THE SYNTAX OF PHONOLOGY

by

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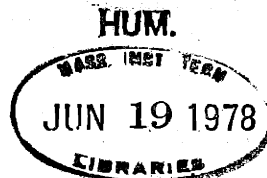DOCTOR OF PHILOSOPHY

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ABSTRACT

This study is concerned with the proper modes of syntactic influence in phonology. It is proposed and proven that this influence is direct, not mediated by string boundaries. A contextually applicable constituency predicate is developed to handle certain phonological rules applicable at gross phrasal levels which clearly have syntactic conditioning. Instanced are French <u>liaison</u>, Hebrew accentuation, and Italian <u>raddoppiamento</u>. A special case of the required notion of constituency is standard level theory. Thus a general account of domains of application results.

There are certain structural configurations which universally, it is suggested, block rules of a certain independently specifiable type. Blockage of rules across trace, parenthesis, and edge of sentence is blamed on a single structural defect, so that rule failure at any one of the three will entail failure at the other two. Extensive case studies: English phrasal phonology and the Irish initial mutations. Finally, a single rule of English, final dental palatalization, is inspected in detail as an example of a rule not subject to any lexico-syntactic conditioning, and predictably so: The rule is conditioned by prosodic structure instead.

A recurrent theme is the inadequacy and inappropriateness of terminal boundary symbols as a segmental means of reflecting non-segmental information.

Thesis Supervisor: Noam Chomsky

Title: Professor of Linguistics

For the things which are seen are temporal;
But the things which are not seen are eternal.

                              2 Cor. iv 18.

The omission or the addition of one letter
might mean the destruction of the whole world.

                              Talmud.

The world is nothing but a big gimmick.

                              Hendrix.

Téora seithir góa: Bés, dóig, toimte!

To Albert Hofmann

ACKNOWLEDGEMENTS

CONTENTS

INTRODUCTION

This thesis began as a collection of smallish, more or less discrete studies in the general area suggested by the rather vague title—syntax-phonology interactions. As my understanding of the several topics grew, or seemed to grow, so did their mutual dependence and relevance, until the conclusion I was reaching about each one became tied up in a really organic way with the conclusions I found myself reaching about the rest.

I think it would be wrong to say that the four parts of this thesis, taken together, go to show any one coherent thing. However, it develops that there is an overriding theme. In each of the individual problems I examined, a fairly natural account in terms of word boundaries in the manner of Chomsky and Halle (1968) (henceforth SPE) immediately offered itself. Upon closer inspection, it turned out to be wrong. In fundamental, though unobvious respects, it looked like boundaries were not doing what they were supposed to be doing. I began to wonder what boundaries did right. It now seems to me that they and all other boundary symbols are dispensible, and in fact rather misleading notational devices at best.

Let us consider briefly what a word boundary is. A word boundary, in SPE theory, is a way of encoding terminally certain selected aspects of pre-terminal syntactic structure. We should know, first, exactly what the boundary conventions do.

SPE is quite clear, to a point:

BC I: The boundary # is automatically inserted at the beginning and end of every string dominated by a major category, i.e. by one of the lexical categories "noun," "verb," "adjective," or by a category such as "sentence," "noun phrase," "verb phrase," which dominates a lexical category.

(SPE, p. 366.)

Operating in conjunction with BC I is a convention of #-telescoping, unfortunately never openly enunciated in SPE. As interpolated by Selkirk (1972), it goes

BC II: In a sequence $W \#]_X \#]_Y Z$ or $W [_Y \# [_X \# Z$,

where $Y \neq \bar{S}$, delete the "inner" word boundary.

(Selkirk, p. 12.)

An example.

(1) John has a large trout in his pocket.

Let's suppose that this sentence comes with the bracketing in (2).

(2) $[_{\bar{S}} [_S [_{\bar{N}}$ John $]_{\bar{N}} [_{\bar{V}} [_{\bar{V}} [_V$ has $]_V]_{\bar{V}} [_{\bar{N}} [_{DET}$ a $]_{DET} [_{\bar{N}}$

$[_{\bar{A}} [_{\bar{A}} [_A$ large $]_A]_{\bar{A}}]_{\bar{A}} [_N$ trout $]_N]_{\bar{N}}]_{\bar{N}} [_{PP} [_P$ in $]_P$

$[_{\bar{N}} [_{DET}$ his $]_{DET}]_{PRO} [_{\bar{N}}$ pocket $]_N]_{\bar{N}}]_{\bar{N}}]_{PP}]_{\bar{V}}]_S]_{\bar{S}}$

BC I provides for boundaries as in (2)'.

(2)' [#[#[#John#] [#[#[#has#]#]#]#[# [a] [#
S S N   N V V V   V V N DET   DET N

[#[#[#large#]#]#] [#trout#]#]#] [#[in]
A A A   A A A N   N N N PP P   P

[# [[his]] [#[#pocket#]#]#]#] #]#]#]
N DET PRO   PRO DET N N   N N N PP V S S

BC II disposes of most of these, resulting in (2)".

(2)" [#[#[John#] [#[[has]#] [# [a] [#
S S N   N V V V   V V N DET   DET N

[[[large]]#] [#trout]]#] [#[in]
A A A   A A A N   N N N PP P   P

[# [[his]] [#[pocket]]]]]#]#]
N DET PRO   PRO DET N N   N N N PP V S S

BC I and BC II, together, allow a three-way junctural
contrast. At any given spot in a terminal string preceding or
following all the segments of a word, we will either find ##,
#, or no word boundaries at all. As I prove in Part III, it
transpires that one word boundary separates two adjacent
words a and b dominated by immediately pre-terminal nodes A
and B if and only if either A or B (but not both) belongs to
a non-lexical category and is immediately dominated by a
node dominating the other. If both belong to non-lexical
categories and every node dominating one dominates the other,
no word boundaries show up between a and b. In all other
situations, a and b are separated by two word boundaries.

Point one is that the specific SPE boundary conventions
turn out to be inappropriate in individual cases--they en-

code in the terminal string the _wrong_ pre-terminal infor-
mation. We could fix them up if we wanted to, once we found
out what the right pre-terminal information was. On the as-
sumption that phonological rules are sensitive only to ter-
minal symbols, we presumably want to.

Point two is that this assumption is unnecessary and in
fact leads to complications. There is a list of what look
to be universal juncture effects--predictable blockages of
high-level phonological rules in certain structural config-
urations. Assuming that we can recast these structural con-
figurations in boundary-theoretic terms and then refer to
the boundaries in rules, it ought to puzzle us that these
juncture effects do not vary universally, since a boundary
is supposed to be a phonological unit which can be paren-
thesized or omitted, like any other unit, in a structural
description.

Putting the matter somewhat differently, there are two
separate questions.

(i) Does boundary theory as presently constituted ex-
press the aspects of tree geometry which are really and tru-
ly relevant to the proper functioning of phonological rules?

(ii) To what extent are these genuinely relevant factors
to be regarded as contextually specifiable, and to what ex-
tent can we deposit them in universal grammar?

The answer to (i) is no, and I demonstrate it below by
example. The answer to (ii) is that, to a surprisingly
great extent, boundary effects in phonology are predictable

from the form of each rule, together with a universal state-
ment.

An analogy might be apropos. Suppose someone inven ed
a formalism whereby the A-over-A condition could be wrieben
into every transformational rule subject to it. Clearly
this new capability would be a step backward, since it would
imply that A-over-A effects are not universal. Pointing
to rules which appear to counterexemplify A-over-A is a
fairly feeble defense--on independent grounds we know that
such exceptions (rules of anaphora and so forth) belong to
components of rules which we do not expect to be subject
to structural constraints on string operations in the first
place.

I'm suggesting that, since juncture effects in phonolo-
gy are largely predictable, it is a bad idea to invent a
system of contextual symbols to stipulate them rule by rule.

In a similar way, having installed terminal boundary
symbols looking very much like (and in fact being, in SPE
theory) any other phonological units, we are tempted to try
to manipulate them as we must the others. Such operations,
and the phenomena requiring them, turn out to be (as far as
anyone knows) prohibited and non-occurring, respectively.
There are some famous counterexamples, like French liaison.
I'll deal with these in the text.

