Z (and $Z \neq X$ and $Z \neq Y$), meaning that X and Y share only some but not all elements.

Let A and B be two languages, and let x be a sentence in A representing the idea i(x), and y a sentence in B representing the idea i(y). Assume that x and y are a translation-equivalent pair, then x translates y adequately and y translates x adequately. The relation between i(x) and i(y) (more precisely, from i(x) to i(y)) is exactly one of the following: (i) i(x) = i(y), (ii) $i(x) \subseteq i(y)$; (iii) $i(y) \subseteq i(x)$, and (iv) $i(x) \cap i(y) = i(z)$, where i(z) is the idea expressed or expressible by some sentence z in either A or B. In situation (i), we say that the translation

sentence x *preserves* the idea i(y) as i(x); in situation (ii) we
say that the translation sentence x *reduces* the idea i(y) to i(x);
in situation (iii) x is said to *augment* the idea i(y) to i(x); and
in situation (iv) x is said to *approximate* the idea i(y) by i(x).
The relation between i(y) and i(x) is the reverse of that between
i(x) and i(y).

In accordance with this view on translation, if x and y are
a pair of translation-equivalent sentences, then x and y are
merely two different expressions of the same idea if x preserves
the idea of y (i.e., i(x) = i(y)), otherwise x and y are expressions
of two distinct ideas.

Now, given any pair of translation-equivalent sentences, how
can we decide whether they represent one single idea or two
separate ideas? Let us use a concrete example. Faced with (1a)
and (1b), how can we determine whether (1a) and (1b) correspond
to identical or distinct ideas? All one can say is that a
(perfect) bilingual of English and Chinese would use (1a) and
(1b) under 'comparable' speech situations, but one does not know
whether 'comparable' means 'exactly the same' or 'roughly the
same.' In order to determine whether two comparable speech
situations in two languages are identical or merely similar, one
would have to assume that the two translation-equivalent sentences
are one and the same idea or are two separate ideas, and that would
get us into a circular argument (see Quine 1960 for indeterminacy
in translation). Other criteria would not get us far either. For
example, even if the logician can find two identical or distinct
formulas for the two sentences compared (cf. Montague 1974), or
even if the psychologist can find two identical or distinct
subject responses in his experiment (cf. Kay and McDaniel 1978),
it remains a question whether the logician's formulas or the
psychologist's experiments are adequate measurement of the sameness
or difference between the two sentences (see Lewis 1946 for
inscrutability of object). In fact, in his attempt to define
truth, Tarski (1952) has shown that truths do not reside in the
sentences of an object language but must be asserted by individual
statements in the meta-language. The same would seem to apply to
the sameness between ideas within a language or across languages.
Consequently, we must make some arbitrary decision and declare,
for any given pair of translation-equivalent sentences, whether
they correspond to the same idea or not.

To achieve this, we start out by dividing our previous term
idea into two terms: concept (idea, notion, thought, etc.) and
meaning (semantic value). Technically, a sentence in a specific
language has a meaning but does not by itself represent a concept.
By contrast, a set of translation-equivalent sentences represents
a concept but does not have a meaning. So used, the term meaning
denotes the same thing as our previous term idea; the term concept
will now be explicated.

If X and Y are two sets, then we can obtain their intersection
X $\cap$ Y as indicated before. We can also obtain their union X $\cup$ Y

by collecting elements which are in X or in Y or in both. The
operation of intersection and union can be applied to any number
of sets. Let A_1 , A_2 , ... , A_n be n number of languages, and
let $x = \{x_{A1}$, x_{A2} , ... , $x_{An}\}$ be the set of n translation-
equivalent sentences, with each x_{Ak} being a sentence in some A_k.
Then the set x can be viewed as an abstract sentence whose
concrete manifestations in particular languages are x_{A1} ,
x_{A2} , ... , x_{An}. We want to define the concept that x
represents. Let $m(x_{A1})$, $m(x_{A2})$, ... , $m(x_{An})$ be the meanings
of x_{A1} , x_{A2} , ... , x_{An}. Then the intersection $m(x_{A1}) \cap$
$m(x_{A2}) \cap \ . \ . \ . \cap m(x_{An})$ and the union $m(x_{A1}) \cup m(x_{A2}) \cup \ . \ . \ .$
$\cup \ m(x_{An})$ can be obtained. Let us call the former min(x), or the
minimum semantic content of x, and the latter max(x), or the
maximum semantic content of x. Then, we define the concept that
x represents to be $c(x) = < min(x), max(x) >$, which is a pair
with its two elements fixed in order, that is, an ordered pair.
As a hypothetical example, consider the three meanings $m(x_{A1}) =$
$\{a, b, c, d\}$, and $m(x_{A2}) = \{a, b, c, g\}$, and $m(x_{A3}) = \{a, b, c, h\}$.
Then $min(x) = m(x_{A1}) \cap m(x_{A2}) \cap m(x_{A3}) = \{a, b, c\}$, and $max(x) =$
$m(x_{A1}) \cup m(x_{A2}) \cup m(x_{A3}) = \{a, b, c, d, g, h\}$. Therefore,
$c(x) = < \{a, b, c\}, \{a, b, c, d, g, h\} >$.
    It is clear that for any x_{Ak} , $m(x_{Ak})$ includes min(x) as
its subset and at the same time is included as a subset in max(x),
that is, $min(x) \subseteq m(x_{Ak}) \subseteq max(x)$. Thus, min(x) is the lower
bound and max(x) is the upper bound of a continuum within which
$m(x_{Ak})$ is located. If A_1 , A_2 , ... , A_n are separate
contemporary languages (or dialects), then this continuum may be
viewed as the flexibility range within which the same concept
c(x) can be particularized as various $m(x_{Ak})$'s in various A_k's.
If A_1 , A_2 , ... , A_n are successive historical stages of the
same language, then this continuum may be regarded as the mutation
range within which the same concept c(x) may be particularized
as the meaning $m(x_{Ai})$ at one stage A_i but as the meaning $m(x_{Aj})$
at a later stage A_j. In the former case, we say that the various
sentences x_{A1} , x_{A2} , ... , x_{An} symbolize the concept c(x)
through the meanings $m(x_{A1})$, $m(x_{A2})$, ... , $m(x_{An})$. In the
latter case, we say that a symbolic change has taken place with
the result that whereas c(x) is particularized as $m(x_{Ai})$ at time
A_i , it is particularized as $m(x_{Aj})$ at a later time A_j.

Given a sentence x_{Ak} symbolizing a concept $c(x)$ through the meaning $m(x_{Ak})$, it has been standard to refer to a study of how x_{Ak} acquires $m(x_{Ak})$ by the term semantics. It is thus appropriate to refer to a study of how $m(x_{Ak})$ particularizes $c(x)$ by the term symbolics. Similarly, whereas semantic change refers to a mutation from $m(x_{Ai})$ to $m(x_{Aj})$ for a supposedly fixed x_{Ak}, symbolic change refers to a mutation from $m(x_{Ai})$ to $m(x_{Aj})$ for a supposedly fixed $c(x)$.

In the following space, we will undertake to illustrate the terms meaning, concept, and symbolic change by using a concrete example, namely, the triple of sentences (1a), (1b), and (1c) cited previously.

To begin with, how do we determine what meanings (1b) and (1c) have, since they are not English sentences? We decide to treat the meanings of their literal translations into English, namely (1b') and (1c'), as their own meanings. Thus, m(1b) = m(1b'), m(1c) = m(1c'), and of course m(1a) = m(1a). The next step is to break each one of these meanings into a set whose elements are its submeanings. But the set of translation-equivalent sentences formed by (1a), (1b), and (1c) is too small, and so whatever conclusion we arrive at based just on these sentences cannot be reliable. Therefore, we have to substantially increase the size of this set by adding to it many more translation-equivalent sentences. Instead of actually doing that, we will analyze these three sentences utilizing knowledge that we would have gained if we had actually studied a sufficient number of languages with respect to the concept particularized in (1a), (1b), and (1c).

Now we invoke the arbitrary power of a metalanguage and declare that syntactic and morphological differences among (the literal English translations of) the translation-equivalent sentences reflect difference in meaning. Accordingly, one clear meaning contrast among the three sentences (1a), (1b), and (1c) is this: the Korean sentence particularizes the concept of showing as a single overt causative event, the Mandarin sentence expresses it as a sequence of three temporally-structured events (cf. Tai MS), and finally the English sentence treats it as a single event which is neither an overt causation nor an overt sequence of temporally-structured events.

Since (1a), (1b), and (1c) are mutual translations, they should share some meaning, according to our theory of translation. What is it that they share? Roughly, the shared meaning is (a) 'I made the book visible to him.' If I showed someone a book, he might still not have seen it. Thus, (b) 'as a result he saw the book' is an implicature (à la Grice) but not a part of the meaning of (1a) I showed him the book. However, (b) is more a part of the meaning of (1b) than just an implicature. The same

seems true with the Korean sentence (1c). Therefore, we can
hypothesize that (a) is the shared meaning of (1a), (1b) and (1c),
but (1b) and (1c) also share the meaning (b).

The Korean sentence (1c) is a causative construction, and
causative constructions have been rather extensively studied
(cf. Shibatani 1976). Such notions as control (Givón 1975),
directness (Shibatani 1973, Yang 1974, Maran and Clifton 1976,
Kachru 1976), accidentalness (Vichit-Vadakan 1976) form a short
list of features of causation. Based on this short list, we
characterize the extra semantic feature in the Korean sentence
(1c) as follows: (d) causation: agent-controlled, direct,
neutral w.r.t. accidental/intentional.

The meanings m(1a), m(1b), m(1c) can now be tentatively
analyzed as follows:

m(1a) = {(a) 'I made the book visible to him'};
m(1b) = {(a) 'I made the book visible to him,'
     (b) 'as a result he saw the book,'
     (c) 'I took the book and gave it to him'};
m(1c) = {(a) 'I made the book visible to him,'
     (b) 'as a result he saw the book,'
     (d) 'causation: agent-controlled, direct, neutral}.

Clearly, min(1) = {a} and max(1) = {a, b, c, d}. Therefore,
c(1) = < min(1), max(1) > = < {a}, {a, b, c, d} >. Thus, {a}
and {a, b, c, d} form a continuum, which can be viewed as a
flexibility range in symbolization or as a mutation range in
symbolic change, with respect to the concept c(1). In symbolic
change, c(1) is particularized as (oc-1b) wǔ shì zhī yǐ shū,
I showed him with the book, in Old Chinese, but as (1b) in
Mandarin.   Therefore, a symbolic change in Chinese has occurred
with the result that m(1b) = {a, b, c} replaced m(oc-1b) = {a}
as the particularized shape of c(1). In the history of the
English language, show in Old English had the sense 'to look at.'
In about 1200 show suddenly changed from this sense to the
causative sense 'to cause to see.' However, the periphrastic
construction make someone do something was already in use about
1175. Therefore, it seems likely that before show acquired its
causative sense, the semantic values 'show' and 'cause to see'
share the periphrastic syntactic construction. If this is
correct, then the symbolic change affecting English in connection
with c(1) would have been from {a, b, d} to {a}.

Before concluding, it may be worth mentioning that our
notion concerning the distinction between concept and meaning is
closely analogous to the ideas proto-type (Labov 1973), scene
(Fillmore 1979), category (Chafe 1979), schema (Rumelhart 1978),
natural category (Rosch et al. 1976), model (Miller 1979), fuzzy
set (Zadeh 1965, Lakoff 1972), etc. The intended contribution of
this paper, however, is to formalize a portion of these parallel
ideas in order to make it possible to talk about symbolics and
symbolic change in explicit and exact terms.

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CASE-MARKING AND θ-MARKING IN MALAYALAM: IMPLICATIONS FOR THE PROJECTION PRINCIPLE

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In section 1 of this paper, I shall examine some instances of Case-marking in Malayalam which have a bearing on the claim of the theory of Government and Binding (GB) that Tense (and not the Verb) Case-marks the subject of a sentence (Chomsky 1981). I shall show that Case-marking of the subject in Malayalam is determined by either the Verb or Tense/Aspect, or both, depending on inherent properties of these elements; but in the unmarked case, the verb Case-marks the subject. I shall suggest how GB might try to accommodate itself to this fact.

In section 2, I shall present Malayalam examples of a phenomenon which I shall call "dual projection", where the θ-frame and the Case-frame of a sentence are determined by different lexical items. The Case-frame is determined by the head of the VP (as is normal); but the θ-frame is determined by the head of an NP which is embedded in the VP. I shall suggest that this can be partly explained if we claim that selection may be 'non-local', whereas Case-marking must be 'local'. I shall show that the "dual projection" sentences have serious implications for the Projection Principle (Chomsky 1981).

1

Evidence that the Malayalam verb must be allowed to Case-mark the subject has been presented in Mohanan (1982a). The evidence is very straightforward. Malayalam has a small class of verbs, such as wiśakk 'be hungry' and weedanikk 'have pain', which induce Dative Case-marking of the subject as opposed to the normal Nominative Case-marking:[1]

(1) a. awaḷkkə wiśakkunnu
       she-d   be hungry-Pres.Ind.
       'She is hungry.'
    b. awaḷ   pookunnu
       she-n   go-Pres.Ind.
       'She goes.'

The Dative Case on the subject shows up in association with the <u>wiṣakk</u>-class of verbs, irrespective of whether the sentence is tensed or non-tensed:

(2) a.   awan [awaḷkkə wiṣakkuwaan] praarttʰiccu
        he-n   she-d   be hungry-Inf. pray-Past
        'He prayed for her to be hungry.'
   b.   awan [awaḷ   pookuwaan] praarttʰiccu
        he-n   she-n go-Inf.   pray-Past
        'He prayed for her to go.'

Obviously, it is the Verb, and not Tense, which Case-marks the subject in these cases.

However, the Tense/Aspect system also plays a role in the Case-marking of the subject. Thus Malayalam has an aspect-marker <u>-aam</u> (signifying permission) which requires a Dative subject:

(3) a.   niŋaḷkkə pookaam
        you-d   go-Permission
        'You may go.'
   b.* niŋaḷ   pookaam
        you-n   go-Permission
(4) a.   niŋaḷkkə wiṣakkaam
        you-d   be hungry-Permission
        'You are permitted to be hungry.'
   b.* niŋaḷ   wiṣakkaam
        you-n   be hungry-Permission

Notice that the difference between <u>wiṣakk</u> and <u>pook</u> as regards the Case they assign to the subject (cf.(1a) and (1b)) is neutralized here by <u>-aam</u>, which gives an invariable Dative Case to the subject. We can describe this situation by saying that the Case assigned by Aspect (or Tense) overrides the Case assigned by the Verb, a fact which can be formally encoded as follows. Suppose we say that every verb stem is associated with a feature (or set of features) specifying a Case-frame, and that <u>some</u> of the tense- and aspect-markers are also associated with such features. The Feature Percolation Conventions motivated in Lieber (1980) say that (in lexical structure) it is the features of the affix (and not of the stem) which percolate upward to the dominating branching node; but that if the affix does not specify a value for a certain feature, the value given to it by the stem will percolate upward. Given these conventions, the Case-marking of the subject will take place correctly, as shown below:

(5)

wiṡakk-unnu
[Dative Subject]

wiṡakk
[Dative Subject]

-unnu
[∅-Case Subject]

(6)

pook-unnu
[Nominative Subject]

pook
[Nominative Subject]

-unnu
[∅-Case Subject]

(7)

wiṡakk-aam
[Dative Subject]

wiṡakk
[Dative Subject]

-aam
[Dative Subject]

(8)

pook-aam
[Dative Subject]

pook
[Nominative Subject]

-aam
[Dative Subject]

If a tense- or aspect-marker has its own values for the Case-assignment features, it blocks the values of the corresponding features of the verb; otherwise it is 'transparent' and lets the verb's Case-assignment features go through.

However there is an interesting instance of Case-marking in Malayalam which argues that the feature percolation mechanism is inadequate for expressing the way the lexical features of complex forms are determined. The verb iṡtappeṭ 'like' takes a Nominative subject normally, but takes a Dative subject in the past tense:

(9) a. awaḷ   awane   iṡtappeṭunnu
       she-n   he-a   like-Prés.Ind.
       'She likes him.'
    b.* awalkkə awane   iṡtappeṭunnu
         she-d   he-a   like-Prés.Ind.

c. awaḷkkə   awane   iṣṭappeṭṭu
    she-d   he-a   like-Past
    'She liked him.'
d.? awaḷ   awane   iṣṭappeṭṭu
    she-n   he-a   like-Past

The point to note is that the feature of taking a
Dative subject cannot be associated either with the
verbal stem iṣṭappeṭ (which takes a Nominative subject
normally) or with the past tense-marker (which is
normally a 'transparent' affix in Malayalam, inducing
no Case-marking of its own on the subject). This fea-
ture, therefore, will have to be generated by means of
a rule which becomes applicable at the point where
iṣṭappeṭ and the past tense-marker are put together.
In other words, we need a mechanism to associate fea-
tures directly with complex forms. This should not be
very surprising when we consider that such a mechanism
is needed anyway for lexical semantics; since the
semantic features of a complex lexical item are not
always determined by the semantic features of its com-
ponent elements.

I now wish to suggest that there is an example
closer home (for English speakers) of a Case-frame
being associated directly with complex verbal forms.
It is well-known that the introduction of Passive
morphology brings about a change in the properties of
the verb; this change has been variously analyzed as a
change in the verb's grammatical relations (Relational
Grammar and Lexical Functional Grammar) and as a change
in the verb's Case-marking properties (GB). Simplifying
a little, we can think of Passive morphology as con-
sisting of an auxiliary be and a suffix -en which gets
attached to the verb. Neither the auxiliary nor the
suffix is peculiar to the Passive. The auxiliary
occurs also with the Present Participle of the verb,
which governs the same grammatical (or Case) relations
as the simple active verb. The -en suffix also forms
the Perfect Participle, which (again) has the gram-
matical (or Case) relations of the active verb. There-
fore the relations of the Passive sentence can be
associated with the verb only at the point where it
has been composed with both the suffix and the
auxiliary.2

Whether (or not) the above observation about the
English Passive is correct, we can make the following
generalization on the basis of the Malayalam data
alone: both the verb and the Auxiliary play a role in
determining the Case-frame of a sentence. There are
three ways in which they may do this: the Auxiliary

may be 'transparent', letting the verb's inherent pro-
perties dictate the Case-marking entirely; the Auxi-
liary may be partially 'opaque' — it may modify the
verb's Case-marking in terms of its own Case-marking
feature;[3] the verb and the Auxiliary may compositely
determine Case-marking.

We may also make another observation in the light
of the Malayalam data: in the unmarked case, the verb
determines the Case-frame of the sentence, including
the Case of the subject. If we extend this observation
to English, we can see that there is a better alter-
native to the GB claim that Tense Case-marks the sub-
ject: we can say that Tense in English is 'transparent'
and lets the verb Case-mark the subject; but that
Infinitive and -ing block the verb-induced Case, and
assign no Case of their own.[4]

2

Malayalam has several instances of the relation exem-
plified in English by the sentence pairs:

(10) a. I permitted him to leave.
     b. I gave him permission to leave.

I give below some Malayalam examples:

(11) a. raajaawə mantriye   aṭiccu
         king-n   minister-a beat-Past
         'The king beat the minister.'
     b. raajaawə mantrikkə   oru ati[5] koduṭṭu
         king-n   minister-d one beating-a give-Past
         'The king gave the minister a beating.'
(12) a. raajaawə awane rakšiccu
         king-n   he-a   protect-Past
         'The king protected him.'
     b. raajaawə awanə rakša   nalki
         king-n   he-d   protection-a give-Past
         'The king gave him protection.'
(13) a. raajaawə awane sneehikkunnu
         king-n   he-a   love-Pres.
         'The king loves him.'
     b. raajaawinə awanoodə sneeham unṭə
         king-d   he-d2   love-n   have-Pres.
         'The king has love towards him.'
(14) a. raajaawə mantriye   pookkuwaan anuwadiccu
         king-n   minister-a go-Inf.   permit-Past
         'The king allowed the minister to leave.'

b. raajaawə ma̱ntrikkə pookuwaan
   king-n   minister-d go-Inf.
   anuwaadam koduttu
   permission-a give-Past
   'The king gave the minister permission to
   leave.'

    There are some very interesting facts about Case-
marking and θ-marking in these sentences; but before
we begin to examine them I wish to establish two points.
First, in the (b) sentences of (10)-(14), the deverbal
nominal - e.g. raksa in (12b) - is not part of a [ᵥ N-V]
compound, but an NP which is a sister of V. This should
be obvious in the English example (10b), since gave and
permission are not contiguous; also in a case like (11b),
where the nominal takes an article as a modifier — oru
ati 'a beating'. But in the other cases also, the
nominal can take a modifier (an adjective), cf.

(12b') raajaawə   awanə puurṇamaaya
      king-n   he-d   complete
      raksa   nalki
      protection-a give-Past
      'The king gave him complete protection.'
(13b') raajaawinə awanoodə walutaaya
      king-d   he-d2   great
      sneeham   untə
      love-n   have-Pres.
      'The king has great love towards him.'
(14b') raajaawə   ma̱ntrikkə pookuwaan
      king-n   minister-d go-Inf.
      takkataaya anuwaadam koduttu
      appropriate permission-a give-Past
      'The king gave the minister appropriate
      permission to leave.'

And the nominal can be moved away from the V by the
application of scrambling:

(12b") raksa   awanə   raajaawə   nalki
(13b") sneeham raajaawinə awanoodə untə
(14b") raajaawə pookuwaan anuwaadam ma̱ntrikkə
                                   koduttu

    The second point I wish to make is that none of
the arguments of the deverbal nominal is within the NP
containing that nominal. The arguments are realized
as complements of the V or (as the case may be) as the
subject of the sentence. E.g., (10b) has the structure
(15a), and not (as one might suppose) (15b); and (13b)
has the structure (16a),[6] and not (16b).

(15) a.
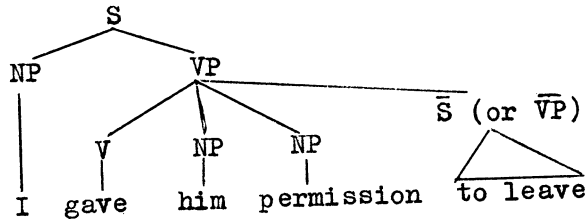
b.
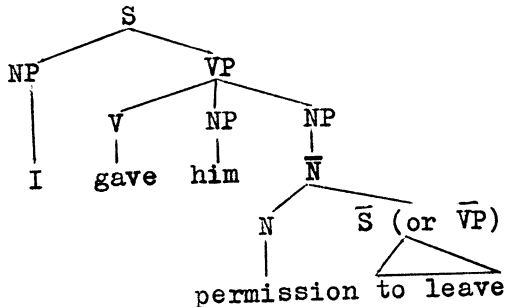
(16) a.
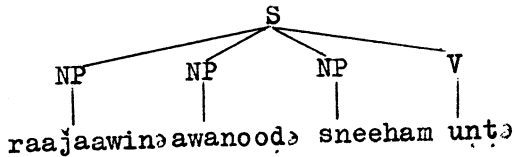
b.
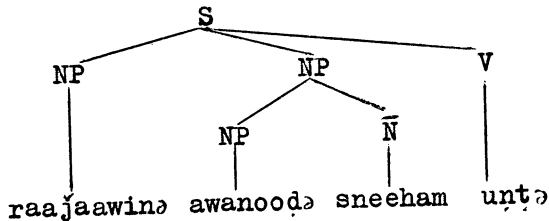
Evidence in support of the structure (15a) (rather than (15b)) comes from the fact that the sentence in question shows the Dative Alternation:

(17) a. I gave him permission to leave. (= (10b))
    b. I gave permission to him to leave.

If permission to leave were a single constituent (constituting an NP), as shown in (15b), it is difficult to imagine how to him could be introduced into

the middle of it. Consider also a sentence which is similar to (17b), namely (18):

(18) He received permission from me to leave.

Now, to-NP is a PP for which give is subcategorized, and from-NP is a PP for which receive is subcategorized (cf. I gave a book to him; I received a book from him). (18) makes the same point as (17b), namely that a PP for which the V is subcategorized cannot very well be embedded within one of the V's NP complements.

In the case of the Malayalam example, we can choose (16a) over (16b) on the ground that awanooda can be moved away from sneeham by scrambling:

(19) awanoodɔ   raajaawinə   sneeham   untə

In Malayalam, scrambling cannot move a part of an NP outside that NP:

(20) a. ñaan   [NPawante   peŋŋale]   sneehiccu
         I-n   he-g   sister-a   love-Past
         'I loved his sister.'
     b.* awante   ñaan   peŋŋale   sneehiccu

     Having established the correct structure of the (b) sentences of (10)-(14), let us note the following facts:

     (i) The Case-frame of the sentences is determined by the verb. This is more apparent in the Malayalam examples, since Malayalam has a more developed Case system. The 'normal' Case-frames of the verbs which occur in (11b) and (12b) are illustrated below:

(21) a. raajaawə   awanə   oru   toppi   koduttu
         king-n   he-d   one   cap-a   give-Past
         'The king gave him a cap.'
     b. raajaawə   awanə   oru   toppi   nalki
         king-n   he-d   one   cap-a   give-Past
         (same as above)

Compare (21a) and (21b) with (11b) and (12b), respectively. Note that one of the positions in the Case-frame of the verb is occupied by the deverbal nominal; the position it chooses seems (invariably) to be that of the Theme-argument of the verb. The arguments of the deverbal nominal are accommodated in the remaining positions of the verb's Case-frame. In making this

'accommodation', the thematic frame of the deverbal
nominal is 'aligned' (in ways which I cannot yet
formalize) with the thematic frame of the verb; this
is best illustrated by the two related sentences,
(17b) and (18). In forming these sentences, the
subject and the direct object of <u>permit</u> are inter-
preted as Source and Goal, respectively; and they are
assigned to the positions of Source and Goal in the
Case-frames of <u>give</u> and <u>receive</u>.

(ii) The sentences (13b) and (14b) illustrate
the situation where the Case-frame of the verb cannot
accommodate all the arguments of the deverbal nominal.
The 'normal' Case-frame of <u>kodukk</u> is illustrated in
(21a); that of <u>untə</u> is illustrated below:

(22) raajaawinə   oru   toppi   untə
     king-d   one   cap-n   have-Pres.
     'The king has a cap.'

Compare (21a) and (22) with (14b) and (13b), respec-
tively. The extra argument in (14b) presents no prob-
lem for Case-marking, since it is sentential and there-
fore needs no Case. In (13b), the extra argument is
marked Second Dative.[7] The general Case-marking stra-
tegy in such situations seems to be to give the extra
argument a Case which is determined by its thematic
role.

But the fact of there being an "extra" argument
highlights an important aspect of the structure of
(13b) and (14b) (and indeed, of all the (b) sentences
of (10)-(14)); namely that in each of them, the selec-
tional frame (thematic structure) which underlies the
sentence is that of the deverbal nominal. Thus,
<u>kodukk</u> 'give' is not strictly subcategorized to take
a sentential complement; yet in (14b) it must take
such a complement, because a sentential argument is
part of the selectional frame of <u>anuwaadam</u> 'permis-
sion'. Similarly, <u>untə</u> 'have' does not take an argu-
ment in the Second Dative; yet (13b) is incomplete
without this argument, because it is required by the
θ-frame of <u>sneeham</u> 'love'. I.e., in sentences of
this type, the selectional frame is projected by the
deverbal nominal, whereas the Case-frame is projected
by the verb.[8] Let us call this phenomenon "dual pro-
jection".

"Dual projection" sentences have several impor-
tant consequences for the Projection Principle of
Chomsky (1981). This principle claims that syntactic
representations at all three levels — D-structure,

S-structure, and LF — are projections of the thematic
structures (hence the subcategorizations) of lexical
items. The Projection Principle is stated as follows
(see (5) and (6) of chapter 2, Chomsky (1981)):

(23) Given either of the following structural con-
figurations, where α is an immediate consti-
tuent of γ :

$$[_\gamma \cdots \alpha \cdots \beta \cdots]$$

$$[_\gamma \cdots \beta \cdots \alpha \cdots]$$

    (i) If β is an immediate constituent of γ
at L_i, and $\gamma = \bar{\alpha}$, then α θ-marks
β in γ

    (ii) if α selects β in γ as a lexical pro-
perty, then α selects β in γ at L_i

    (iii) if α selects β in γ at L_i, then α
selects β in γ at L_j.

(The variables L_i and L_j range over the "syntactic
levels": LF, D-structure, S-structure.) Case (i) of
(23) claims that a lexical category θ-marks all its
complements. This is clearly untrue of the "dual
projection" sentences, where the verb's complements
are not θ-marked by the verb, but by the head of one
of its complements. (The most striking cases of this
type are sentences like (13b) and (14b), where there
are more arguments in the operative θ-frame than the
verb's Case-frame can accommodate.) Case (ii) says
that if a lexical or phrasal category is lexically
specified as θ-marking a certain position (which it
c-commands), it does this at every syntactic level.
This condition is irrelevant to the "dual projection"
sentences, since the deverbal nominal does not
c-command the positions it θ-marks; also, it is dif-
ficult to imagine how the "quirky" θ-marking that we
are considering can be expressed by a lexical speci-
fication. (Case (iii) simply stipulates that θ-mark-
ing is the same at all three syntactic levels.)
    We see that "dual projection" sentences pose a
serious problem for Chomsky's (1981) version of the
Projection Principle. Yet obviously, we cannot
abandon the central insight of such a principle,
namely that syntactic structure is determined by the
thematic structure of lexical items <u>in some way</u>. What
we must clearly abandon is the notion that the map-
ping between thematic structure and syntactic

structure is a trivial one. To determine the exact
nature of this mapping will be a challenging task for
future research.

I wish to suggest that we can make a beginning
by considering again an insight expressed in <u>Aspects</u>
(Chomsky 1965); namely that selectional restriction is
not "strictly local". Suppose we say that one of the
important differences between Case-marking and θ-mark-
ing is the following: Case-marking by a lexical cate-
gory must be 'local' with respect to the maximal pro-
jection of that category; but θ-marking by a lexical
category may be 'non-local'. Of course, the next
most pressing (and interesting) question is: in what
precise ways may θ-marking be non-local?

Notes

1. n = Nominative; a = Accusative; d = Dative;
   d2 = Second Dative; g = Genitive.

2. This conclusion can be avoided only by claiming
   that English has two homophonous verb forms, a
   Perfect Participle and a Passive Participle, each
   governing a different Case-frame (or set of gram-
   matical relations). This (indeed) is the proposal
   of Lexical Functional Grammar (Bresnan 1982).
   (The invariable identity of form of the Perfect
   and Passive participles can perhaps be explained
   by positing a morphological process of zero-
   affixation which derives one from the other.)

3. The Auxiliary is never totally 'opaque'. Thus,
   the Malayalam aspect-marker -aam imposes its own
   Case on the subject but otherwise lets the verb's
   Case-marking features go through. In fact, I have
   not been able to find an auxiliary element which
   affects the Case-marking of more than one position
   in the sentence. A thought which may be relevant
   at this point is the following: Suppose my obser-
   vation about the English Passive is incorrect.
   Then we have no counterexample (that I am aware
   of) to the following generalization: the Auxiliary
   can affect the case only of the subject, not of
   any position in the VP. It would be interesting
   to find an explanation for this.

4. Such a move has the following implications. If
   the positions which a category Case-marks must be
   a subset of the positions it governs, we must now
   say that the verb also governs the subject. Mohanan
   (1982a) discusses some facts about pronominal

interpretation in Malayalam which (in fact) suggest this conclusion. While there is no difficulty in devising a way to make the verb govern the subject (one can simply treat S as a projection of V), this move (in turn) has implications for the distribution of PRO, which may necessitate an extensive revision of GB theory.
Another tack to pursue would be to delink Case-marking and government. This would make it possible for the subject to be Case-marked by the verb but not governed by it. (But see, again, the evidence of Mohanan (1982a).)

5. The Accusative is overtly marked only in the case of an animate NP. Since the Nominative Case-marker is 'Ø', oru ati in the example above is indistinguishable from the Nominative.

6. Malayalam probably has a "flat" structure; see Mohanan (1982b) for some evidence.

7. The Second Dative is used in Malayalam for the Goal of certain types of directional verbs, e.g.
   ñaan   awanoodə   oru   kaaryam   paraññu
   I-n   he-d2   one   matter-a   say-Past
   'I spoke to him about a (certain) matter.'

8. This fact has been independently noted by K.P. Mohanan (Mohanan 1982c).

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Markers of Definiteness in Indo-Aryan
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Introduction

It is a known fact that Indo-Aryan languages do not have articles comparable to English article the and indefinite article a(an). Is the absence of overt markers in any way an indication of the absence of definiteness/indefiniteness? Definiteness is a semantic notion which is conveyed not only through morphological and syntactical markings but also through discourse mechanisms. Now that we can study the grammar beyond the sentence level, we find explanations for some linguistic phenomena that were previously unclear. Definiteness is such a phenomenon. This paper focuses on the linguistic means by which definiteness is expressed in Indo-Aryan. It deals with definiteness from two viewpoints: formal and functional. It seeks to state the distribution of definiteness and the functional conditions which govern its distribution.

The literature on definiteness in Indian languages is scarce. The recent paper by Masica (1980) is perhaps the only study that deals directly with the subject. The article compares the definiteness marking in South Asian languages including Dravidian and Munda and suggests its marking on direct objects as a feature for the areal typology of South Asian and Central Asian languages. His analysis includes more languages but is less unified.

Before turning to analysis let us clarify the concept of definiteness. How does it differ from indefiniteness semantically? It seems that the speaker, when using a definite noun phrase, issues a form of instruction to the hearer to "locate" or place the referent of the definite NP within one of a number of pragmatically defined sets of objects. He locates the referent in the sense that he understands that the object-referred to is a member of this set. Thus the primary function of definiteness is to identify a particular referent of the definite NP. Identity of the noun phrase may be established on the basis of (1) linguistic context; (2) spatio-temporal context of the speech act, i.e. non-linguistic; or (3) previous shared experience of speaker and adressee.

(1) (Marathi) ekā   gāvat ek šetkarī hotā. tyālā ek mulgī hotī
              one village-in one farmer was. to-him one girl was
              eke divši šetkaryāne mulilā   šetāt paṭhvile.
              one day farmer(agent) the daughter farm-in sent

    (Hindi)   ek   gāvmē   ek kisān thā. Uskī ek laṛkī thī.
              one village-in one farmer was his one daughter was
              ek din kisānne larkī ko   khet mē bhejā.
              one day farmer the daughter farm in sent

    (Bengali) æk grāme   æk čaša chilo. tār ekṭi meye chilo
              one village-in one farmer was his one daughter was
              æk din čāšā   meye ke   khete pāṭhālo.
              one day farmer the daughter farm-in sent.

              "There was a farmer in a village. He had a daughter."
              One day the farmer sent the daughter to the farm."

(2) (Marathi)   sitāna kāl ek āmbā khāllā. āmbā cavdār hotā.
     (Hindi)   sitāne kal ek ām   khāyā. ām svādistha thā.
     (Bengali)   šitā kāl ꬱektā ām   khelo. āmṭā šuššādu chilo.
             Sita yesterday one mango ate. mango tasty was.
          "Sita ate a mango yesterday. The mango was tasty."

Notice the second occurrence of farmer, daughter and mango in
Hindi and Marathi. Though these noun phrases are without deter-
miners they are definite. The hearer can identify these noun phrases
because they have been referred to in earlier sentences. Bengali,
however, uses t̄a (t̄i) to mark specificity as in amṭā and ꬱektā.

(3) (Marathi)   āj   tu   devḻāt jašil ?
     (Hindi)   āj   tu   mandir jāyegī?
     (Bengali)   āj   tumi mandire jābe?
             today you   temple go(fut.)?
          "Will you go to the temple today?"

The temple refers to the unique temple within the village that
speaker and hearer are in or come from. The hearer can locate the
referent using his knowledge about the situation, i.e. extralinguis-
tic context.

(4) (Marathi)   sinemā canglā hotā?
     (Hindi)   sinemā acchā thā?
     (Bengali)   sinemāṭā bhālo chilo?
             the movie good was
          "Was the movie good?"

The appropriateness of the first mention of the definite noun
sinema depends basically on speaker and hearer sharing knowledge
about its referent. The speaker knows that the hearer has seen a
movie and assumes that the hearer can identify the movie he is
referring to.
     Thus interpretation of definite reference for the hearer consists
in the task of identifying the correct set which the speaker intends
and in locating the referent in it (Hawkins 1976). On the other
hand, indefinite descriptions are pragmatically neutral with respect
to the set of identification and referent location aspects of defin-
iteness. In fact indefinite mention may function to mark a noniden-
tifiable referent. But its positive function is to establish a new
"file" in the hearer's consciousness (Du Bois 1980:220). In the
examples (1) and (2) the indefinite ek serves both to assure the
hearer that he need not look elsewhere to identify the referents (as
they are nonidentifiable) and to encourage him to establish new
cognitive files for a particular village, for a particular farmer,
for a particular girl and for a particular mango.
     When the speaker assumes that the hearer knows the referent
(when in fact he does not) and hence does not specify or somehow
fails to provide the hearer with information necessary for estab-
lishing a new file, the ambiguity occurs. This specifically happens

in sentences where the new file is opened with a definite mention.
For example:

(5) (Marathi)   rāmṅa   kāl   s̲i̲n̲e̲m̲ā̲   pahilā.
     (Hindi)   rāmne   kal   s̲i̲n̲e̲m̲ā̲   dekhā.
     (Bengali)   rām   kal   s̲i̲n̲e̲m̲ā̲   dekheche.
                 Ram yesterday movie   saw

     "Ram saw $\begin{Bmatrix} *a \\ *the \end{Bmatrix}$ movie yesterday."

     Special measures are taken to provide the missing information.
The nouns are further specified as ek sinemā 'a movie' or to sinemā
'the (that) movie'.
     So far we have seen that ∅-forms presuppose identifiability and
mark definiteness, whereas ek-forms which are neutral to identifia-
bility and establish new files mark indefiniteness. In addition,
Bengali seems to use t̲ā̲ for marking specificity of indefinite as
well as definite nouns.

```
                          Nouns
              _____/       _____
      Definite                              Indefinite
    [identifiable]                           [neutral]
      /      \                              /        \
specific  non-specific           specific      non-specific

āmt̲ā̲       ∅                  æktā̲  ām          ∅
```
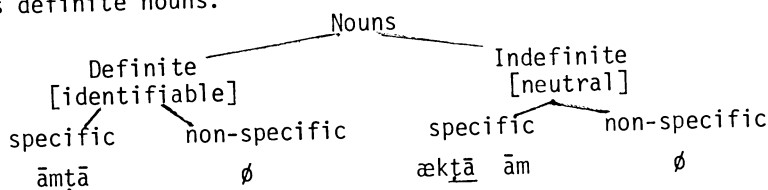
     Now let us look at the linguistic devices that Indo-Aryan
languages use in marking definiteness. We know that proper names,
definite pronouns, possessive noun phrases, in fact all nouns in
oblique case relations, if not marked specifically for indefinite-
ness, presuppose identifiability and are definite. "If a noun phrase
is said to be identifiable, this means simply that the hearer can
establish a link between the noun phrase and the concept it refers
to" (Du Bois, 218). The demonstratives in Indo-Aryan which are
formally undifferentiated from the third personal pronouns except
for stress, have the function of marking definiteness. For example
to maṇus means "the man." They convey definiteness through their
anaphoric/cataphoric pronominal function or through their article-
like function. A great many 'that's' and 'one's' have to be trans-
lated by definite articles.
     What other devices are there besides the ones mentioned so far?
One common device is the marking of the Definite Objects. Most Indo-
Aryan languages mark Direct Objects as well as Indirect Objects with
the same suffix. Hindi uses ko, Marathi uses lā and Bengali uses
ke. Consider some of the following examples.

Definiteness through morphological marking:

	Direct object NP's without definite markers	Direct object NP's with definite markers

(6) (M.) mi āj kutrā vikat ghetlā.　　　　mi āj <u>kutryālā</u> vikat ghetla.
　　 (H.) mainẽ āj kuttā xaridā.　　　　　　mainẽ <u>āj</u> <u>kutteko</u> xaridā.
　　 (B.) āmi āj ekṭā kukur kinechi.　　　　āmi āj <u>kukurṭā</u> kinechi.
　　　　　 I today one dog　 bought　　　 I today the dog　 bought
　　　　 "I bought a dog today."　　　　 "I bought the dog today."

(7) (M.) mi āj dhobī pāhilā.　　　　　　　mi āj dhobyālā pāhila.
　　 (H.) mainẽ āj dhobī dekhā.　　　　　　mainẽ <u>āj</u> dhobīko dekhā.
　　 (B.) āmi aj dhopā dekhechi.　　　　　āmi āj <u>dhopāke</u> dekhechi.
　　　　　 I today washerman saw　　　　　 I today the washerman saw
　　　 "I saw a washerman today."　　　 "I saw the washerman today."

(8) (M.) mi āj pustak vācla.　　　　　 *mi āj　 pustakālā vācla.
　　 (H.) mainẽ āj kitāb parī.　　　　　　 ? mainẽ āj <u>kitābko</u> paṛā.
　　 (B.) āmī āj boi poṛechi.　　　　　āmi　 āj　 <u>boiṭā</u> poṛechi.
　　　　　 I today book　 read　　　　　　 I today the book　 read
　　　 "I read $\left\{\begin{array}{l} *a \\ *the \end{array}\right\}$ book today."　　 "I read the book today."
　　　 (I did the act of reading.)

The underlined NP's in column (2) with the definite object marker are identifiable and therefore definite. However, there seems to be a constraint on the use of the marker. The inanimate object nouns in Hindi and Marathi do not use the marker and become nonspecific in their interpretation as in (8). The nonspecific object noun with its verb constitutes a unitary predicate concept, what may be called an object conflation (Du Bois, 259). The object nouns of this type are generally bound to verbs and form a complex idea which is essentially verbal (Masica 1980). Because of the compound verb formation this category of nouns is quite important in Indo-Aryan.

The rules regarding compound verb formation or object conflation seem to be language particular. There may occur differences in the usage of the marker. Some nouns with the marker may be more easily acceptable in one Indo-Aryan language than in another, as in example (8). On the other hand, animate and human nouns in most Indo-Aryan languages seem to use the marker regularly. In Bengali only human objects take <u>ke</u>. This is perhaps due to the fact that they are not often made part of object conflation or compound verbs. Humans are generally too independently salient to be conflated with a verb (Du Bois, 220). This can be seen in example (9).

(9)　(Marathi)　　? mi <u>dukāndar</u> āṇlā.
　　 (Hindi)　　　? mainẽ <u>dukāndār</u> <u>lāyā</u>.
　　 (Bengali)　　? āmi　<u>dokāndār</u> enechi.
　　　　　　　 I shopkeeper　 brought
　　　　　 "I brought the shopkeeper here."

(10) (Marathi)   *mi <u>sitā</u> pāhilī.
      (Hindi)   *mainĕ <u>sitā</u> dekhī.
      (Bengali)   *āmi <u>šitā</u> dekhechi.
                   I Sita   saw
              "I saw Sita."

In Bengali the sentence is all right if <u>Sita</u> is taken to refer
to the Goddess, where the reference is to the concept of god.
     Proper nouns as in (10), being always definite, use the marker
obligatorily.
     We have seen that the nouns in the second column are definite.
That does not mean the nouns in the first column are indefinite.
They seem to mark nonreferentiality rather than nonidentifiability,
and they open up new files in the hearer's consciousness.
     The use of the marker is obligatory for Indirect Objects. Be-
tween the Indirect Object and the Direct Object, the former has the
first claim to the marker. Once the Indirect Object is marked,
the Direct Object remains unmarked. The object-NP does not make
reference to the definite or indefinite nouns. They rather become
part of the predicate conflation.

(11) (Marathi)   mi <u>dhobyālā</u> sādī dilī.
      (Hindi)   mainĕ <u>dhobiko</u> sāṛī di.
      (Bengali)   āmi <u>dhopāke</u> šaṛī diechi.
                   I the washerman sari gave

          "I gave $\begin{Bmatrix} \text{*a} \\ \text{*the} \end{Bmatrix}$ sari to the washerman."

Indirect objects are likely to be personal nouns and personal
nouns as we have seen before demand overt marking.
     Thus from the above examples and their discussion, it seems
that the use of the marker is more dependent on semantic considera-
tion of the nouns such as human, animate, inanimate, etc., rather
than on the syntactic form of the noun phrase. In other words, the
use of the marker is not incompatible with unidentified or indef-
inite noun phrases. See examples (12) and (13).

(12) (Marathi)   *titha mi   <u>ekā pustakālā</u> pāhila.
      (Hindi)   *vahā mainĕ <u>ek   kitābko</u> dekhā.
      (Bengali)   *okhāne āmi ꬺ<u>ektā   boitā</u> dekhechi.
                   there I   one   book   saw
              "I saw a book there."
(13) (Marathi)   titha mi <u>ekā mānsālā</u> baghitla.
      (Hindi)   vahā mainĕ <u>ek ādmiko</u>   dekhā.
      (Bengali)   okhāne āmi ꬺ<u>ektā lokke</u> dekhechi.
                   there I   one man   saw
              "I saw a man there."

In Bengali <u>ke</u> is a human   object <u>marker</u>; and since <u>tā</u> is spec-
ificity marker, as mentioned earlier, ꬺ<u>ektā</u> and <u>loktā</u> together are
impossible.

In the absence of the marker, some Direct Object nouns (with appropriate verbs) have generic interpretation. This is consistent with the fact that both definite and generic nouns are identified. "A plural generic mention provides access to a concept in the interlocutor's mind which is representative of the whole class and since there is only one such concept, any mention which is understood as generic will be "identified" with this concept." (Du Bois, 225). Notice, the generic subject nouns are also without determiners, as in (15).

(14)　(Marathi)　tu <u>vāgh</u> pahilās?
　　　(Hindi)　　tune <u>bāgh</u> dekhā?
　　　(Bengali)　tumi <u>bāgh</u> dekhecho?
　　　　　　　　you tiger　saw
　　　　　　　"Have you seen a tiger?"
(15)　(Marathi)　<u>whel</u> stanpāyī prāṇi āhe.
　　　(Hindi)　　<u>whel</u> stanpāyī prāṇi hai.
　　　(Bengali)　<u>timi</u> stɔnnopāyi prāni.
　　　　　　　　whale　mammal　　(is)
　　　　　　　"A whale is a mammal."

Unmarked preverbal NP's as in (16) are ambiguous between definite and indefinite. They are disambiguated by marking indefiniteness with <u>ek</u> as in (17), and definite with the demonstrative pronouns comparable to English 'that' or 'those', as in (18). Bengali, however, prefers to use the specificity marker <u>tā</u>.

(16)　(Marathi)　mi <u>santra khālla</u>.
　　　(Hindi)　　mainẽ <u>santrā khāyā</u>.
　　　(Bengali)　āmi <u>lebu kheyechi</u>.
　　　　　　　　I orange　ate
　　　　　　　"I have eaten an orange."
　　　　　　　"I ate the orange."
(17)　(Marathi)　mi　<u>ek santra</u> khālla.
　　　(Hindi)　　mainẽ　<u>ek santrā</u> khāyā.
　　　(Bengali)　āmi　<u>æktā lebu</u> kheyechi.
　　　　　　　　I　one orange　ate
　　　　　　　"I ate an orange."
(18)　(Marathi)　mi　<u>te santra</u>　khālla.
　　　(Hindi)　　mainẽ <u>vah santrā</u> khāyā.
　　　(Bengali)　āmi　<u>lebutā</u> kheyechi.
　　　　　　　　I　the orange　ate
　　　　　　　"I ate $\begin{Bmatrix} that \\ the \end{Bmatrix}$ orange."

Definiteness through Syntactic Processes

Besides the morphological marking, some syntactic processes also convey Definiteness. In this paper I will discuss two such processes: 1. Relativization and 2. Topicalization.

Relativization

Indo-Aryan has both restrictive and nonrestrictive relative clauses. I will discuss only the restrictive relative clauses. The relative clause occurs in two dominant positions: (A) preceding the main clause and (B) following the main clause.

A. Relative clause precedes the main clause:

(19) (Marathi) jo māṇus titha ubhā āhe to majhā bhāu āhe.
     (Hindi)   Jo ādmi vahā khaḍā hai vah merā bhāi hai.
     (Bengali) Je loktā okhāne ḍariye āche śe amār bhāi.
               who man there standing is that/he my brother
     "The man who is standing there is my brother."