Now of course it will be possible to lay substantive
constraints on the use of boundaries such that these abuses
will be ruled out. And there will be a boundary theory

which ferries to the terminal string exactly the right and universally useful pre-terminal information. Such a constrained boundary theory will merely be an imperspicuous way of saying that the pre-terminal information itself conditions rules. Which is my point.

In Part I I examine, with reference to English, blockage of phonological rules at (i) trace, (ii) parenthesis, and (iii) the end of a sentence. Take, for example, monosyllabic destressing.

(3) *You're not as sick of your thesis as I'm t of mine.

(4) *I'm, they tell me, cute.

(5) *I think, therefore I'm.

There is a structural generalization uniting these blockages. The local phrase marker at each blockage site has a single geometric property, which I won't spill here. Let us assume for the time being that it exists. We then expect to find implicational clustering of effects analogous to those exemplified in (3)-(5) wherever any one is found. This is actually so, as I demonstrate in case studies of English (Part I) and Irish (Part II).

An interesting counterexample is discussed in Part IV. Part IV is devoted entirely to a rather poorly understood rule of English phonology, final dental palatalization before word-initial yod: coul[ǰ]l you and the like. Among other interesting properties, this rule does not fail where monosyllabic destressing does.

(6)　John want[š] your body more than he doe[ž] your mind.

(7)　Thi[š], you see, seems to be quite reasonable

　　　English.

(8)　Try i[č], you won't die.

What I show is that this fact is, at a certain level of analy-
sis, to be expected. Palatalization stands to monosyllabic
destressing with respect to juncture effects as, say, pro-
nominalization stands to WH-movement with respect to A-over-A:
In a sense its exceptional status, since independently pre-
dictable, actually supports the theory it apparently counter-
exemplifies.

As for the truly contextual function of boundaries, note
that boundary theory presupposes a well-articulated theory of
linguistically significant units. For every linguistically
significant unit and level of phonological rules indigenous
to it, boundary theory has a corresponding terminal symbol to
demarcate it. Suitably elaborated, this level theory is,
according to me, just about all we need to talk about in the
way of contextual specification in the ordinary cases.

(In Part III, it becomes necessary to say that there is
a unit which has no correlate in standard theory. This is the
clitic group. By this means I wish to express the familiar
but still curious status of clitics as, in various senses, in-
termediate between affixes and independent words.)

There are extraordinary cases. Many reasonably well-
documented examples of rules of phrase-level phonology exist
which have the property of "selective" application. This

means that they are sensitive, in obscure ways, to genuine-
ly contextual non-phonological information which level theory
alone is not rich enough to provide. Boundaries, in case
after case, turn out to be inappropriate. I believe that it
is possible to say reasonably strong things about the uni-
versal possibilities for non-phonological conditioning of
phonological rules, and have them appear to stick. In Part
III I propose what seems to me, with my experience with se-
lective sandhi, to be the appropriate theoretical vocabulary
making such rules expressible and the non-occurring ones
inexpressible. The key notion is one of constituency, and it
develops that normal level theory, which I also sketch, is
actually a special case of it. Case studies: French liaison,
Hebrew accentuation, Italian raddoppiamento.

The pioneer in this area is, of course, Lisa Selkirk,
and I would like to publicly avow my admiration for and in-
debtedness to her and her work.

I make very few theoretical assumptions of especially
controversial status in what follows. The ones I make are
mainly of convenience, rather than of necessity. In order
to have reasonably precise syntactic groundwork, for instance,
I make use throughout of $\bar{X}$ theory, the only existing account
of phrase structure which says much. Bar theory is spelled
out most comprehensively in Jackendoff (1977). In principle,
empirical consequences of this decision could arise, particu-
larly in connection with the selective processes of Part III.
In fact, they do not. My point is only that the reader's

opinion of this easy assumption and similar ones should not interfere with my larger case. Pretty broadly and unequivocally, though, I accept most of the basic notions of standard generative transformational theory.

PART I

AGAINST BOUNDARIES

Before I say anything, I note the naked fact that phonological and morphological rules have idiosyncratic domains of application: The morpheme, the syllable, the word, possibly the phrase, and the sentence come to mind. Rules of phonology and morphology are not very different from other kinds in this respect; syntactic and semantic rules of course show analogous boundedness, proving themselves to be limited to certain phrasal categories, sentences, utterances, or discourses. In syntax and semantics, this unavoidable observation is perceived as an incitement to discover properties of the various rules, or, better, of the various components to which they belong, from which their limitations will follow. A fatalistic and slightly empty solution to the problem, which no one even thinks to propose, would be to set up ad hoc boundary symbols flanking each sort of domain, which our rules can now pay attention to as it appears necessary. By doing merely this we seem to be condemning ourselves to a lack of insight into the several systems of rules.

This same solution, however, is precisely the one generally accepted among phonologists. In order to implement it, as will become increasingly evident in the following pages, one quickly finds the need for a great deal of theoretical machinery to place boundaries, to delete most of them when

they pile up, and to ignore the rest of them when they get
in the way. All of this comes from assuming that boundaries
exist as items of vocabulary on a par with the others.

How, in practice, are boundaries actually referred to in
rules? They are generally very handy in demarcating margins
of domains of application. It is extraordinarily nice to be
able to write ___$, ___+, ___#, possibly ___##, and the mir-
ror images. Recognition of this fact provoked the first ap-
pearance of real juncture symbols (Sapir and Swadesh 1939,
Trager and Bloch 1941). It is not nearly as clear that re-
ference to internal boundaries is necessary, i.e. that there
are structural descriptions of the form

(1)   ... [+F] B [+G] ...

, with specified terminal material on either side of a speci-
fied boundary B. Where B = ## or greater, (1) is of course ut-
terly unexemplified. I offer most of the remainder of this
work against the possibility of B = #. Examples of (1) where
B = $ are strikingly absent, to my knowledge. As for B = +, an
apparent problem, most of the examples known to me involve
allomorphy cooccurring with the presence of specific affixes
preceding or following the alleged boundary. It seems to me
that an easy and sensible thing to say for such cases is that
the alternations are conditioned by the affixes themselves,
rather than by the boundaries, whose presence is in turn con-
ditioned by the affixes.

In any case, as Kiparsky (1974) demonstrates, his alter-
nation condition, according to which neutralization processes

apply only to derived forms--i.e. only where an alternating form (but see Mascaro 1976) reveals the un-neutralized segment--<u>predicts</u> the presence in many rules of what's usually written +. In other words, it lets us get away with not writing it in, often simplifying matters considerably. Kiparsky cites, for instance, trisyllabic laxing, which has hundreds of exceptions: nightingale, overture, Rotenberg, etc. Since these don't feel very exceptional, it is not a nice solution to mark them as such. In fact <u>all</u> non-derived words fail to incur trisyllabic laxing. But writing in a required morpheme boundary is, by anyone's standards, a really ugly alternative, since a complex disjunction of them is actually necessary,

$$(2) \quad \underline{\quad} \langle + \rangle_a \ C_o \ \langle + \rangle_b \ V \ \langle + \rangle_c \ C_o \langle + \rangle_d \ V \quad C_o$$

Condition: a <u>or</u> b <u>or</u> c <u>or</u> d

as examples like (a) anti+pathy, (b) san+ity, (c) penal+ty, and (d) omin+ous show. The problem, along with the boundaries, disappears given the alternation condition.

Now if it is true that essential internal boundaries are not found, we might bother to ask ourselves why not. Let us imagine that there are separable components of syllabic rules, morpheme-level rules, word-level rules, phrase-level rules, and sentence-level rules, each of which has the obvious domain of application, henceforth often abbreviated σ, m, f, w, and S. Clearly we need to be able to represent the notion <u>edge of domain</u> in our rules in one way or another.

Let me use the notations __∅, ∅__, meaning 'at right edge!',
'at left edge!', interpreted identically for each component L
of rules.

(3)
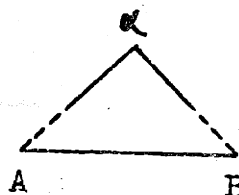
ᾱ, A, and B are nodes, A and B terminal ones. A and B are
ultimately dominated by ᾱ. Nothing to the left of A or to
the right of B is dominated by ᾱ. Whether ᾱ = σ, m, w, f, or
S, to say that a rule is restricted to ᾱ is to say that its
effects and conditioning are limited to the string consisting
of A, B, and everything in between, written $\overline{AB}$. The left
edge of domain ᾱ is at A. The right edge of domain ᾱ is at B.

Thus, in a word-level rule, __∅ is equivalent to the
usual __#. Etc.

Note, though, that ∅ is not a boundary. It is just an
extension of the usual 'null' notation, given sense now that
domains end at the left and right. One can't perform opera-
tions on it. It would be absurd to delete it, for instance,
just as it would be to try to delete the focus bar.

Without boundaries as terminal symbols, it follows more
generally that there will be no "readjustment rules" (SPE,
pp. 371 ff.) with the power to insert them, delete them, or
shuffle them around in various ways. Such rules have rarely
been proposed; see Part III below. These few examples aside,
the fact is that boundaries, if phonological units, behave

remarkably unlike other phonological units, serving in rules only in contextual capacities. The same observation is made in Pyle (1972): It is probably not necessary to put a boundary on either side of an arrow in a phonological rule. If there are no boundaries, it is not even possible.

If we banish boundaries, it also follows that rules with the effect of (1) will be nonexistent. A morphological rule whose environment is ___ $\emptyset$ [+F] is already senseless, because the presence of specified material to the right of the morphological edge contradicts the assumption that the rule is a morphological one. Similarly for the other types of rules.

One thing that falls out of this theory without coaxing is a universal implication to the effect that a rule which is strong enough to breach an edge at level n will also breach edges at the finer levels of structure. It is usually very easy to set up boundary-strength hierarchies saying exactly this. (See, for instance, Stanley (1969), McCawley (1965), Ramanujan (1967), Kenstowicz and Kisseberth (1977).) Why is it so easy? The idea of boundary "strength" is not quite circular. To say, e.g., that # is "stronger" than + means a little more than merely that (generally speaking) any rule that applies across # applies across +, even though, in a theory which uses boundaries, we could just as well say that + is stronger than #: Notice that every word uniquely contains at least one morpheme.

In fact this is true of linguistically significant units

in general. If unit γ uniquely contains at least one unit ψ,
a γ-boundary is "stronger" than a ψ-boundary. Every mor-
pheme uniquely contains at least one segment, every word at
least one morpheme, every phrase at least one word, and every
sentence at least one phrase.

The syllable does not possess such implicational virtues.
Neither a segment (obviously), a morpheme (-Z), nor a word
('d) need uniquely contain a syllable, though a sentence, it
seems to me, must. But it does not follow from this last
that every syllable must uniquely contain at least one word,
or one morpheme, or even (due to ambisyllabicity, on which see
Kahn (1976), or else below, and against which Selkirk (to ap-
pear)) one segment.

My point is that the syllable lies outside the contain-
ment hierarchy. Interestingly, it also lies outside the
strength hierarchy. There are structurally-conditioned rules,
like French liaison (below), which apply across junctures of
one sort or another, but fail at syllable edge, just as across
zero. (Why liaison does not apply across zero I'll give an
account of in Part III.) A syllable, it appears, does not
count as a structural unit in the same way as the other struc-
tural units.

There is a third respect in which the syllable differs
from other units, this one, I believe, at the root of the
other two. A syllable has no status at all in the lexicon—
it is presumably established by rule in the phonology.
There are powerful indications that syllable construction

à la Kahn (1976) follows most phonological rules, indeed preceding only the syllabic ones. Note that, unlike true lexical units, the syllable is entirely dissociated from meaning. Note that there are no syllables which have to be marked as exceptions to otherwise valid generalizations. Note that syllables are not borrowed or lost.

I assume, then, that the syllable exists only outside the lexicon, of which hierarchical organization is a property in reasonably good standing. Supposing that it gets built quite late, after rules at the other levels, the syllable's status as a non-entity with respect to rules at them is explained.

A little more generally, it is useful and probably necessary to recognize an independent hierarchy of prosodic units, including the syllable, the foot, the stress group, etc. These, one presumes, bear strict containment relations to one another, but none to any member of the lexical hierarchy. (Morris Halle suggests to me that the segment may have dual membership, occupying the lowest position in both lexical and prosodic hierarchies.)

Restricting our attention to lexical units, the strength and containment rankings mentioned above are automatically related in this theory, and it follows that the syllable's exclusion from the latter should entail its exclusion from the former. An S-level rule, for instance, is one applying within $[_S \quad]$. By _definition_ its operation includes the

lesser domains f, w, m.

$$[_S \quad [_f \quad [_w \quad [_m \qquad] \qquad] \qquad] \qquad]$$

m-rules

w-rules
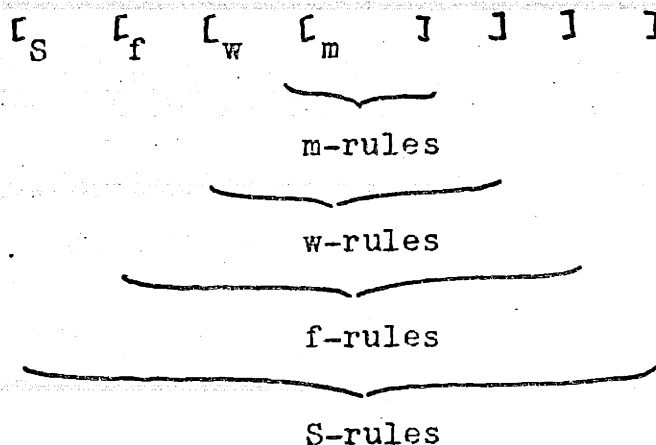
f-rules

S-rules

Note that speaking of "boundaries" #, $\dagger$, at the edges of these internal domains makes it <u>possible</u> to refer to them in the structural descriptions of high-level rules. This pseudo-possibility is what gives rise to the notation (B) and to implicational strength hierarchies to eliminate it. But their import is an automatic consequence of the nonexistence of boundaries.

Likewise an f-level rule is one applying within $[_f \qquad]$. Etc.

Generally, it follows from the fact that R is a ψ-level rule that R will apply anywhere inside a ψ. There are many sorts of extenuating circumstances blunting the force of this implication. For instance, in Irish (Part II, below), the initial mutations, affecting the leftmost segments of words, are triggered by what appear to be word-sized abstract features. So, of course, the mutations will not be found applying across edges smaller than w, since the flanking units

will never bear the instigating features.

Opposed to the notion edge is the notion juncture.
Where the relation is-an-edge-of is unary, the relation is-a-
juncture-between is binary; there will be no domain-final or
domain-initial juncture. This is intended to correspond to
the observation that there are rules at a given level L which
involve only pairs of adjacent terminal symbols, and not the
L-last one alone or the L-first one alone. Prime terminal
elements p and q are adjacent with respect to some level of
representation L if and only if p and q are productions of
the rules of L and no r also produced by the rules of L falls
between p and q in the terminal string containing them. As
we will see, mere stringwise adjacency

$$(4) \quad \ldots \quad \underset{[+F]}{p} \quad \underset{[+G]}{q} \quad \ldots$$

does not guarantee that a rule operating at L whose structur-
al description appears as

$$(5) \quad \ldots \quad [+F] \, [+G] \quad \ldots$$

will apply.

I'd like to say that there is such a thing as a normal
juncture, in essence a locally well-formed tree, whose
presence the application of rules of a certain independently
justifiable type requires. These rules respect normality of
juncture by their very nature, unlike edge of domain, which
is genuinely contextual. That is, we know right away from
the form of a rule that it will fail across an abnormal
juncture, an object which we will soon encounter. A normal

juncture is not, I would say, the same thing as any known property of the terminal string (I'm talking about boundaries), as will become less unobvious below.