B. Relative Clause follows the main clause:

(20) (Marathi) to māṇus majhā bhāu āhe jo titha ubhā ahe.
     (Hindi)   vah ādmi merā bhāi hai . Jo vahā kharā hai.
     (Bengali) ?? oi loktā amār bhāi Je okhāne ḍariye āche.
               that man my brother who there standing is
     "That man is my brother who is standing there."

As seen in (20) the construction where the relative clause follows the main clause is unnatural in Bengali.

These restrictive relative clauses have quite a few variants depending on the application of the rules, Relativization, NP-deletion, Pronominalization, and also on the permutation of the elements in both the clauses, relative and main. The variants are not semantically equivalent and they fulfill different discourse functions depending on the presuppositions, linguistic context, and pragmatic considerations. Here I will discuss considerations only for the order of clauses. The function of the S-initial relative clauses is to establish the identity of the noun to which they refer. An individual or a thing is mentioned for the first time. The absence of the NP in the relative clause but its presence in the main clause violates the function of the relative clause (21), which is to identify an NP. One has to introduce the noun in the first clause, then talk about it in the second clause.

(21) (Marathi) ? jo titha ubhā āhe to māṇus majhā bhāu ahe.
     (Hindi)   ? Jo vahā kharā hai vah ādmi merā bhāi hai.
     (Bengali) ? Je okhāne ḍarīye āche śe loktā amār bhāi
               who there standing is that man my brother
     "The one who is standing there (that man) is my brother."

The function of jo is not that of a relative pronoun. It simply marks a topic in a conversation (Junghare 1980). The speaker assumes that the hearer does not know about the standing man (the information in the first jo-clause) and, furthermore, does not know that he is the speaker's brother. Since the identity of the referent described by the S-initial relative clause is not presupposed, NP's in these clauses are [-definite] unless specifically marked.

The function of the Main-Rel. construction seems to be assertive. The relative clause asserts some attribute of an individual who is already identified. Notice the following sentence is questionable in the light of discourse.

(22) (Marathi) ? <u>to</u> mājhā bhāu āhe <u>jo mānus</u> titha ubhā āhe.
      (Hindi)   ?? <u>vah</u> merā bhāi hai <u>jo ādmī</u> vahā kharā hai.
      (Bengali) * <u>še</u> āmār bhāi <u>je loktā</u> okhāne dāriye āche.
             he my brother who man there standing is
        "He is my brother (the man) who is standing there."

In (22) the occurrence of the anaphoric pronouns (<u>to, vah</u> and <u>še</u>) in the sentence initial position implies that the discourse is quite advanced and the NP has been specified a long time before, hence it is redundant to introduce the antecedent nouns with the topic markers in the relative clause that occurs after the main clause, because the function of such a relative clause is to give more information about a specified noun. The Relative Clause construction in which a relative clause follows the main clause contains information which is presupposed. Notice for example (20) can answer the following two Marathi questions but (19) cannot.

(23) to mānus kon āhe ?
     that man who is
     "Who is that man?"
(24) to mānus kon āhe jo titha ubhā āhe?
     that man who is who there standing is
     "Who is that man who is standing there?"

Since the second type of Relative Clause construction in which the relative clause follows the main clause has presuppositions: (1) someone is there, and (2) someone is in the standing position, the construction is surely [+definite].

It seems that the relation between Relative Clause constructions and definiteness can be described in terms of a continuum, the Rel.-Main clause sequence being on the left, Main-Rel. sequence somewhere in the middle, and non-restrictive relative clauses on the right.

    Correlative (jo--to)-----Restrictive (to--jo)-----Non-Rest.
     least definite ─────────────────────>───────────── most definite

Definiteness through Topicalization and Word Order

The notion of dividing a sentence in two parts for the study of information structure is well known in linguistics. The <u>topic</u> refers to what the speaker is talking about in the structure, while <u>comment</u> refers to what the speaker has to say about the topic. This distinction of <u>topic</u> and <u>comment</u> is parallel to Clark's (1977) "Given" and "New" contract. Clark has pointed out that when speakers-hearers engage in talk, they abide by this contract, that is, the speaker is responsible for making syntactically as "Given" that information that he thinks the listener already knows, and makes as "New" that which he thinks the listener does not know. English with

its fixed word order uses syntactic devices such as definite articles, cleft constructions and anaphoric pronouns to mark given elements. Indefinite articles and pseudo cleft constructions mark the information that is new to the listener. Indo-Aryan languages use different devices such as topicalization of relative clauses and other elements at the sentence level as well as word-order variation to fulfill similar communicative purposes.

Topicalization is an important process in Indo-Aryan languages in which the topicalized element is brought into sentence initial position. The underlined noun phrases in (25) are indefinite. But when they are topicalized as in (26), they can have the definite interpretation.

(25) (Marathi) bāget phula hotī.
      (Hindi)   bagičemē phul the.
      (Bengali) bāgāne phul chilo.
                 garden-in flowers were
                 "There were flowers in the garden."

(26) (Marathi) phula hoti bāget.
      (Hindi)   phul the bagičeme.
      (Bengali) phul chilo bāgāne.
                 flowers were garden-in
                 "The flowers were in the garden."

But the NP in (27) seems to be problematic. It can be interpreted as definite as well as indefinite.

(27) (Marathi) pakṣi uḍālā.
      (Hindi)   pakṣī uṛā.   "$\begin{Bmatrix} A \\ The \end{Bmatrix}$ bird flew."
      (Bengali) pākhī uṛeche.
                 bird   flew

This dual status of the NP is due to the fact that it can be subject as well as topic. As the subject of the sentence it can be indefinite but as the topic it must be an identified NP whose referent can be found in linguistic, situational or universal context of discourse. Initial NP's are not always topics. As non-topic subjects they can be definite or indefinite.

Masica (1980:6) mentions the following example (from Khanna 1971:29) as an exception of the topicalized NP that is not definite.

(Hindi) ek bāt mãi ne noṭ kī -- "One thing I noted"

The NP ek bāt is not really indefinite in the sense of 'any thing'. Rather it makes a reference to 'one particular thing.' Note the meaning of the NP in the non-topicalized construction. mainẽ ek bāt noṭ ki means "I noted one particular thing" rather than "I noted a thing." In the above sentence the NP is topicalized for emphasis. Object NP's that are overtly marked for indefiniteness occur in sentence initial position only for marking specificity. The meaning of ek 'a' changes to 'one' as in (28).

(28) (Marathi) ek pustak wācla tyāna.
      (Hindi)   ek kitāb paṛī usne.   "He read one book."
      (Bengali) ǣkṭā boi poṛeche še.
                 one book read   he

The statement in (28) answers the question: "How many books did he read?"

Definiteness and Word Order Variation

Indo-Aryan languages are identified as SOV type since their favored unmarked order is SOV. Many SOV languages such as Dravidian have what appears to be a rather free word order. There is greater freedom in the positioning of elements and the order in which they are placed. In these languages, where there is a great deal of word order variation, word order is not strictly required to serve a grammatical function. That is to say a change in the order of nominal constituents does not signal a change in the relationship between the predicate and its arguments. In these languages, the word order rather serves a pragmatic function and the word-order variation is used for communicative purposes and discourse strategies. Appropriate mention of the noun phrases on the part of the speaker and the correct identification of those NP's on the part of the hearer is necessary for the discourse continuity. The word order variation may provide such a clue to the identification of NP's. Let us examine the six possible variants of SOV in which the object is not specified for its definiteness.

(a) mi santra khālla.   [what did you eat?]
(b) mi khālla santra.   [who ate the orange?]
(c) santra mi khālla.   [who ate the orange?]
(d) santra khālla mi.   [who ate the orange?]
(e) khālla mi santra.
(f) khālla santra mi.

"I ate $\begin{Bmatrix} an \\ the \end{Bmatrix}$ orange.

It seems that the focus falls on the preverbal element. The noun unspecified for definiteness occurs only in preverbal position. In any other position it is definite. Let us examine the sentence with four constituents.

(a) mi rāmlā pustak dila.   (g) pustak mi rāmlā dila.
(b) mi pustak rāmlā dila.   (h) pustak rāmlā mi dila.
(c) mi dila rāmlā pustak.   (i) pustak dila mi rāmlā.
(d) rāmlā mi pustak dila.   (j) dila mi rāmlā pustak.
(e) rāmlā mi dila pustak.   (k) dila rāmlā mi pustak.
(f) rāmlā pustak mi dila.   (l) dila pustak mi rāmlā.

"I gave $\begin{Bmatrix} a \\ the \end{Bmatrix}$ book to Ram."

All the variants except one (a) have presuppositions. The variants with presuppositions are definite constructions. Therefore the nouns in these variants though unmarked, are definite. The exception is the unmarked or normal word order of the form: Subj.-Ind.Obj.-Dir.Obj.-Verb, as in (a). A sentence with this order of elements does not have any presuppositions. It is this order which is normally used to begin a discourse. And during the initial stage of a discourse, if the noun is not marked for indefiniteness, it can be interpreted as definite or indefinite.

The non-marked noun in preverbal positon, because of its conflatory nature with the verb, can be intepreted as part of the compound verb. In other positions there can be no possibility of the noun conflating with other elements, subject, indirect object, or adverbs. Therefore the unmarked nouns in other positions are interpreted as definite whoe referents are known to the addressee.

Summary and Conclusion

On the basis of the comparative study of these three languages, the following general comments can be made. In Indo-Aryan languages Definiteness seems to be an unmarked category and Indefiniteness seems to be a marked one. All singular nouns not overtly marked as Indefinite are interpreted as Definite, except in preverbal position where they may be Indefinite because of compounding. The unmarked nature of Definiteness is perhaps a universal phenomenon. Definiteness is an unmarked category in English, as has been suggested by Du Bois (1980:254). From a discourse point of view, it seems reasonable that a speaker would not repeat the referent if the hearer can identify it easily.

Indo-Aryan languages seem to mark definite objects obligatorily if they are animate. On the other hand, the marking of definite inanimate objects is not so strict and the inanimate object nouns are therefore subject to various interpretations: Generic (Indefinite) Specific (Unidentified or Indefinite), Definite or Non-specific (not making reference to any object, but just a part of compound verb category). For clarity, these nouns have to be specifically marked as Indefinite (with ek-) or Definite (with the proper demonstrative).

In terms of the marking of definite objects, these languages seem to take three positions. Bengali seems to mark definiteness much more than Hindi and Marathi. Its use of the marker ṭā, (ti) is almost like the English article 'the'. Marathi seems to be the most relaxed one, marking definiteness least. Hindi stands somewhere in between. With regards to Topicalization of relative clauses and of other elements within a sentence, Marathi makes more use of the topicalization process than Hindi and Bengali. Bengali seems to be strict and does not allow these processes as freely as Marathi. Hindi again falls in between. Similarly, Marathi allows more word order variation than both Hindi and Bengali. The relationship between definiteness, topicalization, and word order variation in Indo-Aryan languages suggests the following generalization.

The more topicalization and word order variation a language allows, the less restricted its marking of definiteness.

If a language can use already available devices (such as topicalization and word order variation) for marking definiteness, there is no need for that language to invent new ones (such as markers or articles). On the marking scale of definiteness, Marathi stands at the low end, Bengali at the upper end (marking the definite status most) and Hindi near the middle.

This study raises several questions which are worth pursuing. Does the strict marking of Definiteness in Bengali and not so strict marking in Marathi represent two different stages in the development of Indo-Aryan? Does Marathi represent an earlier stage and Bengali the advanced one? We know that Sanskrit allows a great deal of word

order variation and seems to be relaxed in its marking of definite-
ness, i.e. does not use particles or articles for marking definite-
ness. Or is it the case that relaxed marking of definiteness in
Marathi is due to the areal influence of Dravidian? And if it is
the case, how is the Definiteness marked in Dravidian? Do Dravidian
languages use markers or do they use other devices? Does Indo-Aryan
as a whole, the Eastern branch of Indo-European, represent an earlier
stage and the Western Indo-European languages (English, French, etc.)
the advanced stage in the development of Definite marking in Indo-
European? Homeric Greek did not have definite articles, as has been
mentioned by Hawkins (1976).

There are many other stones left unturned in this study. We
know that the genericness or specificity of a noun is related to the
kind of verb it occurs with, and furthermore on the tense and aspect
of the verb (Chafe 1970) (ex.: hattyālā śengdāṇe āvaḍtāt vs.
hattyālā śengdāṇe āvaḍle or hattyāna mājhi gāḍi toḍli). Therefore
the study of Definite marking requires a detailed examination of the
verbs, tenses and aspect and their relationship with nouns. Also,
an extensive study of noun status in discourse of all the Indo-Aryan
languages is needed before we can come up with solid generalizations.[1]

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LINGUISTIC COMPETENCE AND FOLK THEORIES OF LANGUAGE:
TWO ENGLISH HEDGES

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In the ordinary sense in which we say that words like chair and table are ABOUT furniture, hedges are words about language and speech.[1] There is nothing remarkable in this; language is part of our environment, and we have words about most things in our environment. The linguistically interesting aspect of hedges is that, although they are about language, they are not exactly used to talk about language as we would say that chair and table are used to talk about furniture or, for example, gerund and entailment are used to talk about language. When we use a word like chair or table or gerund or entailment, chairs, tables, gerunds, and entailments do not become ipso facto part of what is said. With hedges it is different; when we use a hedge like loosely speaking, the notion of 'loose speech' which this expression invokes becomes part of the combinatorial semantics of the sentence and utterance in which it occurs. A familiar (if probably vacuous) combinatorial semantic rule is

(SR) If adjective a denotes class A and noun n denotes class N, then the denotation of the expression an is the intersection of the classes A and N.

I wish to claim that the notion of 'loose speech' is part of the combinatorial semantics of sentences containing the expression loosely speaking in the same way in which the notion of class intersection is claimed by proponents of (SR) to be part of the combinatorial semantics of an expression like red chair.
A hedged sentence, when uttered, often contains a comment on itself or on its utterance or on some part thereof. For example,

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[1] The present paper is based on a much longer work on hedges which is still in progress but part of which has been made semi-public in a typescript ms. (Kay, 1979) of which the subtitle was 'hedges revisited.' The word 'revisited' referred to the well known paper of George Lakoff (1972). In Kay (1979) I discuss in detail Lakoff's approach to hedges and my own agreements with and divergences from that approach; space does not permit a recapitulation of that discussion here. Also in that (1979) paper there are references to personal communications and advice from many people whose contributions cannot be recited here, although all have helped shape my view of the subject. I must acknowledge, however, a very general intellectual debt to Charles Fillmore and George Lakoff.

when someone says, <u>Loosely</u> <u>speaking</u> <u>France</u> <u>is</u> <u>hexagonal</u>, part of
what they have uttered is a certain kind of comment on the locution
<u>France</u> <u>is</u> <u>hexagonal</u>. In this sort of metalinguistic comment, the
words that are the subject of the comment occur both in their fami-
liar role as part of the linguistic stream and in a theoretically
unfamiliar role as part of the world the utterance is about. Such
metalinguistic reference seems unaccounted for (an perhaps unac-
countable for) in standard theories of semantics that are based on a
context-free, recursive definition of truth for sentences, and in
which linguistic objects and world objects (or objects in a model)
belong to disjoint realms. The problem, I believe, goes beyond
that of indexicality as usually conceived, and although it would
be interesting to investigate in detail the relation between the
kinds of facts to be discussed here and discussions of indexicality
within model theoretic semantics (e.g., Kaplan 1977), that compari-
son will not be attempted. The omission might be justified by
appeal to limitations of space, but such a plea would be less than
candid, as I suspect that the phenomena I will describe constitute
a principled set of exceptions to any theory of natural language
meaning that makes a rigorous separation between truth conditional
meaning for linguisitc types (i.e., sentences), normally called
'semantics,' and other aspects of meaning, frequently called 'prag-
matics' (see, for example, Gazdar 1979: 2f). The latter claim
would, to be sure, require considerable clarification before a demon-
stration could be begun. In the present paper I must content my-
self with presenting a few facts and some timid empirical generali-
zations.

The principal conceptual tool I will employ for stating these
empirical generalizations will be that of 'folk theory.' The term
is borrowed from anthropology. In describing the system of know-
ledge and belief of another culture, an anthropologist speaks of
that culture's folk theory of botany, the emotions, language, and
so on. Anthropologists discover such folk theories by analysis of
the use of words in the native language. The guiding idea is the
familiar one that any natural lexicon implies a tacit, structured
conceptualization of the stuff that the words of that lexicon are
about. What the words we shall be concerned with here are about is
language and speech, and the folk theory we shall be looking for is
the tacit and mostly unconscious theory of language and speech we
invoke when we employ certain parts of the lexicon of English.

The present essay is thus in the first instance lexicographical.
But we will see that in the domain of hedges, lexicography is in-
separable from combinatorial semantics because the schemata or
folk theories that constitute the semantic content of the hedges
as lexical items serve as combinatorial structures for putting to-
gether the meaning of the sentences in which the hedges occur.
Hence, world knowledge about language--what I have called folk
theories of language--may at times be part of knowledge OF language.

Knowledge of language, linguistic competence, is commonly
distinguished from knowledge of the world. Linguists do not gen-
erally consider it a matter of interest that the language we are
competent in is also in our world and therefore a thing of which we

have world knowledge, i.e., a folk theory. Certainly linguists do not often ask whether world knowledge of language bears some special relation, that other sorts of world knowledge do not bear, to the knowledge that constitutes linguistic competence. Perhaps the question is not posed because the answer is considered obvious, namely NO. The facts to be considered below suggest, however, that the folk theory of language presupposed by various hedges should be interpreted both as world knowledge ABOUT language and as knowledge OF language--i.e., as part of linguistic competence.

The data to be considered in this paper concern two hedges, loosely speaking and technically. The concept 'folk theory' will figure in the analysis of the meaning of each of these expressions. The comparison of the two analyses will reveal a not altogether obvious difference between the folk theories that constitute our tacit knowledge of the world (as realized in word meanings) and consciously formulated theories: folk theories, like conscious theories, answer to a requirement of local consistency but, unlike conscious theories, folk theories answer to no requirement for global consistency.

Loosely Speaking

The hedge loosely speaking may be employed in the service of a variety of semantic and/or pragmatic functions which, from a traditional point of view, appear disconcertingly diverse. Let us consider some of the possible semantic-pragmatic roles of loosely speaking in the response of Anthropologist A to Layman L in the following dialogue.

(1) L: Where did the first human beings live?
    A: Loosely speaking the first human beings
       lived in Kenya.

First, believing the evolutionary process to be inherently gradual, A may consider the expression 'the first human beings' to be semantically ill-formed and hence devoid of the capapcity for non-vacuous reference. If A had this problem, believing that 'the first human beings' couldn't possibly refer to anything, he might reply more fully

(2) A: Strictly speaking, one can't really talk
       about 'the first human beings,' but loosely
       speaking, the first human beings...

Secondly (and alternatively), A may think that 'the first human beings' is a normal referring expression, but not the one that picks out exactly the entity about which he wishes to assert 'lived in Kenya. For example, A may consider it important to distinguish in this context 'the first human beings' and 'the first human beings known to science. If this were A's reason for hedging with loosely speaking, his fuller answer might be along the lines

(3) A: Strictly speaking, we can only talk of
the first human population known to
science, but loosely speaking, the first
human beings...

A's problem may be not with 'the first human beings' but rather
with 'in Kenya.' A third motivation for loosely speaking could then
be that A considers the unhedged sentence 'The first human beings
lived in Kenya' to have a reading which presupposes the modern
nation of Kenya to have existed at the time the first human beings
were alive. Such fastidious pedantry might motivate a longer reply
along the lines

(4) A: Loosely speaking, in Kenya. Strictly speaking
in the place now called Kenya.

Fourthly, and perhaps most typically, A may think that the un-
hedged sentence 'The first human beings lived in Kenya' oversimpli-
fies or otherwise distorts the pertinent facts, but is nonetheless
the best he can do given the exigencies of the conversational situ-
ation. Sometimes the demands of Gricean Quantity (Say no more than
necessary) and Manner (Be brief) require a sacrifice in Quality
(Tell the truth). In our present example, the relevant facts might
involve sites not only in Kenya but also in Uganda and Tanzania,
fossils of uncertain relation to each other, and so on. Loosely
speaking can be and probably often is used to apologize for this sort
of deficiency in Quality, induced by the demands of Quantity and
Manner. The fuller version of A's reply could be something like

(5) A: Loosely speaking in Kenya. Strictly speaking,
we are dealing here with a complex situation
involving sites mainly in Kenya, but also in
Tanzania and Uganda, and with a set of fossils
which may not all represent the same species...

Examples (2-5) illustrate four distinct kinds of 'loose speech'
that the hedge loosely speaking may reflect in (1): (i) the use of
an incoherent description in an act of reference (2); (ii) the use of
a coherent but 'wrong' description in an act of reference (3); (iii)
the utterance of a sentence that (the speaker feels) permits an un-
intended interpretation that contains a false presupposition (4); and
(iv) the utterance of a sentence that is defective in Gricean Quality,
that is, in truth (5).

What, then, does loosely speaking mean? George Lakoff (1972)
gives the example

(6) (a) A whale is a fish. (FALSE)
(b) Loosely speaking, a whale is a fish. (TRUE)

and argues that the semantic function of loosely speaking is that of
a predicate modifier which, through selection of certain features

of meaning internal to the intension of a category word like fish, maps it into another category-type intension. But we see that this cannot be correct, since in (1) loosely speaking does a variety of things that have nothing to do with the modification of a category word. Furthermore, it may do several of these things simultaneously: in uttering his part of (1), A might be bothered by any combination of the factors discussed in connection with (2-5) (except of course those combinations containing both (2) and (3), since these happen to be mutually exclusive.). Thus the semantic scope of loosely speaking must be at least as broad as the entire sentence it accompanies, e.g., in (6)(b) the sentence a whale is a fish. Since presence or absence of loosely speaking in a sentence such as (6) may affect our judgment of its truth, the classical view holds that loosely speaking must make a contribution to the semantics of the SENTENCE in which it occurs. Since the scope of loosely speaking must be at least as broad as the whole sentence it accompanies, its scope must be that whole sentence, and one is tempted to conclude that the semantics of loosely speaking is a function from sentence intensions to sentence intensions--i.e., from the set of worlds in which whales are fish (the null set in some theories) to the set of worlds that are like this one with respect to the fishiness of whales. But nothing of this sort can be right because, as we saw in connection with (2), loosely speaking sometimes functions to comment directly on the FORM of the sentence it accompanies.

Moreover, when (6)(a) is changed to (6)(b) by the addition of loosely speaking the reason that our judgment changes from false to true is not that a false proposition P (= a whale is a fish) has been changed into some true proposition P'. Rather, we abstain from judging (6)(b) false because we understand (6)(b) both to assert the sentence a whale is a fish and to express a reservation regarding the adequacy of that assertion. If the dimension of adequacy is taken to be that of truth (tightness of 'word-to-world fit') as seems to be the relevant dimension in the case of (6)(b), then we have no trouble accepting a judgment of true. In the general case, however, the dimension of adequacy directly addressed by the hedge loosely speaking need not be that of truth: the loose speech referred to may involve laxness in obedience to the rules of language, as in (2) and perhaps (3), or even looseness with respect to stylistic canons, as in (4). Of the four examples, (2-5), only (5) directly concerns truth, and even in this case, we do not experience (1) as expressing some proposition P', which is distinct from but closely related to 'The first human beings lived in Kenya,' and which is exactly true.

The empirical claim about loosely speaking that I have attempted to develop may be summarized as follows:

(7) For any sentence S of the form loosely speaking
P, where P is a declarative sentence, an utterance
of S constitutes two acts:
(i) an act of asserting P,
(ii) an act of warning that (i) is in some
way a deviant (loose) act of assertion.

Probably the most typical way for an assertion to be deviant is in terms of Quality, but, as we have seen an assertion may have other kinds of defects about which loosely speaking warns.

If (7) is even approximately correct, expressions such as loosely speaking present an interesting challenge to current formal theories of semantics and pragmatics. If loosely speaking means what (7) says it means, this is surely its literal meaning (not figurative, ironic, etc.). Although (7) specifies the literal meaning of loosely speaking, (7) does not consist of a specification of truth conditions of either S or P, but rather expresses a warning to the addressee that he should be wary in his acceptance of the assertion of P. If (7) is correct, literal meaning and truth conditions cannot always be the same thing, not even almost the same thing.

It is not obvious how the meaning of an expression like loosely speaking is to be captured in a theory of the generally accepted kind, where the truth conditional meaning of a sentence is established in terms of a possible world semantics independent of pragmatic considerations, and no feedback from pragmatic reasoning to literal meaning is countenanced. But even supposing that with sufficient ingenuity we could develop an account of loosely speaking within this kind of framework, it is not clear that we should wish to do so. If we look at the different kinds of semantic-pragmatic functions that may be accomplished by loosely speaking (illustrated, though by no means exhausted, in (2-5)), we find that they constitute, from the traditional view, a disparate collection. Another way to view the same matter is to notice--as the reader may already have done--that (7)(ii) is stated far too broadly. Loosely speaking doesn't point to just any kind of deviance in an act of asserting. For example, acts of assertion that deviate because they contain uninterpretable indexicals or because they fail to answer a question just posed are not examples of 'loose speech.'

(8) (a) Jack and John were running and $\left\{\begin{array}{c}\text{*loosely speaking he}\\ \text{one of them}\end{array}\right\}$ fell down.

   (b) A: When did Mary get her car tuned up?
       B: *Loosely speaking, because the engine was knocking.

I have spoken informally of the various kinds of 'loose' speech represented by examples (2-5), and in this informal usage I think lies the key to the semantic unity of the expression loosely speaking. I suggest that what enables us to speak informally about 'loose' speech in connection with all of these examples is what constitutes the actual semantic unity of the expression loosely speaking. In every utterance of a sentence like (1), the linguistic act of asserting that the first human beings lived in Kenya is talked ABOUT (in the same familiar sense in which we say that in the utterance of a sentence like trout eat flies trout are talked about). That is, when we say Loosely speaking P we bring to bear part of our world knowledge of what it is to assert something, or, as I

would prefer to say, we bring to bear part of our folk theory of language and speech--the part that concerns assertion. We have knowledge, beliefs and schematizations of language and speech just as we have knowledge, beliefs and schematizations of everything else in our experience. When we use a hedge like <u>loosely speaking</u> in an utterance we use it to talk about some other part of that same utterance, and so at one level we use our world knowledge of language and speech in the same way we use our world knowledge about zoology when we employ the word <u>trout</u> or <u>fly</u>. <u>Loosely speaking</u> interprets the utterance in which it occurs as a world object according to a particular folk theory of utterance, which is part of our larger folk theory of language and speech.

To speak loosely is to assert something not quite true. Typically, loose speech is speech that would be true in a world slightly different from the one we are describing, but in some cases we characterize our speech as loose if it fails to achieve precise truth because of some defect in its construction. Expert theories of language and speech normally make a strict distinction between locutions that don't (quite) state propositions and locutions that state propositions that aren't (quite) true; but not all parts of our unconscious folk theory of language and speech insist on this distinction; <u>loosely speaking</u> appears to invoke such an area of the folk theory.

Technically

<u>Technically</u>, used as a hedge, has a meaning that may be roughly glossed 'as stipulated by those persons in whom society has vested the right to so stipulate.' Thus when we say, <u>Technically, a whale is a mammal</u>, we appeal to the fact that systematic biologists have decreed that, whatever we common folk may say, whales are mammals. One line of evidence for this analysis of <u>technically</u> comes from pairs of synonyms--or near synonyms-- of which one member belongs to an authoritative jargon; in such pairs only the member from the jargon takes the hedge <u>technically</u>.

(9) (a) Technically, that's a rodent. (order <u>Rodentia</u>)
    (b) *Technically, that's a varmint.

(10) (a) Technically, that's an insect. (order <u>Insecta</u>)
     (b) *Technically, that's a bug.

The (b) versions may be heard as attempts at humor precisely because the words <u>varmint</u> and <u>bug</u>, not only belong to no technical jargon, but, on the contrary, are markedly colloquial.

Further, if we hear a sentence like

(11) Technically, street lights are health hazards.

our reaction is to wonder WHO has decreed that street lights are health hazards and BY WHAT AUTHORITY. If we learn that the Surgeon General of the United States has done so, even if we reject his arguments and therefore question the wisdom of the stipulation, we

cannot legitimately deny the claim expressed in (11). If, on the other hand, we learn that an individual genius has proclaimed street lights to be health hazards on grounds we consider impeccable, we will surely agree that street lights are in fact health hazards, but we may well deplore that the claim expressed in (11) is not the case.

Lakoff (1972) attributes to Eleanor Rosch a revealing example similar to the following,

(12) Technically, a TV set is a piece of furniture.

pointing out that the sentence can have different truth values in different contexts, if there exist in society two distinct bodies with the authority to make such stipulations about TV sets and furniture. For example, moving companies might designate TV sets as furniture, while the insurance industry excludes TV sets from furniture.

Given this account of the meaning of technically, we may ask whether technically displays the two properties of hedges, previously discussed, that provide problems for standard formal semantics. These, it will be recalled, are (a) that the lexical meaning of a hedge may become one of the organizing schemata of the combinatorial semantics of the sentence in which the hedge occurs, and (b) that a hedged sentence may contain a metalinguistic comment regarding the way in which a word or phrase of the sentence is being used in that sentence.

Regarding property (a), if we sketch the logical structure of (12) in terms of our intuitive account of technically, we get something with the rough structure of (13), in which we find that the effect of the word technically is not confined to a single element but is distributed throughout the quantificational and predicational structure of the sentence.

(13) There is an x such that Society has authorized x to stipulate the meaning of TV set, and Society has authorized x to stipulate the meaning of furniture, and x has stipulated the former to be included in the latter.

The precise wording of (13) is not intended to be taken literally; the point of (13) is just that most of the 'logic syntax' of (12) comes from the word technically. The lexical meaning of technically provides the structural skeleton of the meaning of sentences, like (12), in which it occurs. In this respect, technically acts like 'logical' words (e.g., all, and, not) are supposed to act. But we noted that technically is a substantive, world-knowledge-embodying word; in fact it is precisely by virtue of the folk theory it embodies regarding language, society, and the social division of linguistic labor that technically achieves its organizing function in sentences like (12). Semantics and mere lexicography find themselves confounded.

That technically displays property (b)--regarding metalinguistic comments in which the linguistic item(s) MENTIONED are simultaneously USED as a regular linguistic counters--is not apparent from the examples so far given (9-12). One reason for this is that since the target words

(e.g., TV set and furniture in (12)) appear with the generic indefinite article, the examples conduce to a straightforward interpretation in which these words are mentioned, but not also used. Consider, however, the following.

(14) The movers have come for your furniture, which
technically includes TV sets.

Here the word furniture is both used and mentioned: furniture is used in the ordinary way as the lexical head of a definite noun phrase, your furniture, to pick out a set of world objects; furniture is simultaneously mentioned as the topic of a metalinguistic comment, which informs us that, by stipulation of relevant authorities, the extension of furniture includes TV sets.

Comparison of loosely speaking and technically

In the case of each of the two hedges considered, I have sought to explain both its lexical meaning and its combinatorial semantic function in terms of a metatheory in which there is a linguistic system disjoint from the world whose elements (words, sentences) may be combined to represent objects and states of affairs in the world via the meanings or intensions of those elements. The sentence Snow is white is true... This general schematization of language is familiar as an informal sketch of the basic intuitions that lie behind the formidable accomplishments of that tradition of semantic theorizing which descends from Frege via Tarski to the modern proponents of model theory, including in particular the various versions most relevant to linguists arising from the work of Richard Montague. In this framework, words may refer to or represent world objects because the former have intensions that may be matched by the actual properties of the latter.

This conscious theory of language, and particularly of reference, has recently been opposed by the baptismal-causal theory of Kripke (1971) and Putnam (1975). The reader may have noticed that in discussing the meaning of technically, I had recourse to Putnam's phrase 'the division of linguistic labor' (1975: 145ff). The part of the folk theory of language which technically invokes seems in its main lines to agree with the theory of Kripe and Putnam, especially Putnam's version. On this view, a word refers, not via an intension it contains, but on account of someone having once stipulated that henceforth this word shall designate some ostensively presented thing or thing-type. Putnam's idea that we have unconscious recourse, in using a word like gold, to the notion of some expert or official who has the right and the knowledge to diagnose real world gold in a presented sample is especially close to the account I have given above of that aspect of the folk theory of language which underlies the use of technically.

Thus when we use loosely speaking, we are taking a Fregean view of language and, moreover, because of property (a), we are organizing the semantics of our utterance in accord with Fregean notions. On the other hand, when we use the hedge technically, we are taking a Putnamian view of language and are organizing the semantics of our utterance along Putnamian lines. If a natural language like English has a formal seman-

tics that employs logical schemata such as conjunction, negation, etc., to compose the meaning of a sentence from the meaning of its parts, then we must number among that same array of structure-composing schemata such substantive folk beliefs about language as those implicitly underlying the explicit theories of reference associated with scholars like Frege and Putnam. These are the combinatorial semantic schemata invoked by loosely speaking and technically respectively.

Folk theories

I have written throughout this paper in terms of a single folk theory of language and latterly pointed out that this 'theory' differs from conscious theories in that it is not internally consistent. I could as easily have written that English encodes a variety of different folk theories of language. The distinction would have been merely terminological and the same conclusions would have been reached. There are two points here: the first is that a folk theory does not present a globally consistent whole the way a conscious, expert theory does. This should surprise no one, since it is precisely the conscious reflection characteristic of expert theorizing that is generally considered to produce its global coherence. The second point is that folk theories are not 'believed' in the way conscious theories are but are used or presupposed as the occasion of thought or communication demands. The penetration of these folk theories of language into the semantic structure of language, via hedges, appears to present several challenges to the generally accepted framework of much current semantic theory.

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Bengali Conjunctive Participle Constructions*

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This paper is concerned with the conjunctive participle construction, a characteristic device for linking sequences of clauses in Bengali.[1] According to Masica 1976, similar constructions are also found in many languages of the Indian subcontinent both related and unrelated to Bengali, and also in Amharic, Japanese and Korean, Persian, Finno-Ugric and Altaic languages, Greek, Latvian, and Slavic languages, among others.

I shall first describe some basic formal properties of Bengali conjunctive participle constructions. Then I shall take up the problem of clausal compatibility; i.e., how speakers determine that two or more given clauses may be linked through conjunctive participle formation into a single sentential matrix.

1. Formal Properties

(1) a. se baari giye kaapor chere   caan korlo
        he house go-CP clothes leave-CP bath did
        'He went home, took off his clothes, and had a
                          bath'

    b. osudh   kheye jodur phal   holo
        medicine eat-CP Jodu-G benefit became
        'Jodu was benefited by taking medicine'

    c. lu   lege   se maaraa gaelo
        heatstroke affect-CP he died
        'He got heatstroke and died'

Some examples of Bengali conjunctive participle constructions (CPCs) are given in (1). Each consists of one or more nonfinite clauses plus a single finite clause. We may hypothetically represent an underlying form for the first example, (1a), as shown in (2):

(2) [[se baari jaa-] [se kaapor chaar-] se caan korlo][2]
     he house go   he clothes leave   he bath did

Comparing (1a) with (2) it is seen that the verbs of the two leftmost clauses are reduced; they take that special nonfinite form known as the conjunctive participle. In the rightmost clause, however, the verb is not reduced and serves as the finite predicate of the overall sentence (1a).

Together with the reduction of clausal verbs we notice that only one of the underlying clause subjects in (2) surfaces in (1a). This results from the operation of a subject deletion process that is identity conditioned. Notice that the deletion process operates on all but one underlying instance of the common subject NP. Ostensibly, the undeleted instance of the common subject may surface in any of the component clauses of the CPC. This is illustrated in (3):

(3) a. <u>se</u> baari giye kaapor chere   caan korlo (=1a)
        he house go-CP clothes leave-CP bath did

    b. baari giye <u>se</u> kaapor chere   caan korlo
        house go-CP he clothes leave-CP bath did
    c. baari giye kaapor chere   <u>se</u> caan korlo
        house go-CP clothes leave-CP he bath did

There are two possible accounts for the preceding. The first is that the subject exempted from deletion, or controlling subject, originates in any of the component clauses of the construction. The other possibility is that the controlling subject always belongs to a particular clause, serves as the antecedent for the deletion of subjects in noncontrolling clauses and subsequently may be moved out of its original position. In fact, it is the latter which obtains. The way to test for this is to consider CPCs in which the common subjects of the component clauses differ in case marking. Consider (4) and (5), the hypothetical underlying structures corresponding respectively to (1b) and (1c).

    (4) [[jodu osudh   khaa-] jodur phal   holo]
         Jodu medicine eat   Jodu-G benefit became

    (5) [[taar lu   laag-] se maaraa gaelo]
         his   heatstroke affect he died

In (4) and (5), the leftmost and rightmost clauses have coreferential subjects, but these subjects take different case markings. In (4), the leftmost clause has a direct case subject while the rightmost clause has an indirect or 'dative' subject.[3] In (1b), the surface version of (4), we see that the controlling subject is the subject of the rightmost clause, the finite clause. The subject of the leftmost or nonfinite clause is deleted. Observe, in (6), that the sentence is unacceptable if the nonfinite clause subject is made the controlling subject:

    (6) *jodu osudh   kheye phal   holo
        Jodu medicine eat-CP benefit became

Now consider (5). Here the leftmost or nonfinite clause has an indirect ('dative') case subject. The rightmost or finite clause has a direct case subject. In (1c), the surface form of (5), the controlling subject turns out to be once again the finite clause subject. Example (7), in which the subject of the nonfinite clause has been made the controlling subject, is unacceptable:

    (7) *taar lu   lege   maaraa gaelo
        his heatstroke affect-CP died

This evidence indicates that, when deletion under identity of subject NPs occurs in CPCs, the controlling subject is invariably the subject of the finite clause. Therefore it is to be inferred that sentences such as (3a) and (3b) arise by the movement of the controlling subject out of its original position to the head of a nonfinite clause.

2. The Same Subject Condition

    Let us consider how speakers determine that two or more given

clauses are suitable to be linked into a single CPC. Writers on
several languages of the Indian subcontinent (Dwarikesh 1971 and
McGregor 1977:38ff. on Hindi; Arden 1969:200 and Rhenius 1845 on
Tamil; Forbes 1862:139; Chatterji 1926:1003, and Dimock et al. 1976:
190-191 on Bengali) have postulated a single formal condition on
the formation of CPCs. This is given in (8):

> (8) The Same Subject Condition (SSC)
>   A CPC may be formed only when the subjects of
>   all component clauses are coreferential.

All Bengali CPCs illustrated so far in this paper conform to
the SSC. Consider (9a), the hypothetical underlying structure of
which is given in (9b):

> (9) a. omuk   unnisso pancaas saale   jomi kine
>   so-and-so 1900   50   year-L land buy-CP
>   kuri bacchor pare anek laabhe   bikri korlo
>   20 year   later many profit-L sale did
>   'So-and-so bought some land in 1950 and twenty
>   years later sold it off for a big profit'
>   b. [[omuk   unnisso pancaas saale   jomi kin-]
>   so-and-so 1900   50   year-L land buy
>   omuk   kuri bacchor pare   anek laabhe   bikri
>   so-and-so 20 year   later many profit-L sale
>   korlo]
>   did

The SCC accounts for the acceptability of (9a) because each of
its component clauses, as shown in (9b), shares a common subject
omuk 'so-and-so'. At the same time, the SSC accounts for the un-
acceptability of (10a):

> (10) a, *jodu thele   modhu pore gaelo
>   Jodu push-CP Modhu fell-down
>   'Jodu gave a push and Modhu fell down'
>   b. [[jodu thel-] modhu pore gaelo]
>   Jodu push   Modhu fell-down

As (10b) shows, (10a) consists of two clauses which do not share
a coreferential subject, and therefore by the SSC the resulting sen-
tence (10a) must be unacceptable.

The SSC is adequate in many instances, such as (9) and (10).
However, it does not account for the judgments of Bengali speakers
in all cases. In other words, we cannot say that speakers determine
clausal compatibility for the purposes of conjunctive participle for-
mation purely in accordance with the SSC. Compare (9) with (11):

> (11) a. *omuk   gato bacchor iurope   giye ei   bacchor
>   so-and-so last year   Europe-L go-CP this year
>   hankane   giyeche
>   Hong Kong-L has-gone
>   'So-and-so went to Europe last year and this year
>   he's gone to Hong Kong'
>   b. [[omuk   gato bacchor iurope   jaa-] omuk
>   so-and-so last year   Europe-L go   so-and-so

```
        ei    bacchor hankane        giyeche]
        this year    Hong Kong-L has-gone
```

Unlike (9a), (11a) is unacceptable even though, as (11b) shows, it conforms to the SSC. This demonstrates that the SSC cannot be the sole condition on the subjoining of clauses in CPCs. But what is worse, the SSC cannot be even a partial condition on CPCs. This is because many Bengali CPCs are acceptable even though the SSC is violated. Consider (12):

```
(12) a. ceaar bhenge giye    modhu pore gaelo
        chair break down-CP  Modhu fell off
        'The chair broke and Modhu fell off'
     b. [[ceaar bhenge jaa-]  modhu pore gaelo
        chair break down      Modhu fell off
```

(12b), the hypothetical underlying structure of (12a), shows that the two component clauses have distinct subjects. Yet the CPC (12a) is acceptable. On the face of it, this acceptability is dif-ficult to understand, since the partially similar sentence (10a) (repeated here for convenience) is unacceptable:

```
(10) a. *jodu thele    modhu pore gaelo
        Jodu push-CP Modhu fell down
        'Jodu gave a push and Modhu fell down'
```

It needs to be emphasized that an example like (10a) is not con-troversial in terms of informants' judgments, nor is it stylistically peculiar. CPCs which violate the SSC are found not only in the spoken register of Bengali, but also occur in the written language. Examples are found even in the work of some of the most conservative and respected writers. Karpushkin 1964:88-89 has culled a number of literary instances including those in (13):

```
(13) a. ghare  bristi prabes   kariyaa jal  bosiyaache
        room-L rain    entrance do-CP   water has-sat
        'The rain having come into the room, water
            accumulated' (Bankim Chandra Chatterji,
                          Racanabali v. 2, Calcutta 1954:60)
     b. raannaagharer paase   praacir bhaangiyaa katokgulo
        kitchen-G      beside wall    break-up   some
        it    jaro       haiyaa     aache
        brick piled-up become-CP are
        'Beside the kitchen, a wall having broken, some
        bricks are piled up' (R. Tagore, Galpaguccha,
                          Calcutta 1954:165)
```

It would seem desirable to formulate a condition on the forma-tion of CPCs which would account for the same data which the SSC accounts for, and which would also account for those acceptable sentences that the SSC does not account for. With this goal in mind, I have collected and analyzed a number of counterinstances to the SSC. In general, they have fallen into several broad types.

3. Counterinstances

First, in many counterinstances to the SSC, the subjects of each

of the component clauses are inanimate. As in (14):

(14) a. ṭaeks beṛe giye   anek jiniser daam
       tax   increase-CP   many thing-G price
       beṛe giyeche
       has-increased
       'Taxes have gone up and the prices of many
                   things have increased'
    b. ottodhik baroph poṛe   samosto sosso nasṭo
       excessive snow   fall-CP entire   crop   ruined
       hoye giyeche   'Heavy snow has fallen and all
       has become   the crops have been destroyed'
    c. ghare brisṭi prabes   kore kaaṭher mejhe
       room-L rain entrance do-CP wood-G floor
       nasṭo holo   'The rain entered the room and
       ruiṇed became   the wooden floor was ruined'

By constrast, in many sentences which violate the SSC and which are unacceptable, the subjects of the various component clauses are animate; as in (15):

(15) a. *aalaauddin diper upore haat buliye ekṭi jin
       Aladdin   lamp-G upon hand rub-CP one   genie
       beriye   uthlo
       go out-CP rose
       'Aladdin stroked the lamp with his hand and a
                  genie came out'
    b. *Jodu thele   modhu poṛe gaelo   (=10a)
       Jodu push-CP Modhu fell down
       'Jodu gave a push and Modhu fell down'

There is also a class of counterinstances to the SSC in which one clause has an animate subject. However, the referent of this subject NP does not 'do' the denoted action. Rather, the action happens to him. Illustrations are given in (16):

(16) a. bomaa pheṭe   aekjon   maaraa gaelo
       bomb explode-CP one-person died
       'A bomb exploded and one person died'
    b. ceaar bheṅge giye   modhu poṛe gaelo   (=12a)
       chair break down-CP Modhu fell off
       'The chair broke and Modhu fell off'

It is possible for even more than one component clause in a CPC to have an animate subject to whom the denoted action 'happens'; and in these instances, too, the SSC may be violated. (17) is an instance in point:

(17) a. baabaa maaraa giye se anek muskile   porlo
       father die-CP   he many difficulty-L fell
       'His father died and he fell into many difficulties'

As another class of counterinstances to the SSC, we observe numer- ous examples in which one of the component clauses is an indirect or 'da- tive' subject construction. Some illustrations are given in (18):

(18) a. ṭaeks beṛe giye   aneker kasto   hoyeche
       tax   increase-CP many-G difficulty has-become

'Taxes have increased and many people have had
difficulties'
b. bristi pore   caasider   laabh holo
   rain fall-CP farmers-G profit became
   'It rained and the farmers profited'

4. Volitionality

In Bengali, indirect or 'dative' subject predicates tend
to contrast with formally and semantically similar direct sub-
ject predicates. Indirect subject constructions invariably ex-
press activities viewed or spoken of as nonvolitional. Consider
(19) and (20):

(19) a. *se ghum bhaanglo   b. taar ghum bhaanglo
        he sleep broke   his sleep broke
        'He awoke'   'He awoke'

(20) a. se upos bhaanglo   b.?*taar upos bhaanglo
         he fast broke   his fast broke
         'He broke his fast'   'He broke his fast'

In each of these instances the (a) examples show direct sub-
ject predicates and the (b) examples exhibit the corresponding in-
direct subject predicates. The act of waking up denoted in (19)
is a nonvolitional act. Notice that an indirect subject expression,
(19b), is used to express it; the direct counterpart (19a) is
starred. Contrastingly, the act of breaking a fast denoted in (20)
is typically a volitional one. It tends to be expressed by the
direct subject construction (20a). One speaker has remarked to me
that the indirect subject counterpart (20b) is possible only in a
situation in which the subject breaks his fast in a nonvolitional
way, e.g., by being force-fed. Examples (19) and (20) are cited
here as only a small and illustrative part of the total evidence
that, in Bengali, the direct/indirect subject distinction at the
formal level correlates with a volitional/nonvolitional distinc-
tion at the semantic level. For the complete evidence see Klaiman
1980a and 1980b.[4]

In light of these facts we may take note that (18a,b) contrast
with (21a,b) respectively:

(21) a. *taeks bere giye   aneke kasto   peyeche
         tax   increase-CP many   trouble   have-found
         'Taxes have increased and many people have
                            taken trouble'
     b. *bristi pore   caasiraa laabh korlo
         rain fall-CP farmers profit have-made
         'It rained and the farmers have taken advantage'

The indirect subject clauses that we observe in (18a,b) are
replaced in (21a,b) by their respective direct subject counter-
parts. The results differ slightly in meaning; compare the reading
assigned to (21a) with that assigned to (18a), and also compare the
reading assigned to (21b) with that assigned to (18b). In each of
the examples in (21), the finite clause expresses an action which

is viewed as deliberate or volitional from the standpoint of the subject's involvement. It seems reasonable to hypothesize that the contrast in acceptability between the corresponding examples of (18) and (21) is due to the factor of volitionality. In all the acceptable counterinstances to the SSC which have been presented so far in this paper--exes. (14), (16), (17) and (18)--each subjoined clause expresses a nonvolitional activity. Therefore it would seem that the SSC only applies in CPCs in which at least one component clause expresses a volitional act. Now let us look at some additional data.