Defining the proper notion of <u>juncture</u> involves taking trees rather seriously as geometric objects. Look, for instance, at this one.

(6)
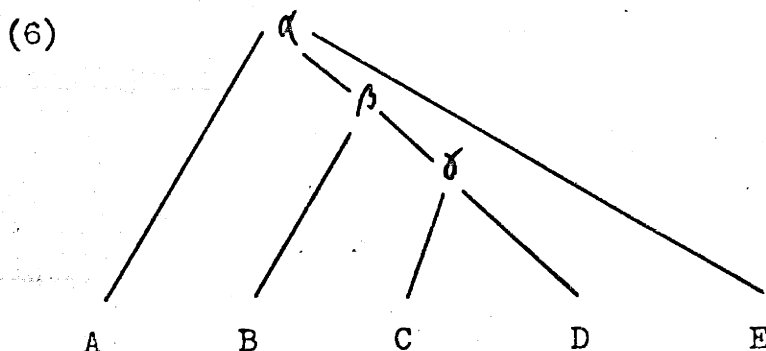
$\alpha - \gamma$, A - E are vocabulary provided by the rules of some level of structure L. α, let's say, is the root of the tree. A - E are prime terminal lexical units U. Let us consider these nodes as points. Now we will complete (6) by drawing $\overline{AE}$, containing the terminal string.

(7)
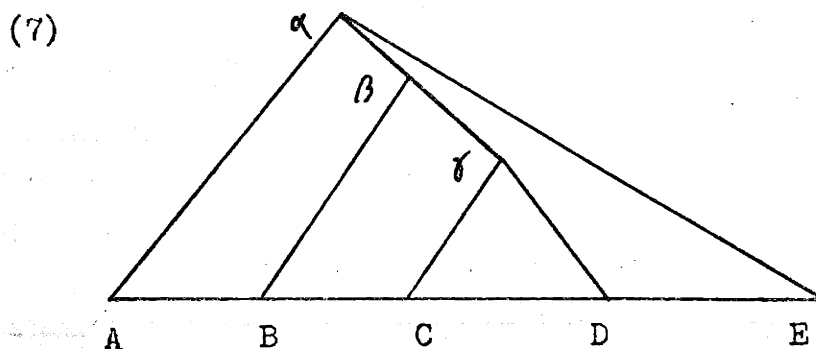
We'll say that a <u>U-juncture</u> is the empty interior of a simple closed curve whose n vertices are from $\left\{ V_T^L \cup V_N^L \right\}$, n-1 of whose sides are the lines of the usual L-marker, and

whose nth side contains the terminal string. Since a U-junction is empty, exactly two adjacent members of V_T^L, U-units, are among the vertices of the curve bounding it. In fact a U-juncture is just a minimal area bounded by the lines of an L-marker. The four defined by (7) are (left to right) the interiors of:

(8) (i) κ AB β

(ii) β BC γ

(iii) γ CD

(iv) $\kappa \beta \gamma$ DE

The exterior of the large triangle κ AE is not a U-juncture, because it is not the interior of anything. The interior of κ AE is not a U-juncture, because it is not empty. Notice that every consecutive pair (i, i+1) of terminal elements 1, 2,..., n determines a segment $\overline{i\ i+1}$ which is one side of the curve bounding a different U-juncture: In (8), $\overline{AB}$, $\overline{BC}$, $\overline{CD}$, $\overline{DE}$ (the only roman letters in each example) are sides of (i), (ii), (iii), and (iv), respectively, and only them, respectively. Notice also that there is no U-juncture to the left of A or to the right of E. (Since U is a (prime) terminal element, there are no f-junctures--phrasal nodes are not terminal. Prosodic units, too, do not come with U-junctures.)

We might ask if there can exist a pair of consecutive units (U_i, U_{i+1}) such that no curve containing them bounds a U-juncture. It is very convenient to say that there can, and that rules at level L will not involve both U_i and U_{i+1} in such circumstances. U_i and U_{i+1} will then meet at the

"abnormal" juncture which I alluded to.

I will often have occasion below to speak loosely of the U-juncture "between" U_i and U_{i+1}, or of application "across" U-juncture.

Let me restrict my attention to high-level phonological and morphological rules. (By "high-level morphology" I understand local morphological alternations conditioned by factors in the signal at large. Examples will come up.) Here L is S, syntactic structure at the level of the sentence; our trees are standard phrase markers. Thus we're talking about w-junctures, since words are the prime terminal elements here.

Let us assume that the rules in question require the presence of normal w-junctures. If any constant terms of the structural description of such a rule are satisfied by material belonging to two consecutive members of V_T not on the curve bounding any one w-juncture, the rule will block. An "abnormal" juncture, then, cannot be breached by rule.

The first case I want to consider is the English a/an alternation. First, though, I'm going to digress at length, delivering a number of slightly desultory remarks on some problems connected with the rule responsible, revolving mainly around its proper form and direction. These will ultimately turn out to be important to the larger discussion. The first remark concerns the various forms, from the twelfth century to the present, of the synchronic rule evidently behind this alternation.

Even in its modern form, the a/an rule is usually sup-

posed to be taking _an_ to _a_, and is also usually supposed to
have steadily generalized in applicability over the years. It
is true that _an_ is found in progressively diminishing numbers
of environments, but this is an entirely different thing from
saying that some rule is applying more. Actually writing a
rule representing in itself each of the several intermediate
stages is not a pleasant business, as we shall see. I'd like
to suggest that these rules are in fact a good deal simpler
than they appear. Various independent processes, it turns
out, conspire to feed each _a/an_ rule (or bleed it, depending
on its directionality--more on this below), accounting in
large part for its apparent complexity, while the rule's his-
torical evolution consists mainly in coming to be couched in
terms of the feature [$\pm$syll] instead of the feature [$\pm$cons].

OE had no indefinite article until around 1150, when
the numeral _án_ 'one' began to discharge that office. In its
capacity as article, _án_ became "proclitic and toneless" (OED)
while the numeral retained its length and wound up going
through the vowel shift. Preconsonantal _a_ is found in mid-
lands dialects from the beginning, as soon as the non-numeri-
cal sense developed. Still, _an_ occurs preconsonantally
through 1300 or so. _An_ before [w] disappeared early, though
before [y] (_an yere_, etc.) it remained until the fifteenth
century, sometimes even later.

(9)   1611 Bible _Acts_ vii 27   An eunuch of great au-
         thority.

(_Such an one_ is an isolated archaism, found very late.) Even

after the fifteenth century, <u>an</u> occurs before unstressed
[yùw]: <u>an eulogium</u>, <u>an united appeal</u>.

      (10) 1823 Lingard <u>History of England</u> VI 219 An eu-
           logium on his talents.

      (11) 1847 Tennyson <u>The Princess</u> i 149 All wild to
           found an University For maidens.

Evidently there are still speakers who have <u>an</u> before [yùw].

    OED records <u>an</u> before <u>h</u> (and possibly [h]), even begin-
ning a stressed syllable, as late as the eighteenth century:
<u>An house</u>, <u>an heifer</u>.

      (12) 1611 Bible <u>Acts</u> vii 47 But Solomon built him an
           house.

      (13) 1732 Pope <u>Essay on Man</u> iv 78 Nor in an hermitage
           set.

<u>An</u> before unstressed [hV̀] is rather hardy. <u>An historic</u> mo-
ment, <u>an heretical proposal</u> still have their utterers, al-
though most speakers, and nearly all American speakers (I'd
say), have no <u>an</u> before [h].

    Thus we can bet on at least the following stages in the
history of the alternation. (To the left of each slash is
the post-<u>a</u> environment; to the right, the complementary one.
V = vowel, C = consonant, G = non-glottal glide.)