5. Refining the Hypothesis

Consider the examples in (22):

(22) a. tinte begun bheje samosto tel phuriye gaelo
3-pc eggplant fry-CP entire oil was-used-up
'All the oil was used up frying three eggplants'

b. apaaresaan koreo   omuk   baaclo   naa
operation do-CP-although so-and-so survived not
'Although an operation was performed, so-and-so
                    did not survive'

c. oaasaartaa bodliye   kaltaa   thik hoye gaelo
the-washer change-CP the-tap okay became
'By changing the washer the tap became all right'

All these CPCs violate the SSC. Moreover, in each instance the reduced (leftmost) clause expresses a volitional action. However, no NP denoting the doer of that action is specified in any instance. It is interesting that, in all examples of this type, unacceptability arises if any doer subject is specified. Thus compare (22a-c) respectively with (23a-c):

(23) a. *raadhuni tinte begun   bheje samosto tel
cook   3-pc eggplant fry-CP entire oil
phuriye gaelo   'The cook fried three eggplants
was-used-up   and all the oil was used up'

b. *daaktaar bos apaaresaan koreo   omuk
Doctor Bose operation do-CP-although so-and-so
baaclo   naa   'Although Dr. Bose performed an
survived not   operation, so-and-so did not
                    survive'

c. *mistri oaasaartaa bodliye   kaltaa thik
plumber the-washer change   the-tap okay
hoye gaelo   'The plumber changed the washer and
became   the tap became all right'

Why should the specification of the doer subject affect the acceptability of these CPCs? To achieve some insight into the matter, let's consider the following examples:

(24) a. bomaa phete   aekjon maaraa gaelo (=16a)
bomb explode-itr.-CP 1-person died
'A bomb exploded and one person died'

b. *PLOraa bomaa phaatiye   aekjon   maaraa gaelo
PLOs bomb explode-tr.-CP 1-person died
'The PLOs exploded a bomb and one person died'

    c. PLOraa bomaa phaaṭiye   jan smithke maarlo
        PLOs   bomb explode-tr.-CP John Smith-O killed
        'The PLOs exploded a bomb and killed John Smith'

In (24a), each of the component clauses in the CPC expresses
a nonvolitional activity. The sentence is acceptable even though
it violates the SSC. (24b) also violates the SSC, but it is un-
acceptable. Notice that the reduced (leftmost) clause denotes a
volitional activity and has a specified subject. Without alter-
ing this clause, the only way to make the sentence acceptable is
as shown in (24c); namely, to modify the finite clause so that the
specified subject of the reduced clause becomes the common subject
of both clauses.

This would seem to indicate that, when a specified subject can
be assigned to a clause denoting volitional activity, then that
same subject must also serve as the subject of the other component
clauses in the CPC. Consider (25):

    (25) a. aajkaal   praay pet   keṭe   baacchaa
              these days often stomach cut-CP child
              prasob   hay   'These days children are often
              childbirth becomes   born by Caesarian section'
          b. *peṭ   keṭe   se ekṭi baacchaa prosob   korlo
              stomach cut-CP she one   child   childbirth did
             'She had a child by Caesarian section'

(25a) is similar to the examples shown earlier in (22), and
is acceptable. The reduced or leftmost clause expresses a voli-
tional act and the finite or rightmost clause is an indirect sub-
ject construction. However, neither clause has a specified subject.
But consider (25b). This sentence is unacceptable because it must
be interpreted as if the unspecified subject of the leftmost clause
were deleted under identity to the subject of the rightmost clause.
In other words, the sentence must be interpreted to mean that the sub-
ject both performed the Caesarian operation and gave birth to the
child. The sentence is starred because its meaning is nonsensical.

From this piece of evidence and from the earlier data, we can
deduce that the presence or absence of a specified volitional subject
in a Bengali CPC is significant. In other words, what counts in de-
termining clausal compatibility is not whether all component clauses
have coreferential subjects, but whether any one component clause de-
notes volitional action and can be interpreted as having a specified
subject.

6. Conclusion

In light of these facts it is proposed that the putative SSC on
the formation of CPCs be replaced by a different constraint as given in
(26):

    (26) The No Double-Agent Condition (NDAC)
              Any specified, volitional subject in a CPC
              is construed as the subject of each component
              clause.

The NDAC accounts for the same range of CPCs whose acceptability

is accounted for by the SSC. At the same time, the NDAC is more adequate in that it also accounts for numerous CPCs whose acceptability is ruled out under the SSC. All the acceptable counterinstances to the SSC that have been cited in this paper are accounted for under the NDAC. So far I am not aware of any firm exceptions to it.

This does not mean that the NDAC accounts completely for clausal compatibility in Bengali CPCs; i.e., how speakers determine whether two or more given clauses can be united within one sentenial matrix. The fact that sentence (11) is unacceptable could not be accounted for under the SSC, as we indicated earlier; but it cannot be accounted for under the NDAC either. We have not discovered all the conditions on the formation of conjunctive participle constructions. Rather, the NDAC is a partial step forward in our progress toward that goal and an improvement over our previous understanding.

Footnotes

*I am indebted to the Center for Asian Studies at Arizona State University for providing its assistance and to Betty Parker of the Center for producing the typed version of this paper. Errors are my sole responsibility. The paper is based on a chapter of my dissertation (Klaiman 1980a). Abbreviations: CP=conjunctive participle, CPC=conjunctive participle construction, itr.=intransitive, G=genitive case, L=locative case, NDAC=No Double-Agent Condition, O=objective case, SSC=same subject condition, tr.=transitive.

[1]Bengali is a modern Indo-Aryan language spoken in the Indian state of West Bengal (population: nearly 45 million) and in neighboring Bangladesh (population: nearly 80 million) (figures taken from Sen 1978:435). Except where otherwise indicated, all examples in the present paper are taken from the standard colloquial dialect of the language, which is the form of Bengali spoken in Calcutta and its environs.

[2]According to Masica 1976:108, CPC formation is often seen as a process of sentence conjunction. But in Bengali there is formal evidence that the process is really one of subordination. (This is the reason the individual clauses are bracketed as they are in (2).) The formal evidence in question arises from the fact that subordinate and nonsubordinate clauses in the language differ in their behavior in several ways. For instance, sentential negative particles are placed preverbally in the former and postverbally in the latter. Also, some verbs (e.g., existential aach-/thaak- 'be, exist' and the negative verb na-/naa ha- 'not become') have separate allomorphs, one of which occurs in nonsubordinate clauses, the other in subordinate. In these and some other respects, Bengali CP clauses behave like subordinate clauses, while true conjoined clauses behave otherwise.

[3]In the grammatical descriptions of many languages, the term 'dative subject' is applied to the oblique case NP which is, typically, said to 'experience' (as opposed to 'do') the actions denoted by some class of verbs. The term 'dative' is used because the 'dative subject' is typically marked with the 'dative' case, i.e., the case of the indirect object. But in modern Bengali, the case markings of the indirect object and the 'dative' subject differ; the former is

the Objective case and the latter the Genitive case. Hence the term 'indirect subject'.
[4]For evidence of similar patterning in Georgian, Russian and Japanese, see Klaiman 1981.

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On the Correspondence between Linguistic Tone and Musical Melody
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1. Introduction.
      In this paper I offer a reinterpretation of a study by
Richards 1972. Richards looked at a song in Hausa, a tone language,
and attempted to determine the degree to which the musical melody
resembles the linguistic tones of the lyrics. His result was un-
ambiguous but unimpressive: the correspondence was statistically
significant but admitted many deviations. This contrasts with
what we know from other studies of the correspondence between tunes
and tones, where in general the result is that the correspondence
is either extremely close or practically nonexistent. My reinter-
pretation of Richards' data will suggest that for this particular
Hausa song, a comparison should be made not between the musical
melody and the phonemic tone levels but rather between the musical
melody and a normalized _intonational_ realization of the lyrics. I
will try to conclude something from this about the nature of
phonetic representations.

2. A Study of Wakar Indefendan, "The Song of Independence".
      The song Richards examined was performed and recorded in
1964, at the height of its popularity in northern Nigeria. Rich-
ards compared the pitch displacements in the song with those that
would be predicted in the spoken language on the basis of the pho-
nemic tone differences in Hausa. Hausa has two underlying tones,
H and L, which can combine on some heavy syllables to give a
sequence HL, or falling. Falling tones have rather low frequency
(in the present song, there are only 22 falls, as compared to 488
highs and 337 lows), and so to simplify his study Richards counted
falling tones as low. He looked at the first 52 lines, about one
quarter of the song in question, and the first two lines appear
in (1):

(1)

S. bàb- bár ká - sàr Shê- hù dàŋ Hô - dí - yò

Nà - jé- ri - yà tá tsàa- rè gàs- ki - yá

      The way that Richards tabulates the tune-tone correspon-
dence is the following. To normalize the shift from one musical
note to the next with the concurrent move from the phonemic tone
level of one syllable to the next, Richards translates musical note
sequences and phonemic tone sequences into a different notation,
as follows: for a transition between a relatively H pitch and a
relatively L one, Richards writes F; for a transition between a

relatively L pitch and a relatively H one, Richards writes R, for rising; for level movements from syllable to syllable, Richards writes L. Note that in Richards' system L stands for level, not for low tone. A sample transcription using these new symbols is given in (2). This is a transcription of the first line in (1). Where there is a sequence of notes on a single syllable of lyrics, I count only the first note. I assume that Richards does the same, but he does not mention this explicitly. One other simplification Richards makes is to count phonemic falling tones as if they were low tones; this simplification is arbitrary, but it does not throw off the count severely, since falling tones make up only 22 of the 847 tones Richards counts in the complete text.

(2) Sample transcription:

> Tune: R L L L L L L F F
> Tone: R L L L F R L L F

Richards now counts the number of tune-to-tone correspondences in the actual song and compares these to the number of correspondences that would occur by chance, that is, if the poet did not aim for a fit between the linguistic tones and the musical ones. Richards' results are statistically significant but woefully unimpressive. The attested cases of perfect matches between tune and tone are a mere 53.4% of the total. Although the percentage of matches to be expected by chance is lower, it is only somewhat lower, in the range of 35% to 40%, depending on how one calculates. The difference is large enough to be statistically significant but nonetheless not really very large at all. Only slightly more than half of the musical notes actually coincide with the phonemic ones. This is a puzzling finding, for at least two reasons. First, we wonder why a composer would seek to make the melody conform to the phonemic tone pattern to such a limited extent. Poets are entitled to nod, but not 46.6% of the time. Second, in other cases in the literature, it seems that composers do not behave like this at all. Either they pay rather close attention to the tones or they ignore them altogether. In the next section I summarize the literature I have encountered on this topic in African Languages.

3. Survey of other sources.
Greenberg 1949 reports on one Hausa song, a popular song two lines of which he transcribes. He notes that there is no correspondence between the musical and linguistic tones. Within each foot (of two or three syllables), the syllables are all on the same musical note, regardless of the linguistic tones of the lyrics. However, Greenberg refers to Maloney, who has found correspondences between linguistic tones and music in a number of West African languages, including Hausa. Unfortunately, I have not yet been able to consult this source.

Jones 1959 and Schneider 1943-44, 1961 concur that there is a dramatic correspondence between tune and text in Ewe, a Kwa language of Togo and Ghana. Jones, in fact, notes an agreement of slightly more than 90% between the linguistic tones and the musical pitches of the one song for which he calculates the correspondences. This is all the more impressive because Ewe has three phonemic tones, High, Mid, and Low. Jones and Schneider disagree, somewhat bitterly, over the reasons behind the small minority of cases in which tune and tone don't match. Schneider presents a number of hypotheses, the most interesting of which has to do with tone being overridden by meter: H may be L in song on a metrically important syllable, and L may be H in song on a metrical point of secondary value.

Finally, Simmons 1980 takes the close correspondence between tune and tone for granted in Efik, a Benue-Congo language of Nigeria, presumably because the correspondence is so exact, though Simmons offers no actual tabulations. And Simmons raises the further question of whether it is the tones of the lyrics that set the pattern for the melody or the tones of the melody that narrow down the choice of words for the lyrics to tonally matching ones.

Thus, with the exception of Richards, all of the sources quoted find either a clear tune-tone correspondence or a clear lack of it. In the remainder of this paper I will arbue that the Hausa song which Richards analyzed does in fact exhibit a remarkable correspondence between musical melody and linguistic pitch, but that this correspondence is obscured by paying attention to the phonemic tone levels H and L. Rather, these tone levels must be adjested by rules which are in fact related to the rules of Hausa intonation. The basic point can be seen by observing that most of the violations of the tune-tone correspondence in the following text, taken from four more lines of Wakar Indefendan, are of one of two kinds. First, the third syllable of the proper noun Nigeria is marked as L in the phonemic transcription, but it is on the same note as the preceding high-toned syllable in the song. This is simply a consistent transcription error on Richards' part: phonemically, the third syllable of Nigeria is actually high, not low. The second violation involves the fact that in practically every sequence HLH, L in the song is realized on the same level as preceding H, rather than being relatively low. The deviant L tones are encircled.

(3)

Thus, what appears to be the case is that the poet patterned the melody not after the phonemic tone levels _per se_, but after these levels modified by a rule that makes a L equivalent in fundamental frequency to a preceding H. Interestingly, as I learned from a study reported in Leben 1983, the L in HLH is quite variable in where it is plotted with respect to the preceding and following tones; in actual Hausa intonation curves it is not unheard of for L to be not only as high as the following H but even as high as the preceding H. This being the case, it is possible that the poet is modeling the song on the basis of a version of Hausa intonation that is indeed stylized, but that is clearly adopted from one of the options taken in normal speech. However, before we can consider this topic any further, we must first take a closer look at Hausa intonation in general.

4. Intonation in Hausa.

The standard view of Hausa intonation is that, to a first approximation, a low tone lowers the register for a following high tone. As a result, the H on the right in HLH is, on the average, lower in fundamental frequency than the rightmost H in HLH. This is true, but in addition, many investigators have either assumed or claimed that the reason behind this downdrifting pattern is that in a sequence of tones, a low tone is plotted, say, two steps below a preceding H, while a H is plotted only one step above a preceding L. This point of view emerges from work focused on Hausa in particular, such as Hodge and Hause 1944, Meyers 1976, Miller and Tench 1980, 1981, as well as from general works concerned with the formalization of the downdrift phenomenon, such as Schachter and Fromkin 1968, Clements 1979, and Hyman 1979. However, there are signs that the lowering effect of a L on a following H is independent of where that L is plotted on the actual intonational curve. The results are reported in greater detail in Leben 1983, but the basic phenomenon is the following.

In fluent speech, L tones are extremely variable in their realization. Quite often they are no lower than the following H, as Meyers 1976 has observed. And in quite a few cases L is actually higher than the following H. Thus, the following sort of picture, traced from an oscillographic print-out using the pitch extraction system of the Phonology Laboratory at the University of California, Berkeley, is not atypical:

(4)

yáa cí ⟨gàbá⟩ ⟨dà⟩ céèwáa...

'he went on to say...'

Note that the encircled syllables are approximately as high as the preceding high-toned syllables, even though they themselves are phonemically low (as can be judged by eliciting them in isolation, or in very slow speech).

As I propose in Leben 1983, I believe that this phenomenon shows that the mapping of low tones onto intonation contours ought to be regarded as separate from the effect that low tones have in many languages of lowering the register for the following H. That is, first we map high tones onto a register, then subsequently, we add the values for L, as depicted in (5):

(5) a. H plotting
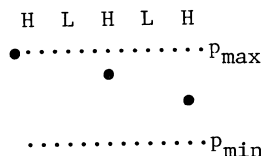
b. L plotting (arrow shows possible range of variation)
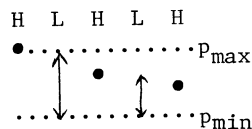
In the light of this, one interpretation of the song is that the composer is aware of the fact that L in HLH is more variable in its realization than other tones. What he is consistently doing in the present song, then, is assigning L to one of the

extremes that it occupies in real discourse. On this view, the
mapping of the encircled L's is not exceptional at all, and the
tune-tone correspondence for the song is near 100%.

5. Discussion.

This seems to tell us something about this piece of Hausa
music, namely that its tune reflects Hausa intontation and not
just Hausa tone. But a reasonable question to ask is whether
this has any wider significance. I think that it possibly does.
Bloomfield held the view that between physical phonetics and the
level he called phonemics there was no linguistically significant
level. He reasoned that only phonemics provided a non-arbitrary
criterion for factoring certain properties out of the speech sig-
nal and leaving others in. The criterion was based on whether the
property in question was distinctive or not, in the sense of dis-
tinctive that Bloomfield defined. Hence, if you tried to repre-
sent a level somewhere between the phonemic level and the level
of physical phonetics, there was no apparent non-arbitrary criter-
ion that you could use in order to sort out the properties that
you left in the phonetic transcription from the properties that
you chose to omit from the transcription.

Nowadays, we have more or less abandoned Bloomfield's
assumptions on this matter, and we speak of a so-called level of
"systematic phonetic" representation. Unfortunately, the proper-
ties of this level are not defined in any language-independent
way, and so really Bloomfield's point still ought to hold. I
think that the musical structure of this Hausa song may help to
provide a non-arbitrary basis for the notion "systematic phonetic
representation", for at the very least this song shows that the
composer was not relying solely on phonemic tones. In particular
the composer was making use of certain characteristics of Hausa
intonation contours. But the song melody is obviously not identi-
cal with the actual physical fundamental frequency patterns of
spoken utterances, nor can the melody be directly derived from
these fundamental frequency patterns. Thus, the composer seems to
be drawing representations from a level that is more superficial
than the phonemic level yet more abstract than the physical sig-
nal. I suggest that the study of songs like these may help to show
which characteristics of an intonation contour are salient to Hausa
speakers, so that they can draw on these even though they are not
contrastive in any way.

We can gather from work on metrics by Kiparsky and others
that poets are able to draw on rather abstract properties of phono-
logical representations in putting together their poems. I think
it might be possible to interpret the present study in somewhat
the same vein, only here we have an art form that taps linguistic
representations which are abstract, but which are nonetheless
closer to the surface than the level of phonemic or underlying
representation.

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A comparative study of topic and focus
constructions with special reference to Tzotzil
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0.0 Introduction

In this paper, I will discuss the manifestations of the
pragmatic categories "topic" and "focus" in a number of languages.
A functional rather than structural definition of these categories
makes it possible to recognize as topic and focus marking
syntactic phenomena not traditionally associated with these
categories; for instance, it will be shown that the ergative case
marks topics and the zero article marks focuses. This conclusion
cannot be reached deductively using a purely structural analysis
but is unavoidable given the functional parallels between
constructions with these markings and constructions with markings
traditionally associated with topics and focuses (Kuno 1973, Givón
1979).

1.0 Argument and sentence types

Studies of pragmatic concepts such as "givenness" and
"definiteness" (cf. Chafe 1976, Givón 1978) have dealt primarily
with the marking accorded individual arguments such as pronouns
and articles. On the other hand, work such as Prince (1980, 1982)
gives a detailed account of various functionally distinct types of
non-canonical word order in English; such constructions are
marked, so that the functional concepts discussed are only
designed to be applicable to a small subset of the sentences of
English. The approach taken here is somewhat different; it is
designed to explain the marking accorded sentences rather than
individual arguments and to accomodate all sentence types rather
than only marked constructions. This is a formidable task and the
present work is only a first attempt; the ultimate goal is a model
of sentence-level grammar making reference only to pragmatic
categories. This is of course an idealization, similar to the
idealization of an autonomous syntactic component made by
exponents of generative-transformational grammar (cf. Chomsky
1975) which has led to a greatly increased understanding of this
component; a model of language integrating the syntactic and
pragmatic components will not be possible until the properties of
each in isolation are understood. There is no universally
accepted functional metalanguage and the authors of the grammars I
have consulted in preparing this study (with the exception of Kuno
(1973) and Kiss (1981)) are unwilling or unable to indicate the
marking associated with this or that pragmatic category in the
languages they describe, so that it will often be necessary here
to communicate marking of pragmatic categories through reference
to syntactic categories.

Argument/constituent types may be divided into three categories according to their pragmatic status. The argument types described here may be nominal, verbal, adjectival, or adverbial, while the constituent types are verb+argument:

1) Arguments or constituents which are entirely presuppositional or given, that is, carried over from the immediately preceding discourse; pronouns most commonly fill this role, which will be referred to as "unmarked topic".

2) Arguments or constituents which interact with and effect some change in the presuppositions involved in the discourse beyond the immediate clause. Typically, these change the temporal or spatial context, add a character to the discourse register, change the state of some character, or single out an element of a presupposed set to the exclusion of the other members of this set.

3) "Accessory" arguments, which have no effect upon the discourse beyond the immediate clause.

The above definitions must be understood to represent ideal types, as the distinction between these categories is not always clear in a given language.

Sentences may be divided into four basic types:

A) Those containing two type 2 arguments or constituents; one of these is a "marked topic" ("marked" as opposed to "unmarked", not as opposed to "not having marking"), which sets a temporal, spatial, or personal context for the remainder of the sentence (Chafe 1976); the other brings about some change in the information status of the characters, context, or presuppositions involved in the surrounding discourse and leaves the information status of the marked topic relatively unaffected. If the marked topic has a syntactic role, it will usually be a subject. In a given language, the marked topic may not necessarily receive marking associated specifically with topics; a type A sentence may for example have two type 2 arguments in the predicate, neither of which are marked as topic. However, a type 2 argument may not be marked as a marked topic unless there is another type 2 argument in the clause.

B) The subject and the predicate form a single type 2 constituent; the predicate indicates a change in the information status of the subject rather than any other character, context, or set in the discourse register.

C) The subject is type 1 and the predicate is type 2 or 3.

D) The subject is type 2 and the predicate is type 1 or 3.

Section 2 below discusses sentence types A, B, and C; focus constructions such as sentence type D will be discussed in section 3.

2.0 Different types of subject-topic marking
2.1 Type A vs. types B and C.

Languages may mark the distinction between sentence types A, B, and C through word order or case-marking. Many languages distinguish A from B and C by means of word order by putting

marked but not unmarked topics in initial position; these topics
will not necessarily be subjects although they will tend to be,
since subjects are generally the most topical nominal arguments
(Givón 1979). Languages at the VS end of the "VS to SV continuum"
(Givón 1977) are of this type.

Type A sentences require two type 2 arguments; such sentences
are more likely in some situations than in others. A sentence
beginning a subsection of the discourse is likely to change the
set of characters or the temporal or spatial context presupposed
in the discourse; data taken from a text is divided by the author
or editor into subsections (impartially if not entirely
objectively) by means of paragraphing. In the Mayan language
Tzotzil, which is solidly VOS, subjects and objects may be
topicalized by being put in initial position and being marked with
the enclitic -e; topicalization of subject and object is
considerably more common in the first sentence of a paragraph than
in other sentences, as is shown in Table 1 (1):

Table 1	Topics	Not topics	Topic %
Paragraph-initial	70	305	18.7
Other	160	2320	6.4

$$\text{chi-square}=65.62, p .01$$

Prince (1980) shows that left-dislocation in English (which
is a marked topicalization in the terminology used here) is
similarly especially likely in paragraph-initial sentences.

Givón (1977) has pointed out a parallel situation in Early
Biblical Hebrew. The narrative tense and VS order were used for
the "continuity" function when the same subject was maintained and
the events were in narrative sequence; SV order and the perfect
tense were used when there was a personal or temporal
reorientation; the new subject would be a preverbal marked topic
and the time reference would move backwards.

Givón (1979) has also shown that in a number of languages the
syntactic role of direct object and its unmarked position are
associated with introducing and reintroducing characters; thus
direct objects are likely to be type 2 arguments, so that
transitive sentences are likely to be type A while intransitive
sentences are likely to be type B. In Tzotzil, the presence of an
overt direct object makes subject topicalization much more common,
as can be seen in Table 2:

Table 2	Topics	Not topics	Topic %
Overt object	59	201	22.7
No overt object	148	1240	10.7

$$\text{chi-square}=28.85, p .01$$

Prince (1982) reports a similar situation in English; here
characters are normally introduced or reintroduced in sentence-
final position so that this position tends to be occupied by type
2 arguments. Type 2 arguments with OTHER positions in canonical

word order may then be left-dislocated; however, there is no
purpose in left-dislocating a type 2 argument from final position
and in fact no left-dislocations occur from this position.
       Prince (1982:8) also finds that topicalization in English has
the function of marking "an open proposition as Chafe-given in the
discourse" (Chafe 1976); for example, see 1 (Terkel 1974:191):

       1) Q: Do all the long-haired guys bug you?
          A: I don't want my son to have it. Now, the sideburns I
             wear because I do TV commercials and stuff. I'm in the
             modeling field.

Filling the open sentence "I wear the sideburns for X/some reason"
interacts with the presuppositions involved in the surroundings
discourse so that "the sideburns" becomes a marked topic.
       Ergative case-marking also distinguishes type A sentences
from types B and C; in an ergative case-marking system, transitive
objects and intransitive subjects receive the same marking (the
absolutive case) while transitive subjects receive different
marking (the ergative case) (Dixon 1979); it was argued above that
transitive subjects in general serve as marked topics, so that the
ergative case marks marked topics. Hopper and Thompson (1980)
give numerous examples of languages where ergative case-marking is
restricted to certain types of transitive clauses, most commonly
those with perfective verbs, those in which the object is
affected, and those in which the object is referential. All of
these are likely to be type A, i.e., have a marked topic as
subject, because the predicate has some effect upon elements of the
discourse other than the subject; perfective verbs change the
temporal context, verbs affecting their objects change the
information   status of the object, and referential objects add a
new character to the discourse register. On the other hand,
imperfective verbs, verbs not affecting their objects, and
predicates with non-referential objects primarily affect the
information status of the subject so that clauses with these
features are likely to be type B and their subjects marked with
the absolutive.
       In many ergative languages, the alternation between ergative
and nominative transitive subjects is governed by a universal
implicational pattern known as the "animacy hierarchy"
(Silverstein 1976). 1st and 2nd person pronouns are highest on
this hierarchy, followed by 3rd person pronouns, animate noun
phrases, and finally inanimate noun phrases. The higher positions
of the hierarchy are associated with nominative marking of the
transitive subject while the lower positions are associated with
ergative marking of the transitive subject; the most common "split
ergative" patterns restrict ergative marking to nominal transitive
subjects or nominal and 3rd person pronominal transitive subjects.
A typical example of this is found in the Australian language
Ngawun (Breen 1976):

2) panya-ŋka ṇantu-lpuṇu tyalaṛu
   woman-ERG hold -pres   baby ="The woman is holding the
                                                        baby."
3) ŋayu ṭiṇi ṭaya-lpuṇu
   I(NOM)tree cut -pres="I'm cutting firewood."

This pattern is completely predictable if, as suggested here, the
ergative case marks marked topics; since pronouns (especially the
more "topical" 1st and 2nd person pronouns, cf. DeLancey (1981),
Kuno and Kaburaki (1977)) are likely to be type 1 rather than type
2 arguments, they will normally be unmarked rather than marked
topics, and so do not receive ergative marking; thus "split
ergative" marking is a grammaticalized topicalization of the type
described by Givón (1976, 1979).

2.2 Types A and C vs. B

Many languages mark the subject-topic of types A and C the
same, while the subject of type B is marked differently; those
distinguishing these types via word order are midway on the VS to
SV continuum. Typical examples are 17th century Spanish (cf.
Myhill 1982) and the Mayan language Chorti; in these languages, as
in more strictly VS languages such as Tzotzil, marked topics are
preverbal while subjects of type B sentences are not; thus
transitive subjects still have a higher likelihood of being
preverbal than intransitive subjects, as can be seen in Table 3 (2):

Table 3

Spanish	SV	VS	SV%
Transitive subjects	648	352	65%
Intransitive subjects	142	180	46%
		chi-square=43.40, p .01	
Chorti			
Transitive subjects	14	1	93%
Intransitive subjects	55	90	38%
		chi-square=17.01, p .01	

In these languages, in contrast to strictly verb-initial
languages, unmarked topic-subjects such as non-contrastive
pronouns and noun phrases with determiners are normally preverbal
(Myhill 1982); as a result, text counts show about an even split
between SV and VS order in these languages.
    The same distinction between type A and C sentences on the
one hand and type B sentences on the other is marked in Japanese
by the alternation between the "topic" marker -wa and the
"subject" marker -ga (Kuno 1973); unmarked topic -wa has been
referred to as "anaphoric" while marked topic -wa has been
referred to as "contrastive". Sentence 4 is an example of the use
of anaphoric -wa:

4) John- wa   hon- o yonda
   John-TOPIC book-ACC read="Speaking of John, he read the
                                                 book."

In a sentence with a contrastive -wa, both the topic and the
remainder of the sentence (the "comment") contrast with some other
(mentioned or unmentioned) topic and comment. Thus the comment
affects the presuppositions involved in the surrounding discourse
and so the sentence is type A; this is illustrated in sentence 5,
where the subject "many people" is not anaphoric:

5) Oozei-no hito - wa   party-ni kimasita ga   omosiroi
      many   people-TOPIC party-to   came   but interesting
   hito - wa   hitori   mo imasen deshita
   people-TOPIC one person even   was not
   "Many people came to the party, but there were none who
   were interesting."

When the subject is not anaphoric and there is no contrast (type
B), the subject is marked by -ga, as in sentence 6:

6) Sora-ga akai
   sky   red="Look! The sky is red!"

2.3 Types A, B, and C vs. D

     The final stage in the VS to SV continuum may be exemplified
by modern English and French; here, subjects of sentence types A,
B, and C are all preverbal and the only remaining VS patterns are
some type D sentences such as existentials and presentatives
(Givón 1977). A parallel situation involving case-marking may be
found in Filipino languages where every sentence has some argument
with a "subject-topic" case-marker except existentials (Schachter
1976) or in Finnish where subjects are in the partitive in certain
existential constructions and in the nominative otherwise (Itkonen
1979).

3.0 Focus marking

     In this section, I will discuss some general characteristics
of focus marking. Focus marking may be at the level of the
sentence or the verb phrase and chooses a single argument which
represents one or another of a set of values; this set is
pragmatically or semantically implied in the discourse but need
not be explicitly stated (Prince 1981). The remainder of the
sentence or verb phrase is held constant or presupposed. The
position associated with sentential focusing may be anywhere in
the sentence; for instance, French focuses are clefted and
referential focuses in Persian may be in sentence-initial
position, separated from the verb (Haeri pc). On the other hand,
verb phrase focus marking is limited to reordering and marking

within the verb phrase. Nevertheless, it is possible that
sentential focus and verb phrase focus will be associated with the
same position in a given language; this is the case for instance
in Tzotzil, Hungarian (Kiss 1981) and Basque (Lafitte 1944).

The choice of a given item to be focused may be placed on a
continuum according to the extent to which it violates
presuppositions about which item would be chosen. At one end of
the continuum are choices where no presuppositions are violated;
the choice is "neutral". On the other end of the continuum are
choices which are "contrastive", that is, where the choice of item
contradicts presuppositions about which item would be chosen. For
the sake of exposition, the following discussion will be limited
to a characterization of the endpoints of this continuum, that is,
"neutral" and "contrastive" choices; this of course is an
oversimplification of the situation.

Focus marking may be either marked or unmarked, depending on
the construction type and the language. For an argument to
receive unmarked focus marking, it may be a marked or an unmarked
choice; for an argument to receive marked focus marking, it must
be contrastive.

Certain argument types are inherently more likely than others
to be focused. The answer to a question is focused; this choice
is usually neutral and hence receives unmarked focus marking but
not marked focus marking. In English, focus intonation is
unmarked but focus movement and clefting are marked (Harries-
Delisle 1978): thus answers to questions receive sentential stress
but are not focused unless contrast is implied (Givón 1979). In
Tzotzil, focusing places arguments in preverbal position and is a
marked construction (only 160 out of 3,323 (4.8%) subjects and
objects in the data base base were focused); answers to questions
are not normally focused, as can be seen in 7 (Laughlin 1980:5):

```
7) Q:  Bu   ch - a  -bat  Lol?
        where inc-2abs-go  Larry="Where are you going, Larry?"
   A:  Ch - i  -bat ta   Jobel
        inc-1abs-go  to San Cristóbal="I'm going to San
                                            Cristóbal."
```

Focused arguments are typically contrastive, as for example in
sentence 8 (Laughlin 1980:84):

```
8)  L - i  - y  -ak' - b -otikótik  j  -ve7el-tikótik
    cmp-1abs-3erg-give-dat- excl.  1erg-meal - excl.
        7un,      pero muk'  x - i  -ve7-otikótik
    phrase-final  but  not  sta-1abs-eat-  excl.
        7un,      y - u7n  naka chij  t - z  -ti7-ik
    phrase-final 3erg-because just mutton inc-3erg-eat-pl.
    te yo7e
     there
    "He gave us a meal, but we didn't eat, because just mutton
    they eat there."
```

On the other hand, in Hungarian (Kiefer 1966) and Basque (Lafitte 1944), marking of focus by word order is completely unmarked and so answers to questions must be focused by being put directly before the verb. So, in Basque, to answer the question "Who threw the vase?", it is necessary to say (Lafitte 1944:47):

9) Untzi- a - 0   aita - k   aurdiki du
     vase-the-ABS father-ERG throw aux="<u>Father</u> threw the
                                                            vase."

Sentence 9 may also be translated as a marked focus construction: "It was father who threw the vase."

3.1 Focusing and the zero article

In many languages, the zero article on a direct object marks unmarked focus in that an object with the zero article is a member of a set; the set consists of those objects which are to some extent predictable from the semantic nature of the verb in that they are characteristically associated with the verb; the verb and object are semantically cognate. Each verb has a set of characteristic objects associated with it; when a non-specific object is selected from this set, it is given the zero article; when an object is taken from outside this set, it receives an article whether it is specific or not. See for example sentences 10-12 from Hungarian (Károly 1972:97):

10)*Ceruzá- t néz
     pencil-ACC see(3s)=*"He sees pencil."
11) Egy ceruzá- t néz   .
      a   pencil-ACC see(3s)="He sees a pencil."
12) Néz   egy ceruzá- t
     see(3s)   a   pencil-ACC="He sees a pencil."

Note that the direct object with an article may be focused or not. On the other hand, semantically predictable non-specific objects take the zero article; such objects are focused by a grammatical rule whether they are contrastive or not (Kiss 1981) (3), since they form part of a set and focusing is unmarked (A. Pap pc):

13) Inge -t   próbál
     shirt-ACC try on(3s)="He's trying on a (non-specific)
                                                        shirt."
14)*Próbál   inge - t
     try on(3s) shirt-ACC="He's trying on a (non-specific)
                                                        shirt."

In fact, in Hungarian, ALL articleless verbal complements, which have the characteristic of being semantically cognate to the verb, are focused (Kiss 1981):

16)*János ment   mozi -ba (4)
     John went(3s) movie-to="John went to the (non-specific)
                                                    movies."
17) János mozi -ba ment
     John movie-to went(3s)="John went to the (non-specific)
                                                    movies."

Many languages allow direct objects to "incorporate " into
the verbal complex, becoming essentially part of the verb and so
being subject to word-internal phonological rules; the verb may be
formally marked as intransitive. Typically, incorporated direct
objects are non-referential, non-specific, and semantically
cognate with the verb; the verb-object construction is essentially
a single lexical item. This is the situation for example in the
Oceanic languages Fijian (Arms 1974), Mokilese (Harrison 1976),
and Woleian (Sohn 1975), the Austro-Asiatic language Temiar
(Benjamin 1976) and the American Indian languages Paiute and
Nahuatl (Sapir 1911). Thus "object incorporation" (Mardirussian
1976) is a type of verb phrase focusing.

Unlike other Mayan languages such as Yucatec (Bricker 1977),
Tzotzil does not have object incorporation; however, the same
principle applies since objects with no article are both
semantically cognate to the verb and non-specific. These objects
may be predictable or contrastive; if they are predictable, they
occur in postverbal position while if they are contrastive, they
are focused. Thus the percentage of focus movement in Tzotzil
of articleless objects is fairly high. On the other hand, objects
which have articles may have them either because they are specific
or because they are non-specific but not semantically cognate to
the verb; all the objects which are not semantically cognate to
the verb will not be part of a set and thus not eligible to be
focused (5), so that the percentage of focused objects with
articles is considerably lower (see Table 4):

Table 4
Objects	Focused	Not focused	Focus %
With article	28	767	3.5
No article	38	387	8.9

chi-square=15.89

Just as the definite or indefinite article may mark verbal
complements which are unpredictable in that they are not
semantically cognate to the verb, they may also mark complements
which are unpredictable in that they are contrastive. A typical
example of this is shown in sentences 18-19 from Hungarian (A. Pap
pc):

18) Egy borbély volt
     a   barber was(3s)="He was a <u>barber</u>." (as opposed to
                                               anything else)

19) Borbély volt
    barber was(3s)="He was a barber." (neutral statement of
                                                    fact)

Stockwell, Bowen, and Martin (1965) report a similar situation in
Spanish.
    In Persian, where -ra marks definite and specific (and
normally non-cognate) direct objects, it may also distinguish
contrastive objects from predictable objects (Haeri pc):

20) Man ingilisi-ra khoob yad gereftam
    I English   well   learned ="I learned English
    well." (as opposed to some other language)
21) Man ingilisi khoob yad gereftam
    I English well   learned ="I learned English well."
                                    (neutral statement of fact)

3.2 Focus and aspect

    There is a clear connection between focus and aspect;
perfective verbs characteristically move the time reference
forward (Hopper and Thompson 1980); on the other hand, in
sentences with focused nominals, the verb must be presupposed
information and hence the time reference CANNOT move forward;
put another way, both focused nominals and perfective verbs
normally represent the main assertion of the clause they occur in
(Hopper 1979). Thus focused nominals and perfective aspect are
incompatible. Focusing in Tzotzil is considerably more likely
when the verb is imperfective than when it is perfective (see
Table 5):

Table 5
Subjects and objects	Focused	Not focused	Focus %
Perfective	43	837	4.9
Imperfective	43	477	8.3

                              chi-square=6.49, p .01

    In Hungarian, perfectivity is marked by verbal prefixes which
are stressed and occupy focus position; the presence of a focused
nominal forces the prefix out of this position; this means that
the verb has been displaced as the main assertion of the clause
(Kiss 1981):

22) A csapat el -vesztette a döntő- t
     the team away-lost(3s) the final-ACC="The team lost
                                             the final."
23) A csapat a döntő- t vesztette el
     the team the final-ACC lost(3s) away="It was the final
                                             that the team lost."

    Similarly, in Russian, the imperfective form of the verb is
used if some other arguments represents the main assertion of the

sentence even if the action described is perfective in meaning (Forsyth 1970):

24) Kto pisal   Vaynu i   Mir?
    who wrote(imp.) war and peace="Who wrote War and
                                                 Peace?"
25) Tolstoy pisal   Vaynu i   Mir
    Tolstoy wrote(imp.) war and peace="Tolstoy wrote War
                                                 and Peace."

4.0 Conclusion

The pragmatic categories "topic" and "focus" are still poorly understood and their manifestation in individual languages is obscured by interaction with marking of syntactic categories; comparative study shows, however, that in a given language this interaction consistently produces one of a number of patterns of word order or case-marking; for example, in every language with variable order of subject and verb which I have investigated, type A subjects are most likely to be preverbal, followed by type C subjects, and finally type B subjects. This word order typology was devised through study of languages in which the verb almost invariably precedes the object; the situation in OV languages is obviously quite different and remains to be investigated. Similarly, the "active" case-marking system of languages such as Guaraní (Gregores and Suárez 1967), where (in the traditional view, cf. DeLancey 1981) intransitive subjects are marked as transitive subjects if they are agentive and objects if they are not, may be entirely semantic in nature; if this is the case, the typology suggested here is not applicable to such systems (but see Munro and Gordon 1982, where it is argued that semantic and pragmatic factors in such systems are not necessarily independent); this question obviously must be left open here pending further research.

The connection between marked and unmarked topicalization has been common knowledge for some time (cf. Givón 1977); however, while marked focusing has received some attention (Harries-Delisle 1978, Prince 1978), unmarked focusing such as exists in Basque and Hungarian has been described as an idiosyncratic property of certain languages and to my knowledge no attempt has been made to relate the facts regarding unmarked focusing in various languages to each other or to what is known about marked focusing. It appears that a number of interesting generalizations may be made about the characteristics of marked and unmarked focus constructions so that a focus system such as that of Hungarian might be a grammaticalized version of a focus system such as that of Tzotzil. It would then be necessary to investigate the details of and motivation for this grammaticalization.

The typology of sentence types outlined in section 1 accounts for the data considered here. It must of course be recognized as preliminary in nature, since each sentence type has subtypes and

may be represented in a number of different ways; hopefully, further research will reveal correlations between various subtypes and representations. This work is also preliminary in that it cannot yet have any pretensions of explanatory adequacy. To say that the ergative case marks marked topics simply unifies two descriptive categories; this is a step in the right direction but does not in itself tell us exactly what these descriptive categories represent. It seems likely that they are manifestations of a category which is universal to human language, and which is described (doubtless imprecisely and inaccurately) in section 1 above; although work such as Li and Thompson (1976) and Van Valin and Foley (1980) has made it clear that there are no universal syntactic categories, there is no evidence to suggest that there are no universal pragmatic and semantic categories and in fact such categories are assumed to exist in various theoretical frameworks such as Van Valin and Foley's "Role and Reference Grammar" and Dik's (1981) "Functional Grammar" (6). The ultimate object of research here must be these categories rather than the details of their manifestation in one language or another (although the nature of the former may only be deduced through observation of the latter); thus it is the typology itself which is of primary theoretical interest, as it reveals the nature of the categories basic to the organization of language.

Footnotes

1. This and all data from Tzotzil is taken from Laughlin (1977, 1980).
2. This data is taken from, respectively, Cervantes (1605) and Fought (1972).
3. It is possible for an articleless verbal complement to occur outside of focus position, as this position may only be occupied by one argument or constituent; cf. fn. 4 and Kiss (1981) for details.
4. This sentence is only grammatical if "János" is given focus intonation, so that it means "It is John who went to the movies."; "moziba" is then displaced from focus position (cf. sentences 22-23).
5. That is, they will not be eligible to receive verb phrase focus; they may of course still receive sentence focus. Articleless objects, on the other hand, will always be eligible for both kinds of focus.
6. Linguists working within the framework of generative-transformational grammar (cf. Chomsky 1981) have incorporated thematic (semantic) roles into their model of language and it is probably only a matter of time until they do the same with pragmatic roles; cf. Kiss 1981 for such a discussion of Hungarian.

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On Direct and Oblique Cases

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One purpose of this paper is to utilize recent advances in the understanding of grammatical relations in order to test a standard assumption of structuralist and traditional analysis of morphological cases. * Another is to use the results of that testing –– the pattern of successes and failures of the restated structuralist hypotheses –– to draw more general conclusions about the nature of the linguistic sign and the organization of grammar. Another is to advance a new typological principle for classifying case systems and case languages.

The concept to be tested is the division of morphological cases into direct and oblique, or grammatical and local, or the like. Such a distinction is made by most approaches which bring syntactic function into an account of case meanings. It underlies the Indo-European tradition and hence the generalizing works of Kurylowicz (1949) and Jakobson (1936, 1958). This distinction evolved out of the classical division of cases into nominative (direct) vs. oblique (all non-nominative cases), combined with the classical distinction of case functions into those due to rection –– roughly, government [1] –– vs. others. The theoretical evolution seems to have involved identification, or at least approximation, of the two notions of direct and governed (casus rectus). The label 'direct' or 'grammatical' is, in the best-known structuralist approaches, applied to the nominative and accusative (Jakobson: 'direct') and sometimes also to the genitive (Kurylowicz: 'grammatical'). To summarize and simplify the structuralist literature: the terminology varies, the definitions vary, and the cases admitted as direct or grammatical or whatever vary. But that literature gives the impression of debating these issues as one. I therefore assume that underlying the variety of contributions to the question is a uniform assumption: that cases can somehow be divided into two sets, one marking syntactic relations in the abstract and the other bearing more semantics.

This paper tests, not some particular definition, but that general assumption. It tests it empirically, surveying case uses in several languages in an attempt to discern some division which is evidently equivalent to the structuralist distinction of direct from oblique cases. In other words, it seeks only a functional bifurcation within case paradigms, without stipulating which cases are to have which function. This is necessary because the traditional and structuralist literature does not give adequate criteria for unambiguous classification of cases, as is particularly clear when we venture outside of Indo-European. In addition, common sense dictates that we test the shared assumption of a functional dichotomy, which was clearly the essential theoretical notion, rather than the details of its various positive

characterizations. The conclusion will be that a direct-oblique dichotomy does exist, but it does not apply to morphological cases.

The above characterization of the structuralist position does not apply to Hjelmslev (1935, 1937), who correctly saw the question of whether there exist direct and oblique cases to be an empirical, language-specific one, found that 'dans certaines langues il existe des cas qui par leur signification se prêtent mieux que certains autres à exprimer les relations locales et concrètes' (1935:61), Indo-European being such a language, and declared the assumption that all languages would exhibit such a bifurcation 'idiosynchronic' and aprioristic.

Nor does this characterization do justice to Kurylowicz, whose analysis of case functions was surprisingly modern. He gives the following definitions ((1949)1960:135ff., restated in the terms used here):

> primary function of a case = assignment by the syntax, or residual lexical assignment
> secondary function of a case = assignment by the lexicon (non-residual)
>
> syntactic function = governed function
> semantic function = non-governed function
>
> grammatical case = one whose primary function is syntactic
> concrete case = one whose primary function is semantic

For instance, the Indo-European accusative is a grammatical case because its function as direct object is governed (Kurylowicz: syntactic) and residual (it is the neutral or unmarked object case; Kurylowicz: primary function). Kurylowicz also ranked case functions by relative centrality, a notion corresponding to subcategorization as that is defined below; he lumps this notion with the distinction of grammatical vs. concrete (p. 140). The approach taken in this paper differs from Kurylowicz's in using less lumping (it distinguishes residual lexical case marking from syntactic case marking, and subcategorization from the other notions). More important, while Kurylowicz evidently assumed that there is something that can be labeled by the expressions <u>grammatical</u> <u>case</u> and <u>concrete</u> <u>case</u>, this is exactly what the present paper questions.

I will distinguish between <u>government</u> and <u>subcategorization</u> in the following fashion. Government refers to determination, by verb or adposition, of the morphological form of the complement. For instance, the Russian verb <u>ljubit'</u> 'love' governs the accusative of its object; the verb <u>interesovat'sja</u> 'be interested in' governs the instrumental; <u>udivljat'sja</u> 'be surprised' governs the dative; <u>serdit'sja</u> 'get angry' governs the preposition <u>na</u> 'on(to), at' plus accusative; and so on. The difference in cases here does not reflect a difference in surface syntactic relations, since all the objects are of the type that can be called first objects; it is not due to any difference in semantic roles, since for verbs of emotion the objects are uniformly sources of emotion. The case or preposition is simply required by the verb, and this is stated in the lexicon. 2)

Subcategorization (I omit <u>strict,</u> but the sense intended is that of <u>strict subcategorization</u> in Chomsky 1965) is the requirement that the complement be present, without reference to its morphological form. All the verbs just mentioned subcategorize their first objects, as well as governing the cases of those objects. Clearly, whatever is governed is also subcategorized. The converse is not true, although subcategorized but ungoverned relations are rather few. Three examples are the goal of a verb of motion (illustrated in (1)), the locative phrase with a verb of existence, location, or stance (in (2)), and the predicate nominal in certain fixed construction types (in (3)). These are subcategorized but not governed: the verb requires the presence of the goal, locative, or predicate nominal, but does not dictate its form. The case or adposition is determined by semantics, and the speaker has a choice.

(1) He went { home
               into the library
               across the plaza

(2) I lived { in Berkeley
               on Walnut
               near Peet's

(3) Russian   Našli ego { p'janogo   (accusative)
                             p'janym   (instrumental)

              found him   drunk
              '(We) found him drunk'

(Government and subcategorization are also known as morphological government and syntactic government respectively.)

This paper tests two hypotheses. The first is that all languages will exhibit a fairly clear bifurcation among their cases. There will be one set of cases (here to be called 'direct') which will be used for governed or subcategorized actants. The remainder (the 'oblique' cases) will be used for non-governed or non-subcategorized actants. (This hypothesis does not claim that the set of direct cases will be cross-linguistically uniform. Nor does it claim that the basis for division will always be government, or will always be subcategorization. It claims only that there will be a discernable bifurcation among cases, along these lines.)

The second hypothesis is based on the observation that some languages grammaticalize the notion of 'core arguments', clearly distinguishing semantic roles from the level serving as input to lexical subcategorization; while others do not, apparently relying on semantic roles alone and giving few or no syntactic arguments for subjecthood and objecthood. (The observation is due to Van Valin 1981; see also Kibrik 1982. The terms in which it has been expressed here are due to Anthony C. Woodbury, personal communication.) The hypothesis is that this typological distinction will have some consequences for case functions: that languages with clearly grammaticalized core arguments will most clearly distinguish direct from oblique cases.

To test these hypotheses, I surveyed case functions in a number of languages. This paper reports on five of them: modern Russian, Old Russian, Chechen-Ingush (an ergative language of the North Central Caucasus), Nanai (Manchu-Tungus family, Altaic stock; an accusative language of far eastern Siberia), and Finnish. 3)

Case functions. Table 1 summarizes case functions for the five languages. Information about government and subcategorization is given there in compressed form: the 'Governed' and 'Non-subcategorized' columns should be self-explanatory; the 'Weakly governed' column uses the term traditional in Russian grammar for functions which are subcategorized for but not governed. These are functions like those shown in (1)-(3) above. The 'Overlap' column reports the presence of both governed and non-governed, or both subcategorized and non-subcategorized, functions of the given case. An x in any two of the first three columns means an x in the 'Overlap' column. The 'Adposition' column shows whether the case occurs as the object of a preposition or postposition. In languages having more than one adpositional case, the cases appear to be governed by the adpositions. This is most clearly true of Russian and Old Russian. For languages having a single adpositional case (Chechen-Ingush, Nanai), there is no need to have each adposition govern that case in the lexicon: it can be assigned automatically by a syntactic rule. The picture is more complex for languages like Finnish, where one case — the genitive — is the unmarked adpositional one and could conceivably be assigned by syntactic rule, while others are evidently governed. This discussion indicates that some notion of the governed or non-governed status of adnominal cases can be gained by checking the number of entries in that column: if a language has more than one, government is probably involved somewhere.

The 'Adnominal' column indicates whether the given case marks the dependent noun in a noun phrase, as does the Indo-European genitive:

(4) Latin   amor   patris
              love   father-GEN
              'a father's love'; 'love for one's father'

(5) Russian   dom   brata
              house   brother-GEN
              'brother's house'

The sole adnominal case of a language like Chechen-Ingush or Nanai appears to be non-governed, assigned by syntactic rule. When a language has more than one adnominal case, government appears to be involved at least part of the time, although the question requires more study. For Finnish, the genitive is the unmarked adnominal case and possibly assigned syntactically; others may be governed. (See also note 7 and the relevant discussion below.) The ambiguity in (4), where the genitive can be interpreted as either subjective or objective, shows that the genitive is

Table 1. Case functions in five languages. 3) (See Appendix for details.)

Language	Case	Governed	Weakly governed	Non-subcategorized	Overlap (clause level)	Adposition	Adnominal
Russian	Nominative	x	x*	x*	x	x	
	Genitive	x	x	x	x	x	x
	Dative	x	x*	x*	x	x	(x)
	Accusative	x	x*	x	x	x	x
	Instrumental	x	x	x	x	x	
	Prepositional 1	—	—	—	x		
	Prepositional 2	—	—	—	x		
	Genitive 2	x				x	
Old Russian	Nominative	x	x*	x*	x	x	
	Genitive	x	x	x	x	x	x
	Dative	x	x	x	x	x	x
	Accusative	x	x	x	x	x	
	Instrumental			x	x	x	
	Locative			x			
Chechen-Ingush	Nominative	(x)				?	
	Genitive	(x)					x
	Dative	x				x	
	Ergative						
	Instrumental			x			
	Locative			(x)	(x)		
	Comparative		x	x	(x)		
	Allative		?		?		

(Table 1, cont.)

Language	Case	Governed	Weakly governed	Non-subcategorized	Overlap (clause level)	Adposition	Adnominal
Nanai	Nominative	x				x(h)	x(h)
	Accusative	x					
	Instrumental						
	Dative	x		x			
	Locative		x	x	x		
	Ablative		?	x	x		
	Lative		x	x	?		
Finnish	Nominative	x	(x)	(x)	(x)		
	Genitive	x	?			x	x
	Accusative	x		x	x		
	Partitive	x	x	x	x	x	x
	Essive	x	x	x	x		
	Translative	x	x	x	x		
	Inessive		x	x	x		
	Elative	x	x		x		
	Illative	x	x	x	x		
	Adessive	x	x	x	x		x
	Ablative	x	x	x	x		
	Allative	x	x	x	x		
	Abessive			x	x		
	Comitative			x			
	Instructive		?	x	?		

Key: x = occurs; (x) = marginal; x* = agreement only; x(h) = head-marked

assigned to the adnominal configuration, not to particular roles
of the adnominal dependent; this is a strong argument that the
case is syntactically assigned, hence not governed. This discus-
sion shows that, for adnominal cases as for adpositional cases,
the number of x's in the column gives some idea of whether
government is present.

Presentation of complete data on the functions of each case
in all languages surveyed is impossible in a paper of this size.
The following examples give some idea of the multiple functions
of some of the cases. (They also illustrate some of the dilemmas
that are created by trying to make such a classification.) The
Appendix gives an overview of each language, without examples.

(6)-(9) illustrate the Russian nominative, in its functions
as subject (shown in (6)), 'weakly governed' (subcategorized but
not governed) predicate adjective (in (7)), non-subcategorized
predicate adjective (8), and object of preposition (9). (Here
and below I ignore the non-syntactic functions of the nominative:
citation form and direct address.) Nominatives are underlined.

(6) On sidit v biblioteke
     he sits   in library   'He's sitting in the library'

(7) On vernulsja p'janyj
     he returned   drunk   'He came back drunk'

(8) Golodnyj on ploxo rabotaet
     hungry   he badly works   'He works badly hungry'

(9) Čto on za čelovek?
     what he for person   'What kind of person is he?'

The nominative on predicate nouns and adjectives actually repre-
sents agreement rather than direct assignment of a particular
case (these constructions are analyzed in Nichols 1981); and the
preposition in (9), although historically a representative of the
preposition za 'for', 'behind', can synchronically be analyzed as
simply a component of the complex pronominal modifier čto za
'what kind of'. These functions, then, though relevant to a com-
plete account of the uses of the nominative, are suspect, or at
least represent a different phenomenon from the use of the nomina-
tive as subject. The nominative   subject is to be regarded as
governed because it contrasts with other cases, the choice being
dictated by the verb. One of the contrasting cases is the dative,
governed by the verbs taking the inverse construction:

(10) Mne   nravitsja   èta pesnja
      me-DAT like-3sg   this song-NOM   'I like this song'

The functions of the Russian genitive are illustrated in (11)-
(16): subject (11), direct object (12), oblique first object (13),
time expressions (14, 15), adnominal (16), object of preposition
(17). Genitives are underlined.

(11) Moego brata   ne bylo doma
     my   brother not was home   'My brother wasn't at home'

(12) Ja <u>ètogo</u> ne znal
      I   this   not knew   'I didn't know that'

(13) Želaju vam <u>sčast'ja</u>
      I wish you happiness   'I wish you happiness'

(14) On rodilsja <u>pervogo</u> sentjabrja
      he was born first   of September
                          'He was born September 1'

(15) <u>Pervogo</u> sentjabrja posle dolgoj zasuxi nakonec pošel dožd'
      first   September after long drought finally went rain

      'On September 1, after a long drought it finally rained'

(16) dom   <u>brata</u>
      house of brother   'brother's house'

(17) bez <u>deneg</u> 'without money'   iz <u>doma</u> 'out of the house'

(11) and (12) illustrate the genitive subjects and direct objects
that can be triggered by negation in Slavic languages. These pre-
sent problems for a theory of case assignment, since the genitive
is not governed by the verb but evidently assigned by a syntactic
rule restricted to the governed-and-subcategorized relations of
subject and direct object. They are often thought of as replacing
the nominative or accusative assigned by the lexicon. They are
clearly different from the genitive object of (13), which is gov-
erned by the verb and does not alternate with the accusative.
(14) is a 'weakly governed' genitive: the verb <u>rodit'sja</u> 'be born'
forms a stable construction with temporal expressions, which it
therefore subcategorizes; but the morphological form of the tempo-
ral expression is determined by its own semantics (dates are geni-
tive, days of the week take <u>v</u> 'in' plus accusative, years take
<u>v</u> plus prepositional, and so on), not by the verb. (15) shows the
genitive in a true time adverbial, not subcategorized by the verb.
     (16) repeats (5), showing the typical adnominal construction
in Russian. The genitive is clearly the unmarked adnominal case,
although with deverbal nouns other cases or prepositions may be
used. 4)   (17) shows examples of the genitive governed
by prepositions.
     The instrumental is governed as object by a number of verbs.
(20) gives examples of a few of the verbs in one semantic class
governing the instrumental: verbs of interest and similar posi-
tive attitudes. (21) and (22) show two of the many adverbial
functions of this case: the instrumental of instrument (21) and
the instrumental on a time-adverbial predicate nominal (22). (23)
shows some of the prepositions governing the instrumental.
Instrumentals are underlined in these examples.

(20) On zanimaetsja/interesujetsja/vosxiščaetsja <u>muzykoj</u>
      he is occupied is interested is delighted music

      'He is occupied with (interested in, delighted with) music'

(21) On piŝet <u>karandaŝom</u>
     he writes pencil   'He writes with a pencil'

(22) On ŝil zdes' <u>rebenkom</u>
     he lived here child   'He lived here as a child'

(23) s <u>bratom</u> 'with brother'   za <u>derevom</u> 'behind the tree'

     Classification of the instrumental agent in a passive construction is problematical. Clearly the instrumental case is not
governed. Less clear is whether the syntactic relation is subcategorized or not. The underlying relation of agent is clearly
subcategorized by the verb; but one hesitates to say that the
derived relations produced by passivization are subcategorized.
Nevertheless, somewhat unhappily, I do adopt that analysis, on
the grounds that since passivization can be described as verbgoverned in Russian its resultant grammatical relations can be
described as subcategorized. The truth of the matter is that the
classificatory principles used here were not designed to
accommodate derived grammatical relations.

     The Russian (and, generally, Slavic) instrumental is famed
for its multifunctionality: for fuller descriptions see e.g.
Wierzbicka 1980, Mrazek 1964, Worth 1958.

     The next few examples illustrate Chechen-Ingush cases. (24)
and (25) show the nominative functioning as intransitive subject
and as direct object: 5)

(24c) san waŝa   c'a vo:ʃu
      my brother-NOM home comes   'My brother is coming home'

(25i) na:nas   biera: kuoĉ   t'aju:x
      mother-ERG child-DAT shirt-NOM puts on

      'Mother dresses child in shirt'

The only other function of the nominative is predicate nominal
with the verb 'be' and a few others:

(26i) yz   bwolxluo   va
      he-NOM   worker-NOM   is   'He is a worker'

Such nominatives are clearly subcategorized, and probably
governed: since the appearance of the nominative is not due to
agreement (there being no case agreement in this language), they
could be provided for in the lexicon. Non-governed and nonsubcategorized predicate nominals, analogous to Russian (7), (8),
and (22), apparently do not occur in Chechen-Ingush, which uses
biclausal constructions with the verb 'be':

(27i) yz   mogaŝ   voacaŝ   balxa   vaxar
      he-NOM   well-NOM   not being   to work   went

      'He went to work sick'

(28i) bier   dolaŝ   yz   jurta   vä:xar
      child-NOM being   he-NOM   in village   lived
      'As a child he lived in a village'

Thus the nominative is arguably always governed.

(25) above shows a three-place verb, and illustrates the dative second object of that verb. (29) shows another three-place verb with the same case valence, and (30) a three-place verb with both dative and allative possible for the second object.

(29i) Sy vašas   šien luolaxuočunna urs   tiexar
      my brother-ERG refl neighbor-DAT knife-NOM struck

'My brother stabbed his neighbor'

(30c) Cuo   { cünga } a:xča   delira
             { cunna }

      he-ERG   him-ALL money-NOM gave
                    DAT

'He gave him money'

(31) shows the dative subject of an inverse verb.

(31i) Suona yz   gu
      I-DAT him-NOM see   'I see him'

All postpositions take the dative (if we disregard a few frozen and dialect constructions apparently using the nominative). Since this pattern is evidently triggered by syntactic configurations rather than the postpositions themselves, it must be the result of a syntactic rule rather than government.

These functions exhaust the uses of the nominative, ergative, and dative cases of Chechen-Ingush. These three cases are then exclusively subcategorized and, in their clause-level functions, arguably always governed. The same can be said for the allative, which is apparently restricted to the indirect objects represented by (30) and the oblique first object of a very small set of verbs:

(32c) San vaša   süöga häžira
      my brother-NOM me-ALL looked

'My brother looked at me'

The locative is used both for non-governed, subcategorized place complements (33) and first objects of a very small set of verbs (e.g. (34)):

(33c) Govr   xix   je:lira
      horse-NOM water-LOC went (out, over, etc.)

'The horse crossed the river'

(34c) Iza   qüöru šien lu:laxuočux
      he-NOM fears refl neighbor-LOC

'He is afraid of his neighbor'

Such examples show that Chechen-Ingush is strikingly different from Indo-European, both in the simplicity of statements that can be made about the functions of individual cases and in the simplicity of statements about the case valence of configurations

such as three-place predicates, adnominal constructions, postpositional constructions, etc.

A final example comes from Nanai, where the nominative is used in adnominal constructions and as object of postpositions. (35) shows the adnominal construction.

(35) nai   dili-ni
      person-NOM   head 3sg   'person's head; human head'

In its adnominal and adpositional functions, the nominative is properly equivalent to a caseless noun: those syntactic relations are marked, not by a case on the dependent noun, but by possessive suffixes on the head noun or preposition. In (35), the third person singular marker -ni is what marks the syntactic relation. (This is what is meant by the entry head-marked on Table 1.) Although it is clear that, of the seven inflectional forms available to nouns, these represent the nominative, and they must be taken into consideration when assessing the functions of the nominative, still there is a sense in which they do not count: the real marker of the syntactic relation is on the head of the construction.

All the preceding examples show the kind of data considered in this study, and the basis for determining government and subcategorization. They also show that refined concepts of government, subcategorization, and grammatical relations do not obviously simplify the task of classifying cases and case functions. On the contrary, they introduce complexity in a variety of ways. It is indispensable, for example, to discuss clause-level and phrase-level functions separately. It becomes evident that the genitive objects of (12) and (13) are phenomena of different orders. So, perhaps, are the type of non-government and non-subcategorization found in adverbials, on the one hand, and that found in adnominals, on the other. Derived syntactic relations do not fit the classificatory scheme. The question arises of whether the unmarked case for a given syntactic function is best described as governed (since it is associated, albeit perhaps only residually, with particular lexemes) or as assigned by regular syntactic rules (since the association with particular lexemes is unmarked and residual). Another dilemma is whether to regard agreement as an instance of the particular case assigned, or as in itself a morphological entity, consisting of a disjunction of cases.

This brings us to the broader question of delimiting the object of study. For single-language descriptions, it may be expedient to start with syntactic relations and ask how they are signaled by cases. From this perspective, it is artificial to segment cases off from adpositions, conjunctions, adverbs, and the like. For instance, in modern Russian, the inventory of formal markings of nominal syntactic relations includes the six non-prepositional cases, preposition + case complexes, agreement, other regular disjunctions of cases, the caseless adjective form, conjunction + agreement complexes, adverbs, and possibly more.

These are all problems imposed by the classificatory system, which is evidently ill-suited to language. It will be of little

surprise that both hypotheses based on it fail. However, the particular patterns of failure give us two new typological principles, to be presented below.

Hypothesis 1. Even if we simplify the picture by restricting discussion to the clause relations, the first hypothesis — that languages distinguish direct from oblique cases as governed and non-governed, or subcategorized and non-subcategorized — is falsified. Case functions differ widely, but they turn out to fall into two basic types, which I call overlapping and complementary. An overlapping case has both governed and non-governed, or both sub-categorized and non-subcategorized, functions. The Russian nominative, instrumental, and genitive shown above illustrate overlap: all have both kinds of functions. Table 1 shows at a glance that in general the Russian cases are prone to overlap: five of the six cases having clause functions show overlap. Old Russian tends even more strongly toward overlap, for a variety of historical reasons (greater productivity of genitive adverbials; more uses of independent cases, where modern Russian uses prepositions). In general, then, Russian disproves the first hypothesis. Other languages with a high incidence of overlap include Finnish, shown on Table 1, and Latin, not shown.

In contrast, Chechen-Ingush and Nanai display a great deal of complementarity. Case functions are neatly split, with overlap being limited, often marginal.

We might, then, wish to say that Chechen-Ingush and Nanai distinguish direct from oblique cases. The direct cases of Chechen-Ingush are nominative, dative, ergative, and allative; the oblique cases are instrumental and comparative; genitive and locative require further study. For Nanai, the direct cases are nominative, accusative, and dative; the oblique cases are instrumental and ablative; the lative is direct if subcategorization is criterial, oblique if government is; the locative shows clear overlap if subcategorization is criterial, but is oblique if government is criterial.

These lists of cases depart from the typical structuralist understanding of direct and oblique in two respects. First, cases with local-sounding names such as allative, locative, and lative are disconcertingly strong candidates for direct status. Second, the number of direct cases is much higher than the two or three usually admitted in the literature: Chechen-Ingush has up to six candidates for direct status, and Nanai has up to five. We have found evidence for a direct-oblique distinction at the price of considerable distortion of the substance and intent of the standard distinction.

On the other hand, the facts suggest a new typological principle: The world's case languages can be divided into those, like Russian, with considerable overlap, and those, like Chechen-Ingush, with little or no overlap.

A high incidence of overlap in case functions means that the language abounds in oblique objects — objects whose case, though

governed by the verb, is metaphorically motivated by other functions of the same case. (For instance, Russian verbs of negative emotion — 'fear', 'be ashamed', etc. — govern the genitive, a case which etymologically goes back to the Indo-European ablative, used for motion-away-from, the two functions being linked by a metaphor of aversion.) Complementarity means that there are few or no such oblique objects. (For instance, Chechen-Ingush has only a handful of verbs taking oblique objects, and a very limited set of cases so used; the motivation of the oblique objects involves no metaphor, but only syntactic analogy and reinterpretation of adverbials as objects.) 6) Put differently, overlap means that many different cases can mark one and the same syntactic relation; complementarity means fewer markers per syntactic relation. The same distribution holds when we turn our attention from clause actants to adnominal dependents and objects of adpositions. In Russian, which abounds in overlap and oblique objects, almost every case can be governed by adpositions and several can be adnominal. But Nanai and Chechen-Ingush have one adnominal case and one adpositional case each. 7)

The abundant overlap and frequent metaphor found in Indo-European and Finnish case systems appears to be unusual in northern Eurasia, where most case systems seem to be more like those of Chechen-Ingush and Nanai. Overlap could be just a genetic peculiarity of Indo-European, spread as an areal feature to Baltic Finnic. (In general the case functions of Finnish and Estonian are strikingly reminiscent of those of Baltic and Slavic languages.) However, there is a more compelling historical explanation. The great majority of the north Eurasian case languages are agglutinating; Indo-European is inflecting. If inflecting languages evolve out of agglutinating languages by phonological change, then we can say with some confidence that the case system of Indo-European is older than the case systems of Uralic, Altaic, or North Caucasian. Then the overlap, metaphor, and general intricacy of the case rules in Indo-European is the natural consequence of the antiquity of the case systems: there has been more time to develop metaphor, to grammaticalize idioms, etc. (In Baltic Finnic it may be due to areal interaction.)

Hypothesis 2. The second hypothesis was that languages which grammaticalize the status of core argument (subject, object) will tend to have complementary case systems, while those that do not will prefer overlap. This hypothesis also fails, but most interestingly. Russian clearly grammaticalizes the notions of subject and object: numerous syntactic rules refer to these relations, and several rules change and/or create them. Yet Russian displays consistent overlap. Nanai just barely grammaticalizes these relations: only a few rules refer to them, none create or change them, and subject and object can be directly derived from semantic roles alone. Yet Nanai cases are highly complementary. Chechen-Ingush gives almost no evidence for syntactic relations as distinct from semantic roles, and the entire language can be adequately described

without using the terms 'subject' and 'object'. Yet Chechen-Ingush cases are, if anything, even more strongly complementary. So the second hypothesis is not merely falsified, but actually reversed: the stronger the grammaticalization of subject and object, the more widespread is overlap in case functions. It is intriguing that the languages with rampant overlap -- Indo-European languages, including prominently Slavic -- are the ones that have given rise to the theoretical distinction of direct vs. oblique.

Conclusions. The two hypotheses fail because both are based on a simplistic view of cases as autonomous morphological entities somehow correlated with autonomous syntactic relations. I suggest that we look at morphology and syntax not as autonomous levels of grammar but in another way: as function and coding technique. Syntax is function, case is coding technique. Viewed in this way, the grammars of languages do give evidence for a distinction of direct vs. oblique. But that distinction ceases to pertain to morphological paradigms, having rather to do with the coding of syntactic relations. On this view, coded functions comprise such notions as direct subject, oblique subject; direct object, oblique object (both are first objects); direct second object, oblique second object (both are traditional indirect objects); and so on. These coded functions, and the particular cases used on them, are shown for the languages studied here in Table 2. Other coded functions may be detectable. Russian, for instance, has direct instruments (in the instrumental case) contrasting with oblique instruments (e.g. iz 'out of' + genitive, for the verb 'shoot'). It has a direct adnominal case -- the genitive -- contrasting with several oblique adnominal forms (see again (15), (19)).

This method of classification captures in a single notion both contrasts in morphological form and differences in syntactic behavior: the first four to six entries on Table 2 constitute the beginning of an accessibility and/or control hierarchy for most of these languages (while neither morphology alone nor syntax alone ordinarily suffices to establish accessibility or control). It is important to emphasize that this approach does not make claims about cases per se. It does not, for example, ask whether the Russian dative or the Chechen-Ingush dative is a direct case (since it marks the direct second object) or an oblique case (since it marks the oblique subject). Rather, it asks what cases mark oblique subjects in Chechen-Ingush, and so on.

This approach has several advantages over received views. Classification is motivated and unambiguous, in contrast to the standard notions of direct vs. oblique, whose membership often had to be decided by fiat. The classification in Table 2 is based on universal notions, whose universality is demonstrated by the fact that they apply with equal ease to overlapping and complementary case systems. Yet it easily handles language-specific patterns. (In contrast, the standard notion of direct vs. oblique cases represents an attempt to impose a universal dichotomy on morphological paradigms, which are known to be highly idiosyncratic.)

Table 2. Functions and codings.

coded function	Russian	Chechen-Ingush*	Finnish	Nanai
direct subject	nom ~ gen	erg/nom	nom ~ part	nom
oblique subject	dat PP	dat gen	all	?
direct 1st object	acc ~ gen	nom	acc ~ part	acc ~ nom
oblique 1st object	various and PP	all loc	various	?
direct 2nd object	dat	dat ~ all	all	dat
oblique 2nd object	PP	PP adverb	?	?
direct adnominal	gen	gen	gen	nom (head-marked)
oblique adnominal	dat instr	——	part, etc.	——

KEY: case ~ case   Principled grammatical alternation

case/case   Illusory alternation due to syntactic analysis *

PP   prepositional phrase, postpositional phrase

cases: nom(inative), gen(itive), erg(ative), dat(ive), etc.

* Disjunctions for Chechen-Ingush are the outcome of forcing an ergative language into an accusative terminological mold. They disappear on an ergative analysis of coded functions:

 direct S/O   nominative

 oblique S/O   dative, allative, locative

 direct A   ergative

 oblique A   dative, genitive

Most important, it links the notion of grammatical relation with the notion of morphological coding, fusing the two into a single type of entity. Thus, although <u>direct</u> coincides with <u>residual</u>, the two notions are not at all the same: <u>direct</u> pertains to coding, <u>residual</u> to the mechanics of grammar and lexicon.

This approach has the important advantage of letting us describe an ergative language as ergative, regardless of whether the syntax of control, word order, etc. turns out to be accusative. It therefore unifies the description of ergative languages and gives ergativity a deeper, more central status in the grammar than that of superficial morphological marking. Such a description is shown at the bottom of Table 2, for Chechen-Ingush.

Talking about direct and oblique relations means more than just working a bit of syntax into morphology. The difference between direct and oblique is neither syntactic nor morphological. It is a difference in the nature and delicacy of coding — that is, a difference pertaining not to levels of grammar but to the linguistic sign function. The direct markings simply signal the presence of the given syntactic relation: subject, first object, or whatever. The oblique ones not only signal the syntactic relation; they also index semantic properties of the dependent noun and/or the verb. For instance, the dative inverse subject marks not only subjecthood but also the fact that the subject is an experiencer and/or that the verb is one of perception, modality, cognition, or the like. The instrumental first object of Russian tells us not only that we are dealing with an object, but furthermore that it is the manipulated object of a verb of motion or control. Oblique marking, in other words, contributes additional semantic information to the basic syntactic information.

It now becomes evident that the traditional and structuralist strategy was to use the label 'direct' for the cases used to mark direct subjects and direct first objects, and sometimes also direct adnominals. The 'oblique' cases were those used for all oblique relations, all adverbials, and direct second objects (the traditional indirect object). Why should the markers of direct second objects have been excluded from the set of direct cases, when their function is so clearly to mark a syntactic relation? I submit that it is because the indirect object always carries more semantic information than does either subject or direct object: it is inherent in indirect objects that they will be goals or recipients, hence also often experiencers. In contrast, the direct subject is virtually unlimited as to semantic role, and the direct object is not obviously restricted to roles (with the apparent exception of Chechen-Ingush, where direct objects are always patients).

The structuralist analysis, then, attempted to distinguish the more semantic cases from those simply marking syntactic relations. The more semantic information a case carried, the more likely it was to be labeled 'oblique'. From this paper's perspective, the structuralist position lumped relative semanticity with direct vs. oblique relations. It also conflated two distinct kinds of semanticity. There are two ways in which a case may carry

semantic information: directly, by indexing the semantic role of its noun and/or the lexical semantics of its verb; and indirectly, when it marks a syntactic relation which is inherently restricted as to semantic role or verb class. These two were lumped in the implicit structuralist understanding of semanticity.

In other words, the structuralist position seems to have been based on an understanding like that of direct vs. oblique function. It erred simply in lumping these with two types of semanticity, in assuming autonomy of levels, and in attempting to apply the labels direct and oblique as invariant descriptions of cases. I have argued that these assumptions make the structuralist position untenable. It is interesting that the structuralists could sense, and partly codify, a notion whose full explication was precluded by the structuralist commitment to invariance and autonomy of levels. 8)

In contrast to that position, the approach taken here sees autonomous levels of syntax and morphology as indispensable conceptual tools for proper description of languages, but not as necessarily real elements of grammar. Once we see the autonomy of levels to be a descriptive convenience and conceptual distinction rather than a necessary property of language, the structuralist assumption of invariance in the relation of case to syntax simply evaporates. It becomes uninteresting to ask what the dative always or typically 'means', whether it is an oblique case, whether a dative oblique subject is a subject or an indirect object, and whether direct cases are assigned lexically or syntactically. Rather, we simply ascertain what kinds of oblique objects exist in a given language, and then ask whether they can passivize, control reflexivization, and the like. In the end, this approach encourages attention to language-specific differences. It does this by rejecting the assumption that there is a universal inventory of syntactic or semantic relations which cases simply exist to mark, along with the assumption that cases have autonomous meanings which motivate their use to mark one or another syntactic or semantic relation. To accept that assumption would be to reify the analytic tools of syntactic relations and semantic roles.

Summary. The two hypotheses about direct and oblique cases fail, but they fail revealingly. The failure of the first hypothesis points to a new typological principle for classifying case systems and case functions. The failure of the second hypothesis has several implications worth testing, for example the question of whether variable marking of core relations is not perhaps favored by clear grammaticalization of those relations. The ultimate reason for the failure of the two hypotheses is that the notions of direct and oblique, which the structuralist position intuits and attempts to explicate, properly pertain not to morphological cases but to coded functions. More specifically, there are three reasons for their failure. The first is the structuralist assumptions of invariance and autonomy placed on the original notions. The second is properly a subcase of the first: the condition of autonomy of levels entails that morphological case will be seen as a distinct

kind of entity and investigated in isolation, when properly the question of nominal marking requires study of adpositions, agreement, conjunctions, and the like. (As was hinted above, this assumption of the autonomy of morphological case is due not solely to reification of conceptual tools, but also to the analytic, form-to-function approach that dominates the structuralist literature. It is also worth pointing out that both Hjelmslev and Kurylowicz saw the artificiality of isolating cases from other markers of nominal relations.) The third reason is the inherent heterogeneity of case functions, a problem which is not lessened but only revealed more clearly by recent theoretical advances.

The function-and-coding approach allows us to make cross-linguistic sense of, and impose terminological order on, what must otherwise be seen as very messy language-specific variation in case functions. In this particular corner of grammar it has proven expedient to seek coded functions, i.e. grammatical signs, using autonomous levels of grammar as a descriptive convenience without ascribing reality to them; the implications for the more general practice of grammar should be obvious. The structuralist notion of direct and oblique cases fails to the extent that it reifies the conceptual tools of analysis, and succeeds to the extent that it captures coded functions.

Appendix:

Clause-level case functions in the languages shown on Table 1.[3]

KEY:
subj.	= subject	pred. nom.	= predicate nominal
obj.	object	inv.	inverse
dir.	direct	indir.	indirect

* = case by agreement   C = under certain conditions only

locus = place expression with verb of existence or location
object (contrasts with **dir. obj.** and **indir.obj.**) = an oblique object (most examples are first objects)

Language, Case	Governed functions	Weakly governed	Non-subcategorized
Russian			
Nominative	subj.	pred. nom.*	pred. nom. *
Genitive	subj. (neg.) dir.obj.(neg.) object	time	time
Dative	inv. subj. indir. obj. object	pred. nom. *	pred. nom. *
Accusative	dir. obj.	pred. nom. *	pred. nom. *, time
Instrumental	pred. nom. [9] object	pred. nom. agent in passive	pred. nom. means, time, place, etc.

(Russian, continued)

Prepositional 1, 2: used as objects of prepositions only

| Genitive 2 | object | —— | —— |

Old Russian

Nominative	subj.	pred. nom. *	pred. nom. *
Genitive	subj. (neg.) dir. obj. (neg.) object	source, goal of motion	time, reason
Dative	inv. subj. indir. obj. object	goal of motion	dative absolute
Accusative	dir. obj.	goal of motion	time
Instrumental	object	pred. nom. agent in passive	means, time, place, etc.
Locative	——	——	time

Chechen-Ingush

Nominative	S/O		
Genitive	inv. subj.		
Dative	inv. subj. ind. obj.		
Ergative	A		
Instrumental			instrument, means, comitative
Locative	(object)	locus	(place)
Comparative	(object)		object of comparison
Allative	object indir. obj.	?	

Nanai

Nominative	subj.		
Accusative	dir. obj.		
Instrumental			instrument, means
Dative	indir. obj.		dative absolute
Locative		locus	place
Ablative		source?	source
Lative		goal	

Finnish

Nominative	subj. dir. obj. C	(time)	(time)
Genitive	inv. subj.		
Accusative	dir. obj.	time?	time
Partitive	subj. C dir. obj. C object		partitive absolute
Essive	 pred. nom.[9]	time locus pred. nom.	time place pred. nom.
Translative	pred. nom.[9]	pred. nom.	pred. nom. forms adverbs
Inessive		locus	place, time
Elative	object	time source pred. nom.	
Illative	object	goal	time
Adessive	object inv. subj.	locus	time instrument manner
Ablative	pred. nom. [9]	source	time
Allative	ind. obj.	goal	
Abessive			'without'
Comitative			comitative
Instructive		time	manner

Footnotes

1   Government in my sense, and in the traditional sense, is not quite equivalent to the notion of rection as used primarily in the Indo-European literature. Government is the lexical determination of case by verbs or adpositions; rection also includes the relation of a noun to the adnominal case used with it. Sometimes this can be described as government, for instance where deverbal nouns preserve some of the case government of the source verb; but the term rection is also applied to possessive adnominal genitives, where there is no lexical determination of case.

2   Case government may be described as arbitrary, but it is not random. As will be mentioned below, the same case is often governed by a whole set of semantically related verbs, and the choice of case is often motivated by metaphor. (One mechanism of metaphor is illustrated below. Another is described in Nichols 1983.)

3   Sources used: Russian: Field and text work; my own files and teaching materials. Old Russian: Borkovskij ed. 1978:345ff., Borkovskij & Kuznecov 1965:454ff., text surveys. (Table 1 represents the oldest stage of the language, without distinguishing native East Slavic from its Church Slavonic admixture.) Chechen-Ingush: Field and text work; Maciev 1961:578ff.; Jakovlev 1940: Ch. 2. Nanai: Avrorin 1959, 1968. Finnish: Eliot 1890:131ff., Serebrennikov & Kert 1958:55ff.

4   All these adnominal forms appear to be non-governed, assigned by a syntactic rule sensitive to syntactic or semantic function: the genitive is the unmarked case; preposition k 'to(ward)' plus dative is used for the object of a verbal noun of emotion (shown in (i) below); the instrumental is used for the subject of a verbal noun of action when an object (genitive) is present (in (ii)):

(i)   ljubov' k otcu   ljubov' otca
      love   to father   but   love   father-GEN
      'love for (one's) father'   'a father's love'

(ii) otkrytie   Ameriki   Kolumbom
      discovery America-GEN Columbus-INSTR
      'the discovery of America by Columbus'

5   The letters c and i following example numbers indicate whether the example is in Chechen or Ingush.

6   Chechen-Ingush oblique objects are described in Nichols 1983.

7   The claim about Chechen-Ingush adnominal cases requires some comment. In this language, verbal nouns preserve intact the case government of the source verb, including even the case of the subject. We could then claim that all cases have adnominal functions. But here, in speaking of adnominal cases, I refer only to those which reflect the adnominal construction and not those which preserve the verbal government. The various cases shown in the Russian examples in note 4 are all different from the cases governed by the corresponding verbs, thus I consider them adnominal cases.

8   Gary Holland has pointed out to me that the structuralist adherence to autonomy of levels in the study of case can be traced to Meillet's insistence on the autonomy of the word. Meillet's theoretical pronouncements underlie the entire early twentieth-century Indo-European tradition and consequently determine the structuralist stance.

9   A governed predicate nominal can also be called an object. An example is Russian

    on   kazalsja   molodym
    he   seemed   young-INSTR   'he seemed young'

where the verb kazat'sja 'seem' requires the instrumental.

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Functional Reasons
for the Fall and Rise of V-S Structures in French
A Quantitative Approach
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Scholars have characterized French word order in various ways depending on their breadth of diachronic perspective and on their methodological and theoretical orientation. Old French has been classified as verb-second (Frappier 1972, Einhorn 1974, vonWartburg 1934), verb-center (Kukenheim 1967, Crabb 1969), or even as having a great 'liberty' of word order (Foulet 1930). A few theories have been posited to account for Old French word order and its subsequent evolution, some based on the 'internal rhythmic structure' of the language; some claiming word order to be, at first, a purely stylistic device with increasing syntactic conditioning (Ewert 1969). Lehmann (1972) and M. Harris (1978) have suggested that Old French word order is an example of Vennemann's TVX theory (1974), claiming French to have evolved from SOV to SVO via a period where the verb moved medially (as initial position was filled by the sentence topic) and then became syntactically fixed in second position (as the most common topics, subjects, became grammaticalized in first position).[1] Most recently, Galambos (1982) has convincingly argued against Lehmann, claiming French SVO word order to be the result of the standardization of subjects in first position (being quite common and unstressed as topics by the 13th century), which evolved from a previous VSO stage (with the initial person-marked verb a natural topic in the absence of subject pronouns, and the first type of topic generalized in first position). Although Galambos' theory is appealing, it seems to overlook the fact that V-S structures in French have never disappeared; on the contrary, in certain environments, they even seem to be on the rise.

This paper will examine the role that V-S structures have played in the evolution of French and discuss their function in Modern French. Possible reasons for the decline of V-S structures in certain types of clauses and rise of such structures in new environments will be postulated, and a claim will be made that new V-S patterns can be explained at least in part by:

1) the change in pragmatic function of certain elements through influence by other elements with a more retricted grammatical distribution, and

2) the rise of complex sentence structures, which has resulted in use of V-S word order as a pragmatic device to lessen processing difficulties.

Data consists of 3500 declarative clauses in 8 French prose texts[2] from the early XIIIc to the XXc. The framework used in this study is a functional one, originating with Sapir's notion of language drift (1921). Data was analyzed using the variable rule program as developed by Cedergren and Sankoff (1974) and Sankoff and Labov (1979).

A look at the data shows a surprising distribution of V-S structures in French - they do not consistently decrease in application and they show an increase in application in the most modern text.

1. % of V-S Structures:

|	1210	1230	1300	1389	1448	1529	1690	1981
Texts	1	2	3	4	5	6	7	8
n=	440	861	727	265	246	323	236	284
% V-S	19	16	15	30	18	7	3	7

Assuming language to be a coherent, rule-governed system which evolves without loss of communication from one period to the next, we would hope to find that the disparate percentage figures reflect some evolutionary trends in certain grammatical structures and that the seeming inconsistency of V-S application over the periods is due to a unique interweaving of grammatical and functional patterns. Closer examination of the data does, in fact, show the emergence of certain patterns. First, there is a decline in effect of initial adverbial modifiers on V-S word order over the centuries. This is also the case with initial objects.

2. Adverbs in Initial Position:

Texts	1	2	3	4	5	6	7	8
n=	87	132	56	82	56	46	25	54
% V-S	93	86	84	60	46	32	4	3

On the other hand, there is a surprising rise in the effect of 'et' on V-S structures in the XIVc and XVc and a rise in V-S structures out of relative and subordinate clauses after the XVc while V-S structures in main clauses show a definite decrease in application after the XIVc:

3. 'et' alone or Adverb in Initial Position:

% of V-S

— = 'et'

-- = adverb

Texts
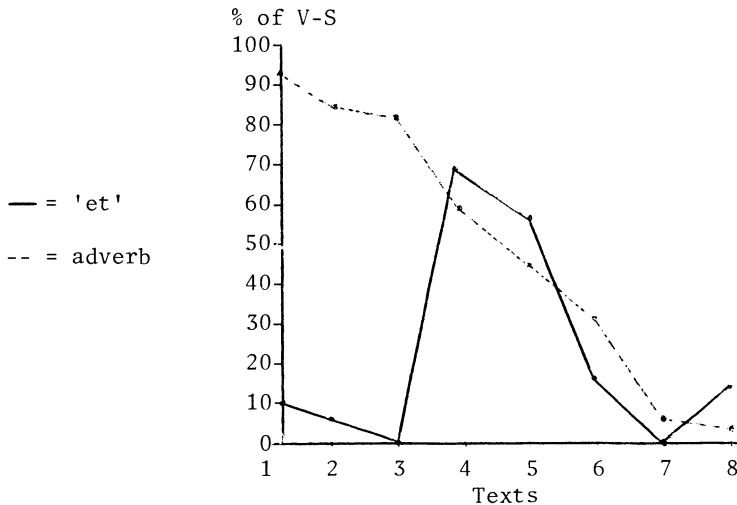
4. Main or Subordinate Clause:

% of V-S

——— = Sub. Clause

-- = Main Clause
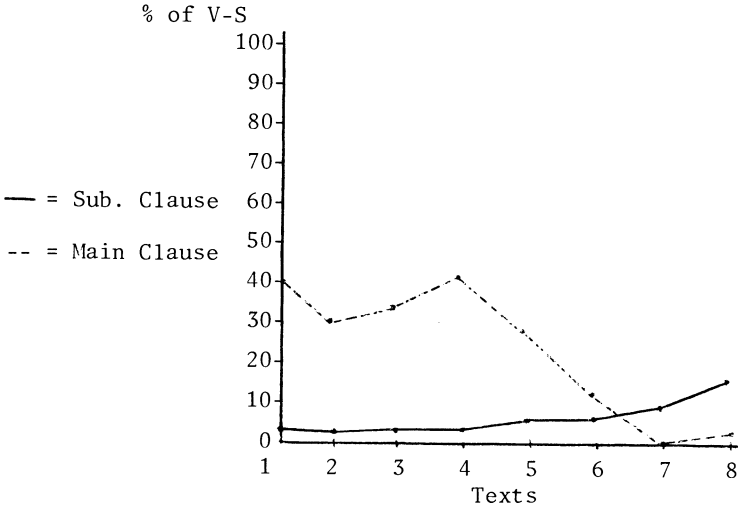
The pragmatic function of certain grammatical elements suggests a few reasons for these changes in V-S patterns.

It is a well-known fact that Old French lacked the extensive subordinating apparatus that Modern French possesses; Rychner (1970: 82)   phrases this as a "rareté du passage d'une phrase à l'autre sur un rapport logique: cause, explication, concession, supposition, fin." More commonly found is a simple adverb in first position acting in a subordinating capacity by linking a clause with the preceding one, in other words, establishing the first clause as the topic of the new construction. This suggestion is supported by the fact that the types of adverbs which do **not** influence V-S word order are, in fact, those which do **not** function as topic markers (certes, sans faille, onques, neporquant, etc):

5. <u>Maintenant</u> li six message s'agenoillent a lor piez.
   (Now the six messengers fall to their knees.)

Types of direct objects found in initial position are those having the pragmatic function of either indicating topic (like adverbs) or focus (using Kuno's definition of focus (1974) as a syntactically marked element to be rejected or accepted as a value for the variable in a presupposed predication):

6. <u>Ceste promesse</u> fist li rois.
                  focus
   (This promise made the king.)

To understand the seemingly strange behavoir of 'et', it is necessary to look at another element, 'si'. The distribution of 'si' as an adverb is unique from that of the other adverbs. 'si',

from Latin 'sic', has been considered by many scholars to be a 'predicative' or 'copulative' indicator. Rychner states that 'si' establishes its clauses as a predicate of the preceding clause. It is also found after subject noun phrases, often long and complex, indicating its extended use as a topic marker and creating at least one problem for verb-second supporters.

7. Li dux de Venise qui ot a nom Henris Dandole, et ere mult
   sages et mult prouz, si les honora mult, il et autres gens.
   (The duke of Venise, whose name was Henris Dandole, and was
   very wise and very powerful, si honored them greatly, he
   and others.)

Concerning V-S patterns, a comparison of the effect of 'et' alone in initial position, of 'si' alone, and of 'et' and 'si' together, suggests that 'et' may have acquired an adverbial nature and that 'si' was gradually replaced by 'et'. Contact between 'et' and 'si' is seen from the XIIIc-→ XIVc, with V-S structures occurring 11/13 times. Instead of acting solely as a coordinator, it is possible that 'et' was now being used to establish or help establish one clause as the predicate or comment of the preceding one. In her extensive study of Middle French word order (6600 sentences from 28 texts), Brown (1980) suggested that a proposition may be one of the terms of a complex proposition organized around one verb, and that the two clauses may condition each other to form only one syntactic sentence (p. 14). The present data seems to indicate that 'et' became functionally important in creating such complex propositions in the XIV and XV centuries. We also find that, whereas 'et' and 'si' both occur in main clauses, only 'et' is extensively found in subordinate and coordinate clauses, which may help to explain, at least in part, the extension of V-S structures into these clause types.

8. V-S Structures with 'et' and 'si' in Initial Position:

Texts	1	2	3	4	5	6	7	8
% V-S 'et'	23	15	15	66	56	18	4	14
'et' alone	6/60	5/118	0/96	22/45	14/26	4/27	0/16	1/7
'si' alone	6/6	25/25	58/60	2/2	1/1	1/2	*	*
'et' & 'si' together	1/1	8/9	2/2	0/1	*	*	*	*

The pronounced rise in V-S structures in subordinate clauses is still largely unaccounted for, however. Written Modern French, besides being characterized by a wealth of subordinating conjunctions, reflects a considerable rise in complex sentence structures: relative clause embeddings are prevalent, and even the less favored center-embedded clauses can be found. We would hope that an understanding of the organization of these complex sentence structures might explain more fully the appearance of V-S structures in these environments. The difficulties in processing center-embedded

clauses was first noted by Chomsky (1961) and later treated exten-
sively by Kuno (1974). This difficulty of comprehension, according
to Kuno, is due to the "limitation on the human capacity of tempo-
rary memory"(p.120). In Tok Pisin, G. Sankoff (1980) has suggested
that the particle 'ia', placed on both sides of a relative clause,
functions, in part, to give the listener perceptual cues as to the
limitation of the relative clause and thereby aid in processing
difficulties. Slobin (1977) has indicated that languages may use a
variety of devices (markers, prepositions, word order, pronouns,
inflections, etc.) to aid in processibility. In Modern French, V-S
structures seem to function as such a device for the processing of
complex sentences with embedded relative clauses, with embedded
clauses statistically significant at a .025 level in their effect
on word order in text 8:

9. C'est ainsi que tous les niveaux que distin$\overset{V}{\text{gue}}$ la lingui$\overset{S}{\text{stique}}$
   descriptive dans l'étude de langue sont depuis longtemps l'ob-
   jet de recherches comparatives impliquant des populations dé-
   favorisées.
   (It's in this way that all the levels that distinguishes des-
   criptive linguistics in the study of language have been for a
   long time the object of comparative studies involving disfa-
   vored groups.)

Heaviness of NP was also statistically significant, with not one
V-S structure with subject=pronoun in text 8.[3]
     Givón (1977) has shown that certain Bantu languages evolving
from VSO to SVO (i.e. Swahili, Dzamba) exhibit SVO first in main
clauses and much later in subordinate and relative clauses:

10. Dzamba: oPetelo a-nyamuzi imo-koudo imu-bundaki zaki.
    (Dik 1980:178)(Peter he-sold the-alligator that-caught Jack.)

Dik (1980) has suggested that this is due to the fact that rela-
tive markers/pronouns and subordinators behave like topic or focus
constituents, what he calls P1 constituents, and, having these
functions, are more likely to hold their preverbal position longer
than other elements without these functions. Since V-S structures
in Modern French subordinate and relative clauses seem to be more
productive and innovative than in previous periods (at least from
the XIIIc), we are less likely to hypothesize this phenomenon as
the last stage of French on its way to SVO order but, rather, as a
possible first sign of VSO word order. In spoken French, VSO traits
are also widely attested in phonological as well as in syntactic
ways, by the dropping of certain unstressed subject pronouns, the
use of 'Est-ce que..?' and the existence of dummy locatives and
pronouns.
     By modifying either Vennemann's TVX theory or Dik's P1VSO
stage to allow 'T' or 'P1' to include focus elements and even the
verb (if it is functioning as topic or focus), we have a viable

description of Old French word order. As subjects became grammati-
calized in first position, we find the emergence of dummy subject
pronouns, the disintegration of first position to mark function
and the rise of such devices as dislocation and cleft construc-
tions to mark elements for topic and focus. At least in the writ-
ten language, the rise of complex sentence structures has resul-
ted in V-S patterns to aid processibility, indicating that the
hearer must wait further for the relevant information of the sen-
tence. At the same time, we see the emergence of new V-S patterns
in subordinate and relative clauses to functionally mark the verb
as the topic of its clause, or a clause the topic of another
clause. The same phenomenon can also be seen in Modern Spanish:

11. ...y esta no es más que una opinión mía que no tiene usted...
       (...and this is nothing more than my opinion that don't have
              you...) (Grosso 1974:40)
12. Compare a. Les trois recherches qui s'attachent à l'étude du
                langage actif comparent les sujets sous l'angle
                de la structure qu'_ils_ _produisent_.
                                    topic+relevant information
                (The three studies concerning the study of lan-
                 guage production compare the subjects from the
                 perspective of the structure they produce.)
         with b. Mais contrairement à l'étude du langage actif,
                celle du langage réceptif suppose que soit délimi-
                té à priori le répertoire d'éléments de la langue
                constituant les épreuves de compréhension.
                (But, contrary to the study of language production,
                 that of language reception assumes that might be
                 delineated a priori the repertory of elements of
                 the language making up the tests in comprehension)

In example 11, we see that whereas the subject of the relative
clause is an inherently topical pronoun in sentence a. with the
verb containing the more relevant information, in sentence b.,
the verb is much more topical than the subject noun phrase in
post-verbal position.
     Of course, this study is only a preliminary attempt to ex-
plain the functional role of V-S structures in French. Work using
a framework involving 1)the various functions of propositions in
the construction of discourse models (Pollack 1983), and 2)the
notions of semantic and referential link to create discourse co-
hesion (Reinhart 1980), comparing both written and oral data,
would be fruitful in this area. Understanding the evolution of
such structures as V-S in French will help us not only to better
understand how French has evolved, but also help us to understand
the nature of languages in general as they constantly generalize
functional structures to grammatical status and then create new
or revived grammatical ways to express those same functions in
the language.