      (14) (i)   C   / V, G, h
           (ii)   C, w   / V, y, h
         (iii)   C, G   / V, yùw, h
          (iv)   C, G, hV́ / V, yùw, hV̀
           (v)   C, G, h / V

We might, in a straightforward manner, attempt to write a rule
for each stage. In that case we would be committed to rules
(15)(i-v) below. (In order to avoid question-begging, I have
written first the environment of an _an_ → _a_ rule, then the en-
vironment of an _a_ → _an_ rule. Having them both to look at
will come in handy later.)

(15)   (i) ___ [+cons]

(ii) ___ $\begin{bmatrix} -syll \\ \langle \begin{smallmatrix} -cons \\ +back \end{smallmatrix} \rangle \end{bmatrix}$

(iii) ___ $\begin{bmatrix} -syll \\ -low \\ \langle \begin{smallmatrix} -cons \\ _a -back \end{smallmatrix} \rangle \end{bmatrix}$ $\left\langle \begin{array}{c} \langle_b [\begin{smallmatrix} +high \\ +back \end{smallmatrix}] \rangle \\ _a \langle_b [+stress] \rangle \end{array} \right\rangle$

(iv) ___ $\begin{bmatrix} -syll \\ \langle_a +low \rangle \\ \langle -back \rangle \\ \langle_b -cons \rangle \end{bmatrix}$ $\left\langle \begin{array}{c} \langle_c [\begin{smallmatrix} +high \\ +back \end{smallmatrix}] \rangle \\ \langle_{a,c} [+stress] \rangle \\ _b \end{array} \right\rangle$

(v) ___ [-syll]

(i)' ___ [-cons]

(ii)' ___ $\begin{bmatrix} -cons \\ \langle \begin{smallmatrix} -syll \\ -back \end{smallmatrix} \rangle \end{bmatrix}$

(iii)' ___ $\begin{bmatrix} -cons \\ \langle -syll \rangle \\ -back \\ \langle_a +high \rangle \end{bmatrix}$ $\left\langle [\begin{smallmatrix} +high \\ +back \\ _a -stress \end{smallmatrix}] \right\rangle$

(iv)' ___ $\begin{bmatrix} -cons \\ \langle -syll \rangle \\ _b \langle_a -back \rangle \end{bmatrix}$ $\left\langle [\begin{smallmatrix} +high \\ +back \\ _a \langle_b -stress \rangle \end{smallmatrix}] \right\rangle$

(v)' ___ [+syll]

These are merely literal representations of the environments
more perspicuously stated in (14), as the reader may verify if
she really wants to. What I have just done nicely obscures,
I think, the real nature of these rules. Let us first consi-
der the variable operation of (15)(iv) before h. In most di-
alects [h] is somewhat lenited, at least, in exactly the en-
vironment ___V. It is very easy to suppose that initial h-de-
letion accompanies and feeds the a/an rule. Thus an will ap-
pear only to the extent that [h] disappears. It is an im-
portant fact, to my mind, that an before (written, anyway) h
is found especially in British dialects, among exactly which
h-deletion has real documentation. In the dialects where
initial [h] is entirely gone, whether by deletion or relexi-
calization, an is always present before orthographic h; see
Sivertsen (1960), Orton and Dieth (1962). It is tempting to
make similar conjectures even about some of the writers con-
tributing to stage (iii): Initial [h] was always gone, so
an was always present. This last obviously requires re-
search.

     To give an analogous example, speakers of some dialects
of Australian English regularly and unilaterally palatalize
dentals before yod, intervening "boundaries" notwithstanding.
(More on this in Part IV.) Thus we hear:

     (16)   thi[š] yo-yo

     (17)   tho[ž] yellow balloons

     (18)   fluen[č] Yiddish

     (19)   my thir[ǰ] urologist

The unpalatalized versions are ungrammatical in connected speech. However, the word and behaves strangely: an[ǰ] results only optionally. It develops, of course, that [ænd] alternates on a sporadic basis with [æn]. In the same way, jus[č] you is found only as often as ju[š] you, simply because [ǰʌst] alternates freely with [ǰʌs]. I'm suggesting that sporadic h-deletion correlates with sporadic an in a similar fashion.

It is a disturbing thought that, in this otherwise reasonable account, the morphological rule relating an and a must follow the surely crudely phonetic rule of h-deletion. This problem I'll remedy below.

Next, let us look at the problem of an before [yùw]. Why is an ùnited appeal so different, even to the ear of someone who does not speak this way, from *an únified theory, still more from *an Yàstrzemski fan, and especially from *an wòn ton recipe? Before I answer this question, I would like to point out the curious fact that while many speakers of English have words whose initial segments are [nyù-] (neurotic, Newtonian), and some have initial sequences in [nyú-] (new, neuron, Newton), there are no English words in [nyV-], where V is not [u], or in [nw-].

My idea is that an φ is grammatical, where φ is a permissible initial sequence, to the extent that nφ is also a permissible initial sequence. That this should be so follows from the theory of Kahn (1976), together with the assumption that a is the underlying form of the article, in

the following way.

Let us start with the string a urologist [ə yùwrálə̀jist]. Kahn's system provides for this the syllabification shown in (20).

(20)   ə y ù w r á l ə j i s t

This happens by his rule II, a rather general syllabification convention.

II (a) $C_1 \ldots C_n V \rightarrow C_1 \ldots C_i C_{i+1} \ldots C_n V$

where $C_{i+1} \ldots C_n$ is a member of the set of permissible initial clusters but $C_i C_{i+1} \ldots C_n$ is not.

(b) $V C_1 \ldots C_n \rightarrow V C_1 \ldots C_i C_{i+1} \ldots C_n$

where $C_1 \ldots C_i$ is a member of the set of permissible final clusters but $C_1 \ldots C_i C_{i+1}$ is not.

(An x at the end of a line signifies the absence of a syllabic association, in Kahn's notation.)

Now suppose we'd blindly inserted an n, by an agency so far unelucidated: [ənyùwrálə̀jist]. The domain of application of Kahn's II is "the word" (Kahn, p. 55); conceivably a proclitic (see Part III) item begins a "word." Better still, the status of the epenthetic n with respect to word edge is not so clear. Let us just assume that II can apply to it.

To the extent, small or nil for most people, that initial

[ny-] is "permissible" (presumably at the surface), IIa gives

(21) ə n y ù w...
      | \ \ | /
      σ   σ

If [ny-] is not good, IIa does not allow <u>urologist</u> to appro-

priate an initial <u>n</u>. By IIb we end up with

(22) ə n y ù w...
      \ /   \ | /
      σ   σ

Kahn also has a rule III, applying only in "normal-rate and

faster speech":

III   In   [-cons] C C$_o$ $\begin{bmatrix} V \\ -str \end{bmatrix}$
           | \ | /
           σ$_1$ σ$_2$
      associate C and σ$_1$.

This rule is designed to account for the ambisyllabicity of,

for instance, the <u>n</u> in <u>money</u> in reasonably un-deliberate

speech.

(23) m ʌ n i   →   m ʌ n i
      \ / \ /   \ | / \ /
      σ   σ   σ   σ

From (21), then, III gives (24),

(24) ə n y ù w...
      \ / \ \ | /
      σ   σ

since the <u>u</u> is unstressed.   (22) stays as it is.   Now if we

merely install a surface filter (25)

(25) * [$_{DET}$ ə n
              \ / \
              σ   x

to the effect that the <u>n</u> of <u>an</u> must be ambisyllabic, we auto-

matically reject (22), due ultimately to the fact that [nyuw-]

is impermissible, and II will not apply; we reject *an únified, since III will not apply; and we reject *an wòn ton, since IIa will not apply. Therefore, it seems to me that we can dispense with the [yùw] condition altogether, as it follows from the possibility of [nyuw-]. I am assuming, of course, that Kahn's rules are applicable as is from stage (iii) on.

I do seem to be predicting that those who say an urologist in connected speech will be quite able to say a urologist when speaking more deliberately--say, across pause; this because III will fail. I do not know if this is so. Also, even for those who have consistent initial [ny-], an urologist, etc., will be thrown out for independent reasons, having to do with the palatalization of dentals before word-initial yod. I discuss this in Part IV.

The new versions of (15)(i-v) are these.