NOTES

[1] Here and throughout this paper, I will be using topic in the Chafian sense, as "an element setting the spatial, temporal, or individual framework within which the main predication holds." (1976:50)

[2] Text 1: Villehardouin, J. "La Conquête de Constantinople." 1210. ms. no. 687. Bibliothèque des ducs de Bourgogne. Reprinted in Société de l'Histoire de France. ed. M.A. Paris. Paris: Jules Renouard, 1838.
Text 2: La Mort le Roi Artu, 1230-35. ms. no. 342 du fonds français, Bibliothèque Nationale. In Etude sur la Mort le Roi Artu. éd. J. Frappier. Paris: Droz, 1972.
Text 3: de Clari, R. "La Conquête de Constantinople", 1300. ms. no. 487. Bibliothèque Nationale de Copenhagen. In Classiques français. ed. P. Lauer. Paris: Librairie Ancienne Honoré Champion, 1956.
Text 4: Froissart, J. "Chroniques de France", 1389-92. In Chroniques de J. Froissart, V. 14, ed. K. de Lettenhove. Bruxelles: Victor Devaux et Cie, 1872.
Text 5: du Clercq, J. "Mémoires", 1448-67. Bibliothèque de St. Vaast d'Assas. In Collection des Mémoires Relatifs à l'Histoire de France. Tome XI, ed. M. Petitot. Paris: Foucault, 1820.
Text 6: Tory, G. Champfleury, 1529. British Museum. cote 60.e. 14. In Classiques de la Renaissance en France. ed. M.A. Screech. Paris: S.R. Publishers, 1970.
Text 7: Vaugelas, C.F. Nouvelles Remarques sur la Langue Françoise. 1690. Paris: Guillaume Desprez, Reprinted: Genève: Slatkine Reprints, 1972.
Text 8: Page, M. "Problématiques des recherches comparatives sur la langue d'enfants issus de milieux différents". In Etudes sur la langue parlée des enfants québécois 1969-1980. ed. Gilles Gagné & Michel Page. Montréal: Les Presses de l'Université de Montréal, 1981.

[3] Pronominal subjects were disfavored in all texts, supporting several other studies on word order, including Silva Corvalán's study on Mexican-American Spanish (1981), Dik (1980), and Galambos' study of Old French (1982), who all indicated the strong topic function of subject pronouns.

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Development of Word Associations in a Second Language
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Introduction. A major question in the field of
second language acquisition is: In what ways is second
language (L2) acquisition similar to first language (L1)
acquisition? Comparisons between L1 and adult L2 acqui-
sition are important not only for understanding the ac-
quisition process of L2, but also that of L1. They help
answer the question of the roles of cognitive develop-
ment versus language experience in L1 acquisition. A
similarity in any component of the two processes rules
out cognitive factors for that component. The similar-
ity must reflect a universal property of language ac-
quisition or a property inherent in the specific lan-
guage. On the other hand, differences between L1 acqui-
sition and adult L2 acquisition could be the result of
cognitive factors or affective factors, such as moti-
vation.

Most of the work in L2 research has focused on
phonology, morphology, syntax, and, more recently,
discourse, which are areas that have interested lin-
guists in general. The lexicon has been neglected,
as noted by Levenston (1979), Meara (1980), and Cohen
and Aphek (1981).

There are several reasons to turn our attention
to the lexicon. First, it is a major component of lan-
guage. Second, students often cite vocabulary as one
of the biggest problems in L2 learning. Understanding
the acquisition process may have pedagogical implica-
tions. Third, there is sufficient research on certain
aspects of the lexicon in L1 to allow comparisons be-
tween L1 and L2, once we have the corresponding data for
L2.

The aspect of the lexicon delt with in the present
paper is lexical organization as reflected in word as-
sociations. It has been studied extensively in native
speakers (NS). (See Cramer, 1968.) The methodology
basically involves giving subjects a list of words and
asking them to respond to each word with the first word
that comes to mind. Definite patterns appear when one
compares stimuli and responses. These patterns are
suprisingly stable within a given language group.

Three important classifications of responses are:
paradigmatic (Pr), syntagmatic (Syn), and phonological
(Ph)(also known as "clang"). A Pr response is of the
same word class as its stimulus. A Syn response is of
a different word class. These two types are mutually
exclusive, i.e. a response cannot be both Pr and Syn.
A Ph response, as the term implies, resembles its stim-

ulus in sound. A Ph response can be either Pr or Syn, although the term is usually used for a response that has no apparent semantic or syntactic connection to the stimulus. As an example, suppose red were a stimulus. Blue would be a Pr response, apple a Syn response, and bed a phonological response.

Word association patterns within a language vary depending on a number of factors. One is the word class of the stimulus. Adult NSs of English give about 80% Pr responses to nouns, about 50% to verbs and adjectives, and about 20% to prepositions (Deese, 1962; Palermo, 1963).

Frequency in normal usage is also related to Pr responses in English, at least for adjectives. Deese (1962) reported a correlation of .40 between frequency of Pr responses and frequency of usage for adjectives, but no correlation for other word classes. Stolz and Tiffany (1972) found that adult NSs of English gave about 43% Syn responses to adjectives they knew, but 64% Syn responses to unfamiliar adjectives. These two studies suggest that experience or familiarity with an adjective relate to responding paradigmatically.

Another factor related to response patterns is age. One age difference is that young children give many more Ph responses than older children or adults (Entwisle, Forsyth, and Muuss, 1964). Another age difference is the proportion of Pr and Syn responses. Children tend to give Syn responses and adults Pr ones. For example, blue would be a typical adult response to red, and apple a typical child response. From about age six to ten, children's responses become more adult-like, i.e. the number of Pr responses increases. This change is referred to as the syntagmatic-paradigmatic shift. It seems to be cross-linguistic. It has been reported for a variety of languages including French (Rosenzweig and Menahem, 1962), Slovak (Maršálová, 1975), and Kpella, a language in Liberia (Sharp and Cole, 1972).

Two types of explanations have been given for the Syn-Pr shift. One is language experience. This has been proposed, in varying forms, by a number of researchers (Brown and Berko, 1960; Ervin-Tripp, 1967; McNeill, 1963; and Entwisle et al., 1964). The second explanation is cognitive development. Francis (1972) supports this one. She claims that the shift is due to a "reorganization of the mental filing system" resulting from changes in cognitive abilities. As evidence of these cognitive changes, she cites changes in the speech and thinking of children at this age.

A look at the development of adult L2 word associations might resolve the conflict between the two explanations. If Francis is right, one should not find

the Syn-Pr shift in the adults' associations. If the explanation is language experience, one might find the shift. Failure to find the shift does not automatically support a cognitive explanation since it might be language experience in general, and not just experience with the specific language.

Word association patterns differ depending on the language. As far as the Pr-Syn distinction is concerned, English produces more Pr responses than Polish, French, or Navaho (Shugar and Gepner-Więcko, 1971; Rosenzweig and Menahem, 1962; Ervin and Landar, 1963).

The number of studies on Pr and Syn response patterns in bilinguals or L2 learners is limited. Shugar and Gepner-Więcko examined the associations in English and Polish of advanced Polish university students majoring in English. They found that the percentage of Pr responses on the English test approximated that of American norms and on the Polish test - that of Polish norms. These findings suggest that the tendency to give a Syn or Pr response depends on the language, not the speaker.

Vertogradskaja (1975) considered changes in response patterns as L2 (Russian) proficiency increased. She reported more Syn responses at the lowest level and an increase in Pr responses in the two higher levels. These results seem to parallel those found for children. Unfortunately, her study had only 20 subjects and 30 stimuli, and her report is too brief to evaluate.

The number of Ph responses in L2 associations has also been noted. Reigel and Zivian (1972) found more Ph responses at the lower levels of German (the L2) than at the higher levels. Meara (1978) reported a large number of Ph responses from lower level French L2 learners.

There is other research supporting the idea that students at lower proficiency levels use form versus meaning to handle vocabulary. Henning (1973) looked at errors in remembering L2 vocabulary in English and Persian. He found that level correlated positively with semantic errors and negatively with phonological errors.

Purpose of the study. In order to compare the development of lexical organization in L1 and L2, the word association patterns of L2 learners at different proficiency levels were examined. The study attempts to answer the question: Do the Pr, Syn, and Ph response patterns of L2 learners change with proficiency in the same way as the patterns change in L1 with age?

Two predictions were made. First, a shift from predominantly Syn responses at the lower levels to more Pr responses at the higher levels was predicted. In other words, L2 development was expected to parallel

that of L1. This prediction was based on Vertograd-
skaja's limited study and on the findings described
above that Pr responses correlate with familiarity of
adjective stimuli. The second prediction was that low-
er level subjects would give a larger number of Ph re-
sponses and that this would decrease as proficiency
increased. This prediction was based on the work cited
above by Reigel and Zivian (1972), Meara (1978), and
Henning (1973).

Procedures. The subjects were 73 American univer-
sity students studying Russian and 17 NSs of Russian who
had recently emigrated from the Soviet Union. The stu-
dents were all NSs of English. They were divided into
four proficiency levels based on the class level: 29
first-year students, 15 second-year students, 17 third
-year students, and 12 fourth-year students.

Two word association tests were given, one in Rus-
sian and one in English. They were translation equiv-
alents. Each consisted of 70 common words of the fol-
lowing word classes: 20 nouns, 20 adjectives, 20 verbs,
and 10 prepositions. Subjects were told to "read each
word and write the first Russian/English word that
comes to mind." Responses were to be single words.
Subjects were encouraged to work quickly.

The NSs of Russian, who received only the Russian
version, were tested individually or in small groups.
The students were tested at the beginning of their
regular Russian class. The Russian version was given
first, followed about ten days later by the English
version. Subjects took less than ten minutes to com-
plete the L1 tests and less than fifteen for the L2
tests.

Each response was classified as either Pr or Syn,
depending on whether it was of the same word class as
its stimulus. In addition, each Russian response was
rated as Ph or not Ph based on fixed phonological cri-
teria.

Results. One-way analysis of variance and Scheffé
tests were used to compare the five proficiency levels
(four L2 and the NSs) with respect to the two classi-
fications of responses, Pr versus Syn and Ph. The
results for the Pr-Syn distinction are presented in
Table 1. The only significant difference between the
groups is for the noun stimuli. Even for this class of
stimuli, the significant difference is between the L2
learners and the NSs, but not between the different L2
groups.

These results do no support the original prediction
that there would be developmental changes in the L2
responses similar to those in L1, i.e. a Syn-Pr shift.
In fact, there do not appear to be any developmental
changes in the Pr-Syn pattern. It is interesting

to note that the L2 percentages are similar to those
reported by Deese and Palermo for adult NSs of English
(see above).

Table 1. Mean Percentages of Pr Responses.

Level	1	2	3	4	NS	F	p
Stimulus							
noun	65	77	69	66	35	9.40	.000
adj.	32	44	37	43	32	.74	.570
verb	47	37	38	45	35	.71	.583
prep.	24	35	38	30	28	.70	.590

The comparison of the five groups with respect to
Ph responses shows a different pattern (Table 2). Sig-
nificant differences exist between the groups for each
class of stimuli. Furthermore, the differences are not
just between the NSs and the L2 learners, but between
the L2 levels as well. The largest number of Ph re-
sponses were given by the lower level learners, and this
number decreases as proficiency increases. Based on
the Scheffé test, which determines significantly dif-
ferent subgroups, the levels can be grouped as follows
for noun, adjective, and verb stimuli. Each line repre-

```
      NOUNS:          ADJECTIVES:      VERBS:
     NS 4 3 2          NS 4 3          NS 3 2 4
        4 3 2 1          4 3 2            3 2 4 1
                            2 1
```

sents a subgroup, and the order from left to right in-
dicates increasing order of the mean percentages. Thus
for nouns, levels 4, 3, and 2 and NS do not differ sig-
nificantly from each other, nor do levels 4, 3, 2, and
1. However, level 1 and NS are significantly different.
There is a clear progression related to proficiency
level in the three classes of stimuli. It is not per-
fect for verbs in that level 4 is higher than 2 and 3.
However, the three levels together form an intermediate
group between level 1 and NS and do not differ much
from each other.

Table 2. Mean Percentages of Ph Responses.

Level	1	2	3	4	NS	F	p
Stimulus							
noun	18	13	11	9	6	5.04	.001
adj.	25	18	9	7	2	9.40	.000
verb	32	22	22	26	13	3.79	.007
prep.	13	5	5	3	8	3.56	.010

The results for Ph responses confirm the prediction
that subjects at lower proficiency levels would give
more Ph responses and that this type of response
would decrease at the higher levels.

Pearson correlation tests were used to determine
whether subjects tended to give consistantly Pr or Syn
responses. The results (Table 3) indicate an "individ-

ual style" for the L2 learners and, even more strongly, for the NSs. In other words, a subject who tends to give a Pr response to one word class of stimuli also tends to give Pr responses to the other word classes.

Table 3. Pearson Correlations for Pr Responses.

	L2 Learners			Native Speakers		
	adj.	verb	prep.	adj.	verb	prep.
noun	.59	.59	.59	.90	.95	.87
adj.		.70	.66		.86	.85
verb			.64			.87

$$p \leq .001$$

The lack of significant differences between the L2 groups in Pr responses and the similarity with means from previous research for English raise the question of transfer. Do the L2 learners transfer the patterns of Pr-Syn responses from their native language (English) to the L2 (Russian)? We can begin to answer this question by comparing the responses to the English version with those of the Russian one. Table 4 presents mean percentages of Pr responses from the American students in English and Russian, from published norms for English, and for the NSs of Russian.

Table 4. Mean Percentages of Pr Responses.

	English	norms*	Russian L2	Russian NS
noun	78	79	69	35
adj.	64	50	37	32
verb	52	48	43	35
prep.	60	22	30	28

*The figures for nouns, adjectives, and verbs are from Deese (1962), the one for prepositions is from Palermo (1963).

The comparison does not give a clear and consistent picture. For noun stimuli, it looks as though the L2 learners do transfer the tendency to give Pr responses. The figures for verbs could also be interpreted in this way, although the difference between the two Russian means is not significant. The percentages for adjectives and prepositions are more confusing. The L2 Russian percentages are close to the published English norms, but not to those from the English test used in this study. Is it the result of the test used in this study? Or is the tendency to transfer absent? The results for the noun stimuli make it difficult to answer "yes" to the last question.

Pearson correlations between Pr responses in English and in Russian were used to examine the question of transfer. The correlation coefficients for each word class of stimuli were as follows: .53 for nouns, .48 for adjectives, .41 for verbs, and .64 for prepositions. Although the correlations are not as strong

as those for the Pr responses within a language (see
Table 3), the L2 learners do appear to have an "indi-
vidual style" that is employed regardless of the lan-
guage of the test.

Conclusions. The developmental changes in Ph re-
sponses found in this study parallel those reported in
L1 development. This implies that the development is
not related to cognitive development or to exposure to
language in general. It is related to experience with
the specific language. At the early stages of language
acquisition, either L1 or L2, a person has only a very
limited "semantic network." The number of known words
is small, and the network of meanings connected to
individual words is incomplete. As a result, semantics
is not an efficient basis for organization. However,
sound or form is efficient, especially since there are
so few items (words) to organize. As the speaker devel-
ops the semantic component of the specific language,
there is both sufficient material and motivation (effi-
ciency) to change the criterion for the organization
of the lexicon from form to meaning.

The lack of developmental changes in Pr-Syn re-
sponse patterns in the present study does not parallel
the development found in L1, i.e. the Syn-Pr shift.
The dissimilarity suggests that the shift in L1 is
related either to cognitive development of to experience
with language in general.

The results reported here suggest two possible
lines of further research. If experience with the lan-
guage is related to the development of word associa-
tions, the type of experience may be crucial. Verto-
gradskaja's results (see above) suggest similarity
between L1 and L2 development. One difference between
her study and the present one is the acquisition situ-
ation. Her subjects were in the environment where the
L2 was spoken. The subjects in the present study were
not. Most, if not all, of their experience with Rus-
sian was in the classroom. One direction of research
would be to compare the development of associations
for subjects acquiring the L2 in a formal setting with[1]
those for subjects in an informal, "natural" setting.

The second line of research concerns the question
of transfer of Pr-Syn response patterns from the native
language. If transfer does take place, response pat-
terns in a given language should differ for groups of
L2 learners of different native languages. Thus, for
example, one could extend the present study by examin-
ing the Russian response patterns of French, Polish,
and Japanese L2 learners and by comparing them with
patterns in the native languages.

Notes.
1. Shugar and Gepner-Więcko's results (see above) suggest that L2 patterns can be like those of native speakers of the target language even though the L2 was learned in a formal environment. However, it is not known from the report whether the subjects (students) had ever been in an English-speaking country or in other "immersion" situations.

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Smash Bang*

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At first glance, the verb <u>smash</u> appears to differ in meaning from <u>break</u> in much the same way that <u>shatter</u> does. A reasonable <u>rough</u> account of the meaning difference between (1a-c) is that (1b-c) are intensive versions of (1a). However, while (1a-b) have intransitive counterparts, (1c) does not, (2c) being strikingly unacceptable.

1.a. Mary broke the vase
 b. Mary shattered the vase
 c. Mary smashed the vase
2.a. The vase broke
 b. The vase shattered
 c.?*The vase smashed

This would seem to be a major difficulty for the many frameworks which derive transitive usages of state-change verbs from intransitive usages, if we are correct in analyzing <u>smash</u> as a state-change verb. Transitive <u>smash</u> would simply have no source. However, there is considerable good reason for analyzing <u>smash</u> not as a state-change verb like <u>break</u>, but rather as <u>a collision</u> verb (of a subtype to be identified below).

First note that there are transitive usages of <u>smash</u> which do have corresponding intransitive usages. If there is an oblique (i.e. prepositional) phrase of the appropriate sort in the VP, <u>smash</u> can appear optionally transitively or intransitively. Here there is an apparent parallelism with <u>break</u> and <u>shatter</u>.

3.a. Mary broke the vase against the wall
 b. Mary shattered the vase against the wall
 c. Mary smashed the vase against the wall
4.a. The vase broke agaist the wall
 b. The vase shattered against the wall
 c. The vase smashed against the wall (contrast 2c)

The parallelism here is, however, only apparent. To see this, compare the semantically anomolous (5a) with the nonanomolous (5b).

5.a.+Mary shattered the vase aginst the wall, but it didn't break
 b. Mary smashed the vase against the wall, but it didn't break

The fact that (5a) is contradictory while (5b) is not immediately suggests that while <u>shatter</u> and <u>break</u> have change of state as part of their semantics, any state-change sense associated with <u>smash</u> is there as a matter of conventional implicature. Further evidence for this is provided by the fact that assumptions as to which object changes state in an event denoted by <u>smash</u> is relativised to real world expectations. In (6-7), the (a-b) sentences clearly entail that it is the hammer which is the broken/shattered object, while the (c) sentences allow, though don't force, one to assume that the more fragile vase is the object which suffers on impact (if anything does).

6.a. Mary broke the hammer against the vase
  b. Mary shattered the hammer against the vase
  c. Mary smashed the hammer against the vase
7.a. The hammer broke against the vase
  b. The hammer shattered against the vase
  c. The hammer smashed against the vase

Note also that if <u>smash</u> is not a true state-change verb, it would not be expected to carve itself a tightly defined niche in the semantic field of state-changes. And indeed, while <u>break</u> and <u>shatter</u> are quite restrictive as to what their patients can be, brittle objects which turn into pieces of brittle objects (ignoring for the moment that objects with functions are said to be broken when they can't perform their function; broken engines, broken radios, etc, might simply be gummed up and not in pieces at all), <u>smash</u> is, on the other hand, extremely liberal as far as possible patients are concerned.

8.a. ?Mary broke the cake
  b. ?Mary shattered the cake
  c. Mary smashed the cake

There is at least one usage that <u>smash</u> has in which the object assumed to have suffered is obligatorily NOT the transitive direct object or intransitive subject. If the PP in the VP is penetrative, like <u>through</u>, instead of imp-active, like <u>against</u>, it is the object of this PP which is taken as the suffering object. Here there is no parallelism with <u>break</u> or <u>shatter</u>. The (a-b) sentences (9-10) are sense-less, unless one can force a locative sense out of the <u>through</u>-phrase.

9.a. ?Mary broke the hammer through the window
  b. ?Mary shattered the hammer through the window
  c. Mary smashed the hammer through the window

10.a. ?The hammer broke through the window
   b. ?The hammer shattered through the window
   c. The hammer smashed through the window

On the basis of these data, I conclude that smash
does not belong to the same class of verbs as break and
shatter. This leaves us with several questions. What
class does smash belong to? Where does this implicature
carried by smash come from? I will turn to these questions
immediately and will also have some observations and com-
ments on the strengthening of the implicature in certain
argument arrays.
   I already stated above that I believe smash to belong
to a subclass of collision verbs. To see the initial plaus-
ibility of this, consider how closely another collision
verb, bang, parallels smash in distribution.

11.a. Mary banged the vase (cf 1c)
   b. ?*The vase banged (cf 2c)
   c. Mary banged the vase against the wall (cf 3c)
   d. The vase banged against the wall (cf 4c)

Even more striking are the facts in (12). While (12a)
and (12b) are acceptable when together is interpreted to
mean "at the same time", (12c) and (12d) have a "mutual"
interpretation (i.e. sword A smashed/banged against sword
B and sword B smashed/banged against sword A). This inter-
pretation is unavailable for (12a) and (12b). Note also,
by the way, that (13) is not redundant, as it would be if
smash had a breakage sense as part of its semantics.

12.a. The sword blades broke together
   b. The sword blades shattered together
   c. The sword blades smashed together
   d. The sword blades banged together

13. The sword blades smashed together and shattered

The most striking usage that smash shares with bang,
and not at all with break or shatter, is as an interjection.
Note also that there are several collision verbs   also
impossible as interjections (e.g. hit or strike).

14. Mary was carrying the vase upstairs, when...
   a. smash!/bang! it fell down the stairs
   b.*break!/*shatter! it fell down the stairs
   c.*hit!/*strike! it fell down the stairs

Not all interjections of this class are equally nat-
ural as verbs, e.g. thud. However, given a sufficiently
literary context, a sentence like (15) is by no means

impossible. So the derivational direction appears to be from interjection to verb. This gives us a class of deint- erjectival verbs to which both <u>smash</u> and <u>bang</u>, along with some others, e.g. <u>slam</u> and <u>crash</u>, belong.

    15. Mary listened as the rain thudded against the
        roof of the car.

But where does the damage implicature carried by <u>smash</u>, which is responsible for the illusion that <u>smash</u> is a state-change verb, come from in the first place? An interesting suggestion given to me by Jerry Sadock (per- sonal communication) is that it may be derived from the sound symbolism of interjective <u>smash</u>. Notice in this regard that interjections have their own appropriacy con- ditions. For instance, the sentences in (16) are decidedly odd.

    16.a. Bang! the feather fell on the floor
       b. Smash! the students slept

More interesting for the problem at hand is that <u>smash</u> seems more appropriate than <u>bang</u> in sentences which describe situations in which an object is destroyed in a collision, while <u>bang</u> seems more appropriate than <u>smash</u> in sentences which describe situations in which two mas- sive objects collide but remain intact. Note too, however, that <u>smash</u> is not as clumsy in (17d) as <u>bang</u> is in (17b). The fact that interjection <u>smash</u> prefers but does not demand destruction correlates nicely with the fact that the verb <u>smash</u> implicates but does not entail destruction.

    17.a. Smash! the wrecking ball drove through the wall
      b.??Bang! the wrecking ball drove through the wall
      c. Bang! the wrecking ball bounced off the wall
      d. ?Smash! the wrecking ball bounced off the wall

The correlation is robust and I believe the hypothesis to be correct that the implicature that <u>smash</u> carries is derived from its interjective usages. However, while there has been some work on formalizing implicatures (cf Gazdar 1979 and works discussed there) and considerable work on relating the semantics of derived forms to the semantics of basic forms, I am unaware of any attempts to formally derive conventional implicatures of verbs from the appro- priacy conditions of interjections. It is not immediately clear what such rules would even look like. I will not offer even a tentative solution and leave the question for those more knowledgeable of the lexicon and the nature of lexical rules.

A problem very different from the one just discussed concerns the difficulty in cancelling the damage implicature in simple transitive sentences. This is a major problem since there would be no content to a claim that a sense associated with a word is an uncancellable conventional implicature. Consider (18).

18.??Mary smashed the vase, but it didn't break

Even sentence (19), a transitive sentence augmented by an instrumental PP instead of what I called above an "impactive" PP (i.e. a PP headed by a P like against), sounds quite odd at first blush.

19. ?Mary smashed the vase with a hammer, but it didn't break

There are several steps in the (partial) unravelling of this mystery that I will propose here. The first is to point out an insight had by Fillmore (1977). Fillmore noticed that for verbs with optional direct object selection, whichever object is chosen as direct object is taken as the more affected object. For example, in (20a) the fence and in (20b) the stick is taken as the more affected object.

20.a. Mary beat the fence with a stick
   b. Mary beat the stick against the fence

I believe, however, that Fillmore is only partly right. I believe that in (20a) the fence, as notional goal and (shallow) direct object, is more of an undergoer than the stick is in (20b). Sentence (20b) seems to be a more neutral discription of an event and, I would claim, is the sentence one would use if one didn't want to take the perspective (Fillmore's term) of either object. (There may be a conversational implicature of perspective resulting from the fact that the alternative construction was not employed). Some slight evidence in favor of my claim is that if one inserts an adverb like viciously into the sentences in (20), it must be associated with the fence in (a) but can be associated with either object in (b).

21.a. Mary beat the fence viciously with the stick
   b. Mary beat the stick viciously against the fence

The point here is that to utter (19) a speaker has made a choice to put the vase in a strong undergoer position which is a choice at odds with a desire to cancel the implicature if there is no further elaboration. In (18) the speaker has gone so far as to suppress mention

of the other object involved in the collision, with the
(intuitively unsurprising) effect of strengthening the
undergoerhood of the direct object and thus the implica-
ture that it has suffered in the collision.

   While I believe this line of reasoning to be correct,
we are still faced with the unsatisfactory result of un-
cancellable conventional implicatures, unless it can be
shown that despite such strengthenings of the implicature
it can yet be cancelled under certain circumstances. The
obvious thing to look for is a way to load the (linguistic)
context in ways that counteract the implicature strength-
ening effects of the choice of argument structures in (18)
and (19). Fortunately for the hypothesis being proposed
here, constructions with the desired effect can be found.

   One construction which immediately improves sentences
with the argument structure in (19) is to add an aspectual
adverb phrase like again and again. Such adverbs have the
effect of drawing the attention away from the results of
actions and putting it on the action itself. Since the
damage implicature carried by smash is an implicature of
result, it is not surprising that such adverbs have the
effect of (re)weakening the implicature. Further, by re-
placing didn't, which is a neutral negative, with wouldn't,
which conveys a sense of refusal to give in, the sentence
becomes even more natural. Finally, by using an anaphor
like the damn thing instead of a simple pronoun, a sense
of frustrated objectives very compatible with the use of
wouldn't crops up, making the sentence more natural still.
For purposes of comparison, note that none of these stra-
tegies prevents (23), a sentence headed by a semantic
state-change verb, from remaining a contradiction.

   22.a. Mary smashed the vase again and again with a
         hammer, but it didn't break
      b. Mary smashed the vase again and again with a
         hammer, but it wouldn't break
      c. Mary smashed the vase again and again with a
         hammer, but the damn thing wouldn't break

   23. +Mary shattered the vase again and again with a
       hammer, but the damn thing wouldn't break

   The additional problem posed by (19) was that mention
of the second object involved in the collision was totally
suppressed with the effect that all the attention seemed
to be thrown onto the implicature, thereby strengthening
it severely. One might hypothesize that if there is some
way of mentioning all the participants in an event other
than by employing a fully fleshed out argument frame, the
effects of this sort of implicature strengthening could
be directly counteracted. And, indeed, this seems to be the

case. In sentence (24) the second object involved in the collision is mentioned in a conjoined VP. If all the tricks developed in (22) are employed, the result is a natural and noncontradictory sentence.

24. Mary grabbed the hammer and smashed the vase again and again, but the damn thing wouldn't break.

So although the damage implicature carried by smash can be severely strengthened in certain argument frames, it remains cancellable.

The signifance of an intensely empirical paper like this one in the greater linguistic scheme of things must be evaluated in terms of the questions it raises and areas it opens up to investigation. By far the most perplexing question raised in this paper is how to derive conventional implicatures of verbs from appropriacy conditions of inter- jections. It will be interesting to see if current theories of the lexicon and lexical rules can come to deal with such phenomena. The issue of how implicatures can be made stronger or weaker played a large role in this paper. A more systematic investigation of this phenomenon than could be offered here would be welcome. Finally, this class of verbs has been fairly ignored in the literature. The argument structure properties investigated in this paper should be of interest to case grammarians, rela- tional grammarians and lexical functional grammarians.

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TYPOLOGICAL PARALLELISM DUE TO SOCIAL CONTACT: GUATÓ AND KADIWÉU

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The purpose of this paper is to show that two genetically un-related languages spoken on the upper Paraguay basin in South America, Guató and Kadiwéu, share the same way of expressing reference to the addressee and that the source of this fact may be found in the contact held in the past by the speakers of both languages.

The Guató people have traditionally lived on the banks of the upper Paraguay, including Gaíba and Uberaba lagoons, as well as along the Cuiabá (São Lourenço) river, a tributary of the Paraguay. Their presence there has been recorded since the 16th century (cf. Schmidt 1905, Métraux 1942, 1945b). For some centuries the Guató have had the Guaikurú (Mbayá, Kadiwéu) as their principal southern neighbors (Métraux 1945a, maps 4 and 5).

The Guató language,[1] apparently a linguistic isolate, in any case without perceptible genetic relationship with Guaikurú, has a system of personal affixes which is characterized by, among other things, the absence of a specific marker for the second person plural: the same prefix gʷa- (and its suffixal counterpart -hi) marks both the second person singular and the second person plural. Although distinction is made by means of different personal prefixes between third person singular and third person plural as well as between first singular and both inclusive and exclusive first plural, the plurality of the addressee is regularly expressed at the level of the nominal or verbal phrase by means of the quantifying word mehẽ, which may be glossed as 'pluralizer of second person'. Thus, for the nominal stem -rɛ 'eye', we have the following possess-ed forms:

(1)a. i-rɛ 'his eyes'
   b. bi-rɛ 'their eyes'
(2)a. a-rɛ-ru 'my eyes'
   b. gi-rɛ 'our (incl.) eyes'
   c. haǰi-rɛ 'our (excl.) eyes'
(3)a. gʷa-rɛ 'your (sg.) eyes'
   b. gʷa-rɛ mehẽ 'your (pl.) eyes'

With verbs we have:

(4)a. na-ki-hi 'you (sg.) are fishing'
   b. na-ki-hi mehẽ 'you (pl.) are fishing'
(5)a. na-gʷa-ǰo-yu 'you (sg.) are seeing me'
   b. na-gʷa-ǰo-yu mehẽ 'you (pl.) are seeing me'

The Kadiwéu are the only surviving section of the Mbayá or

Guaikurú people, which dominated in the 17th and 18th centuries a
large extension of the Chaco, just south of the Guató. The Kadiwéu
language[2] is a member of the Guaikurú linguistic family, which in-
cludes other languages spoken in the Paraguayan and Argentinian
Chaco more to the south and to the west, such as Payaguá, Pilagá,
Toba, Mocoví. As to the person markers Kadiwéu presents a situation
analogous to that of Guató: it has also person marking prefixes
which do not distinguish the second person singular from the second
person plural, and like Guató it expresses the plurality of the
addressee by means of a quantifying word in the nominal or verbal
phrases. Thus, for the nominal stem -opitenigi 'bow' and for the
verbal stems -dopi 'to return' and -owooqo 'to think' we have:

(6)a. Gadopitenigi 'your (sg.) bow'
   b. Gadopitenigi tiwaji 'your (pl.) bow'
(7)a. adopili 'you (sg.) return'
   b. adopili tiwaji 'you (pl.) return'
(8)a. owooqoni 'you (sg.) think'
   b. owooqoni tiwaji 'you (pl.) think'

The quantifier tiwaji occurs systematically as a pluralizer
for the second person, but (differently from Guató mehẽ) it may
also occur with the first and third persons plural, meaning in
these cases 'a larger group' (Griffiths 1976:84):

(9)a. jalokodaGa 'we (two or three) run'
   b. jalokodaGa tiwaji 'we (a larger group) run'
(10)a. oyoqole 'they (two or three) throw it'
   b. oyoqole tiwaji 'they (a larger group) throw it'

Personal markers in Kadiwéu are prefixes as may be inferred
from the comparison of (7) above with (11) and of (8) with (12):

(11)a. idopi 'I return'
   b. dopi 'he returns'
(12)a. jowooqo 'I think'
   b. dowooqo 'he thinks'
   c. nowooqonaGa 'they think'

In Kadiwéu verbs, which are somewhat more complex than those
of Guató, there is a suffix immediately following the stem which
occurs when the subject is second person (singular or plural), first
person plural, or third person plural, but never occurs when the
subject is either first or third person singular. This suffix shows
up as -ni, -li, -mi, -∅, or as lengthening of the final vowel of the
verbal stem. In example (7) it is -li and in (8) it is -ni, whereas
in (9) and (10) it is -∅ and in (12a) it is -ni again, but having
dropped its vowel before the initial vowel of the following suffix
-aGa; examples (11a, b) and (12a, b) are the cases where it does
not occur. Examples (13a-f) exhibit the lengthening of the last
vowel of the stem in contrast with its absence:

(13)a. aloo 'you (sg.) play'
    b. aloo tiwaʝi 'you (pl.) play'
    c. ʝalooɢa 'we play'
    d. nalooɢa 'they play'
    e. ʝalo 'I play'
    f. dalo 'he plays'

    Griffiths in his study of the Kadiwéu verb (1976) considers this suffix -- which I call for brevity's sake suffix -ni -- to be a subject suffix, without further explanation. I assume that it is more specifically a mark of number agreement to plural subjects and that its occurrence in the forms for the second person singular should be due to an extension of the use of the second person plural. That is to say that I suppose that forms like (7a), (8a), and (13a) were originally, in a historically earlier stage of Kadiwéu, second person plural forms marked by the pluralizer suffix -ni, and that other forms, no longer in use, without that suffix, existed for the second person singular. Then specific social conditions led the speakers of the language to use forms of the second person plural systematically in place of the forms of the second person singular, to the point where these latter disappeared. The situation should have been somewhat analogous to the English case, where the pronoun you and the other forms of the second person plural replaced the pronoun thou and the other forms of the second person singular.

    That in Kadiwéu society such conditions could have developped, which would have favored that type of sociolinguistic change, seems to be rather possible, since it is known that the Mbayá society, from which the Kadiwéu descends (Ribeiro 1950:146), was stratified into social classes: chiefs and nobles at one extreme and serfs and slaves at the other. Métraux (1945a:304), who analyzed the historical sources, states that

    "In contrast to the democratic organization of the Pilcomayo River tribes, Mbayá society was rigorously stratified. The a-doption of the horse gave this tribe a decided advantage over its neighbors, which contributed to the formation of a system of classes and even of castes. Unable to absorb its countless prisoners, as most Chaco Indians do, each group maintained its individuality and hegemony by stressing blood purity and the privileges of the conquerors. The subjugated tribes were re-duced to the condition of serfs and slaves, and the heads of the extended Mbayá families constituted a new aristocracy".

    Such a complex social organization was being developed even before the adoption of the horse:

    "A military order composed of outstanding warriors seems to have existed among the pre-equestrian Mbayá, when they were known as Guaicurú" (Métraux 1945a:309).

    On the other hand, there is historical information on the use of special linguistic address forms or other sociolinguistic dif-

ferentiation among peoples who spoke other languages of the Guaikurú family. Thus, of the Mocoví it is reported that they distinguished noblemen and plebeian and that "special grammatical forms were used to address a nobleman" (Métraux 1945a:304). As to the Abipón it is said that their

"noblemen differentiated themselves from other people not by special ornaments, but by certain mannerisms of speech or the profuse use of redundant syllables which gave their language a "noble" turn. Those who addressed them had to add the suffix "in" to words. Moreover, the members of the [noble or military] society had some words peculiar to themselves." (Métraux 1945a:309).

Very little is known of the social organization of the Guató, among whom it is less probable that there existed a social stratification which resembled that of the Mbayá. Chiefdom, however, was hereditary (Métraux 1945b:417). On the other hand, it is known that at least a part of the Guató came to be dominated by the Mbayá and were held as slaves by them (Métraux 1945a:307).

Summing up, it seems that we are confronting an interesting case of typological parallelism in languages genetically independent, but neighbors geographically and, what is more, we have a possible sociolinguistic explanation for it: in the Kadiwéu (Guaikurú) language the nominal and verbal forms of the second person plural came to be used in a ceremonial way to single addressees of higher social classes and then were generalized to all kinds of addressees, having replaced the older singular forms, which disappeared from the present-day language; in Guató the same usage and similar consequences could have developed as a result of the strong social interaction and partial integration of the Guató people as slaves in the network of Guaikurú intertribal dominion.

Footnotes

1. The Guató linguistic data were recorded and are being analyzed by Adair P. Palácio, to whom I thank for discussions on the morphology of this language.
2. The data on the Kadiwéu language were analyzed and published by Glyn and Cynthia Griffiths (1976), but the interpretation given to them in this paper is due to a partial reanalysis by Rodrigues and Sílvia L. B. Braggio (cf. Braggio 1981).

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Vowel Shifts in Common Slavic[*]
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1. Introduction

The purpose of this paper is to attempt to motivate and explain the vowel shifts which occurred during the history of the Common Slavic (CS) dialect of Indo-European. Common Slavic is the term given to the language which existed prior to the dissolution of Slavic linguistic unity sometime during the seventh century. No known records of Common Slavic exist, but it can be reconstructed by examining the evidence available from the contemporary Slavic languages,[1] as well as pertinent data from non-Slavic languages which are historically attested. According to Shevelov (1965), the period of Common Slavic linguistic unity lasted approximately three millenia, from c. 2000 B.C. to its final break-up into recognizably distinct Slavic languages by 1000 A.D.

Many excellent historical grammars of Common Slavic exist, among them Arumaa 1964, Mikkola 1913, and Entwistle and Morison 1949. My primary source for the data to be discussed in this paper is Shevelov 1965 (henceforth Shevelov), which is by far the most detailed treatment of Common Slavic available in a non-Slavic language. Certainly it is the most detailed and ambitious book of its kind in English. Most works are merely content with listing the established Common Slavic form which corresponds to a given PIE source, without giving any indication of intermediate steps. The works of Arumaa, Mikkola, and Entwistle and Morison, along with that of Bidwell 1963, are primarily of this type, and thus they tend to be of limited use to those interested in studying possible intermediate stages in the development of Slavic phonology. Shevelov, on the other hand, goes much more deeply into the evidence for possible intermediate steps, and so his reconstructions will be of particular importance to the topics I will treat in this paper. I will, therefore. essentially assume Shevelov's reconstructions, although the other works mentioned above have been invaluable background sources in providing perspective on the data.[2]

The structure of the paper will be as follows. First, I will give an overview of the developments of the vowels and diphthongs in Common Slavic from PIE to just before the dissolution of Common Slavic unity, following essentially Shevelov's reconstructions. Included in this overview will be a short discussion of the vowel jat' and its effect on the vowel system, the monophthongization of the diphthongs, and finally the developments in the Common Slavic vowel system subsequent to the monophthongization of the diphthongs. An attempt will be made to motivate the changes which occurred. Then, I will show how particle phonology, as developed by Sanford Schane (1982 and class lectures) helps in elucidating some of the observed changes.

2. A Survey of the Vowel Shifts in Common Slavic

2.1. Developments in the Vowels Prior to the Monophthongization of the Diphthongs

It is generally agreed that the PIE vowel system was as follows immediately prior to the development of Common Slavic:

(1) PIE Vowels and Diphthongs

    i: e: o: a: u: i e o a u ei oi ai eu ou au

The consensus of the sources named in the introduction is that the old PIE long diphthongs had coalesced with the short diphthongs by the time of the development of Common Slavic. Shevelov cites the following as the first change in Common Slavic from PIE:

(2) PIE i: e: o: a: u: i e o a u ei oi ai eu ou au

     CS   i: e: $_o$a: u: i e $_o$a u ei $_o$ai eu $_o$au

As can be observed, Shevelov reconstructs the vowel which results from the merger of PIE [o(:)] and [a(:)] as [$_o$a(:)]. Evidently, the vowel was very low, almost like [a(:)], but with some o-type character remaining, perhaps even as an o-type on-glide (Shevelov 1965:150). Most less detailed sources simply state that the above shift resulted in CS [a:] from the merger of PIE [o:] and [a:], and CS [o] from the merger of PIE [o] and [a]. It is true that, ultimately, these are the vowels which resulted, but Shevelov documents [$_o$a(:)] as an intermediate stage that was stable for several hundred years prior to the development of [a:] and [o].[3] We have, then, the following vowel system in Common Slavic following the above change:[4]

(3)   i(:)   u(:)   ei   eu

          e(:)

               $_o$a(:) $_o$ai $_o$au

Later, when examining the above shifts in particle terms, we will see that a very plausible interpretation of [$_o$a(:)] is [ɔ(:)], which, for the sake of convenience, I will use in some of the following discussion.

    Shevelov claims that the above vowel system was inherently unstable due to the fact that the old opposition between [e(:)] and [o(:)] was now lost. The system had become slightly lop-sided. It is this unbalanced nature of the system which, according to Shevelov, motivates the next change:

(4)   PIE e(:)   >   CS $_e$a(:)

The results of this shift give us the following system:

(5)   i(:)   u(:) $_e$ai $_e$au

         $_e$a(:) $_o$a(:) $_o$ai $_o$au

Shevelov is careful to note that, while the merger of PIE [o(:)] and [a(:)] to CS [ɔ (:)] was a shift that resulted in the new phoneme [ɔ (:)], the shift in (4) above was phonetic in nature, only. That is, the phonemic nature of [e(:)] did not change, although its phonetic realization did, probably in an effort to achieve more of an intra—systemic balance. It seems that, phonetically, [e(:)] tended to drift down into a more open vowel in an apparent effort to somehow balance [ɔ (:)]. Interestingly enough, though, a full scale shift of [e(:)] to a lower position did not occur. In the following section I will summarize the major points about the nature of [$_e$a(:)] in Common Slavic.

2.2. On the Nature of jat' in Common Slavic

It is necessary to briefly consider the phonetic nature of the vowel [$_e$a(:)] if we are to gain any insight into the development of the CS vowel system. Traditionally, [$_e$a:] is written as ě and called jat'. I will henceforth follow tradition and write [$_e$a:] as ě.[5] According to Shevelov, ě was 'a vowel of more open and back articulation [than [e:]], but preceded by an on-glide which preserved the original e -type articulation' (Shevelov 1965:164). Thus, jat' could have been something like [$_e$æ].

The problem with ě is that the modern Slavic languages exhibit a bewildering array of reflexes which can be traced back to it. Reflexes have been attested from [a] to [i]. (See Shevelov 1965, p. 166ff. for examples). A number of papers and monographs have dealt specifically with jat': Samilov 1964 surveys various reflexes of jat' and concludes that the most likely phonetic value of ě was either [æ] or [ea]. Indeed, Stankiewicz (1973), in his review of Stieber 1969, quotes the latter's observation that the reflex [æ] still exists in some dialects of Bulgarian, as in the words sn'ag, m'asto, and v'ara (no glosses given). Ivić (1959) notes that all of the following reflexes of ě are found in various Serbocroatian dialects: [e], [i], [je], [ie], [ej], and [jæ] (Ivić 1959:50). Shevelov cites evidence from Bavarian German words borrowed from Slovenian that ě was rendered orthographically as ie:

(6) bě̃l 'white' > <u>Pielach</u> (811 A.D.)
    r̃ěka 'river' > <u>Rieken</u> (982 A.D.) (Shevelov p.168)

Shevelov claims that <u>ie</u> represented the diphthongal character of
ě̃. There are also Finnic loanwords of the period which render ě̃
as <u>ie</u> orthographically (Shevelov 1965:167). Conversely, Shevelov
notes that Common Slavic borrowings of Old High German words with
<u>ia</u> and <u>ie</u> were rendered as ě̃:

(7) CS * cě̃gъl 'brick' < OHG <u>ziagal</u> (<u>ziegal</u>)
    Slovenian <u>špě̃gli</u> 'glasses' < OHG <u>spiegal</u> (Shevelov p.168)

Also, Shevelov cites as indirect evidence for the open nature of ě̃
the fact that Common Slavic borrowings from languages in which the
word had a closed [e:] are rendered in Common Slavic not with ě̃
but with [i:]:

(8) Vulgar Latin <u>mēsa</u> > Old Church Slavic <u>mīsa</u> 'disc'
    Persian <u>dēv-</u> 'demon' > Old Russian <u>divъ</u>(Shevelov p.171)

Shevelov argues that closed [e:] was evidently perceived as being
closer phonetically to [i:] than to ě̃, thereby implying that ě̃ was
quite open. During the later period of CS unity and beyond, there
is further evidence from loanwords that ě̃ was once again gradually
becoming more closed (Newman 1971:326).
    The preceding discussion, while not intended to be an exhaus-
tive investigation of the nature of ě̃, gives sufficient informa-
tion as to how widely varied its pronunciation must have been dur-
ing the CS period. In the following section, I will describe how
the phonetic nature of ě̃ is connected to the monophthongization of
the diphthongs in Common Slavic.

<u>2.3</u>. The <u>Monophthongization</u> <u>of</u> the <u>Diphthongs</u> <u>in</u> Common <u>Slavic</u>
    Recall that in Common Slavic there were four diphthongs fol-
lowing the coalescence of PIE [o(:)] and [a(:)]. Two diphthongs
ended in an [i] off-glide, and two ended in an [u] off-glide.
These sets will be referred to as the i-diphthongs and the u-
diphthongs, respectively:

(9)   ei   eu

                 ₒai (=ɔi)   ₒau (=ɔu)

Now, following the phonetic change which resulted in ě̃, Shevelov
renders the diphthongs as the following:

(10) e^{ai} e^{au}

                       o^{ai} o^{au}

It is unclear as to whether the diphthongs with short $\underline{\overset{\text{y}}{e}}$ as the
first element should be represented as in (9) or (10) above. I
will base my discussion on the representation in (9).

The sources I consulted are generally noncommittal about the
chronology of the monophthongization of the diphthongs. The con-
sensus is that the monophthongization of both sets of diphthongs
occurred at approximately the same time.

The basic tendency in the monophthongization process is that
the i-diphthongs monophthongized to front vowels and the u-
diphthongs monophthongized to back vowels. The u-diphthongs mono-
phthongized to what Shevelov calls $[u:_2]$, which is distinct from
the $[u:]$ which is derived from original PIE $[u:]$. In subsequent
developments the original $[u:]$ ($[u:_1]$) becomes CS $[y:]$ ($=[\dot{i}:]$),
while $[u:_2]$ remains an u-type vowel. (I will consider the shift
of $[u:_1]$ to $[y:]$, and its likely motivation, in more detail
later.)

The i-diphthongs, on the other hand, developed as follows:

(11)   ei > i: o^{ai} > $\overset{\text{y}}{e}$

Notice that whereas the u-diphthongs monophthongized to the same
long vowel, the i-diphthongs had divergent developments, merging
to separate long front vowels.

There is some confusion on Shevelov's part as to the phonetic
value of $[u:_2]$. He realizes that it is not possible to recon-
struct the exact phonetic value, but he does say that $[u:_2]$ was 'a
less back and probably less rounded vowel than $[u:_1]$' (Shevelov
1965:276). He claims that it had a definite u-type quality.
Later, he says that 'when later $[u:_1]$ became $[y:]$ and lost its
rounding, it still did not converge with $[u:_2]$ because of the
former's farther back (and possibly lower) articulation' (Shevelov
1965:276). These quotes are confusing to a certain extent, in
that in the first he claims that $[u:_2]$ was less back and round
than $[u:_1]$, and in the second quote he says $[u:_1]$ was articulated
lower than $[u:_2]$, even though all along he seems to suppose that
$[u:_1]$ was more u-like than $[u:_2]$.