(26) (i) ___ [+cons]   (i)' ___ [-cons]

(ii) ___ $\begin{bmatrix} -syll \\ \langle \begin{smallmatrix} -cons \\ +back \end{smallmatrix} \rangle \end{bmatrix}$ (ii)' ___ $\begin{bmatrix} -cons \\ \langle \begin{smallmatrix} -syll \\ -back \end{smallmatrix} \rangle \end{bmatrix}$

(iii) ___ $\begin{bmatrix} -syll \\ -low \end{bmatrix}$ (iii)' ___ $\begin{bmatrix} -cons \\ \langle \begin{smallmatrix} -syll \\ +low \end{smallmatrix} \rangle \end{bmatrix}$

(iv) ___ [-syll]   (iv)' ___ [+syll]

(v) ___ [-syll]   (v)' ___ [+syll]

It is easy to see, in this rendering, the progression from an alternation based on the feature [±cons] to one based on the feature [±syll]. Notice, incidentally, that there are no

grounds of simplicity on which to base a choice of under-
lying form, nor are there in (15). On the other hand, re-
call that my idea above, at least in the modern dialects (iv)
and (v), works only if a → an.

I'd like to call attention to the fact that (25) appears
to be automatically rejecting all an C, without any help from
(26). This is because, according to II, n is never ambisyl-
labic in such circumstances, while it always is before V. Un-
fortunately, the a/an alternation still happens when there is
no possibility of syllabic association, as we will see below.
Nevertheless, we might be able to get along with a rule of
optional n-insertion stating merely

(27) $\emptyset$ → n / a]$_{DET}$ ___ [+seg]

dispensing with (26)(iv, v) completely, if we put on (25)
the construction that syllabic non-association of n is out
only for reasons of syllable composition, i.e. only if the x
legislated against by (25) arises by Kahn's rules. As we
shall see, the only kinds of extenuating circumstances al-
lowed involve extralinguistic interruptions of the signal,
which undoubtedly take place after (25) anyway. So now
nothing remains of (26). (25) is quite general and accounts
for the modern alternation all by itself, given (27). (27)
is the unmarked rule of external sandhi--"an may occur before
something." Why even this much is necessary will soon be
clearer.

As we have seen and will see further, the problem of the

underlying form of the article, not in itself an especially
fascinating question, interacts with weightier and more in-
teresting matters. I should mention that a small debate
(Hurford 1971, 1972, 1974; Vennemann 1972, 1974) raged some
years ago on this very topic, Hurford championing underlying
an and a synchronic recapitulation of diachrony, Vennemann
pushing for underlying a and an inversion of the historical
process. As nearly as I can make out, nothing more than a
little bit suggestive is said in these papers. Each man
admirably points out the defects in the other's case. Ven-
nemann, for example, cites the generally earlier acquisition
of a as evidence for its lexicalization in that form. Hur-
ford sensibly questions the assumption that underlying forms
are learned early and subsequently left alone. Vennemann men-
tions the modern English construction a whole nother story as
indicating the underlying absence of the n. Hurford observes
that this particular "false" division is completely idiosyn-
cratic and nonproductive. Hurford and Vennemann force each
other to admit the inconclusiveness of the evidence from ME
false analysis, of which examples going both ways (an ewt >
a newt, a nadder > an adder, an otch > a notch, a napron > an
apron) exist. Hurford suggests that an n-deletion rule is
"a more economical and natural rule than a rule specifically
inserting an [n], which must, of course, be fully 'spelled
out' in features by the rule." Vennemann professes ignorance
of Hurford's conception of "naturalness," but observes that,
given Hurford's assumptions, a full spell-out of the n must

be done underlinesomewhere, presumably in the lexicon, rendering any
"economy" illusory. Finally, Vennemann notices that the
a ___ environment is larger, in number of words, than the
an ___ environment, and thinks that this would force lexicali-
zation of a. Against this I might mention the example of li-
aison in French (see Part III), where final consonants are
audible under very restricted phonological and syntactic cir-
cumstances; but the choice of the liaison consonant must
clearly be made on a word-by-word lexical basis. There is no
reason why underlying forms should not be set up on the
strength of everyday but numerically comparatively scarce al-
ternations.

I want to go on now and mention a few interesting facts
which appear to lead again to the conclusion that n is inser-
ted. The notion w-juncture is essential here.

It is important to realize, first, that the a/an rule is
not phonological. Obviously it applies only to the one word.
Further, it is not blocked by pause.

    (28)(a) an...Eskimo Pie

       (b) *a...Eskimo Pie

    (29)(a) *an...snorkel

       (b) a...snorkel

In fact, any kind of non-linguistic material, including
coughs, sneezes, hesitation noises, throat-clearings, etc.,
is permissible in place of the three dots in (28) and (29).
Notice, in the cases of the cough and the hesitation,

(30)   an-- [?ə̥?ə̥]--Eskimo Pie

(31)   a-- [əː]--snorkel

that if the rule were really strictly local, we would get a

before the [-syll] glottal stop and an before the [+syll]

schwa. Presumably all this is so because the rule is morpho-

logical, hence comparatively early, and immune to such last-

minute additions to the signal as pauses and other extralin-

guistic signs of physical and mental distress of various

sorts. With this in mind, consider the parenthetical.

(32)(a) *an--though I hate to admit it--silly idea

     (b) *an--though I hate to admit it--absurd idea

(33)(a)   a--though I hate to admit it--silly idea

     (b) ?a--though I hate to admit it--absurd idea

(34)(a) *an--although I hate to admit it--silly idea

     (b) ?an--although I hate to admit it--absurd idea

(35)(a)   a--although I hate to admit it--silly idea

     (b)   a--although I hate to admit it--absurd idea

Judgments are certainly delicate, but it is curious that a is

so good so often. Generally speaking, a (parenthetical) N̄

goes through, while an (parenthetical) N̄ fails. Whether a

vowel or a consonant begins the parenthetical or the string

that resumes to the right of it doesn't seem to matter.

(The reader who finds all such examples, with a parenthetical

after an article, slightly precious to begin with will prob-

ably agree that including a kind of resumptive article

(36) a--unless I'm mistaken--a really distinct im-

                    provement

improves their naturalness markedly. The point I'll be making about the constituency of the parenthetical is unchanged.)

Parentheticals like those in (32)-(35) definitely belong in the representations of the larger strings. What we would like to see is a way of predicting this peculiar failure of the a/an rule from some property of theirs. The pre-parenthetical pause is irrelevant, as (28) and (29) show.

(My feeling about (33)(b) and (34)(b) is that sheer pedantry is responsible for the temptation to make the long-distance alternation, much like, say, the "proper" consistency of tenses throughout a discourse.)

I should first distinguish three or four classes of interpolated material, only one of which I will consistently refer to as parentheticals. First are the well-known subjacency-transparent clauses, X say and the like (Erteschik 1973):

(37) Who did you say you were?

(38) *Who did you repeat you were?

Note the inversion, the lack of pauses, and the smooth intonation, all of which assure us that this type of "parenthetical" is firmly engaged in the surrounding syntactic structure, though puzzling in familiar ways.

Second is the class of interpolated exclamations and various other expressions of spontaneous emotion.

(39) --hey!--

(40) --what was that?--

(41) --now cut that out!--

These have all the properties of interruptions. They niche

(Ross 1972) absolutely anywhere, and have no semantic relation

at all to the host sentence.

(42) Four--quiet in back!--score and seven years ago...

In place of quiet in back! we can again have things like

throat-clearings, coughs, and so forth, making it a virtual

certainty that interpolations like these are last-minute per-

formance effects, having no structural relation at all to the

sentences they interrupt. In addition to their syntactic and

semantic isolation, they also fail to block sentence phonolo-

gy involving things on either side,

(43) an--good lord, did you see that?--easy recipe

unlike, and here I come to the class of parentheticals I will

be considering,

(44) *an, shall we say, easy recipe

Also compare (42) with (45):

(45) *Four, I think, score and seven years ago...