I would like to propose a different solution, one which
Shevelov gives evidence for, but inexplicably rejects. Recall
that $[o(:)]$ and $[a(:)]$ merged to yield $[_oa(:)]$ ($=[ɔ(:)]$). The
vowel $[ɔ(:)]$ was quite stable in Common Slavic and exhibits none
of the quixotic behavior which $\underline{\overset{\text{y}}{e}}$ exhibits. Therefore, it seems
reasonable to assume that the slot in the vowel system which

previously contained [o:] was now empty (note that we are just considering the long vowels here, since the u-diphthongs mono-phthongized to long vowels):

(12)   i: u:

                        ɏ   ☐

                                ɔ:

Therefore, it would not be at all surprising if the u-diphthongs merged first to [o:], given the apparently empty slot. Let us see what evidence can be brought to bear on this issue.

At several places in Shevelov 1965 there is information which seems to provide convincing evidence that the u-diphthongs merged to [o:] upon monophthongization. First of all, Shevelov notes that in Common Slavic loanwords borrowed from Germanic, $[u:_1]$ renders Germanic [u:], while $[u:_2]$ renders Germanic [o:]. Some examples follow:

(13) Czech <u>tyn</u> 'hedge'   ~   Anglo-Saxon <u>tūn</u> 'hedge'
     Old Church Slavic <u>xyzʑ</u>'chamber'   ~   <u>OHG hūs</u> 'house'

(14) Czech <u>buk</u> 'beech'   ~   Gothic <u>bōka</u>
     Russian <u>Dunaj</u> 'Danube'   ~   Gothic *<u>Dōnawi</u> (Shevelov p. 276)

The correspondences in (13) illustrate the use of $[u:_1]$ to render Germanic [u:] (recall that $[u:_1]$ later became [y:]), and those in (14) illustrate the use of $[u:_2]$ to render Germanic [o:]. Such evidence clearly indicates that the u-diphthongs must have origi-nally monophthongized to a vowel something like [o:]. At any rate, this conclusion seems to be a more reasonable and tran-sparent way of accounting for the facts than to assume that there were two distinct vowels $[u:_1]$ and $[u:_2]$ coexisting at the same time. Since we know that later $[u:_2]$ (=[ō:]) came to occupy the slot vacated by $[u:_1]$ when the latter became [y:], the Slavic spellings above are understandable.

Another bit of evidence that what Shevelov calls $[u:_2]$ was probably [o:] is connected to the progressive palatalization of the velars in Common Slavic.[6] Briefly, this process acted to pala-talize velar consonants in the environment <u>iCV</u> (where C is [+velar]), and it was dependent upon the quality of the vowel fol-lowing the velar. Palatalization did <u>not</u> occur in this environ-ment when the following vowel was $[u:_1]$, but it <u>did</u> occur before the more open back vowels $[_ɔa]$ and $[_ɔa:]$. (Steensland 1975/74 claims palatalization occurred before $[ă(:)]$ and $[o(:)]$, which indicates that he is not assuming the intermediate stage recon-structed by Shevelov.) Interestingly enough, palatalization also occurred before $[u:_2]$ (Shevelov 1965:344). If we assume that $[u:_2]$ was [o:], then the fact that progressive palatalization

occurred before this vowel is captured in a completely natural
way, given that it also occurred before the other non-high back
vowels, but not before [u:$_1$]. But, if we maintain that [u:$_2$] was
more u-like than o-like (i.e. that it is [+high]), then the state-
ment concerning the environment of the change must be complicated.
We would have to say something like: Progressive palatalization
occurred before non-high back vowels <u>and</u> before [u:$_2$]. Since
Shevelov's conception of the phonetic value of [u:$_2$] is essen-
tially vacuous, there is no reason not to assume that [u:$_2$] is
[o:].

The above two arguments are the most persuasive that I can
find, and I think they convincingly establish that the vowel
resulting from the monophthongization of the u-diphthongs was
[o:].[7]

While the above discussion makes intuitively plausible the
idea that the two u-diphthongs, upon monophthongization, moved to
occupy the [o:] slot vacated by the merger of [o:] and [a:], the
situation is not so clear with respect to the monophthongization
of the i-diphthongs. It is reasonable to assume that the i-
diphthongs would monophthongize to an unfilled slot, if possible.
But, since there existed no empty mid vowel slot [e:] into which
the i-diphthongs could move, it was necessary for the resulting
monophthongs to merge with already existing front vowels. Recall
that the front vowel space was already effectively 'filled-up' due
to the fact that ě apparently did not fully lower in response to
the merger of [o(:)] and [a(:)] in the back vowels. As we have
previously seen, [e(:)] could appear in a variety of phonetic
guises between [e(:)] and [a(:)]. It is this fact which I claim
kept the i-diphthongs from monophthongizing to [e:] in a process
similar to the monophthongization of the u-diphthongs to [o:].

Let us now examine in more detail how the monophthongization
of the i-diphthongs took place. Assuming the representations of
the diphthongs as given in (9) above (repeated below as (15)),

(15)   ei   eu

              $_o$ai (=ɔi) $_o$au (=ɔu)

we have the following changes:

(16)   ei > i: eu ⟍
                                        ⟍
                                          ⟍ o:
              ɔi > ě   ɔu ⟋

I will consider the shifts given in (16) in more detail later in
terms of particle theory, but some preliminary thoughts as to the
reasons for the changes would be in order at this point. One rea-
son given in the standard sources for the monophthongization of
the diphthongs in Common Slavic is the fact that there was a ten-
dency in the language towards creating open syllables. By

monophthongizing the diphthongs, syllables were rid of final non-vocalic off-glides. The trend towards open syllables in Common Slavic is not only reflected in the monophthongization of the diphthongs, but in other processes as well. Among these other processes was the elimination of vowel plus nasal sequences: the vowels absorbed the nasality of the following consonants, which were then lost.

Another possible explanation as to why the diphthongs mono-phthongized is that since the diphthongs could not optimize to the maximal diphthongs [ai] and [au] because [a] had merged with [o] to form [ɔ], they optimized by becoming monophthongs. Of course, this presupposes that the diphthongs which existed in Common Slavic were in some sense 'unstable' or non-optimal. But, it is not entirely clear that all of the Common Slavic diphthongs were necessarily non-optimal. After all, [ɔi], while not completely optimal in the sense of Donegan (1978), is nevertheless the most optimal of all the diphthongs in the Common Slavic vowel system, since each element of the diphthong is maximally distant from the other (recalling that [a] does not exist in Common Slavic at this time). It does seem clear, however, that the other diphthongs in Common Slavic were unstable (or non-optimal) to a greater or lesser extent. In the case of [ei], for example, a natural change in Donegan's (1978) sense would be for both elements to assimilate to each other and become more alike, yielding either [e:] or [i:]. Or, in a system containing the vowel [a], another possibe change would be for the elements to maximally dissimilate, yielding [ai]. Since the latter option is impossible in Common Slavic, we would predict that either of the other possible outcomes, [e:] or [i:]. would be the result in this case. As we saw in (15) above, [i:] turns out to be the resulting monophthong. In the following dis-cussion, as well in section 3 (where I will consider the mono-phthongizations in particle terms), I will suggest a possible rea-son that [i:] resulted instead of [e:].

Upon examining the diphthongs in Common Slavic we notice that both [ei] and [ɔu] contain elements which agree in the feature [back]. In [ei], both elements are [-back], while in [ɔu] both are [+back]. In the case of [eu], the first element is [-back] and the second is [+back], and in the case of [ɔi] the first element is [+back] and the second is [-back]. Earlier we mentioned that Common Slavic had a progressive palatalization of velars, but there were also two distinct earlier regressive palatalizations of velars, in which a velar became palatalized when followed by [i:], ě̯, or [j]. The history of the Slavic languages is therefore replete with palatalization (or fronting) processes. According to Shevelov, the second regressive palatalization of velars took place at approximately the same time as the monophthongization of the diphthongs. It is documented that the ě̯ resulting from the monophthongization of [ɔi] fed this palatalization. But note that for [ɔi] to feed the second palatalization, its first element had to be fronted, since palatalization only occurred before front vowels. Let us suppose that a palatalization (or fronting)

process analogous to the second regressive palatalization occurred within the diphthong itself, palatalizing (or umlauting?) [ɔ] to [ɔ̈] before [i], with [ɔ̈] ultimately becoming [æ]. This would give us the diphthong [æ i], which would be capable of feeding the second palatalization because its initial element is now [-back].

Extending the idea of applying a process similar to that of the second regressive palatalization to the diphthong [ei], we can account for the fact that [ei] became [i:] in the same way that we accounted for the development of [ɔi] above. The initial element [e] is raised due to the palatalizing influence of the following [i] off-glide. How, then, do we account for the fact that [æi] ultimately became ě̌? Shevelov says that the original [ɔ i] metathesized to something like [iɔ] (=[i₀a]), which then somehow became [i_e a] and then finally ě̌. He does not seem to account for how we get from [i₀a] to [ī_e a]. Evidently, something akin to palatalization is going on, but notice that after metathesis we no longer have an environment for regressive palatalization, since the [i] off-glide is now sequentially initial. We would have the proper environment for progressive palatalization, but, according to Shevelov's chronology, the progressive palatalization does not take place until well after the second regressive palatalization (fed by [æi]) and after the monophthongization of the diphthongs. So, it would simplify matters if we could extend the second regressive palatalization process to account for the fronting of [ɔ] in [ɔ i] before the sequence is metathesized to put the [i] off-glide sequence initial. Once [æi] is metathesized, we get [iæ], which is a plausible phonetic realization of ě̌.

Therefore, I differ with Shevelov's claim that [ɔ i] was metathesized to [iɔ] and only then somehow became [iæ]. If we were to follow Shevelov's analysis, we would have to adhocly appeal to a type of progressive palatalization occurring simultaneously with the second regressive palatalization solely for the purpose of getting [iæ] from [iɔ]. But, Shevelov himself makes clear that the progressive palatalization occurred much later than the monophthongization of the diphthongs, so it could not have been involved. To be as economical as possible, I feel that it makes more sense to allow the second regressive palatalization to do the work of fronting [ɔ] to [æ] in [ɔ i] before metathesis occurs, since it is documented that the second regressive palatalization is operating at this time, anyway.

Thus, assuming that the above discussion is generally plausible, we can account for the development of the i-diphthongs by saying that each underwent a type of regressive palatalization (or umlauting), in which the initial element was fronted (in the case of [ɔi]) or raised (in the case of [ei]) before the palatal off-glide [i]. Later, I will examine these processes in terms of particle phonology, which affords an explicit formulation of the phenomenon involved.

So much for the i-diphthongs. As far as the u-diphthongs are concerned, it would not appear that any type of palatalization was involved in their development, since they end in [u] off-glides.

I will put off a detailed consideration of their development to
[o:] until the third part of the paper, where I will examine the
interactions of the monophthongization of the u-diphthongs with
that of the i-diphthongs. Note that we must somehow account for
how [eu], which contains both a [-back] and a [+back] element,
loses its frontness in becoming [o:]. Particle notation seems to
offer an elegant way of accounting for the developments.

2.4. Developments Subsequent to the Monophthongization of the Diphthongs

Following the monophthongization of the diphthongs we have
the following system of long and short vowels in Common Slavic:

(17)   i: u:$_1$ i   u

                       o:

         y̨   y̨
         e̥   e̥
             ɔ: ɔ

The next observable change is that [u:$_1$] becomes [y:] (=[ɨ]),[8]
followed closely by the shift of [ɔ:] to [a:] (and [ɔ] to [o]).
Shevelov refers to these shifts collectively as the unconditional
'second delabialization of rounded vowels' (Shevelov
1965:376ff.).[9] He therefore claims that [u:$_1$] became [y:] due to
this principle, whereas I will claim that a more plausible way of
looking at this shift is to claim that [u:$_1$] shifted as result of
being pushed out of the way by [o:]. (Possible motivation for the
shift of [o:] to [u:] will be discussed in part 3 of the paper.).
The following diagram illustrates this shift:

(18)   y: u:$_1$

                        o:
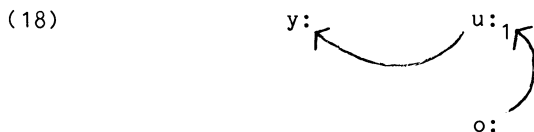
Such 'push chains' of [o:] to [u:] are well established in the
histories of many languages (cf. the Great Vowel Shift in English
(Schane 1982, Lass 1976)). As far as I can tell, few Slavic scho-
lars have proposed a chain to account for the shifts documented
above.[10] Arumaa (1964) claims that [u:$_1$] became [y:] before the
monophthongization of the u-diphthongs to [u:$_2$] (=[o:]). By making
this assertion, which he does not seem to justify, he is unable to
explain what caused such an apparently spontaneous shift. After
all, by anyone's account, [y:] is more marked than [u:], and it is
highly unlikely that such a shift would occur unless motivated in
some way. Shevelov, while documenting that the monophthongization
of the u-diphthongs to [u:$_2$] occurred before [u:$_1$] became [y:],
still does not clearly account for why the shift occurred. He does

not make clear why [u:₂] (whatever its phonetic value) should usurp the position of [u:₁]. Indeed, Shevelov says that [u:₁] and [u:₂] exchanged places and that [u:₁] changed due to the 'purely phonetic character of [u:₁] as compared to [u:₂]' (Shevelov 1965:382). Also, he states that 'because of its back articulation [u:₁] could more easily lose its rounding and still preserve its identity. This was the phonetic condition for the change [u:₁] > [y:]' (Shevelov 1965:382).

It is hard to evaluate what Shevelov means by these statements. They seem confused and vague. I claim that the push chain described above accounts for the data in a way that is motivated and that has been attested in other language families, such as Germanic (cf. Lass' treatment of push chains in Swedish in Lass 1976:78ff.). Interestingly enough, the push chain Lass (1976) describes in the back vowels of Swedish looks remarkably like the shift I am proposing for Common Slavic, except that in Swedish [u:] becomes what Lass writes as [E̤:] (a nonce symbol), since there already existed a [y:] in the Swedish vowel system.

It would be nice if the push chain which we have just described in Common Slavic could somehow be shown to have caused (even indirectly) the subsequent shift of [ɔ:] to [a:]. Other than to speculate that the change of [ɔ:] to [a:] may be due in some way to the tendency for a more marked vowel to become less marked (assuming that [ɔ:] is more marked than [a:]), I will leave that matter at that.

To complete our survey of the development of the Common Slavic vowel system up to the end of the period of Common Slavic unity, we have the following system subsequent to the push chain of [o:] to [u:] and [u:₁] to [y:], and the shifts of [ɔ:] to [a:] and [ɔ] to [o]:

(19)   i: y: u: i   u

          ě̥   ě̥   o

               a:

Sometime after the above shifts, short ě̥ became simply [e]. Note the beginning of the breakdown of length oppositions. This tendency was furthered later by the loss of the jers, [i] and [u], under circumstances beyond the scope of this paper. At this point, setting apart the complex vowel ě̥, we get the coalesence of the remnants of the long and short vowel subsystems, with subsequent loss of all phonemic length distinctions:

(20)   i   y   u

          ě̥   e   o

                    a   (Shevelov p. 461)

Of course, the status of *ě* remains problematic to the present day in the modern Slavic languages.

3. A Particle Analysis of the Vowel Shifts in Common Slavic

In this section I will reexamine several of the vowel shifts discussed in the first part of the paper in terms of particle phonology, a framework being developed by Sanford Schane (1982 and class lectures). Briefly, in particle phonology, vowels are represented abstractly as unordered sequences of primitive 'particles', which represent vowel quality. There are three primitive particles: 'i' represents palatality, 'u' labiality, and 'a' aperture (or height). 'i' and 'u' are tonality particles, and 'a' is an aperture particle.[11] Different combinations of the primitive particles represent different vowels. For example, the long and short vowel systems of PIE would be represented as follows:

(21)   i i   u u   i   u

          ai i   au u   ai   au

                a a   a

The representation of the vowel [e] as 'ai' indicates that this vowel has both palatality and aperture. In general, lower vowels contain more aperture particles than higher vowels, with the highest vowels [i] and [u] containing no aperture particles at all. In the subsystem of long vowels, the single particles appearing separated and to the right of the others denote length. This illustrates the multi- faceted nature of particles in the theory. They behave like cover terms in that 'i', for example, can represent both palatality and length, depending upon the context. The particle 'i' can also represent tenseness in vowel systems where tense/lax distinctions exist. The particle 'a' can represent either lowered height or laxness, depending upon the system (see Schane 1982 for examples from the Great Vowel Shift in English which motivate the multi-faceted nature of particles). Vowels containing 'i' or 'u' are tonality, or chromatic, vowels, while vowels containing only 'a' are achromatic (see Donegan 1978 for further discussion of chromatic and achromatic vowels). The framework also assumes that it is possible for vowels to have no particles at all in their representations. The vowel [ɨ:], for example (which we are writing as [y:]) contains no particles, since the vowel has neither the tonality of [i] nor the labialty of [u]. We will represent it notationally as 'Ø'. We see, then, that in particle phonology the particles represent both segments and features. No claim is made, however, as to the particles having actual phonetic reality. The representations are merely a model for looking at how vowel systems behave.

The analysis I will present presupposes several theory-based

assumptions: Vowel systems, such as the PIE system in (21) above, will be thought of as being made up of vowels with particle representations. In other words, the units which will be examined as undergoing various changes are the <u>particles</u> existing in the system. Symbols like [o] and [e], etc., will only be used as convenient mnemonic devices to refer to the unordered sequences of abstract particles which are recognized as distinct sound units in the language. In the course of historical change, particle representations may break apart (fission) and rejoin to form new combinations (fusion). Certain sequences of particles are assumed to be particularly unstable and susceptible to fission, such as those containing both tonality particles 'i' and 'u' (this assumption is based mainly on the empirical notion that vowels such as [ʉ] ('iu') and [ʊ̆] ('aiu') are, in some sense, more complicated than vowels such as [i] and [e]). It will furthermore be assumed that particles resulting from the fission process may influence other particle representations to a greater or lesser extent. In addition, the framework proposes explicit notational formulations for assimilation (cloning).

Finally, in a more general sense, the model assumes that, regardless of the number of particles we start out with, we should view vowel systems as closed systems, in which no net gain of particles will be allowed unless the gain is sanctioned in some way (cf. the discussion to come with regard to cloning processes in Common Slavic).

Turning now to the Common Slavic data, I will reexamine some of the vowel shifts in the particle framework. As will be seen, the framework enables us to account in an interesting and explicit way for many of the processes affecting vowels in Common Slavic.

<u>3.1.</u> The <u>Monophthongization</u> <u>of</u> <u>the</u> <u>Diphthongs</u> <u>in</u> <u>Common</u> <u>Slavic</u>
In this section I will again consider the monophthongization of the diphthongs in Common Slavic, but this time in terms of particle notation. Recall the previously discussed changes:

(22)   ei > i: eu

         ɔi > ě   ɔu   > o:

How would particle phonology handle these changes? Recall that the general consensus is that all of the monophthongizations probably occurred at the same time. Keeping in mind the earlier discussion concerning the prevalence of palatalization processes in Common Slavic at this time, I would like to suggest that a reasonable starting point for the monophthongizations would be in the i-diphthongs, with the first element palatalizing (or umlauting) under the influence of the following [i] off-glide.[12] In particle notation we have the following:

(23)   aauį   >   aauįi ⌢⟋↗⁻ᵘ   aaiį
         [ɔi]   [ɔ̈i]   [æi]
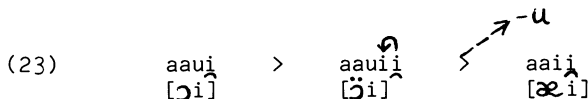
The process in (23) suggests a hypothetical intermediate stage in
the development of e̬ from [ɔi]. The hypothetical stage [ɔ̈i] is
unstable due to the presence of the particles 'i' and 'u' in the
same mora, and thus immediately results in [æi]. The process by
which a copy of the 'i' off-glide particle is copied into the par-
ticle representation of the first mora of the diphthong is called
cloning in particle phonology. Actually, cloning is an explicit
way of looking at assimilation involving vowels. The copying (or
cloning) of the 'i' particle represents the assimilative character
of the change. Note, also, that after cloning the resulting vowel
[ɔ̈i] is so unstable that we presumably have a simultaneous decay
of the old tonality particle 'u', thereby giving [æi], which,
following metathesis, became [iæ], a plausible representation of
e̬ (cf. section 2.3 above).¹³
     How would particle notation account for the change from [ei]
to [i:]? It appears to be a change of the same type that occurred
from [ɔi] to [æi], except that here we have assimilation to
height:

(24)   aiį ⟋↗⁻ᵃ   i i
              [ei] ⌢   [i:]
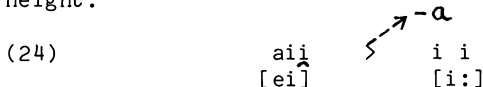
Here, assimilation of the palatal particle 'i' involves discarding
an aperture particle. In doing this we achieve maximum tonality,
in that the aperture particle, whose presence had diluted tonal-
ity, is no longer present. Such a process has been dubbed droning
by Schane (class lectures). Once again, we note that a non-
optimal diphthong has become optimal in some sense--since it could
not become the optimal diphthong [ai], it was forced to mono-
phthongize to [i:].
     We have seen, therefore, that the changes in the i-diphthongs
can be accounted for by appealing to the notion that assimilation
is involved in each case to shift the initial element up one step
towards greater palatality.
     Now that we have looked at the monophthongization of the i-
diphthongs in particle terms, let us examine the monophthongiza-
tion of the u-diphthongs. Recall that both u-diphthongs become
[o:]. Suppose the following occurs in the change from [eu] to
[o:]:

(25)   aiṷ >   aiuṷ ⟋↗⁻ⁱ   auṷ >   au u
         [eu]   [öu]   [ou]   [o:]
           ⁺ᵘ

Once again I propose that a hypothetical stage is involved, [öu],
and also [ou]. As was previously the case with [ɔ̈i], it is pro-
posed that [öu] is very unstable and decays immediately to the

intermediate diphthong [ou], with concomitant loss of 'i'.[14] Where
does the added 'u' particle come from in (25)? Since we have been
claiming that a palatalization process (or umlaut), triggered by a
following 'i', caused the changes observed in the i-diphthongs, we
might want to extend the analysis to claim that something similar
happened in the u-diphthongs. That is, we might want to say that
the observed changes in the Common Slavic diphthongs can be
ascribed to a process of assimilation to tonality (which would
include assimilation to both 'i' and 'u').

But recall that in the change of [ɔi] to [æi] (illustrated
in (23) above), an 'u' particle was cast off when the unstable
diphthong [ɔ̃i] decayed. One might suppose that this stray particle
was picked up by [eu], resulting in [ö̈u], which then decayed to
[ou]. There is something intuitively nice about this second way of
looking at things, in the sense that the stray 'u' continued to
exert influence within the subsystem of diphthongs.

Whichever account is the correct one, we nevertheless get
[ou] following the decay of [ö̈u]. How might we account for the
conversion of [ou] to [o:] in particle terms?

The diphthong [ou] is represented as 'auu' in particle nota-
tion, and [o:] can be represented as 'au u'. Notice that the only
difference is that in [ou] the final element is non-syllabic,
while in [o:] it is not. The number of particles is the same in
either case. In particle phonology we can account for the change
of [ou] to [o:] by the process of fusion. That is, the sequential
realization of the particles 'auu' has changed to a simultaneous
(fused) realization, 'au u'. But, why should [ou] become [o:] and
not [u:]? I claim that [ou], like [ei], is a non-optimal
diphthong. To become an optimal (maximally polarized) diphthong
([au]) is not possible because the vowel [a] does not exist in the
system. Therefore, analogous to what happened to [ei], [ou] mono-
phthongizes, but since the [o:] slot is empty, it need not mono-
phthongize to [u:]. Besides, one would expect that the diphthong
would monophthongize to an empty slot, if possible, in order to
avoid merging with an already existing vowel. In addition, if we
assume that the catalyst in the change of [eu] to [o:] was not
assimilation to 'u', but rather the capture of the stray 'u' par-
ticle which resulted from the earlier change of [ɔi] to [æi] (as
suggested previously), then we could assume that the tendency of
vowels to assimilate to 'u' (if any) in Common Slavic was not as
strong as the tendency of vowels to assimilate to 'i'. Thus, we
could conclude that the lack of motivation of assimilation to 'u',
together with the open slot [o:], conspired to force [eu] to
become [o:].

What about the case of [ɔu] becoming [o:]? A plausible
intermediate step would be the following:

(26) aauu̯ <^ɔ̃⁻ᵃ auu̯ > au u
      [ɔu]   [ou]   [o:]

The step from [ɔ] to [o] is plausible because later in Common
Slavic we actually have the shift of [ɔ] to [o] in the short vowel
system (see section 3.2 below). Therefore, once we get to [ou],
we can apply the reasoning discussed previously in order to
motivate the change of [ou] to [o:].[15]
    At this point let us note that in each subsystem of
diphthongs (i-diphthongs and u-diphthongs) we get a net loss of
one aperture particle per subsystem when the diphthongs mono-
phthongize: one 'a' particle each is lost in the shift of [ei] to
[i:] (see (24) above), and in the shift of [ɔu] to [o:] (see (26)
above). It is interesting to speculate whether there might be
some sort of principle in the system which constrains the changes
in such a way as to allow for the loss of no more than one aper-
ture particle per subsystem. The upshot of such a principle (call
it the Maximum Loss of Aperture Principle for the Common Slavic
diphthongs) seems to be that the tendency in the system towards
simplification (represented via net loss of particles) was not
unbounded or arbitrary with respect to the number of particles
lost, but rather that the changes in the subsystems of the
diphthongs were intimately interrelated, with the changes in each
constraining the other. This interrelationship seems to be
reflected in the balance in the number of particles lost.
    Another interesting fact to note is that, in vowel systems
where tense/lax distinctions are operating, the representation
'au u' can be interpreted either as the long lax counterpart of
[u:], or as a long vowel of the next lower height. That is, 'au u'
can stand for either [ʊ:] or [o:]. Recall that Shevelov, as well
as others, claimed that the u-diphthongs merged to [u:₂], which is
vaguely described as an u-type vowel. It seems possible that the
[o:] which was rendered as [u:₂] in Common Slavic loanwords from
other languages may have been perceived by the speakers of the
time as [ʊ:], which could account for why borrowed [o:] was writ-
ten as u. Does this hint at a beginning tense/lax distinction in
Common Slavic? The data are unclear. As we have seen, it is best
to think of [u:₂] systemically as [o:]. But it is interesting
that the particle framework allows us to capture the ambiguity of
the situation by representing [o:] (which could have been heard as
[ʊ:]) in the particle notation 'au u'. Undoubtedly, more could be
done with this idea.

3.2. Developments Subsequent to the Monophthongization
      of the Diphthongs
    Recall from section 2.4 above that, following the mono-
phthongization of the diphthongs, we had the following system of
vowels:

(27)   i: u:₁   i   u
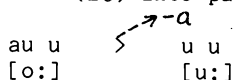

                      o:

        ɛ̯   ɛ̯

                      ɔ: ɔ

Next we suggested the push chain schematized in (18) above, repeated here:

(28)

$$y: \quad\qquad u:_1$$
$$o:$$

Translating the changes in (28) into particle notation we have:

(29)   au u $\overset{-\pi-a}{<}$ u u
         [o:]   [u:]

(30)   u u $\overset{-\pi-2u}{<}$ $\emptyset$
   [u:]   [y:]

Can we account for what motivates the shift of [o:] to [u:]? Donegan (1978) maintains that the more tonality a vowel has, the more likely it is to gain even more tonality and to lose aperture (the 'rich get richer principle'). This appears to be what is happening to [o:] in Common Slavic, as well as in other languages which have vowel shifts of this same general type (cf. the Great Vowel Shift in English). By shifting upwards, the tonality of [o:] becomes less diluted, since the aperture particle 'a' is lost. Given this, the seemingly marked process of [u:$_1$] becoming [y:] is accounted for due to the fact that [u:$_1$] was pushed out of the way when [o:] became [u:]. In other words, the shift of [u:$_1$] to [y:] was motivated in that the shift of [o:] to [u:] occurred. Generally, we would like to say that an unmotivated change can not occur unless some other force in the system causes it to occur. Thus, in the above push chain, a natural change (in the sense of Donegan 1978), [o:] to [u:], causes as a by-product the relatively marked change of [u:$_1$] to [y:]. In another sense, however, both of the shifts above illustrate natural changes to the extent that they result in a net loss of particles.

The two final shifts that I will discuss also illustrate changes from marked to unmarked, which in particle phonology is represented by reducing the number of particles in a vowel representation. The shifts are [ɔ:] to [a:] and [ɔ] to [o]:

(31)   aau u $\overset{-\pi-2u}{<}$ a a
         [ɔ:]   [a:]

(32)   aau $\overset{-\pi-a}{<}$ au
         [ɔ]   [o]
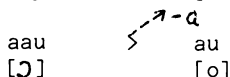
In each case we see that there is a net loss of particles. In general, as was noted earlier, we would like to claim that the

total number of particles in the system should not increase without motivation. Loss of particles can be analyzed as simplification, but net gain of particles should be accounted for in some way (such as through assimilation (cloning)).

Therefore, in the developments subsequent to the monophthongization of the diphthongs, we have had a net loss of two aperture particles, four labiality particles, and no gains. During the time of the monophthongization of the diphthongs, there was also a net loss of particles. Any particles which were added at this time were accounted for by cloning.

4. Conclusions

I have examined the vowel shifts in Common Slavic with the purpose of trying to account in a somewhat principled way for why the changes might have occurred as they did. I have explored several historical arguments concerning the monophthongization of the Common Slavic diphthongs, and have argued against Shevelov's proposal of [u:$_2$] in favor of [o:] as the vowel to which the u-diphthongs monophthongized, and I have also shown how this analysis better accounts for the resulting shift (push chain) of [u:$_1$] to [y:]. In addition, I have proposed reasons as to why the i-diphthongs monophthongized to the vowels that they did.

An important result of the present analysis is that it shows that the shifts in the back vowels in Common Slavic, although superficially quite different from those which are attested in the histories of other languages (cf. the Great Vowel Shift in English, the Old High German Vowel Shift, etc.), were actually remarkably similar to these other shifts, if they are analyzed as in the present study. Indeed, it would appear that all of the above mentioned examples (including Common Slavic) are governed by the same generalization, namely that all involve the raising of [o:] to [u:], with the details of what vowel the original [u:] shifts to depending upon the individual language.[16]

Particle phonology and the conceptions of naturalness suggested by Donegan 1978 offer an insightful way of looking at the shifts. It was proposed that certain shifts in the diphthongs could be analyzed in terms of cloning, as well as by taking into account what constitutes an optimal diphthong. The reasons as to why the non-optimal diphthongs monophthongized in Common Slavic were discussed in the framework. Particle phonology also makes available certain concepts such as fission and fusion which nicely account for several of the observed changes.

Finally, another thought about jat'. It was argued that jat' owes its behavior (i.e. its tendency to lower) in large part to the fact that, in the back vowels, [a(:)] and [o(:)] merged to [ɔ(:)], thereby creating an unbalanced system. Jat' attempted to balance the system, therefore, by lowering. It appears that one can take the behavior of jat' as indirect evidence for the historical reality of [ɔ(:)], which (as was noted earlier) is not generally assumed to have existed by most sources (except for

Shevelov, who wrote the vowel as [$_o$a(:)]). What better explana-
tion is there for the tendency of PIE [e(:)] to lower than its
attempt to balance [ɔ(:)] in the back vowels? If we do not assume
[ɔ (:)] as an intermediate stage, then how can we account for the
behavior of jat'? It therefore seems that Shevelov is right in
setting up [ɔ (:)] as an intermediate stage, especially when we
consider the behavior of the system as a whole.   Even more
interesting is that the intermediate stage [ɔ(:)], rather than
being an unremarkable intermediate step in the history of the Com-
mon Slavic vowels (recalling that, ultimately, [ɔ:] > [a:] and [ɔ]
> [o]), actually had a lasting effect on the system, the evidence
for this being the behavior of jat'.

5. A Consideration of a Broader Question
      At this point I would like to consider briefly a broader
question raised by the above analysis of Common Slavic vowels in
the particle framework. It is apparent that the analysis given in
this paper makes the assumption that systemic vowel change can in
some sense be looked at as analogous to syntagmatic (sequential)
vowel change. But, it is not immediately clear that there is good
reason to approach these two types of change in the same way. For
example, it is not at all uncommon in languages for the two vowels
[a] and [i], occurring in sequence, to merge to form [e] (cf. San-
skrit sandhi phenomena as one example). Much the same can be said
for the merger of the vowels [a] and [o] to [ɔ], and for the
merger of [a] and [u] to form [o]. In the case of the merger of
PIE [a] and [o] to form [ɔ] in Common Slavic, particle theory
would say that 'a' and 'au' fused, yielding the new vowel 'aau'.
In this case, one represents the systemic merger of [o] and [a] to
[ɔ] in much the same way as one might represent the change if the
two vowels were in a sequential timing relationship. So, what I
am in effect assuming, and what the particle framework seems to
claim, is that we can consider diachronic systemic change along
the same lines as syntagmatic change. In the particular case I
just considered, the assumption that these two types of change can
be productively looked at in much the same way notationally seems
to lead to no problems, as long as we keep in mind that we are
really talking about two basically different types of change: sys-
temic change, in which two vowels/phonemes which are configura-
tionally contiguous in the system (but not necessarily contiguous
sequentially) merge to yield a new phoneme in the system; and syn-
tagmatic change, in which two vowels which occur as sequentially
contiguous blend or merge with each other (as in the case of the
Sanskrit sandhi mentioned above). Again, there does not seem to
be any a priori reason to look at these two types of change in the
same way, even though, at this point, I can see no objection to
doing so. The results are the same in one respect in that a new
vowel results in each case, but the two cases differ in scale: in
the systemic change, we are talking about a global, across-the-
board phonemic change, while in the syntagmatic change we are not

necessarily talking about the merger of two phonemes, but rather about a local change involving the juxtaposition of two vowels in sequence. Such a local merger may or may not play a part in affecting the vowel system as a whole.

In a sense, one might want to claim that the ability of particle theory to capture both types of change with the same notation is a potential strength of the framework. Clearly, though, more work needs to be done on this point in the future to work out exactly what the relationship between the two types of change, and the potential consequences for particle theory, will be.

Footnotes

[*] I am indebted to the suggestions and criticisms of Sanford Schane, Ronald Langacker, Margaret Langdon, and Matthew Chen, all of whom patiently read and commented on earlier versions of this paper. Of course, all errors and omissions are exclusively my own.

[1] The Slavic languages include twelve present-day languages: Russian, Belorussian, Ukrainian, Polish, Lower Sorbian, Upper Sorbian, Slovak, Czech, Slovenian, Serbo-Croatian, Macedonian, and Bulgarian.

[2] Vaillant 1950 and Jakobson 1962 are also classic sources which are often cited in the literature, but my limited knowledge of French precluded me from making detailed use of these works.

[3] Evidence for the intermediate stages is also noted by Arumaa 1964:77. The reader is directed to Shevelov 1965 and Arumaa 1964 for the details. It would be beyond the scope of this paper to go into the justification of Shevelov's reconstructions here.

[4] The vowels of the Common Slavic period were also characterized by having certain pitch or tone distinctions which I have ignored. These distinctions do not seem to have played any part in the development of the vowels and diphthongs.

[5] In this paper we will not be specifically concerned with the short reflex of PIE *[e]. I will use the symbol $\check{e}$ to refer to the (long) reflex of PIE *[e:], with the understanding that both long and short jat' share the behavior described in section 2.2.

[6] As noted later in the text, Steensland 1975/74 notes this connection explicitly. The evidence can be found in Shevelov 1965, but not with regard to showing how this evidence can be used to argue that [u:$_2$] was [o:]. Shevelov rejects this claim.

[7] This is essentially Steensland's claim, but while he claims that [u:$_2$] was a fairly open vowel, he never makes the claim that [u:$_2$] was [o:].

[8] Press 1977 discusses in detail the development of the vowel [y:] in the Slavic languages. Note that I will use the traditional Slavic symbol for the vowel [ɨ], which is [y].

[9] It should be noted that Shevelov cites the fact that the second delabialization of rounded vowels did not affect [u:$_2$]. Note, also, that the first delabialization was conditioned only,

and it [10] will be ignored in this paper.

[10] Steensland 1975/74 does note that something like a push chain is going on.

[11] In this paper I will denote particle representations in the body of the text within single quotes.

[12] The cloning solution described in this section was suggested to me by Sanford Schane. It should be noted that the half-moon symbol under a particle denotes that the particle is non-vocalic.

[13] It is possible that the loss of the old tonality particle (as opposed to the new one) in situations like these may be a universal.

[14] Nearly all of the sources note that there are some reflexes of [eu] of the form [i̯u] along side of [u:]. We can account for such cases by supposing that the 'i' particle was not lost in these cases, but that it was retained, giving the representation 'i̯auu' (=[jou]), and presumably then 'i̯au u' (=[jo:]), which then later participated in the push chain, becoming finally [ju:].

[15] The derivations in (24), (25), and (26) above deserve a few extra comments. In (25) and (26), the final step from [ou] to [o:] involves the reinterpretation of the particle representing the off-glide [u] as a length marker. As a result of this reinterpretation, we see that the 'u' particle which had previously represented a glide now represents length and is set off from the other particles by a space. In the case of (24), there is another step in the derivation which is implied but not explicitly shown. This step, too, involves the reinterpretation of the off-glide as a length marker:

$$(24') \qquad \text{ai}\underset{\sim}{\text{i}} \quad \overset{-a}{\underset{\text{7}}{\gtrless}} \quad \text{ii}\underset{\wedge}{} \quad > \quad \text{i i}$$
$$\qquad \qquad \text{[ei]} \qquad \text{[i}\underset{\wedge}{\text{i}}\text{]} \qquad \text{[i:]}$$

As can be seen, the extra step assumes the unstable diphthong [ii̯] as an immediate result of the raising of the first element [e]. Since [ii̯] is so unstable, it immediately results in the stable monophthong [i:] via the reanalysis of the glide as a length marker.

[16] See Lass 1976 for a detailed development of this idea, in which he proposes the notion of a metarule as an abstract schema which can be defined on a grammar for the purpose of expressing a cross-linguistic generalization about changes of this sort.

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PROCESSING OF LINGUISTIC STRESS
IN THE DAMAGED LEFT AND RIGHT HEMISPHERE

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The purpose of this study is to investigate the effect of variations in prosody on the auditory comprehension of English sentences by brain-damaged adults. The area of prosody has a fairly extensive literature in terms of its place in language production. This literature consists of a variety of research studies and clinical and other observations of both children and adults, and from a number of different languages. From such studies we gain the impression that prosodic features are early acquired by children and relatively resistant to loss in language-impaired adults, including people with fairly severe aphasia of one type or another.

There is a considerably smaller body of literature on the place of prosody in language comprehension, particularly in aphasia. I know of only six studies in which the ability of aphasic subjects to comprehend prosodic information has been investigated --one of phonemic pitch accent in Japanese (Sasanuma et al. 1976), one of correct vs. incorrect placement of syllabic stress in Rumanian (Mihailescu et al. 1970), and four in English, including Blumstein and Goodglass' 1972 study of the perception of syllabic stress as a means of making syntactic distinctions between noun-verb pairs like tránsport and transpórt, and semantic-syntactic distinctions between noun-noun phrase pairs like yèllowjacket and yellow jácket. More recently Baum and her colleagues (1982) found that compared to normals, Broca's aphasics were unable to make distinctions between phonemically similar sentences on the basis of either sentential stress or juncture, or to profit from increased stress on functors in sentences.

One impression that emerges from this body of research, as well as from a study preliminary to the current study (Solomon and Aronson 1976), is that however robust prosody may be in terms of resistance to loss, it is a delicate feature to investigate experimentally. When subjects, either normal or brain-damaged, sense that suprasegmental features are being manipulated, they tend to ignore them and rely on the segmental information alone.

The prosodic feature investigated in the current study is English contrastive stress. What contrastive stress is has been a vexed question (Schmerling 1976); it is used here to mean those variations in pitch, loudness and length that have the effect of highlighting or emphasizing differences in meaning between phrases or sentences. Such variations are related to one of the apparent benefits (and perhaps also one of the purposes) of the use of contrastive stress, that of focussing the listener's attention on particular words. For example, in a series of commands such as

(1) Touch the red square.
(2) Touch the blue square.

even if the listener cannot recall (1) after having performed it,
he may be able to perform (2) correctly because the use of con-
trastive stress has drawn his attention to the word blue, whereas
in the absence of such stress he might select a square of another
color.

Contrastive stress may also be of value as a mnemonic de-
vice; its use may allow for retrieval of information lost or ob-
scured by 'noise' in the communication system, either in the lit-
eral, acoustic sense or in the sense of internal interference
with auditory processing, such as may be created by brain damage.
That is, if part of a message is lost, reference to it by use of
contrastive stress in another (usually later) part of the message
may allow for its successful reconstruction.[1]

MATERIALS AND METHODS

Stimuli

The stimuli for this study were two series of commands based
on those found in the Token Test (De Renzi and Vignolo 1962), a
widely used test of auditory comprehension in aphasia that holds
extralinguistic or contextual cues to a minimum. The test uses a
limited repertoire of five colors (blue, green, yellow, white and
red), two shapes (circle and square), and two sizes (large and
small). There are five parts to the test; they consist of com-
mands ranging in length from 'Touch the red square' (typical of
Part I) to 'Touch the large green circle and the small green
square' (typical of Part IV). Such commands seem well suited to
highlighting words by means of contrastive stress. In the cur-
rent study only Parts I and IV were chosen as models for the
stimuli; Part I was chosen because it presents items of a level
of complexity that would allow subjects with even severe impair-
ments of auditory processing to perform at least some items cor-
rectly, and Part IV because it is challenging enough to be likely
to elicit errors from subjects with relatively mild impairments.

In Part I the commands take the following form:

I-1.   Touch the red circle.
I-2. (B) Touch the red square.
I-3. (A) Touch the blue square.
I-4. (AB) Touch the yellow circle.
I-5. (AB) Touch the green square.

The numbers before each of these commands indicate that they are
the first five stimuli used in the first part of the current
study; they are not the identical to those used in any existing
version of the Token Test and thus will be referred as Part I-
type stimuli. These commands were presented to subjects verbally
in separate sets with two distinct readings. One of the readings

used contrastive stress, emphasizing the differences in content
between each succeeding sentence, as suggested by the underlining
in the examples above. In the discussion of results that fol-
lows, this reading will be referred to as the Contrastive Stress
condition.

It may be noted that in a series of sentences such as (I-1)
to (I-5) above, there are three possibilities for the use of con-
trastive stress, depending on the word or words in the sentence
at hand that contrast with words in the immediately preceding
sentence that occupy the same syntactic position. In (I-3), only
the color contrasts with the immediately preceding sentence; it
is labelled and will be referred to as Shift Type A, as indicat-
ed. In (I-2), only the shape contrasts with the preceding sen-
tence; this will be referred to as Shift Type B. In (I-4) and
(I-5) both the color and shape contrast; sentences of this type
will be referred to as Shift Type AB. There were 5 commands us-
ing each of these 3 shift types, arranged randomly in the 15
items that comprised the Part I-type stimuli for the study.
(There were, in fact, 16 stimuli in this part of the study; the
first command, because it contrasts with nothing, was a dummy
item and was unscored).

Precisely the same set of 16 stimuli was used for the other
condition in the study. This reading, the Control condition,
used a uniform rising intonation at the end of each command,[2] ig-
noring the inherent contrasts between commands. This may be what
the authors of the original Token Test, De Renzi and Vignolo
(1962:671), had in mind when they said that the commands should
be read 'without any special prosodic emphasis' (see also De Ren-
zi and Faglioni 1978:42).

In Part IV the commands take the following form:

        IV-1. (X) Touch the small yellow circle
                         and the small blue circle.
        IV-2. (Z) Touch the small red circle
                         and the large white square.
        IV-3. (Y) Touch the large blue square
          .   and the small red square.
          .
          .
        IV-5. (X) Touch the small white square
          .   and the small white circle.
          .
          .
        IV-7. (X) Touch the small red square
                         and the large red square.
        IV-8. (Y) Touch the large blue circle
          .   and the large yellow square.
          .
          .
        IV-10. (Y) Touch the large green circle
                         and the small green square.

When each of the 15 items in this part of the study was presented
in the Contrastive Stress condition, the first object noun phrase

in each sentence was read with the same uniform rising intonation used at the end of the Control condition reading of the Part I-type stimuli. In the second NP, the words that contrast with words occupying the same syntactic position in the first NP were read with contrastive stress, as suggested by the underlining in the examples above. Thus the domain of contrastive stress in the Part IV-type stimuli was the command itself (the second NP with reference to the first), whereas with the Part I-type stimuli it was the immediately preceding stimulus; these may be termed, respectively, intrastimulus versus interstimulus uses of contrastive stress.[3] When presented in the Control condition, the Part IV-type stimuli were read with a uniform rising intonation at the end of each object NP.

Given this 'intrastimulus' use of contrastive stress, there are various possibilities for the realization of stress in the second NP, all of which depend on which words in it contrast with words in the first NP. In (IV-1), for example, there is only one item that is different in the second NP--the color. In (IV-5), only the shape changes, and in (IV-7), only the size. Among the total of 15 Part IV-type stimuli, there are five with only one difference between the first and second object NPs; they have been labelled and will be referred to as Shift Type X. Another five demonstrate differences in two elements--changes in size and color (IV-3), color and shape (IV-8), and size and shape (IV-10); these have been grouped and labelled Shift Type Y. There are also five sentences in which all three elements change in the second NP, such as (IV-2); these will be referred to as Shift Type Z.

All the commands were presented by tape recording, including a screening test to determine whether the subject's auditory comprehension and visual and motor abilities were sufficient to identify separately each of the nine target words (the colors, shapes and sizes) used in the test and to perform the task in general; this also served the function of finding a comfortable listening level for each subject. Half of the subjects heard the sets of stimuli with the Contrastive Stress condition first (first the 16 Part I-, then the 15 Part IV-type stimuli); then they heard the identical stimuli read in the Control condition. The other half of the subjects heard them in the order Control, then Contrastive Stress.

The stimuli were tape recorded with careful attention to loudness levels and intonation. They were then analyzed as to the duration and intensity of both the carrier and target phrase by means of VOCAL, a manipulation program for producing tapes of verbal utterances developed at the Haskins Laboratories. This analysis revealed no significant differences in length between the carrier phrase in either testing condition, and it allowed for comparisons between the same target phrase when read with the two different prosodic styles.

The output tapes produced by the VOCAL program contained equal intervals between separate stimuli--7 seconds for the Part

I-type stimuli and 9 seconds for the longer, Part IV-type com-
mands. When played during testing, the tape could be stopped in-
obtrusively by a remote control switch if it appeared that a sub-
ject would require more time to respond; however, no limit was
placed on the time to respond. Response time information was re-
corded acoustically by means of a second tape recorder for later
analysis. Actual responses were noted as they occurred during
testing and later scored as to correctness in terms of both a
pass-fail criterion and a more sensitive system of weighted (par-
tial-credit) scoring, in which each element of the target phrase
was given a value of 1; thus a correct response to a Part I-type
stimulus yielded a score of 2 (1 each for the color and shape),
and a correct response to one of the Part IV-type stimuli a score
of 6 (1 for each of elements in the two object NPs).

Subjects

There were 42 subjects in the study, all of whom had had a
single cerebrovascular accident of either the left or the right
hemisphere. Discrete site of lesion information was not avail-
able on all subjects, but that was not one of the selection cri-
teria in the design of the study. None of the subjects had a
history of alcohol abuse, mental retardation, senile or presenile
dementia, psychiatric problems or significant hearing loss. All
were right-handed, and all were native speakers of American En-
glish. There were 15 subjects with single lesions of the right
hemisphere and 27 with single lesions of the left hemisphere.
All subjects were neurologically stable at the time of testing,
and all were a minimum of one month post-onset. Some subjects
were as much as 18 years post-stroke, but a careful review of
their medical histories revealed that none of them had had more

Table 1. Demographic Information

| | | Aphasics (Lefts) | | | Non-aphasics |
		All	Severe	Mild	(Rights)
Number:		27	9	18	15
Sex:	Male:	18	6	12	9
	Female:	9	3	6	6
Age in years:	Mean:	57.7	57.4	57.9	55.3
	Range:	38--75	43--75	38--74	32--72
Education in years:	Mean:	13.2	11.7	13.9	12.9
	Range:	8 - 20	8 - 14	9 - 20	9.5-19
Months post-onset:	Mean:	39.1	52.1	32.6	37.9
	Range:	1--217	1.5--134	1--217	6--120

than one neurological event with permanent sequelae. Additional information on time post-onset, as well as other demographic information, is presented in Table 1.