Again unlike insertions of the good lord type, these are se-

mantically related, in obscure ways, to their surroundings.

Also, they both come with a special intonational contour and

induce one on the outside sentence.

(46) This is, as you can see, an example sentence.

(47) This is--good lord!--an example sentence.

(48) This is an example sentence.

Good lord!, on the other hand, merely interrupts the outer contour.

Finally, the "true" parentheticals as you can see, etc., often do not stand alone.

(49) *I conjecture.
*You'll appreciate.
*We can say.
*Though.

It certainly looks like the syntax must register their presence in some way, unlike the fairly trivial good lord cases.

The appropriate derived structure is an interesting problem. As nearly as I can make out, there are no reasons whatever for supposing that the parenthetical is dominated by anything in particular in the surrounding phrase-marker. Rather than make some arbitrary decision, I propose to give up entirely and let the parenthetical clause be geometrically independent of the rest of the sentence, sharing only its terminal string with it:

(50)
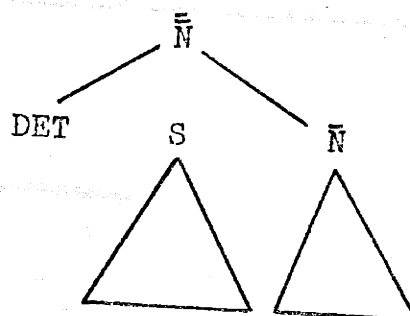
This has the advantage of predicting root phenomena in the parenthetical, which are in fact observable (see Emonds 1974):

(51)   a--{ don't you agree
          { don't forget now } --very important message

In addition, comma intonation (46) can now be given a struc-

tural basis.

We might also think of calling non-restrictive relative clauses parentheticals in this sense, thus accounting for their familiar intonational properties, which are parallel in every respect to those of the interpolated clauses we've been considering. Obvious but still puzzling contrasts like (52) versus (53)

(52) (a) The chutney which John gobbled

(b) The chutney that John gobbled

(53) (a) The chutney, which John gobbled

(b) *The chutney, that John gobbled

turn out to be explained as well. The parenthetical is a root S and cannot begin in a complementizer any more than any other root S.

(54) *That John gobbled the chutney.

But WH-movement can apply in both the (a) examples.

Emonds (1974) has suggested that internal parentheticals come by rule from sentence-final position. He wants to say that (55) and (56) below are related by rule

(55) John blames fluoridation, I think.

(56) John blames, I think, fluoridation.

because only then can he say that the parenthetical is at some point a root S (Emonds 1970).

(57)
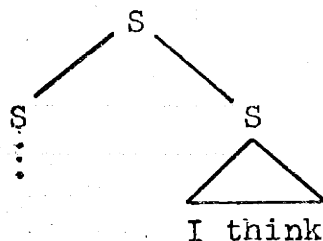
This in turn he wants to say because root transformations actually do apply independently in the two clauses, as in (51) and (58).

(58)   Does John blame fluoridation, do you think?
This idea seems really questionable, first because there are all sorts of non-sentential parentheticals, like however, in sum, without doubt, etc., which there is no special reason for getting from somewhere else.   Notice too the following examples.

(59)   John, however, blames fluoridation, I gather.

(60) *John blames fluoridation, however, I gather.
It is also interesting that a parenthetical in an isolated NP is quite sensible, though there is no sentence for it to have been a right daughter of.

(61)   a noun phrase, to take just one example, like this
                one

Generally, of course, moved constituents show up in sentences that also contain gaps vacated by them.

(62) *Where John.

(63)   John.

Probably the biggest advantage of a no-movement analysis is the movement rule it doesn't require.   This rule seems to be excluded by any reasonably confining theory of transformations.   Notice that deriving examples like (64)

(64)   Everybody's gloating over the fact that Alonzo
       blindly accepts Alix's facetious claim that John,
       I think it is, and his sister blame fluoridation.

from a source like (57) involves violating nearly every con-

straint in the literature, whether one extraposes, in the
manner of Emonds, _and_ _his_ _sister_ _blame_ _fluoridation_ (a non-
constituent), or intraposes _I_ _think_ _it_ _is_.

Finally, the problem of the constituency of the paren-
thetical with respect to its surroundings, concerning which,
as far as I know, there are not even plausibility arguments,
is totally obviated. This issue, I should mention, is en-
tirely distinct from the one about what sorts of constitu-
ents a parenthetical may appear between: This is the
"niching" problem (Ross 1972) and it is unrelated.

Now if we assume that _a_ is the underlying form of the
article and that there is a rule of high-level morphology
on the order of (27), (27) will predictably fail if the seg-
ment after the focus begins a parenthetical.

(65)
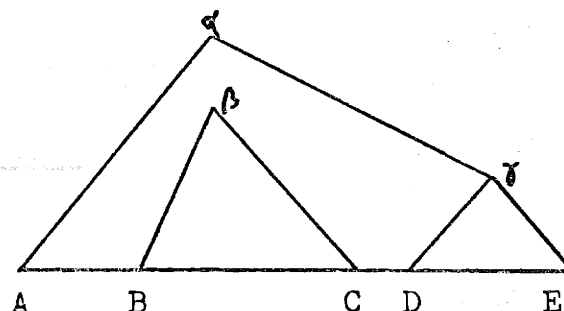
$\alpha AB\beta$ does not bound a w-juncture, because it is not closed.
$\alpha ABC\beta$ does not bound a w-juncture, because it is not closed
either. Even if it were, its interior would not be empty.
$\alpha ABCD\gamma$ does not bound a w-juncture, because, even though it
is closed, its interior is not empty. In fact there is no
w-juncture bounded by any curve containing A and B. Thus a
rule crucially involving terms analyzable as A and B, e.g.

(27), will not apply.

A second type of case is what is usually thought of as
to-cliticization.

    (66)(a) Who does John want t to speak?

       (b) *Who does John wanna speak?

    (67)(a) Who does John want to speak to t?

       (b) Who does John wanna speak to?

As is well-known, to-cliticization (and hence the reduction
it feeds, for a small number of verbs) is blocked across an
extraction site t. The rule looks maybe something like (68)
(see Bresnan 1971, Selkirk 1972):

$$(68) \quad V \ [_{VP} \ \underline{to} \ \rightarrow \ [_{V} \ V \ \underline{to} \ ]_{V} \ [_{VP}$$

Actually, (68) is probably a little muscular. We might
reasonably ask why it is necessary to adjoin--"cliticize"--to
on the left at all. The usual thought (Selkirk 1972) is that
a word belonging to a non-lexical category, e.g. to, may lose
its stress and eventually reduce only if it is a "syntactic
dependent," roughly equivalent to not being a phrase unto it-
self. (I return to this below.) Note, for instance, the be-
havior of the preposition to:

    (69)(a) I'm talking ta you.

       (b) *Who are you talking ta?

But alongside of sentences like (67)(b), there are also sen-
tences like (70), where to is presumably all that remains of
a verb phrase.

    (70) I don't wanna.

Hence it appears necessary to reattach to on the left, making

it once again dependent. Following this operation, to may lose its stress and reduce, and want may suffer its familiar phonological vagaries into the bargain.

Notice, though, that to fails to be a "syntactic dependent" precisely when something has been removed to the right of it. I believe that this is actually the salient feature of the phenomenon, and it falls easily out of the theory I'll elucidate directly. In any case, this exception to one side for the moment, there's very little in the way of saying simply that to can reduce anywhere.

But again, (70) is puzzling--it is an exception to the exception. After all, one can't say

(71) *I don't intend ta.

which is parallel in every respect. We are forced to admit that want is exceptional. (68), incidentally, falsely denies this--it does not distinguish (70) from (71).

Suppose we said merely

(72) want to → wanna

as do Chomsky and Lasnik (to appear)(but see Postal and Pullum 1977). This minimal theory appears to give all the right results without awkward readjunctions. It embodies the observation that the list of contracting verbs is very short. Even when to reduces, it is only want that undergoes segmental alterations.

(73)(a) I meant ta finish.

     (b) *I menna finish.