All of the subjects with left hemisphere lesions had received an independent diagnosis of aphasia, usually from a speech-language pathologist. Subjects with right hemisphere lesions were determined to be not aphasic, on the basis of their performance on the Boston Diagnostic Aphasia Examination (BDAE; Goodglass and Kaplan 1972), which was part of the testing protocol for this study. The subjects with left hemisphere lesions were further subgrouped according to their degree of aphasic impairment, mild or severe; this was done independently by an experienced aphasiologist, on the basis of a recording of some 10 minutes of conversational and expository speech by each subject (i.e. Part I of the BDAE).

RESULTS AND DISCUSSION

Contrastive stress appears to make a difference in the ability of aphasic patients to comprehend relatively short verbal commands, such as those in Part I of the Token Test. In this study the 27 left hemisphere damaged (i.e. aphasic) subjects as a group demonstrated a statistically significant difference in performance with the Part I-type stimuli, as measured by the weighted scoring method (Table 2); this difference favored the Contrastive Stress condition over the Control condition ($p < .05$). There were no significant differences for the two subgroups of mildly and severely impaired aphasic subjects; this is probably

Table 2. Results: Weighted Scores

				Percent Correct					
		Aphasics (Lefts)						Non-aphasics	
Stimulus		All		Severe		Mild		(Rights)	
type	Condition	(n = 27)		(n = 9)		(n = 18)		(n = 15)	
		Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Part I	Control	89.8	16.1	73.3	18.4	98.0	4.6	100	0.0
		**		NS***		NS		NS	
	Stress*	92.5	12.0	80.4	14.3	98.5	3.1	99.8	1.0
Part IV	Control	84.8	15.3	69.2	12.4	92.6	9.7	99.6	0.8
		NS		NS		NS		NS	
	Stress*	84.7	14.6	68.8	11.4	92.7	8.0	99.7	1.0

*Contrastive Stress   **p = .037   ***p = .057

due to the smaller ns involved (18 and 9 subjects, respectively, vs. the aggregated n of 27), the apparent simplicity of this task for the mildly impaired subjects, and the considerable variability in performance of the severely impaired subjects, as suggested by the larger standard deviations for that subgroup as compared to the mildly impaired aphasic subjects. However, it may be noted that the performance of the severely aphasic subjects approached statistical significance (p = .057), the difference again favoring the Contrastive Stress condition. The right hemisphere damaged (i.e. non-aphasic) subjects performed the Part I-type stimuli perfectly (or nearly so) in both conditions.

For the longer, Part IV-type stimuli, there were no significant differences in performance between the Contrastive Stress and Control conditions for any of the subject groups or subgroups. The right hemisphere damaged subjects again performed essentially like normal subjects, making very few mistakes, randomly scattered. The aphasic subjects, both as a group and as subgroups, also performed these longer stimuli with virtually the same level of correctness in the two conditions, but with more errors than the non-aphasic subjects.

One reason for this outcome may be that this task in itself (i.e. in terms of the information content of the segmental phonemes alone) was too difficult for the aphasic subjects to allow the difference in prosodic styles, evident and significant with the Part I-type stimuli, to emerge. Evidence for this may be observed in the lower mean level of correctness in both testing conditions for the longer vs. the shorter stimuli, with the greatest change occurring with the severely impaired aphasics in the Contrastive Stress condition (a decrease in weighted score correctness of 11.6%, from 80.4% to 68.8%; note that this decrease is much greater than the 4.1% decrease in the Control condition).

Performance was also analyzed in terms of the previously mentioned 'shift types' for the Part I- and Part IV-type stimuli, i.e. in terms of which elements change in successive commands in the Part I-type stimuli and which elements change within each of the Part IV-type stimuli. In 72 separate comparisons of performances of the various groups and subgroups of subjects, only one significant difference between the Contrastive Stress and Control conditions emerged.[4] Thus whatever advantage contrastive stress may confer in performing the Part I-type stimuli as a set, it was not observable in subsets of those stimuli, nor did it appear in subsets of the Part IV-type stimuli (perhaps because the size of the subsets is too small for statistical purposes).

In addition to such between-condition comparisons, performance was also examined by shift types within testing conditions. Differences here appeared not in terms of correctness but in response time. For the 15 non-aphasic subjects there was a significant difference in response time for the Part I-type stimuli for Shift Type B as compared to Shift Type A (p < .05) and a difference that approached statistical significance for Shift Type B

compared with Shift Type AB (p = .053). In both cases these differences were realized as faster response times for the type B stimuli; that is, the non-aphasic subjects performed faster when the shape element of the command changed with respect to the immediately preceding stimulus, faster than when either the color (in this case, significantly faster) or both color and shape changed. This difference in response time was not associated with any difference in performance in terms of correctness, but that is not surprising, since these subjects performed at perfect or near-perfect levels.

There was a significant difference in response time with the Part I-type stimuli for the nine severely impaired aphasic subjects as well, but with two interesting differences. These subjects' response times were also significantly different for Shift Type B as compared with Shift Type AB (p < .01) and approached a statistically significant difference with Shift Type B compared to Shift Type A (p = .054), but they were <u>slower</u> rather than faster (as was the case with the right hemisphere damaged subjects). These slower response times for Shift Type B appeared in both testing conditions, but they attained or approached statistical significance in only the Control condition, whereas with the non-aphasic subjects the significant differences for Shift Type B appeared in only the Contrastive Stress condition. Once again these differences were not associated with any differences in correctness of performance.

Because Shift Type B involves the final word of the commands, these combined results suggest that a recency effect is operative with these two groups of subjects. However, the effect is not the same: with the non-aphasic subjects it is positive, in the sense of being associated with decreased processing time, whereas wthe non-aphasic subjects it is positive, in the sense of being associated with decreased processing time, whereas with the severely impaired aphasics the effect is negative, in that it is associated with <u>increased</u> processing time.

The non-aphasic subjects also showed differences in speed of response to the longer, Part IV-type stimuli. In the Contrastive Stress (but not the Control) condition, they performed the Shift Type X commands significantly faster than the other two types (p < .05 in both cases). This outcome agrees with an a priori assumption that a change of only one element presents an easier processing task than either two or three elements' changing.

The 18 mildly impaired aphasic subjects also demonstrated a significant difference in response time in the Contrastive Stress condition for the Part IV-type stimuli, but in a rather surprising way: they performed the Shift Type Z stimuli significantly <u>faster</u> than the Shift Type Y (p < .01). (The same is true for the Shift Type X versus Z comparison, but this difference, also in favor of Type Z, is not statistically significant.) This outcome is counterintuitive; one would expect response times for these presumably more complex stimuli to be greater (i.e. slower) than for commands in which only two elements were changing. One

possible explanation for this result is that subjects (particu-
larly the aphasic subjects) perceived the Shift Type Z commands
as very difficult and, suspecting that they could not perform
them correctly, wanted to be done with them as quickly as possi-
ble. However, analysis of the Part IV shift types in terms of
correctness (as opposed to response time) does not support this.
There was no significant difference in correctness of performance
among the stress types of the Part IV-type stimuli, in either the
Contrastive Stress or Control condition, for either the mildly
impaired aphasic subjects or the non-aphasic subjects.

CONCLUSIONS

The implications of the response time data for various shift
types and groups of subjects, beyond those discussed above, are
not yet clear and warrant further study. However, the implica-
tions of the earlier mentioned results for all the stimuli taken
together (i.e. not considering differences in shift types) seem
quite clear: when given an auditory language processing task that
is manageable, such as stimuli like those in Part I of the Token
Test, aphasic individuals (particularly those more severely im-
paired) perform that task significantly better in the presence of
contrastive linguistic stress than with a neutral reading of the
same material. When the task is more difficult, as in Part IV of
the Token Test, the advantage conferred by contrastive stress
disappears; in fact, if the listener's processing capacities are
already overburdened by segmental information, the addition of
suprasegmental information may result in poorer rather than bet-
ter performance.

Darley et al. (1975) have defined prosody generally as 'all
the variations in time, pitch, and loudness that accomplish em-
phasis, lend interest to speech, and characterize individual and
dialectal modes of expression.' This definition suggests a peri-
pheral role to prosody, that if prosodic features were not pres-
ent in a message, there would be little or no effect (in English,
at least) on the understanding of its semantic content. The
findings of this study, on the other hand, suggest that prosodic
features such as contrastive stress convey not only a speaker's
affect and attitude but also part of the core of his message, and
that even severely impaired aphasic individuals retain the abili-
ty to comprehend the linguistic as well as the paralinguistic in-
formation that prosody conveys.

NOTES

1. In some instances stress is of value only if the listen-
er remembers previous information. In the examples cited, con-
trastive stress might be of little value in performing the second
command correctly if the listener could not recall the first,
particularly if he did not remember whether that sentence con-
tained the word circle or square. Stress might help him to un-

derstand and remember the word <u>blue</u>, but it would not allow him
to make the correct choice of the circle in (2) with any more ac-
curacy than choosing the square.

2. This reading was suggested by Dr. Frederic Darley, to
whom I am indebted.

3. At least two other readings of the Part IV-type stimuli
are possible. With one, the domain of contrastive stress would be
both interstimulus (as is the case with the Part I-type stimuli)
and intrastimulus. The second and third Part IV-type stimuli
would thus be read in the following manner:

> IV-2.   Touch the small red circle
> > and the <u>large</u> <u>white</u> <u>square</u>.
>
> IV-3a. Touch the large <u>blue</u> square
> > and the <u>small</u> <u>red</u> square.

However, such a use of contrastive stress, with emphasis now
placed also on <u>blue</u> in (IV-3a), seems unlikely to assist a per-
son--particularly a brain-damaged person--in performing the com-
mand correctly. Stressing <u>blue</u> may help in the correct selection
of that color, but only if one remembers the second NP in the im-
mediately preceding sentence; that is, the advantage of such a
use of contrastive stress may come only with an additional in-
vestment of memory (and impairments in auditory memory, it should
be noted, are not infrequently a source of auditory processing
deficits in brain-damaged individuals).
Furthermore, that investment may yield diminishing returns:
If a subject were to hear stress on the penultimate word in (IV-
3a), <u>red</u>, and for some reason did not comprehend or hold in mem-
ory the immediately preceding word, <u>small</u>, then the presence of
stress on <u>blue</u> in the first part of the command, together with
the lack of stress on the preceding word, <u>large</u>, might lead him
to suspect that the word not comprehended was also <u>large</u>. That
is, if a subject were to hear

> IV-3b. Touch the large <u>blue</u> square
> > and the _____ <u>red</u> square,

and did not comprehend or hold in memory) the antepenultimate
word <u>small</u> (as suggested by the blank underlining), then his
knowledge of rules of English prosody might lead him to make the
incorrect choice of the large red square.
Another possible reading of the Part IV-type stimuli is to
allow the first object NP to 'forecast' the contrast present in
the second NP. If this were done, then the third stimulus would
be read in the following manner:

> IV-3c. Touch the large <u>blue</u> square
> > and the small <u>red</u> square.

One reason that this reading might be employed, rather than the one adopted in the current study (a uniform rising intonation at the end of the first NP), is that a subject might think that the stimulus was complete and might proceed to respond prematurely; the use of contrastive stress in both NPs would tend to discourage this. In fact, use of the uniform rising intonation did not produce such problems and indeed may have prevented them. Furthermore, subjects were instructed (and, if necessary, reminded) not to respond before the entire command was complete.

    4. With Shift Type AB (both color and shape changing) for the Part I-type stimuli, the severely impaired aphasic subjects made significantly fewer errors in the Contrastive Stress condition.

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Morphological Innovation & Subgrouping:
some Tibeto-Burman notes *

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0. Introduction. This paper attempts to contribute both to the theory and the practice of Tibeto-Burman subgrouping through the examination of the patterns found in the distribution of certain morphological innovations. The use and value of morphological innovations for subgrouping is illustrated with the examples provided by innovative yes/no interrogative particles, negatives, and a periphrastic causative formed from ***tir** 'to send; to cause'.

1.0 Methodology. Subgrouping often constitutes a far more difficult task than the simple discovery of a genetic relationship. It is further complicated by the fact that many of the similarities between closely-related languages are the product not of a common inheritance but of what Sapir termed drift; that is, the common starting point provided by a common origin often conspires with what are universal diachronic tendencies to produce parallel but historically quite independent paths of development among genetically related languages. The picture is still further complicated by the areal convergence produced by widespread multilingualism. Recent work on areal linguistics has shown that multilingualism is capable of producing extensive convergence across genetic boundaries (Emeneau 1956, Masica 1976); it is only reasonable to assume that the widespread multilingualism found among closely-related languages would at least produce as great a degree of convergence.

Nonetheless, the principle behind subgrouping is quite simple: subgrouping is done exclusively on the basis of shared innovations. The value of a shared innovation is further increased if accompanied by a complementarily-shared obsolescence (Greenberg 1957:49); thus, if two languages replace an older morphological construction with a newer one, this double agreement is of increased significance for judging the prior existence of a common historical period. In fact, it is precisely in cases of syntactico-morphological innovation that shared obsolescences coupled with shared innovations are most readily detectable.

The corollary to the above principle is equally simple: since shared retentions can occur independent of a period of common development, their presence cannot constitute subgrouping evidence. Although this corollary is self-evident, even a cursory glance at the discussions of subgrouping in the literature on Tibeto-Burman makes it necessary to state it.

2.0 The yes/no interrogative marker < PTB *ma 'not'. Simon (1942) noted that the −**m** of the Classical Tibetan interrogative marker **ham** was ultimately to be derived from the negative marker PTB/PST *ma ~*mi 'negative'; DeLancey (1978 a,b) laid out the path that the derivation must have taken. The *ma 'negative' is found abundantly attested throughout Tibeto-Burman and Sino-Tibetan and unquestionably reconstructs to the earliest stages of the language family. The *ma 'interrogative marker' , however, is an innovation, whose origins are to be found in the syntax and semantics of disjunctive questions.

Chao (1947:40-1) describes Cantonese disjunctive questions as involving a choice between alternatives. The most elaborate disjunctive question is the A-not-A question which spells out the choice between something and its negative:

> **nee   zek-mu-zek   in° ah?**
> you smoke-not-smoke **in° ah**
> 'Do you smoke?'

The least elaborate disjunctive question again has a positive sentence but the negative disjunct is represented only a single final particle: **mah, mhe°, a°,** or **ah.** Etymologically, the first two particles are related to the negative marker *ma; semantically, Chao describes the tagged on final particle as equivalent to the French **n'est-ce pas?**

Such disjunctive questions are the rule rather than the exception in Tibeto-Burman; examples can be found in language after language. It is obvious that such constructions were the source the widespread *ma interrogative markers. The negative in sentence-final position was reinterpreted as an interrogative marker for yes-no questions. Then, at least in some cases, this yes-no interrogative was extended to use as a general interrogative. Certainly, the functional distribution of the *ma in Tibeto-Burman supports this analysis; wherever the *ma interrogative is found marking general questions, it is also found marking yes-no questions but not vice versa.

Burmic Division: Kukish Section. In Shafer's Kukish Section of the Burmic Division of Tibeto-Burman, question particles derived from *ma 'NEGATIVE' are found in eight of the eleven branches: Southern Chin [Khami, Khimi], Lakher [Lakher], Old Kuki [Langrong, Hrangkhol, Hallam, Tsiru, Aimol, Kolhreng, Kom, Purum], Central Chin [Lushei, Haka (Lai), Bawm-Zo], Northern Chin [Tiddim, Siyin (=Śiyang), Thado], Western Branch [Empeo], Eastern Naga Branch [Angami, Rengma], and Mikir [Mikir]. On the other hand, it is definitely not found in Meithlei of the Meithlei Branch and the data on the Northern Naga Branch shows A-not-A questions but not a sentence final particle from *ma 'NEGATIVE'. This data is summarized in Chart 2.0 found below.

The interpretation of the data is relatively straightforward. Because the eight branches above all have direct evidence of an innovated *ma interrogative particle, this is taken as evidence for grouping them together. No comment can be made on the Luhupa Branch, since no data from Luhupa languages was analyzed. It should be noted that the failure of a language to appear in one of columns of the chart may reflect nothing more than the fact that it was not analyzed. However, in the case of Sema and several Ao dialects of the Eastern Branch, the analysis showed no evidence of such a marker; nonetheless, both Angami and Rengma of the branch did contain evidence. In the case of Ao, it is unlikely that the failure to discover the particle is due to the uneven quality of the data base; instead, it simply does not have evidence of the particle.

Southern Chin	Central Chin
Khami - mo	Lushei -em/-m@
Khimi -mei	Haka (Lai) m@
	Bawm- Zo -mê
Lakher	
	Northern Chin
Lakher -ma	
	Tiddim Chin hiam
Old Kuki	Siyin zi·am
	(=Siyang) mo
Langrong -m@	Thado -m/-ham/-am
Hrangkhol -mo	
Hallam -m@	**Western Branch**
Tsiru -mŏ	
Aimol -mŏ	Empeo -me
Kolhreng -mŏ	
Kom -mo	**Eastern Naga Branch**
Purum -mŏ	
	Angami -ma 'YN Q'
Mikir Branch	Rengma -mu 'NEG. YN Q'
Mikir má 'FRAGEPARTIKEL; oder'	

Meithlei Branch	Northern Naga Branch
⊸	A -not - A questions only

Chart 2.0: The distribution of interrogative particles derived from PST *ma 'NEGATIVE'

Notes: The subgroupings are those of Shafer (1966-7, 1974). No data from the Luhupa Branch was analyzed.

Shafer's Baric [=Bodo-Garo (Barish) & Konyak (Nagish)].   Shafer's Baric division is composed of his Barish Section [=Bodo-Garo] and his Nagish Section [=the Konyak or 'Naked Naga' languages].   The evidence provided for the unity of this subgrouping by the innovated *ma interrogative marker is less than decisive but is nonetheless intriguing.

<div align="center">

Shafer's Baric Division

</div>

Barish Section		Nagish Section
North Central Branch	Western Branch	
Garo -ma	Bodo -ma	Phom me- (ma-)
-mai		'general interrogative'

If the widespread distribution of this particle is attributed to common origin, then it certainly constitutes evidence for the unity of Bodo-Garo with the Konyak languages. Both Bodo-Garo and Konyak are well established

subgroups, and their unity has been suggested by Burling (1983) on the basis of lexical evidence. The presence of non-*ma-derived interrogatives in Deori Chutiya [Bodo-Garo] and in Nocte and Chang [Konyak] might argue for independent but parallel development (after all, the derivational mechanism is relatively simple) of the *ma-interrogatives in Bodo, Garo, and Phom except that in all three cases the present phrase-final interrogative looks etymologically suspiciously like what are non-interrogative sentence-final particles in other languages. Thus, while Garo has final strings such as **sok-ba-ku-ja-ma** 'Has he not arrived?' (Burling 1961:36) in which the final two morphemes are the innovative negative **ja** followed by the innovated interrogative **ma**, itself a former negative , in Deori Chutiya a number of the interrogatives are marked simply by **-ya** 'or not'; simple truncation is certainly sufficient to get from the Bodo **-ja-ma** to the Deori Chutiya **-ya**. The Nocte and the Chang interrogatives similarly look like they can be analyzed as resulting from the truncation of a sentence + *ma interrogative structure, resulting in the reanalysis of the sentence final particle as an interrogative. The very abundance of reputed interrogative particles and the widespread appearance of cognates elsewhere occurring as declarative sentence-final particles argue for this analysis. In any case, such particles do not argue against a common origin for the *ma interrogatives of Garo, Bodo, and Phom. [Note: An obvious question of whether the common origin is at a higher level or just at the Baric Division level remains unanswered.]

Bodish [Tibetan]. This type of innovation occurred independently in some of the Bodish languages including Central Monpa **-mo** [Tsangla Branch], Classical Tibetan **-ham** [Bodish Branch], Jyarung **ma-** (preverbal) [rGyarung Branch], and Kham **ma-** (preverbal). These are in three of Shafer's four branches of his Bodish division, and thus look like they are from one source.

Other. Within Chinese, Mandarin **ma**, Cantonese **ma**, and Haka **-mo** are all sentence-final question particles, but these appear to have been innovated, since Archaic Chinese seems to have no evidence of such a sentence-final interrogative marker.

3.0 Innovated negatives. The universality of the *ma 'NEGATIVE' in Tibeto-Burman makes the presence of an innovated negative relatively easy to identify. In most cases, the value of such an innovation for sub-grouping is increased by the fact that it has totally replaced the older *ma.

<u>Bodo-Garo [=Barish Section]</u>

North Central Branch	Jalpaiguri Branch	South Central Branch	Western Branch	Eastern Branch
Garo -ja	Wanang -ca	Atong -ca	Bodo -ya	Deori Chutiya -a (?)

Chart 3.0a: Bodo-Garo innovated negatives.

The first four are cognate negatives innovated from *ma V *ja. Originally, the *ma was the negative marker and the *ja was a sentential particle serving

another function. The second stage in the process was the reinterpretation
of the –ja as part of the negation marking producing a discontinuous negative
*ma V –ja. The final stage, of course, is the dropping of the original *ma as
redundant. If the –a of Deori Chutiya is cognate, then all branches of Bodo-
Garo share this innovation; if not, only the first four do. In any case, the
innovation provides subgrouping evidence. [Note: the colloquial use of the
French **pas** as a negative marker produces a somewhat similar case].

    Karenic. The Karenic languages provide not one but two distinct innovated
negatives.

<div align="center">

Karenic

</div>

Moulmein Sgaw	təʔ-báʔ	Bassein Pho	ʔə̀ʔ
Bassein Sgaw	təʔ-báʔ	Moulmein Pho	ʔeʔ
		Palaychi	ʔə̀ʠ

Chart 3.0b: Karenic innovated negatives.

Both are innovated but neither appears to have come from the other. The
first is a discontinuous form təʔ...báʔ , which seems to have its origins in
the *ta 'PROHIBITIVE' prefix of Tibeto-Burman combined with a sentence-
final particle *ba. Its origin, I suspect, correlates with the innovation of
SVO < *SOV word order in Karen. As in many of the Tibeto-Burman languages,
the original negative *ma may have been postverbal occurring before a
sentence-final particle. With the shift toward SVO, pressure for a preverbal
negative occurred, providing a motivation for the reinterpretation of the
preverbal *ta 'PROHIBITIVE' as not just a negative marker for imperatives
but as a general imperative.

    4.0 Innovated causatives. Throughout Tibeto-Burman innovated causatives
are found independently derived from various full verbs with meanings such
as 'to send', 'to give', 'to make', etc. Such constructions have evolved from
fully periphrastic structures comparable to the English 'He made him steal
the money'; however, now many of these structures are morphological rather
than syntactic. The periphrastic causative *tir < *tir 'to send' is an
excellent example of such a construction. As is evident from Chart 4.0,
*tir in its purely causative sense is restricted to four subgroups of Chin:
the Central Chin Branch, the Old Kuki Branch, the Lakher Branch, and
to just one language in the Northern Chin Branch [Ralte]. Its occurrence
in Ralte is reported in Wolfenden (1929), but he associates the form not
with *tir but with another etymon. However, as note 6 accompanying Chart
4 makes clear, the Ralte reflex is cognate to the forms in the other three sub-
groups. Thus, the *tir causative is evidence for grouping together at least
these four branches of Chin against the other seven branches of Kuki-chin.
[for chart 4.0, see the next page].

    5.0 Conclusion. This paper has attempted to simultaneously contribute
both to the theory and to the practice of Tibeto-Burman subgrouping. The
actual innovations examined are either expansions of observations found
in the literature or original with this work. The other purpose of the paper

Chart 4:   The Distribution of *tir 'cause'[1,2]

Lakher	*tir 'cause'	*m-t'ir 'iron'
Mara	tua	t'ua
Old Kuki	*tir 'cause;send'	*t'ir 'iron'
Langrong	-tir-	t'ir
Hrangkol	-tir-	tir
Hallam	-tir-	t'ir
Kom	-tir-	t'ir
Central Chin	*tir 'cause'	*t'ir 'iron'
Lushai	-tir	t'ir
Ngente	-tir-	
Zahao	-tir-	
Hmar	-tir-	
[3]Pankhu		t'ir
Laizo (Osburne 1975)	-tér-	
Lai (Haka)	-tar	t'ir
Bawm-Zo (Reichle 1979)	-tir	
Banjogi	-tar, -ter	t'ir
Taungtha		[5]şɣ
Northern Chin		
Thado		t'i
[6]Râltê	-tik=	
Śiyang		tśi
Tiddim Chin		

Notes on Chart 4:

[1]*tir 'causative', or more correctly reflexes of *tir, occur only in the Chin languages. In fact, the distribution is further restricted to just four of Shafer's five Chin subgroups--Lakher, Old Kuki, Central Chin, and Northern Chin; the non-occurrence of *tir in Southern Chin-- the fifth Chin subgroup--may on the one hand be a reflection of the internal subgrouping of the Chin languages or it may just be the result of gaps in our data.

[2]Reflexes of *t'ir 'iron' are provided for their phonological parallelism.

[3]Shafer's Bom, according to Reichle (1979:12), is identical to Pangkhua (=Pankhu).

[4]Bawm-Zo is identical with Shafer's Banjogi (Reichle 1979:12).

[5]Shafer (1974:235) suggests this word is a loan from Southern Kukish, since he would expect śit.

[6]Apparently because of the unexpected final -k, Wolfenden (1929:188) tentatively equated Ralte -tik- with WT ǰug-pa (P. bǰug, F. gǰug, Imp. ʒ'ug) 'to cause, compel,' noting that other instances of variation between -i- and -u- occur. However, the Ralte from -tik-, even the final -k, is a quite regular reflex of *tir.

In this connection, Shafer (1974:250) wrote in reference to final *-r in Northern Chin:
> There is considerable scattered evidence, which I shall not bring together here, to indicate that *-r did not drop without a trace, but that it was replaced by a glottal stop, at least in Thado and Siyang and this shift was perhaps affecting Ralte and Vuite.

Although Shafer had noted 'scattered evidence' of a glottal reflex, later authors found the stop in question to be velar not glottal. In particular, the correspondence of Lushai -r (Central Chin) to Tiddim -k (Northern Chin) was explicitly noted in Ono (1965) and again in Hillard (1974). Solnit (1979:112) went a step farther, providing twenty-one examples of the correspondence of Lushai -r to Tiddim -k; he also gave in passing four corresponding Siyin (=Siyang) forms with final -k:

Lushai	Tiddim Chin	Siyin (=Siyang)	
zuár	-zuak	yuak	'sell'
páar	-paak	pak	'flower (n.)'
záar	-aak	a~ak	'fowl'
pèer	/peek	p'iak	'flat'

In short, the *-r to -k correspondence (or some variant of it) seems to occur in Tiddim Chin (=Kamhau), Siyin (=Siyang), Thado, Ralte, and Vuite, all Northern Chin languages.

Sources: Wolfenden (1929), Shafer (1974), Ono (1965), Hillard (1974), Osburne (1975), Reichle (1979), Solnit (1979)

....

was to point out the value of such innovations for subgrouping. Such morphological constructions have a number of advantages over other

negative markers, and causative markers are typically found even in the most basic grammatical descriptions of a language. Second, the number of forms involved is significantly simplified over what is necessary for phonological innovations: further, the interpretation of 'exceptions' is not as demanding a task. And, finally, it is often the case that such innovations co-occur for obvious reasons with a simultaneous obsolescence, thereby providing dual evidence for a common period of development.[1]

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*I shall be astonished if all my errors should prove minor, and I will be grateful for the corrections of the readers. This paper is a response to the interest generated by my reading of Benedict (forthcoming, 1983) and DeLancey (1978 a,b). I also wish to thank Keith Record for his help. This material is based upon work supported by the National Science Foundation under Grant No. BNS-8203882.

[1]The one caveat, of course, is that parallel but independent morphological innovations do occur. Thus, the value of any given innovation for subgrouping evidence is inversely proportional to the likelihood that it resulted from an independent but parallel path of development.

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Performatives as Indexicals: Resolving the Performadox*

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The performative hypothesis, one of the syntactic/semantic proposals identified with generative semanticists such as Ross (1970) and Sadock (1974),[1] states that among the elements that must be represented in the deep or semantic structure of a given sentence is the intended illocutionary force of the sentence. A performative clause was posited to fill this role. Consider:

(1) The boss is a moron.
(2) I say that the boss is a moron.
(3) Confidentially, the boss is a moron.
(4) Confidentially, I say that the boss is a moron.

Under the performative hypotheses (1) and (3) have abstract performative clauses indicating their illocutionary force.

Boer and Lycan, in their "A performadox in truth conditional semantics" (1980), present a problem (hereafter the performadox) in semantics, as they say; more specifically, however, it is a problem in speech act theory as it relates to semantic theory more generally. The performadox runs as follows: suppose that the abstract clause is not semantically interpreted. It would follow, then, that the adverb in (3) is not interpreted. This is clearly wrong since "confidentially" contributes to the meaning of (3). On the other hand, suppose that the performative clause is interpreted. This predicts that (1) and (2) have the same truth conditions, which is also wrong. On either assumption, the performative hypothesis gets the wrong results. As Boer and Lycan are quick to point out, the obvious implication is that the performative hypothesis is false. But then, there would be no performadox. They accept the (otherwise uninterpretable) adverbials of sentences like (3) as good evidence for the performative hypothesis. The only surface non-performative for which Boer and Lycan accept an underlying performative clause are those such as (3).[2]

Sadock provides a solution by positing two distinct notions of truth. Truth$_1$ is a semantic notion about which we may well have no guiding intuition, since it is a theoretical notion. Truth$_2$ is a pragmatic notion. Truth$_1$ is a property of propositions, while truth$_2$ is a property of assertions. Thus (1) and (2) have the same truth$_2$ value, while they differ on the level of truth$_1$.[3]

We find much in this analysis that is attractive. We think there is something profoundly right about the bi-valent treatment of truth and we agree that pragmatic considerations play a central role, but we are reluctant to espouse two kinds of truth. Rather, we prefer the difference to hinge on two levels of truth evaluation.

Our analysis draws heavily on Kaplan's work on demonstratives and indexicals (1977).[4] Kaplan distinguishes contexts of utterances from circumstances of evaluation because they play two different roles. Consider:

(5) I am hungry.

When you evaluate a sentence like (5) with respect to a possible world, the individual relevant to the determination of the truth value is the individual referred to in the <u>actual</u> context of utterance, and not whoever meets the description or satisfies the property criteria in the circumstance of evaluation. The relevant individual is the agent of the discourse context, and the sentence is true iff that individual is hungry, regardless of the state of her counterpart in w_{183}. The context has served to determine the relevant individual. The circumstance of evaluation provides a list of who has the relevant property. Thus (5) is true with respect to a circumstance of evaluation n, iff 'I' am among those individuals who have the property of being hungry in n.

For this reason, Kaplan espouses a three level semantic system, rather than the traditional two level framework. His levels consist of <u>character</u>, <u>content</u> and <u>circumstance of evaluation</u>. Character can be conceived of as a sort of linguistic meaning rule established by the conventions of a linguistic community; character has the role of determining a content for every context. It is a function from every context to a content. Some expressions have a "stable" character, which is to say that they pick out the same thing across contexts. Context insensitive expressions have this type of character.[5] Indexical elements, on the other hand, typically have characters that are non-stable. For example, the character of the word "I" might informally be construed as a linguistic meaning rule roughly of the form (6).

(6) In each of its utterances, "I" refers to the person
     who utters it (Kaplan 1977:44).

The character of indexical expressions does not pick out the same referent across contexts. Rather, it designates different individuals in different contexts. Thus, "I" designates different individuals in contexts where the agents (utterers) differ.[6]

Character, in tandem with context of utterance (hereafter context), yields the level of content: what was said or the proposition expressed. Content is then what is evaluated at the circumstance of evaluation. Kaplan's framework provides a picture as in (7).

(7) character   content   referents
          +   →   +   → (individuals, etc.)
       context   circumstance

This three-level system allows one to predict some interesting
properties of indexical expressions in general. It can capture
the fact that indexicals vary in content from context to context.
For example, though the character of "I" is the same in every
context, and thus we can understand any occurrence of it, the
contents may differ, since its character functions to pick out,
or refer to, different individuals in different contexts.
Consider now (8) and (9).

(8) I am here now.
(9) Cyndi Welsh is in Berkeley, CA on February 13, 1983.

On one level any utterance of these two sentences have the same
truth conditions. Both are true iff the alphabetically second
author of this paper is located at the place and time referred
to. Clearly both are equally contingent. On the other hand,
there is also some intuitive difference between them. The intui-
tion is that, on the basis of knowing the meanings of the indexi-
cal expressions (and the verb) in (8), we know that it cannot be
uttered falsely; that is, (8) is true in any context in which it
is uttered.
Classical semantics, in dealing with examples such as these,
fails to distinguish between them. Montague (1968) captured the
difference between (8) and (9) by restricting the circumstances
of evaluation so as to include only proper indices: world, agent,
time and place such that the world is the world of the context,
the agent is the agent of the context, who is located at the
place and time of the context. This seems legitimate since im-
proper indices would be like impossible worlds--no such contexts
could exist. However, there is an unfortunate consequence of such
a reform: (8) comes out logically true, and by the principle of
modal generalization (10) is also logically true.

(10) Necessarily I am here now.

But not only is (10) not logically true, it is false. I might
have decided to sit out the BLS meeting, or the BLS might have
rejected the abstract we submitted. In either case and in a
number of other eventualities, I might fail to be here now.
In Kaplan's framework, the distinction between character and
content allows both the contingency and the validity of sentences
such as (8) to be captured. Kaplan writes:

> The bearers of logical truth and of contingency are differ-
> ent entities. It is the <u>character</u> (or the sentence if you
> prefer) that is logically true, producing a true content in
> every context. But it is the <u>content</u> (the proposition, if
> you will) that is contingent or necessary (Kaplan 1977:72).[7]

One more point must be stressed before we move on to the
application of this system to the analysis of performatives.

This framework argues that character necessarily works in tandem with context to determine content, and assumes that contexts are structured entities. There are many types of things that count as contexts. Conversational history between interlocutors can serve as a context rich enough to support anaphora, as can shared contextual experience outside of a discourse context. Since there are many types of context and a rigorous definition is a task yet to be completed, we will confine our discussion to what seems to be the minimum assumption necessary for our purposes: contexts of utterance.[8]

Contexts of utterance are structured entities which are definable in terms of various features. Contexts of utterance consist of: 1) an utterance; 2) an agent of the utterance; 3) an audience; 4) such contexts are spatially located at a place; and 5) temporally located at a point or neighborhood in time; and finally 6) such contexts are embedded in a richer context, a world history, if you will.

Thus, formally such contexts consist of a set of indices that are to be distinguished from the indices of the possible circumstances of evaluation. If c, a context, is a member of the set of contexts C, then c=x, audience, w, p, t, u. That is to say that c is uniquely determined by these indices.[9]

Our solution to the performadox depends on the positions we take on two crucial issues. First we maintain that the verb occurring in a performative sentence is the same verb as that which occurs in its declarative counterpart. That is, 'say' in (2) is the same verb and means the same thing as 'say' in (11).

(11) Kim said that the boss is a moron.

This commits us, in the words of Bach (1975), to the position that "performatives are statements too." Thus in uttering (2) to assert the boss is a moron, one is also stating that one thereby performs that assertion.

We follow Ginet (1979) who defines a type of verb phrase "φ . . . such that a person's φing entails that the person makes an utterance and means something by doing so."[10] Such a φ he calls a "meaning verb phrase." All performative verbs are meaning verbs, but not conversely (e.g., berate you, describe the scene) (Ginet 1979:245). His attempt is to show:

> that a verb phrase 'φ' is performative in certain circumstances because its descriptive meaning (the nature of the act it signifies) is such as to make it possible in those circumstances to φ by means of stating that one thereby φs: φing is the sort of thing that lends itself to being done just by saying that one, in that very act, does it (1979:246).

This position has, as a corollary, "if [performatives] are also statements, then as statements they are true or false" (Bach 1975:230).[11]

This may seem a problematic position to maintain. One of the
motivations for claiming that such sentences do not have truth
values is that they seem to turn out always true, from which the
argument is made that one could then make things true simply by
saying them. Such a conclusion would be enough to make one a
compulsive talker (cf. PTCS:99 fn. 12). This argument is a non
sequitur, however, since it is based on the assumption that it is
the complement clause <u>only</u> that is assessed as to truth value.
We reject this assumption. Under our analysis, it is the truth
value of the proposition expressed by the performative clause
that is of concern here.

The truth conditions of (2) and (11) are as follows: both
have the form of (12).[12]

    (12) X SAY that P

In (12) X is an agent, P is a sentence/proposition and SAY is the
verb <u>to say</u> without number or tense features. In general, utter-
ances of sentences of the form (12) have the following truth con-
ditions.

    (13) (12) is true iff ($\exists$: context $c \, \epsilon \, C$) ($\exists$: time $t \, \epsilon \, T$)
                           ($\exists$: world $w \, \epsilon \, W$) ($\exists$: agent $x \, \epsilon \, X$)
                           ($\exists$: audience $a \, \epsilon \, A$) ($\exists$: place $p \, \epsilon \, P$)

        such that
        SAY $(x, P, \text{in } c, \text{at } t, \text{located at } p, \text{in } w, \text{to } a)$ is
        true.[13]

In the case of (2) the values of indices are as in (14);

    (14) x = speaker
           w = the world of the context
           t = the time of the context
           p = the location of the context
           P = the utterance
           a = unspecified

The context in this situation is the actual context of utterance.
When the context is identical with the relevant circumstance of
evaluation, the proposition expressed by the utterance is true.
Thus (15):

    (15) (2) is true iff:
           SAY $(I, \text{"the boss is a moron," in } c, \text{at } t_c, \text{in } w_c,$
           at p_c) is true.

(2) is true simply on the assumption that it was uttered in the
actual context. Turning now to (11), the truth conditions are
the same as those in (12). The only difference between (2) and

(11) is the relevant context that serves as the circumstance of evaluation. The past tense in (11) indicates that the relevant context is some context previous to the actual context of utterance. Thus (16):

> (16) (11) is true iff ($\exists$: c' $\neq$ c) ($\exists$:t' < t) etc. such
>   that SAY (Kim, P, at c', located at p, at t', in
>   w, to a) is true.

In order to determine the truth value of (11), c' $\neq$ c is the relevant context. We would need to locate c' that temporally preceded c.

Notice that the treatment we are proposing also excludes progressive forms from having performative status, since the progressive indicates a time span broader than that of the context of utterance. Since the context of utterance must coincide exactly with the circumstance of evaluation in order for the trivially true status to arise, the progressive form is automatically predicted not to exhibit performativity in the general case--just the desired result.

It should not be disturbing that the truth conditions given predict that sentences like (2) will be true on any utterance. We have already seen an example of this phenomenon in (8). Applying Kaplan's views to performatives is fairly straightforward. That performatives are always true is a result of the fact that the character of the indexical elements along with a verb with a certain type of meaning produces a true content in every context.

Returning now to (3), speech act adverbials are the one bit of evidence for the performative hypothesis that Boer and Lycan accept. These are the sentences for which Boer and Lycan allow an underlying performative clause. As noted above, if the adverbials do not modify the speech act, rather than the clause they co-occur with, they are semantically uninterpretable. It is also the case that these adverbials are not uninterpretable.[14] There is something semantically anomolous about shouting (3) at the top of your lungs. Most proposals for dealing with this phenomenon have involved building in structure of either a semantic or pragmatic nature to provide something for the adverbial to modify (cf. Davison's (1982) thorough critique of these). Under our analysis, however, since contexts of utterance are structured entities, in and of themselves, we have no need for such structure building. Our treatment amounts to a semantic-pragmatic one since contexts of utterance can contribute semantically in the Kaplan system.[15]

This account seems correct in that it allows for a unified treatment of overt and adverbial instances of performativity. Our proposal is that (1) the semantics of overt performatives involves a Kaplan context of utterance, and (2) speech act adverbials are predicated of features of these contexts. These features may be made explicit as in a performative clause (performa-

tives as "lexicalizations" of contexts of utterances, if you will), or not, as in the case of the example considered. Thus, this analysis amounts to the claim that the utterance of sentences such as (3) have the same truth conditions as their fully explicit counterparts (4). That is, utterances of both are true iff the agent of the context does (17).

    (17) ((adverbial SAY) (x, P)[16]

There is evidence for this claim. Consider (18) and (19):

    (18) Since you wanted to know, I say that Kim is a fool.
    (19) Since you wanted to know, Kim is a fool.

Both can take responses such as (20) and (21), which cast doubt on or call into question the truth of the proposition expressed by the performative, and not the truth of the subordinate clause in (19):

    (20) I don't believe it, you always have an ulterior motive.
    (21) That's not true, you know I don't give a damn.

The fact that the responses are parallel and that they cast doubt on (18) and (19) in the same way indicates that our claim that they have the same semantics is not so farfetched at all.
    The question now arises as to how we avoid the disaster of having these contextual features showing up just anywhere at all. Why do they not figure in (1) giving it the same truth conditions as (2)? That is, how do we avoid the performadox? Since this analysis allows contextual features to be semantically interpreted, what prevents them from being so interpreted in (1)? An analogue to the Boer and Lycan settlement would be that contextual features are only semantically available when they are overtly represented or predicated over. Only in such cases would these features figure in such a way as to affect truth conditions. Thus (1) and (2) would have different semantics: (2) would be trivially true and (1) would be true or false depending on the mental capacity of the boss. While this may have satisfied Boer and Lycan and does seem to have something right about it, there is nothing explanatory about this—it merely declares that the contextual features are sometimes available. Further, there are facts which such a stipulative account does not address. Consider (22):

    (22) That's not true, he's quite intelligent.

This, taken as a response to (1) or (2), calls into question the proposition expressed by the complement clause of (2). This fact has been taken as evidence that (1) and (2) have the same seman-

tics, namely that of the complement clause leaving the preface uninterpreted. We must reject this approach also of course.

We follow Sadock's proposal that given an explicit performative and a <u>that S</u> complement, there are two truth valuable propositions expressed. One, the performative, has as its semantics a Kaplan utterance context. The other is the complement. We further propose that all utterances of declarative sentences have such an utterance context, and that these contexts are semantically available. From all this it might seem to follow that for us (1) and (2) have the same truth conditions. Indeed that is what we hold, as far as the proposition that includes the contextual features. Note carefully, however, just in what this consists. We are not claiming that in (1) the boss is a moron is true iff I say it. Nor that the proposition expressed by the performative clause in (2) is true just in case the boss really is a moron. We claim that the two propositions expressed in each have their own truth conditions. This approach seems to us to allow a straightforward solution to the conflicts evidenced by (1), (2), and (22).

There is a difference between (1) and (2), however. Consider (1), (2) and (11) uttered under normal circumstances (repeated here).

(1) The boss is a moron.
(2) I say that the boss is a moron.
(11) Kim said that the boss is a moron.

(1), (2) and (11) can take (23) and (24) as responses:

(23) That's not true.
(24) That's not true, she's the smartest person I know.

Observe that (23) as a response to (11) exhibits an ambiguity as to whether the negation refers to the proposition expressed by the whole sentence or only to the one expressed by the complement clause. In the case of (1) there seems to be no ambiguity possible with respect to response (23). This is a problem since we hold that semantically, there is another proposition available from the context of utterance. Why isn't (23) an ambiguous response to (1)? Consider (2). Note that it is well-nigh impossible to construe (23) as an ambiguous response to (2). This is so because it is nonsensical to deny that someone who has just said P, has in fact just said P--it follows from the truth conditions and the context of utterance that the speaker must have said what she said. However, there seems intuitively to be something very different about (23) as a response to (1) and as a response to (2). How do we capture this difference?

We suggest that the (real and salient) difference between utterances of (1) and (2) lies not in the realm of truth conditions (we have argued explicitly and crucially against this), but

in the realm of non-natural meaning (meaning$_{nn}$) (Grice 1957), or perhaps equivalently, implicatures (Grice 1975). If someone says (1), it is true trivially that she says it--but that cannot be the point of so saying, since if it were, she could say (2) wherein the ordinarily uninteresting fact of saying one's utterance is at least part of the point--perhaps as a point of emphasis. To spell it out in detail: given that (1) and (2) are truth conditionally equivalent and both available to the competent English speaker/ hearer, the choice between them and the interpretation given them will fall under the relation (be relevant) or the manner (be per- spicuous) maxims, or more generally, perhaps, fall out from the meaning$_{nn}$ of the utterance. In other words there must be some reason for lexicalizing the contextually available information, but that does not make it a matter of semantics.

At this point we should clarify our position on two points in Sadock's resolution of the performadox. First, Sadock uses his two notions of truth, truth$_1$ and truth$_2$, while we use only one truth, roughly his truth$_1$. For us, meaning$_{nn}$ plays a roughly analogous role to truth$_2$. Second, Sadock observes that (25) is conventionally taken as an assertion of P by X.

(25) X say P

This might seem like an embarrassment for our approach, but we don't think it is. We hold that it is just in virtue of their truth conditions and general Gricean principles that (1) and (2) are both taken as conventionally asserting that (1)--on the one hand, in (1) we have suggested that it is the content of the ex- pressed clause that is being meant$_{nn}$, and, on the other hand, more generally, it simply is pointless, qua assertion, to assert something as semantically unproblematic as the performative clause.

Recall now the question of speech act adverbials. We re- jected the Boer and Lycan-like proposal that they, in effect, "trigger" semantic interpretation of the performative or contex- tual features. We say, instead, that the adverbials "trigger" meaning$_{nn}$. That is, the adverbials tell the hearer that the speaker wants the hearer to attend to the (modified) semantic content of the context of utterance in understanding the utter- ance, since, in virtue of that very modification (that is, in virtue of the adverbial itself) the semantics of the context of utterance is no longer so trivial and uninformative.

To conclude, we have argued that the performadox can be solved if one treats performativity as a co-occurrence of indexi- cals and a meaning verb.[17] We have maintained that performatives have truth conditions and explained why they are what they are. We have argued that any utterance of a declarative sentence has (at least) two truth valuable objects in it and that this is a greatly unifying treatment of performatives and other declara- tives. Finally, we have maintained that the difference between an overt performative and its declarative counterpart is not one

of truth conditions, but of non-natural meaning. But let us be
clear about what we have not done. If Gazdar (1979:18) can be
taken as a fair summary of what "the strongest possible performa-
tive hypothesis requires,"[18] then we have accomplished our goal
by taking no position on or offering no support for virtually any
of the eight subclaims Gazdar isolates. In other words, on most
all of the really interesting claims made by the performative
hypothesis concerning, e.g., the nature of illocutionary force,
deletability, higher abstract verbs, we are silent. Whatever
success we have had, then, lies in an area which is not so much
a battlefield as it is merely unoccupied.

Footnotes

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the bad ones are our fault.

[1]But see Katz and Postal (1964:149 fn. 9). For more on the
performative hypothesis, see Sadock (1974) and the references
therein. It must be pointed out that the performative hypothesis
developed within the linguistic research program known as Genera-
tive Semantics, which proposed, among other things, the identity
of logical/semantic structure and deepest syntactic structure as
a working hypothesis. Thus, the performative hypothesis was
both a semantic and a syntactic proposal. We, frankly, are not
all that interested in "semantic representation" as such, being
more concerned with semantics as conditions for truth in a model
(cf. Gazdar 1979:30). About the syntactic part of the performa-
tive hypothesis we will have exactly nothing to say, and may be
characterized as friendly skeptics or agnostics. For discussion,
see Anderson (1971), Fraser (1971) and Mittwoch (1977). See also
the "pragmatic" approach of Leech (1980) and the discussion in
Davison (1982).

[2]There's lots more that could be said about the performadox,
and Boer and Lycan do say just about all of it, "in a paper which
is much too labyrinthine to be adequately paraphrased here"
(Gazdar 1979:34). Or here.

[3]Related ideas, which are in some ways perhaps even closer
to the view we will develop, are put forward by Récanati (1980).
Récanati actually has four truth valuable levels, not just two:
"the propositional content of a declarative locutionary act . . .
the propositional content of any locutionary act . . . the propos-
itional content of any illocutionary act belonging to the asser-
tive genus . . . the propositional content of any illocutionary
act" (1980:216). We ignore the second and fourth, which Récanati
uses in order to clarify some obscurities in the work of John
Searle, and which are not relevant to our concerns here. Récanati

distinguishes "the <u>locutionary act performed</u> in uttering a sen-
tence [which] is the <u>illocutionary act indicated</u> by this sentence,
whether it is actually performed or not . . . from the <u>illocu-</u>
<u>cutionary act performed</u> in uttering this sentence, whether it is
linguistically indicated or not" (1980:210). This distinction
is pressed into service when Récanati declares that "the performa-
tive utterance 'I state that the earth is flat' expresses <u>both</u>
the proposition that I state that the earth is flat <u>and</u> the prop-
osition that the earth is flat"; the former proposition being at
the locutionary level, and the latter at the illocutionary level
(1980:215). The example would be true at the locutionary level
and false at the illocutionary level. Récanati also usefully
(for our purposes) distinguishes between "the (generic) illocu-
tionary act corresponding to the illocutionary force roughly ex-
pressed by the sentence in virtue of its linguistic meaning, as
opposed to the (specific) illocutionary act corresponding to the
illocutionary force expressed by uttering that sentence in such
and such a context (1980:207). In these terms, our approach to
the performadox probably relates to generic indicated illocution-
ary acts, although we are quite frankly unsure about this, and
about what Récanati would actually say about either the performa-
dox or our solution to it.

[4]Spielmann (1980) might seem to be working the same side of
the street as ourselves, but examination of the two papers will
quickly dispel such suspicions. On the other hand, interesting
remarks are made by Silverstein (1978:7-10) in a quite different
context.

[5]Among these expressions with stable characters might be in-
cluded proper names, if viewed with sufficient naivete. This is
somewhat akin to Kripke's (1972) notion of "rigid designation."

[6]This picture is rather more Russellian (1905) than Fregean
(1892), since context-sensitive terms are what Kaplan calls
"directly referential" (1977:44) (DR). DR terms provide their
referents without mediation of the level of sense/content. While
it may seem rather unappealing to allow propositional content to
include individuals as well as intensions, we remind the reader
that "propositional content" is a technical, not an intuitive
notion, so there should be no problem in this context.

[7]McCawley (1982b) suggests that the Kaplan (1977) account of
"I am here now" and the like is not the correct one; he suggests
an alternative based on the work of Erving Goffman. However,
since McCawley has not, to our knowledge, fleshed out his sugges-
tion, and since we know nothing of the Goffman work, we cannot at
this time comment further.

[8]But see the discussion in Lewis (1972) and Lewis (1979).

[9]Gazdar (1979:20) notes
> G. Lakoff's suggestion . . . to allow an assignment coor-
> dinate . . . to specify the value of the variables standing
> for speaker, addressee, etc. and not have such items listed
> in the <u>n</u>-tuple to which the semantic interpretation is

> relativised. Dahl (1972:11) has argued that Lakoff's sug-
> gestion is incoherent: "It is absurd to eliminate the
> contextual coordinates for the assignment coordinate; the
> assignment coordinate is, on the contrary, determined by
> the contextual coordinates."

It should be obvious that we agree with Dahl as against Lakoff;
further, while Lakoff's and our proposals might appear to be
merely inverses of one another (the dread "notational variants"),
we do not think this is the case, and, in any event, Lakoff was
attempting much grander things than we are; see below.

[10]"Means something" is used in Grice's (1957) sense of "non-
natural meaning" (Ginet 1979:260 fn. 1).

[11]Austin (1962) seems to have held the opposing view on this
matter, as does Sadock (1974). There is more discussion, most
taking our side, in Kempson (1975), Harris (1978), Edmunson (1979),
Bierwisch (1980), Spielmann (1980), and Browne (1982).

[12]Note that here, as elsewhere, we are directly addressing
only what might be termed "limiting case" performatives, e.g. say
or declare. Other performatives, e.g. promise or pronounce, will,
we assume, have truth conditions that include those of the limiting
case as a sub-part.

[13]In the limiting case of say, an audience is, strictly
speaking, not necessary.

[14]Bach and Harnish (1979:225 ff.) claim that such utterances
are syntactically ill-formed and semantically uninterpretable.
They attempt a purely inferential/pragmatic account of their use.
We tend to think that if our account, or something like it, is
not right, then theirs, or something like it, probably is. Après
nous, le déluge.

[15]See Gazdar (1979:164-8) for several other pragmatic incur-
sions into truth conditional semantic territory.

[16]We like the semantics for adverbials provided by McConnell-
Ginet (1982), in which adverbials are taken as true "modifiers"
of verb meanings, rather than operators on verb phrases. See
(1982:175) for speech act adverbials.

[17]L. Aqvist, as described by Gazdar (1979:32-33), seems to
have done something not wholly unlike our work, but we cannot say
for sure, not having seen the original. Note that Gazdar finds
the Aqvist work unable to support a strong version of the perfor-
mative hypothesis, a finding we are not unsympathetic to vis a
vis our work.

[18]Gazdar's claims are the following (1979:18):

> (8) For Ǝ sentences S, in all natural languages, the deep
> structure of S
> (a) has a clause containing a PERFORMATIVE verb.
> (b) the subject of this clause is I and the indirect ob-
> ject is you. Ross (1970:passim)
> (c) this clause is the HIGHEST clause in the deep struc-
> ture.
> (d) this clause is DELETABLE when the verb is marked to

allow this (Ross 1970:249), and the deletion transfor-
mation is MEANING–PRESERVING (early generative seman-
tics assumption).

   (e) the verb in the clause is the ONLY verb in the sen-
       tence which is performative.

   (f) the verb represents the ILLOCUTIONARY FORCE of the
       sentence.

   (g) illocutionary force is SEMANTIC.

   (h) Ǝ is the UNIVERSAL quantifier.

In the discussion following our presentation, G. Lakoff asked how
our proposal differed from earlier versions of the performative
hypothesis, in particular his own. We note again that it is
largely in reference to the sorts of issues Gazdar isolates that
our proposal is unlike "full-fledged" performative hypothesis
proposals. That is, the performadox is formulated to be inde-
pendent of many of the issues in Gazdar's (a) – (h), e.g. whether
illocutionary force is semantic, whether there is one such force
per S, etc., and so is our solution to it. If a full-fledged
performative hypothesis can be shown to be true, then, of course,
our work is rendered otiose; on the other hand, by showing that
such a strong performative hypothesis is false, one has not there-
by touched our proposal. In a sense, we are offering the minimal
independently motivated theory we can to solve the performadox.
Putting it still another way, we are offering clam sauce without
the linguini (McCawley 1982a). Cf. also note 9 above.

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INDIRECT OBJECT ADVANCEMENT IN GERMAN [*]

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The aim of this paper is to motivate a rule of indirect object advancement (henceforth 3 to 2 advancement) in German.[1] Such a rule has been motivated for languages which include, among others, Indonesian (Chung 1976), Tzotzil (Aissen - to appear) and English (Perlmutter and Postal 1978). It will be shown that 3 to 2 advancement exists in German, although it is a highly restricted rule occurring with only a handful of predicates. I will show that a relational analysis of the following sentences includes an application of 3 to 2 advancement in that the underlined nominals are initial 3s which advance to 2.

1a) Man   fragt <u>mich</u>   die unmöglichsten Dinge.[2]
    one/NOM asks me/ACC the most impossible things/ACC
    'People ask me the most impossible things.'
1b) Der Professor   fragte <u>mich</u> alle Einzelheiten ab.
    the professor/NOM asked me/ACC all details/ACC   particle
    'The professor examined me on every detail.'
1c) Der Lehrer   hörte <u>mich</u> die Vokabeln   ab.
    the teacher/NOM heard me/ACC the vocabulary items/ACC part.
    'The teacher heard my vocabulary list.'
1d) Er   lehrt <u>unsere Kinder</u>   die deutsche Sprache.
    he/NOM teaches our children/ACC the German language/ACC
    'He teaches our children the German language.'

Since the subsequent discussion will be concerned mainly with the term GRs, I will begin with a listing of the characteristics of German 1s, 2s and 3s relevant for the purposes of this paper.

Characteristics of 1s
    a) Final 1s are marked NOM.
    b) Final 1s trigger person and number agreement on the verb.

Characteristics of 2s
    a) Final 2s are marked ACC.
    b) Only 2s can become 1s of passive sentences with the auxiliary <u>werden</u> (henceforth <u>werden</u>-passive), as in the following sentences.

2a) Das Mädchen   schenkte dem Jungen   ein Buch.
    the girl/NOM gave   the boy/DAT a book/ACC
    'The girl gave the boy a book.'
2b) Ein Buch   wurde dem Jungen von dem Mädchen geschenkt.
    a book/NOM AUX   the boy/DAT by the girl/PP given
    'A book was given to the boy by the girl.'
2c) *Der Junge   wurde von dem Mädchen ein Buch   geschenkt.
    the boy/NOM AUX by the girl/PP a book/ACC given
    'The boy was given a book by the girl.'

Characteristics of 3s
    a)   Final 3s are marked DAT.
    b)   3s which occur in a transitive stratum (one containing a 2 and a 1) with a predicate which allows a werden-passive can advance directly to 1 by what will henceforth be called the bekommen-passive.

The bekommen-passive
    Folsom (1963 and 1966) and Reis (1976) discuss the similarity between sentences such as 3) and werden-passives such as 2b).

    3) Der Junge   bekam von dem Mädchen ein Buch   geschenkt.
       the boy/NOM AUX   by the girl/PP a book/ACC given
   'The boy was given a book by the girl.'

The points of resemblance to werden-passives are both morphological (a. and b.) and distributional (c.)

    a.   The underlying subject appears as object of the preposition von, which likewise marks the passive chomeur in werden-passives.
    b.   The main verb appears as a past participle, as in werden-passives.
    c.   The set of verbs which allows the bekommen-passive is a subset of the set of verbs which allows the werden-passive.

The points of difference are:

    d.   The auxiliary verb is not werden but rather bekommen, kriegen, or erhalten.
    e.   The final 1 of the bekommen-passive corresponds to the DAT object (the 3) of the active (**compare** 2a and 3) while the final 1 of the werden-passive corresponds to the ACC object (the 2) of the active (compare 2a and 2b).

Thus relational networks of werden-passives contain the subnetwork:

    4)
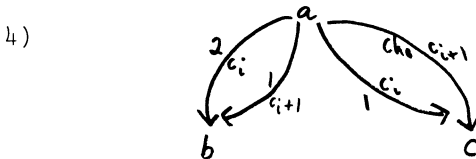
while relational networks of bekommen-passives contain the subnetwork:[3]

    5)
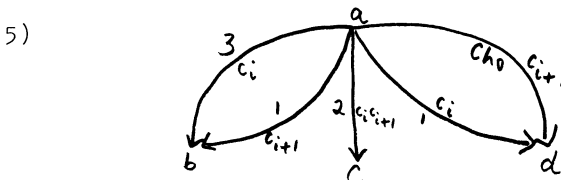
It should be mentioned at this point that the 3s advanced to 1 by the <u>bekommen</u>-passive are not necessarily <u>initial</u> 3s. The following sets of sentences involve <u>Possessor ascension to 3</u> (6), <u>Benefactive to 3 advancement</u> (7) and <u>Adversative to 3 advancement</u> (8). The a) forms of the sentences are those in which no advancement has taken place, the b) forms are those in which the various advancements to 3 have taken place, and the c) forms are the corresponding <u>bekommen</u>-passives. The <u>bekommen</u>-passive can therefore be used as a test for 3-hood but not for initial 3-hood.

6a) Er   legte die Hand   auf den Kopf   des Jungen.
     he/NOM laid the hand/ACC on the head/PP of the boy/GEN
6b) Er   legte dem Jungen die Hand   auf den Kopf.
     he/NOM laid the boy/DAT the hand/ACC on the head/PP
6c) Der Junge   bekam von ihm   die Hand   auf den Kopf
     the boy/NOM AUX by him/PP the hand/ACC on the head/PP
     gelegt.
     laid
     'He laid his hand on the boy's head.'
7a) Er   öffnete die Tür   für die Dame.
     he/NOM opened the door/ACC for the lady/PP
7b) Er   öffnete der Dame   die Tür.
     he/NOM opened the lady/DAT the door/ACC
7c) Die Dame   bekam von ihm   die Tür   geöffnet.
     the lady/NOM AUX by him/PP the door/ACC opened
     'He opened the door for the lady.'
8a) Der Vater   nahm das Spielzeug von dem Jungen weg.
     the father/NOM took the toy/ACC   from the boy/PP away
8b) Der Vater   nahm dem Jungen das Spielzeug weg.
     the father/NOM took the boy/DAT the toy/ACC   away
8c) Der Junge   bekam vom Vater   das Spielzeug
     the boy/NOM AUX by the father/PP the toy/ACC
     weggenommen.
     taken away
     'The father took the toy away from the boy.'

<u>Verbs with two accusative 'objects'</u>
    The small set of German predicates which occur with two nominal arguments in the accusative case includes at least four which can be shown to involve 3 to 2 advancement. These are <u>fragen</u> 'ask', <u>abfragen</u> 'ask, question thoroughly, examine', <u>abhören</u> 'hear'(as in 'hear someone's lessons') and <u>lehren</u> 'teach'. The first three will be discussed as a group, then <u>lehren</u> will be discussed along with two of its synonyms.

<u>Fragen, abfragen and abhören</u>
    Sentences 1abc) appear, at least superficially, to contain two direct objects. The Stratal Uniqueness Law (Perlmutter and Postal 1978) rules out the possibility that both objects are 2s on the same stratum. I claim that in each of these sentences <u>mich</u> 'me' is the initial 3 but the final 2 and that <u>die unmöglichsten Dinge</u>,

<u>alle Einzelheiten</u> and <u>die Vokabeln</u> are initial 2s but final chomeurs.

That <u>mich</u> is at some stage a 3 is indicated by the fact that all three sentences have <u>bekommen</u>-passives in which <u>ich</u> 'I' (the nominative form corresponding to accusative <u>mich</u>) is the final 1.[4]

9a) Ich bekomme die unmöglichsten Dinge   gefragt.
    I/NOM AUX   the most impossible things/ACC asked
    'I get asked the most impossible things.'
9b) Ich bekam vom Professor   alle Einzelheiten abgefragt.
    I/NOM AUX by the prof./PP all details/ACC   examined
    'I was examined on every detail by the professor.'
9c) Ich bekam vom Lehrer   die Vokabeln   abgehört.
    I/NOM AUX by the teacher/PP the vocab./ACC heard.
    'I had my vocabulary list heard by the teacher.'

That <u>die unmöglichsten Dinge</u>, <u>alle Einzelheiten</u> and <u>die Vokabeln</u> are not 3s on a transitive stratum at any stage is indicated by the fact that they cannot occur as final 1s of <u>bekommen</u>-passives.[5]

9d) *Die unmöglichsten Dinge bekommen mich gefragt.
        NOM   AUX   ACC
9e) *Alle Einzelheiten bekommen mich abgefragt.
        NOM   AUX   ACC
9f) *Die Vokabeln bekamen mich abgehört.
        NOM   AUX   ACC

That <u>mich</u> is at some stage a 2 is indicated by the fact that it is marked ACC in 1abc) and that all three sentences have <u>werden</u>-passives with <u>ich</u> 'I' as subject.[6]

10a) Ich werde die unmöglichsten Dinge   gefragt.
     I/NOM AUX the most impossible things/ACC asked
     'I am asked the most impossible things.'
10b) Ich wurde vom Professor   alle Einzelheiten abgefragt.
     I/NOM AUX by the prof./PP all details/ACC   examined
     'I was examined on every detail by the professor.'
10c) Ich wurde vom Lehrer   die Vokabeln   abgehört.
     I/NOM AUX by the teacher/PP the vocab./ACC heard
     'I got my vocabulary list heard by the teacher.'

That <u>die unmöglichsten Dinge</u>, <u>alle Einzelheiten</u> and <u>die Vokabeln</u> are 2s at some stage is indicated by the fact that they too may occur as subjects of <u>werden</u>-passives.

11a) ?Die unmöglichsten Dinge   werden mich gefragt.[7]
     the most impossible things/NOM AUX   me/ACC asked
     'The most impossible things are asked of me.'
11b) Alle Einzelheiten wurden mir   abgefragt.
     all details/NOM   AUX   me/DAT examined
     'Every detail was demanded of me.'

11c) Die Vokabeln   wurden mir   abgehört.
     the vocab. items/NOM AUX   me/DAT heard
     'My vocabulary list was heard.'

What is particularly interesting here is that the initial 3 has
the case marking of a final 3 in 11bc). This indicates that 3 to 2
advancement does not apply if the initial 2 advances to 1. This
holds for abfragen and abhören and also for lehren, as will be de-
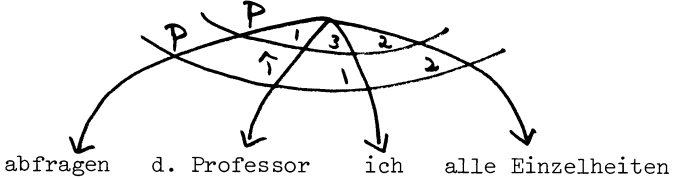monstrated shortly, but not for fragen.
    I suggest the following stratal diagrams for sentences 1b),
9b), 10b) and 11b).

1b')
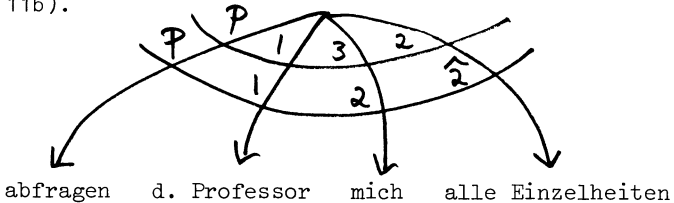
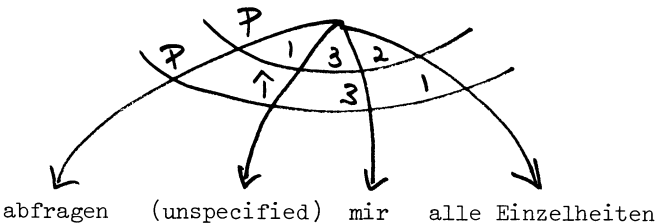abfragen   d. Professor   mich   alle Einzelheiten

    'Der Professor fragte mich alle Einzelheiten ab.'

9b')

abfragen   d. Professor   ich   alle Einzelheiten

    'Ich bekam vom Professor alle Einzelheiten abgefragt.'

10b')

abfragen   d. Professor   ich   alle Einzelheiten

    'Ich wurde vom Professor alle Einzelheiten abgefragt.'

11b')

abfragen   (unspecified)   mir   alle Einzelheiten
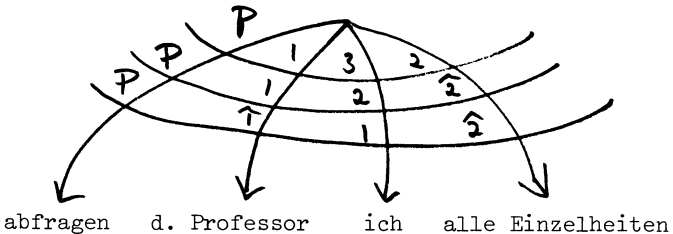
    'Alle Einzelheiten wurden mir abgefragt.'

Lehren 'teach' and its synonyms

 Lehren also belongs to the set of verbs with two ACC arguments
(12a). As with <u>abfragen</u> and <u>abhören</u> the initial 3 does not advance
to 2 if the initial 2 advances to 1 (12b). The <u>bekommen</u>-passive is
possible (12c). Finally, some speakers of German use <u>lehren</u> with a
DAT argument and an ACC argument in the active (12d).

  12a) Er lehrt unsere Kinder die deutsche Sprache.
     he/NOM teaches our children/ACC the German language/ACC
     'He teaches our children the German language.'
  12b) Die deutsche Sprache wird unseren Kindern (*unsere Kinder)
     the Ger. lang./NOM AUX our children/DAT (our children/ACC)
     von ihm gelehrt.
     by him/PP taught
     'The German language is taught to our children by him.'
  12c) Unsere Kinder bekommen von ihm die dt. Sprache gelehrt.
     Our children/NOM AUX by him/PP the Ger. lang./ACC taught
     'Our children get taught the German language by him.'
  12d) Er lehrt unseren Kindern die deutsche Sprache.
     NOM   DAT   ACC
     'He teaches our children the German language.'

Some speakers use the construction exemplified in 12a) while others
use the construction in 12d), which seems to indicate that 3 to 2 ad-
vancement is optional with <u>lehren</u>, although it does not seem to be
optional for individuals.
 The animate object can advance to 2 and then to 1 if and only if
the initial 2 is a subordinate clause.

  12e) *Unsere Kinder werden von ihm die deutsche Sprache gelehrt.
      NOM   AUX PP   ACC
  12f) Unsere Kinder werden gelehrt, die Wahrheit zu sagen.
      NOM   AUX   the truth to tell
     'Our children are taught to tell the truth.'

 That <u>unsere Kinder</u> in 12a) is a 3 at some point is indicated by
the DAT marking of 12b) and 12d) and by the final 1-hood of <u>unsere
Kinder</u> in 12c). That it is a final 2 is indicated by its ACC marking
and by the fact that it can advance to final 1 of a <u>werden</u>-passive
under conditions just specified, cf. 12f). 3 to 2 advancement has
therefore applied in 12a).[8]
 It is interesting to compare <u>lehren</u> with its synonyms <u>beibringen</u>
and <u>unterrichten</u> since the three predicates have different final con-
figurations of arguments. A comparison enables us to test the
claim that initial stratal GRs are predictable on universal semantic
grounds (cf. Perlmutter 1979), since a corollary of that claim is
that predicates with the same meaning assign the same initial GRs to
their arguments.

  12a) Er lehrt unsere Kinder die deutsche Sprache.
     NOM   ACC   ACC

13a) Er bringt unseren Kindern die deutsche Sprache bei.
     NOM   DAT   ACC

14a) Er unterrichtet unsere Kinder in der deutschen Sprache.
     NOM   ACC   PP

Beibringen occurs with a final 3 and a final 2. Both werden- and
bekommen-passives of 13a) are possible.

13b) Unsere Kinder bekommen von ihm die dt. Sprache beigebracht.
        NOM   ACC

13c) Die deutsche Sprache wird unseren Kindern von ihm beigebracht.
      NOM   DAT

3 to 2 advancement does not apply.

13d) *Er bringt unsere Kinder die deutsche Sprache bei.
     NOM   ACC   ACC

Unterrichten occurs in a variety of constructions. When those
being taught are present in the sentence, that being taught appears
as the object of the preposition in 'in', cf 14a). If those being
taught are not present, that being taught appears in the accusative
case with no preposition, cf. 14b), but if the instructee is present,
it appears in the accusative case regardless of the presence or ab-
sence of the object of the instruction, cf. 14a) and 14c).

14b) Er   unterrichtet die deutsche Sprache.
     he/NOM teaches   the German language/ACC

14c) Er   unterrichtet unsere Kinder.
     he/NOM teaches   our children/ACC

I claim that 3 to 2 advancement applies in 14a) and 14c). Unter-
richten differs from fragen, abfragen, abhören and lehren in that
the 2-chomeur resulting from 3 to 2 advancement is not marked ACC
but rather is marked (idiosyncratically?) with the preposition in.
ACC marking of the chomeur renders the sentence ungrammatical.

14d) *Er unterrichtet unsere Kinder die deutsche Sprache.
     NOM   ACC   ACC

Unterrichten occurs in a variety of passive constructions.[9]

15a) Die deutsche Sprache wird von ihm unterrichtet.
        NOM   AUX   PP   taught
     'The German language is taught by him.'

15b) Unsere Kinder werden von ihm unterrichtet.
        NOM   AUX   PP   taught
     'Our children are taught by him.'

15c) Unsere Kinder werden von ihm in der dt. Sprache unterrichtet.
        NOM   AUX   PP   PP   taught
     'Our children are taught the German language by him.'

15d) Unsere Kinder bekommen von ihm die dt. Sprache unterrichtet.
    NOM   AUX   PP   ACC   taught
    'Our children get taught German by him.'

The final 1s of the <u>werden</u>-passives 15a) and 15bc) (<u>die deutsche
Sprache</u> and <u>unsere Kinder</u> respectively) are 2s on a non-final
transitive stratum. On the other hand, 15d) indicates that <u>unsere
Kinder</u> is at some point a 3 on a transitive stratum. This there-
fore indicates strongly that in 14a) <u>unsere Kinder</u> is the initial
3, final 2 and that <u>in der deutschen Sprache</u> is the initial 2,
final 2-chomeur as a result of the advancement of the initial 3 to 2.
<u>Lehren</u>, <u>beibringen</u> and <u>unterrichten</u>, then, all have initial strata
with a 1, a 2 and a 3, despite their quite different final strata.

Summary
    I have shown that a rule of 3 to 2 advancement exists in Ger-
man, that some 2-advancement chomeurs are marked ACC, hence
camouflaged as final 2s, that at least one 2-advancement chomeur
occurs as a prepositional phrase, and that 3 to 2 advancement
interacts with 3 to 1 advancement (<u>bekommen</u>-passive) and 2 to 1 ad-
vancement (<u>werden</u>-passive). 14c) shows that 3 to 2 advancement can
apply even when there is no initial 2. The result is a superficially
transitive sentence. This may well be the case with predicates
other than <u>unterrichten</u>. This, taken together with the prepositional
marking of the chomeur in 14a), which also may well occur with
other predicates, may be an indication that 3 to 2 advancement
occurs with more predicates than those I have discussed here.

Footnotes

1. This paper is written in the framework of Relational Grammar
as presented in Perlmutter and Postal (1977, 1978) and Perlmutter
(1979, 1980).

2. NOM "Nominative", ACC "Accusative", DAT "Dative", GEN "Genitive"
and PP "Prepositional Phrase" refer to grammatical cases and the
morphological marking thereof. AUX is used in this paper to refer
exclusively to <u>passive</u> auxiliaries.

3. Reis (1976) and Edmondson (1978) state that the <u>bekommen</u>-passive
promotes 3s even from non-transitive strata, e.g.

3.1. Er   kriegte von allen gedankt,
     he/NOM got   by all   thanked
     'He was thanked by all,'

corresponding to the active

3.2. Alle   dankten   ihm.
     all/NOM thanked   him/DAT
     'Everyone thanked him.'

The bekommen-passive seems to vary from individual to individual
and from region to region. People also seem to be told at some
point in their education that they should not use it, and the
memory of this admonition affects grammaticality judgments as well.
My informants do not accept the bekommen-passive unless a 2 is also
present on the departure stratum, so they do not accept 3.1. Nor
do they accept bekommen-passives in which a 2 rather than a 3 ad-
vances to 1. Such passives are also claimed by Reis and Edmondson
to be grammatical.

4. GP and MR accept these sentences. WV does not. He also does
not accept 6c), 7c) and 8c). This bears out the observation in the
previous footnote. All three informants are from the Rhineland,
yet WV's use of the bekommen-passive is relatively restricted in
comparison with GP's and MR's, and MR's use of it is more restricted
than GP's. I therefore cannot and consequently do not claim that
all of the sentences in this article are accepted by all speakers
of German. They are all accepted by GP and most are accepted by
MR. WV seems to accept bekommen-passives only when bekommen 'get,
receive' can be understood literally. This also seems to be the
case with at least some Swiss and Austrian speakers.

5. It cannot be argued that any verb with two ACC arguments can
advance one of them to 1 via the bekommen-passive. For instance,
the verb nennen 'name, call' appears with two ACC arguments, cf.
9g), but has only a werden-passive, cf. 9j):

9g) Er   nannte mich   einen Idioten.
     he/NOM called me/ACC an idiot/ACC
9h) *Ich bekam von ihm einen Idioten genannt.
     NOM AUX by him   ACC   called
9i) *Ein Idiot bekam mich von ihm genannt.
         NOM   AUX   ACC
9j) Ich wurde von ihm ein Idiot   genannt.
     I/NOM AUX by him an idiot/NOM(!) called
     'I was called an idiot by him.'

Nor can it be argued that the bekommen-passive advances animate
objects to 1 whenever such objects occur with a second object.
Verbs which occur with an ACC object and a GEN object do not have
bekommen-passives, although they may have werden-passives.

9k) Man   bezichtigte ihn   des Mordes,
     one/NOM accused   him/ACC murder/GEN
     'Someone accused him of murder.'
91) Er   wurde des Mordes bezichtigt.
     he/NOM AUX   murder/GEN accused
     'He was accused of murder.'
9m) *Er bekam des Mordes bezichtigt.
     NOM AUX   GEN

6. GP and MR accept all of these sentences. WV accepts only 10a).

7. I have marked 11a) with ? because there is a problem involved
with the case of the animate object. If the object is omitted then
the resulting sentence 'Die unmöglichsten Dinge werden gefragt' is
perfectly acceptable, so that the point I am trying to make is still
valid, i.e. that die unmöglichsten Dinge is a 2 on a transitive
stratum at some stage. GP accepts 11a) and rejects it if mir is
substituted for mich. MR initially found it difficult to decide
between mich (ACC) and mir (DAT). She was not particularly happy
with either, although less unhappy with mich. When uns 'us', which
has exactly the same morphological form in both ACC and DAT, was
substituted, she accepted the sentence without hesitation. WV does
not accept any of the sentences in 11). I am at somewhat of a loss
to account for the phenomena illustrated in 11). I feel that GP's
acceptance of 11a) somehow hangs together with WV's acceptance of
10a) mentioned in the previous footnote, but what the connection
is I do not know. Another informant (from Schleswig-Holstein) to
whom I put the sentences in 11) also reacted exactly as did GP,
although she rejected many of the other sentences in this paper,
including many accepted by WV. John Newman (personal communication)
has suggested that 11a) is accepted by some people on the basis of
syntactic analogy with sentences like:

     Die unmöglichsten Dinge werden mir gesagt.
     'The most impossible things are said to me.'

and that 11a) is not really grammatical. Certainly it is unexpected,
whereas 11bc) are not. Another possible account of 11a) is that
3 to 2 advancement applies subsequent to the application of 2 to 1
advancement. This certainly accounts for the facts, but how one
would go about demonstrating it I do not know.

8. One occasionally encounters in grammars of German the pro-
position that German abhors two accusatives and tends to avoid the
situation either by using a different verb, e.g. beibringen as in
13), or by changing one of the accusatives to a dative, as in 12d).
So for instance Grebe (1973:514): "Da sich das Sprachgefühl gegen
die Hinzufügung zweier Akkusativobjekte zu einem Verb wehrt, weicht
man ... immer wieder auf die allgemein übliche Grundform mit
Dativ- + Akkusativobjekt aus." While the abhorrence theory might
account for 12d), it does not account for the DAT in 12b), 11b) and
11c), where there is no ACC to trigger the 'change' from ACC to DAT.

The abhorrence theory seems to me to be saying that 12d) contains one more stratum than 12a), while I claim that the opposite is the case.

9. MR and WV do not accept 15d), while it is accepted by GP and some other native speakers I have asked. This is consistent with the observation in footnote 4.

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HOW DO YOU SAY *IT* IN RUSSIAN?
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The aim of this paper is to discuss the English pronoun *it*
and its correspondents in Russian, i.e., "how to say *it* in Rus-
sian." In order to avoid misunderstanding, it should be kept in
mind that the focus here is not on the **interpretation** of pronouns,
but on the **selection** of pronouns — i.e., not how the reference of
a given pronoun should be construed or can be determined, but, ra-
ther, given that a pronoun is needed to fill a specific slot, which
one is appropriate?[1] Accordingly, this paper makes no claims as
to how and where in the grammar pronouns originate. It remains
neutral in the debate which has been carried on in the transfor-
mational literature as to whether pronouns arise by a rule of pro-
nominalization which turns underlying full NPs into pronouns in the
derived structure (cf. Lees and Klima 1963, Langacker 1969, among
others), whether they originate in the base and are thus part of
deep structure and must receive semantic interpretations which as-
sociate them with their antecedents (cf. Dougherty 1969, Jacken-
doff 1972, among others), or whether NPs in underlying structure
contain only referential indices — the argument nouns being sup-
plied from outside the clause — with pronouns serving to fill an
NP identified by a second or subsequent occurrence of the same in-
dex (cf. McCawley 1970, Wasow 1975, among others). The present
discussion is compatible with any of these approaches: all of
these, as well as any other abstract analysis, must eventually
face the question of how to fill the slot which at some point and
through some process has been assigned to a pronoun; non-abstract
analyses, too, must account for why a given pronoun form is ob-
served, and not another. It is hoped that the present paper will
contribute to the understanding of the Russian and English pronoun
systems, and that, through the contrastive approach, it will ad-
vance the understanding of pronoun systems in general by shedding
light on the factors which can serve as bases for distinctions.
Before proceeding, I should make certain terms and assump-
tions clear. The term **ANAPHORA** will be used in its strict sense
to mean "the use of a grammatical substitute to refer to a prece-
ding or previously introduced word or group of words," and not in
some of the looser senses that have appeared in some recent work.
Thus, assuming a normal context for 1 and 2, *it* in 1 would be ana-
phoric, while *it* in 2 would not.

1. It was leaking.
2. It was raining.

An **anaphoric pronoun** thus substitutes for and refers to a previ-
ously introduced N or NP, while a pronoun which stands for some N
or NP which has not been previously introduced is **non-anaphoric**.
I distinguish, also, between the **antecedent** and the **referent** of an

anaphoric pronoun; the ANTECEDENT is the earlier mention of a par-
ticular N or NP, the reintroduction of which gives rise to an ana-
phoric pronoun, while the REFERENT is the absent reiteration of
that N or NP, which is replaced by the anaphoric pronoun. That
these two are not always the same can be seen from 3 and 4.

3. Roger saw $\overset{\text{antecedent}}{\text{a movie}}$, and I saw $\overset{\text{anaphor}}{\text{one}}$ $\overset{\text{referent}}{(=a \ movie)}$, too.

4. Roger saw $\overset{\text{antecedent}}{\text{a movie}}$, and I saw $\overset{\text{anaphor}}{\text{it}}$ $\overset{\text{referent}}{(=the \ movie)}$, too.

The antecedent and referent NPs in 3 are formally identical, and
both are indefinite; in 4, however, they are not formally identi-
cal, the antecedent being indefinite while the referent is defi-
nite. Both 3 and 4 are cases of anaphora, but since the truth-
conditional meanings of 3 and 4 are different, we must conclude
that the distinction between the antecedent and the referent is
a significant one. This is only one illustration of various pos-
sible differences between the antecedent and the referent. Final-
ly, following the practice of assigning features to lexical items
as in, e.g., Katz and Fodor (1963) and Fillmore (1969), I regard
pronouns as being sets of features which are summed up in lexical
items, and have proposed analyzing pronoun selection as a process
of matching the features expressed by the referent with the fea-
tures expressed by the various anaphoric pronouns which the lan-
guage makes available. Some examples of this approach can be seen
in Channon 1980, Channon 1982, and Channon in press. With these
preliminaries in mind, we can move on to consideration of *it* and
its Russian correspondents.

Textbooks and dictionaries of Russian regularly equate the
English word *it* with the Russian *on/ona/ono* (and/or their case
forms).[2] While *on* often does correspond to *it*, there are many
places where they do not correspond. In particular, English *it*
is frequently represented by Russian *èto* (which is usually glossed
as 'this' or 'this/that'), and it is also frequently represented
by Ø in Russian. Each of the Russian forms, in turn, may also
correspond to things other than *it* in English.

In the other work referred to above (1980, 1982, in press) I
have discussed the question of pronoun selection in terms of fea-
tures which must be matched up with the referent (not the antece-
dent) of the pronoun. Among the features relevant to the pronouns
under discussion here are *definite/indefinite, discrete/nondiscrete,
singular/plural, masculine/feminine/neuter, animate/inanimate*, etc.
A partial specification of Russian *on* and English *it* is given in 5.
While the specification for *it* may turn out to be sufficient, the
specification given there for *on* is not, as will be seen later.

5. Russian   *on*:   [definite] [discrete] [singular] [masculine]

              ona: [definite] [discrete] [singular] [feminine]

English *it*: [definite] [discrete] [singular] [inanimate]

Some simple examples showing the correspondence of *on* and *it*
are given in 6-8. Russian nouns have grammatical gender and so
we see three separate forms, *on/ona/ono*, according to whether the
referent noun is masculine, feminine, or neuter.

6. Ja pročital ètot roman včera. *On* (*èto, *∅) očen'
        interesnyj.
     I read that novel yesterday. *It*'s very interesting.
7. Ja pročital ètu knigu včera. *Ona* (*èto, *∅) očen'
        interesnaja.
     I read that book yesterday. *It*'s very interesting.
8. Ja pročital èto pis'mo včera. *Ono* (*èto, *∅) očen'
        interesnoe.
     I read that letter yesterday. *It*'s very interesting.
9. Vy pročitali ètot roman včera? *Èto* (*on/*ona/*ono) očen'
        interesno.
     You read that novel yesterday? *That*'s very interesting.

Note also that the pronoun *èto* cannot be used in any of these
cases, and that ∅ is likewise not possible. There **is** a grammati-
cal use for *èto* in an environment like this, but the antecedent
and the referent (and thus the meaning) are quite different, as
seen in 9. 9 means not that **the novel** is interesting, but rather
that **the fact that the addressee read the novel** is interesting.
No form of *on* can be used here, and the observed *èto* is invariant,
regardless of the gender of the noun representing the thing which
was read.[3] This type of sentence will be discussed further below.
Because *on* is often inappropriate as an anaphor for abstract
or intangible referents, it is sometimes suggested that *on* can
refer only to nouns representing concrete or tangible entities,
and that *èto* is used for other cases instead. The examples in
10-11, among many others, illustrate that this is not so, and that
concrete or *tangible* are not part of the specification of *on*.

10. Beregite svobodu; *ona* (*èto/èta, *∅) doroga.
      Protect freedom; *it*'s precious.
11. Pravil'nost' ètogo podxoda k probleme dokazana ne tem, čto
        vse *ego* (*èto) prinjali. *Ona* (*èto) dokazana tem, čto
        on (*èto) daet vernye rezul'taty.
     The correctness of this approach to the problem is shown
        not by the fact that everyone has accepted *it*. *It* is
        shown by the fact that *it* gives accurate results.
12. Čto èto? *Èto* (*ona) kniga.
     What's that? *It*'s/*That*'s a book.
13. Ja včera pročital novuju knigu Solženicyna. *Èto* (*ona/*on)
        očen' interesnyj roman.
     I read Solzhenitsyn's new book yesterday. *It*'s a very
        interesting novel.

14. Gde moja kniga? *Ona* na stole.
    Where's my book? *It*'s on the table.
15. Počemu vy dumaete, čto takoj mašiny net? *Ona* est'. Ja
        ee videl.
    Why do you think that there is no such machine? *It* exists.
        I saw *it*.
16. Èto (*ono*) ne vaše delo.
    It's/*That*'s not your affair.

On the other hand, some seemingly straightforward cases of
English *it* where the referent is a simple concrete tangible noun
do **not** allow the use of *on* in Russian. Some examples of this can
be seen in 12-13 above, which can appear only with *èto*. In gene-
ral, the occurrence of *on* is very restricted in sentences with
equational-*be* (i.e., sentences of the form 'NP *be* NP,' e.g., 12-
13), though it occurs freely in attributive-*be* sentences (of the
form 'NP *be* ADJ,' cf. 6-8), locational-*be* sentences (of the form
'NP *be* LOC-PHRASE,' cf. 14), or existential-*be* sentences (of the
form 'NP *exist*,' with or without anything following, cf. 15).[4,5]
The examples in 9, 12, and 16 show that *on* requires an explicitly-
stated and named antecedent, an overt noun. A non-linguistic an-
tecedent, along the lines discussed by Hankamer & Sag (1976),
will not do in Russian as an antecedent for *on*, nor will a sen-
tential antecedent, though they are permissible antecedents for
èto. In 9, 12, and 16 there is, of course, an antecedent, but
it is either not named, not a noun, or both.
   While I ascribe the use of *èto* rather than *on* in 13 — even
though there is a named noun antecedent — at least in part to
the fact that the second sentence of 13 is an equational-*be* sen-
tence, there is nevertheless some kind of connection between the
acceptability of *on* and concreteness/tangibility — or perhaps
the relevant feature is more properly *specific/nonspecific* — as
can be seen by comparing 13 with 17 below; 17 is an example very
similar in form to 13, but quite different in meaning.

17. Ja včera pročital novuju knigu Solženicyna. *Ona* očen'
        interesnyj roman Dostoevskogo.
    Yesterday I read Solzhenitsyn's new book. *It*'s a very
        interesting novel by Dostoevsky.

In 13 the referent of *èto* — and the thing which is an interest-
ing novel — is not the tangible book itself, but rather the
contents of that book (and of potentially many others like it),
and thus the putative antecedent *kniga* in the preceding sentence
may not correspond to the referent of the pronoun in the senten-
ce that follows. In 17, on the other hand, what is being referred
to is not the intangible new "book" that Solzhenitsyn has writ-
ten, but rather a physical book which he newly possesses, and
which happens in this instance to be a novel by Dostoevsky; i.e.,
a form of *on* is indeed possible in this sentence just in the

case where *kniga* is taken not as the **contents** of the book, but as **the book itself.** Thus 13, too, really has two possible readings; on the tangible reading of *book*, it is an appropriate antecedent, and *on* can be used as an anaphor, but on the intangible reading, *book* is not an appropriate antecedent (does not match the referent), and *on* does not appear.

The example in 18 illustrates the fact that the antecedent or referent of *on* cannot be an S.

18. Ivan skazal, čto on poedet, no ja ètomu (*emu) ne verju.
    Ivan said that he will go, but I don't believe *it/?that.*
19. Ivan skazal, čto on poedet, no ja emu (*ètomu) ne verju.
    Ivan said that he will go, but I don't believe *him (*it).*

The only pronoun which can appear here in the meaning "but I don't believe it" is a form of *èto* (*ètomu*), and not a form of *ono* (*emu*). The ungrammaticality of *emu* in this reading should not be confused with the grammaticality of *emu* in a totally different reading (cf. 19), in which *ètomu* is ungrammatical. That is the reading which in English would be "but I don't believe **him,**" and the antecedent is then not the subordinate clause, but *Ivan*, and that is why a form of *on* is possible. In English, of course, *it* can without difficulty stand for an entire clause, as in the English version of 18.

In 20-21 we see instances in which English *it* corresponds to Ø in Russian.

20. I hear *it*'s coming out soon, your book.
    Ja slyšal, čto Ø (*ona) skoro vyjdet vaša kniga.
21. Your book, I saw *it* on the table.
    Vašu knigu ja videl Ø (*ee) na stole.
22. Ja slyšal, čto ona skoro vyjdet, vaša kniga.
    I hear *it*'s coming out soon, your book.
23. Gde moja kniga? Vaša kniga? *Ona* na stole.
    Where's my book? Your book? *It*'s on the table.
24. Gde moja kniga? Vaša kniga? Ja ee videl na stole.
    Where's my book? Your book? I saw *it* on the table.
25. Vy ne videli moju knigu? Vašu knigu? Ja ee videl na stole.
    Have you seen my book? Your book? I saw *it* on the table.

The *it* in these examples (20-21) in English is not really anaphoric. It is, rather, a place filler for a dislocated or displaced (but not deleted) NP, which appears elsewhere in the sentence. As such, it is an artificially-induced element in English, called forth by the requirements of English word order; for example, the subject must come before the verb, and if it is moved, say, by right dislocation, it must leave behind a dummy pronoun to satisfy this constraint of English sentence structure. Russian, on the other hand, has no such word order requirement, and the elements of a sentence can appear in almost any surface order,

primarily regulated only by pragmatic considerations. It is thus
entirely expectable that, under conditions of movement within the
clause, no pronoun copy will be left behind, since none is needed
to satisfy any surface word order constraint. In fact, if a copy
were left behind in 20-21 it would be more than unneeded: it would
violate a rule of Russian clause structure which allows only one
subject, one object, etc., per clause at a given time. The situ-
ation in 20-21 should not be confused with that in 22-25, where
a form of *on* can appear; in these examples the NP for which *on* is
an anaphor stands outside the clause in which *on* appears; this is
indicated above by punctuation, but the real evidence is in the
intonation patterns, where 22 differs sharply from 20 and 23-25
differ from 21, and, in the case of 24, in the morphology, since
the nominative *vaša kniga* must be an echo question formed from
the preceding sentence (cf. 23 vs. 25), rather than a dislocated,
topicalized, or "previewed" direct object from the following sen-
tence.

The sentences in 26-27 are parallel to those in 20-21, except
that the dislocated material is not an NP, but an entire S.

26. *It* seems to me that he is right.
    Ø (**èto*) mne kažetsja, čto on prav.
27. *It* is interesting to note that he is still here.
    Ø (**èto/*ono*) interesno zametit', čto on ešče zdes'.

These are, of course, examples of the type which has been called,
among other things, extraposition. To the extent that a corres-
ponding process exists in Russian, it operates without leaving
behind anything to serve even as a nominal subject; no form of
èto, *on*, or any other pronoun can appear here.

It can be seen, then, that English *it* has a number of differ-
ent kinds of uses, and that no single Russian pronoun covers all
of them. Russian *on*, the traditionally-cited equivalent of Eng-
lish *it*, shares only some of them, with (at least) *èto* and Ø also
serving as correspondents of *it* under certain syntactic conditions.
In particular,

   English *it* corresponds to Russian *on* when it is used as an
   anaphoric pronoun whose referent is an NP headed by an N;
   Russian *on* must have an overt antecedent (which must be a
   noun), its referent must be absent, and it cannot stand for
   an S;

   English *it* corresponds to Russian *èto* when it is used as an
   anaphoric pronoun standing for an S, or standing for an ab-
   sent referent when there is not an overtly-named linguistic
   antecedent; generally (though not always) *èto* is used in
   sentences with equational-*be*;

   English *it* corresponds to Russian Ø when it is used non-

anaphorically (in at least one such usage — there may be others which behave differently) as a dummy or filler pronoun standing for a displaced but not absent referent and having no antecedent at all.

At least in Russian, then, we will need some kind of additional features or other means to specify such information as whether or not the pronoun has an antecedent, whether the antecedent has been overtly named, whether the referent is present or absent, and what the linguistic structure of the antecedent is (i.e., whether it is a simple NP or an S).

It also seems clear that we must distinguish among the various different uses of English *it* dealt with here, and perhaps among some others not treated here as well, since these functions are differentiated in some other languages. Finally, although the focus of this paper has been on lexical selection to fill a given pronoun slot, the facts observed here suggest that there will have to be corresponding mechanisms in systems for the interpretation of pronouns which can take account of the kinds of distinctions that have been brought out, i.e., which can correctly distinguish between such pairs of examples as 13 and 17, 18 and 19, etc.

FOOTNOTES

[1] While a great deal has been written on the origin and/or interpretation of pronouns in generative/transformational-type analyses, very little has been done on the choice of surface realizations to fill the slot allotted to a pronoun, and studies of pronouns often tacitly assume that the correct form will somehow appear when and as necessary.

[2] *On/ona/ono* are the forms for the different genders (masculine, feminine, and neuter, respectively) of the 3rd person singular pronoun. Since the distinction among the genders and among the various case forms is not relevant for the discussion at hand, I will, for simplicity, refer to any or all of them collectively as *on* in the rest of this paper.

[3] Of course, the normal English pronoun found here is not *it*, but *that*.

[4] I have no good explanation at this time for why *on* should tend not to occur in equational-*be* sentences, or for why equational-*be* sentences should be different from other kinds of sentences with *be* or from other kinds of sentences in general with regard to this particular factor (occurrence of *on*). It should be noted, also, that *on* is not entirely prohibited in such sentences, cf. i, where both èto and *ona* (from *on*) are possible, with no apparent difference in meaning.

i. Ja včera poznakomilsja s vašej sestroj. *Èto/Ona* očen'
   interesnyj čelovek.
   I met your sister yesterday. *She*'s (??*That*'s) a very
   interesting person.

The status of *on* in equational-*be* sentences remains a topic for
further investigation.

5   A detailed treatment of different types of *be*-sentences in
Russian can be found in Chvany (1975).

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