There are hafta, gonna, but it is not so clear to me that

these are not lexicalized modals, as much as <u>must</u>, <u>will</u>,
though there are some distributional differences. Still,
supposing they aren't, I see nothing wrong with a pair of
rules like (72) producing them.

In my opinion, then, there is no <u>to</u>-cliticization. Thus
it is (72) that fails mysteriously in (66). (It's not so
clear, in fact, in what form cliticization rules are needed
at all. See Part III.)

Given the now standard assumption that a trace is simply
pre-terminal vocabulary dominating no terminal material, this
blockage is not really mysterious.

(74)
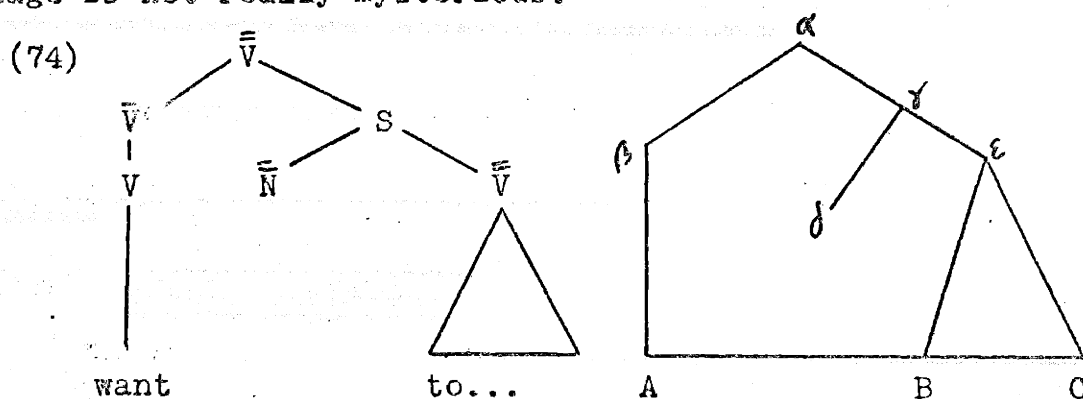
$\alpha\beta AB\epsilon\gamma$ does not bound a w-juncture, because its interior con-
tains δ. $\alpha\beta A\delta\gamma$ does not bound a w-juncture, because it is
not closed. There is no w-juncture bounded by any curve con-
taining both A and B. Therefore (72), whose two constant
terms are <u>want</u> and <u>to</u>, will fail precisely here.

To instance a third rule,

(75) John $\left\{\begin{matrix} \text{is} \\ \text{'s} \end{matrix}\right\}$ more fed up with his thesis than his

committee $\left\{\begin{matrix} \text{is} \\ *\text{'s} \end{matrix}\right\}$ enthusiastic.

auxiliary contraction is of course possible in the first

clause, but not in the second. The literature on this phe-
nomenon is large: See King (1970), Bresnan (1971), Baker and
Brame (1972), Selkirk (1972), Lightfoot (1975). I assume,
following pretty much Selkirk (1972), that contraction of _is_
is fed by the more general monosyllabic destressing rule
(pp. 42 ff.), which, incidentally, is also responsible for
destressing _to_ above.

$$(76) \quad V \rightarrow [\text{-stress}] \; / \; [\# \; W \; [C_0\_\_C_0] \; [(\#)X\acute{V}Y(\#)] \quad Z \; \#]$$

$$\text{Condition:} \quad X \neq T\#\#U$$

What (76) is supposed to say is simple: A monosyllabic de-
pendent gets destressed. A "dependent" is a member of a non-
lexical category ($[C_0\_\_C_0]$ in (76)) which is immediately domi-
nated by a node also dominating a member of a lexical cate-
gory. A lexical category is, of course, one participating in
the bar system and coming, in SPE theory (see introduction),
with flanking #'s. This lexical category, in (76), is in
$[(\#)X\acute{V}Y(\#)]$, providing the condition, which ensures that no
phrase boundary intervenes, is satisfied. Non-lexical cate-
gories are just those which commonly destress and reduce in
ordinary speech: auxiliaries (_will_ [l̩]), determiners (_the_
[ðə]), prepositions (_to_ [tə]), conjunctions (_or_ [r̩]), and
complementizers (_that_ [ðət]). These are quite capable of
bearing stress; but since stress requires unreduced vowels,
the idea, doubtless correct, is to condition reduction on
stresslessness by a rule like (76). The kinds of things
accounted for by (76) are represented in (77) below. I in-
stance once again _to_.

(77) (a) Mary spoke roughly ta Jóhn.

    (b) Mary spoke roughly ta thĕ thrée of them.

    (c) Mary spoke roughly ta thĕ Pãssămăquóddies.

    (d) *John was spoken roughly ta.

    (e) *John was spoken roughly ta bў Máry.

Note that the non-lexical categories, which we might, following Chomsky (1970), classify as $\begin{bmatrix} -N \\ -V \end{bmatrix}$, already have the property that, by virtue of the very phrase structure of English, they are dependents, occurring to the left of their heads. (With respect to conjunctions, the one slightly dubious case, I accept Ross' (1967) account, according to which X-CONJ-X, X an arbitrary category, has the structure (78).)

(78)
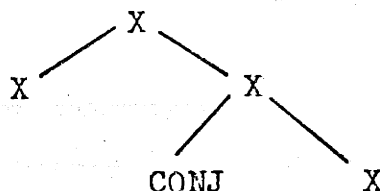
When, then, do they fail to be dependents? When the head has been removed, as in (77)(d,e), (71). But this we have a way of dealing with.

It seems to me that (76) says no more or less than (79).

(79) $V \rightarrow [-\text{stress}] / [_{\begin{bmatrix} -N \\ -V \end{bmatrix}} C_o \underline{\quad} C_o][+\text{seg}]$

According to (79), a monosyllabic non-lexical category de-stresses before something. The stress condition in (76), incidentally, seems to be unnecessary:

(80) Mary spoke roughly ta mĕ.

Now (79), a rule of high-level phonology, ought to pat-

tern with the a/an rule (27) and the wanna rule (72) with respect to failure across abnormal junctures.

Recall (75). I assume that the pre-terminal debris of the extracted (I use a neutral term; see Bresnan (1975), Chomsky (1977)) quantifier in (75) is present in the phrase marker. The result is something like (81).

(81)

```
                    V̿
                  /    \
               AUX      Ā̿
                |      /   \
                |    Q̿      Ā
                |           |
                |           A
                |           |
               is      enthusiastic
```
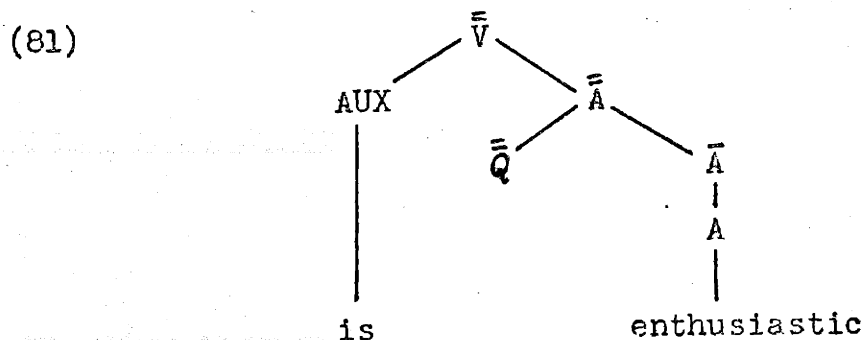
Then is and enthusiastic abut no one w-juncture, and (79) fails as desired. Similarly preposition-stranding facts like (77)(d,e).

Unlike the other non-lexical categories, be does not seem to show up only as a left branch. There are examples like (82), in which be occurs at sentence edge.

(82) I think, therefore I am.

Here, of course, I'm is impossible, even if more phonological material follows.

(83) *I think, therefore I'm. Descartes said that. We might at first think that this is as predicted. There are, to my knowledge, exactly three ways that a juncture can escape normality. They involve trace, parenthesis, and sentence edge, respectively.

(84)
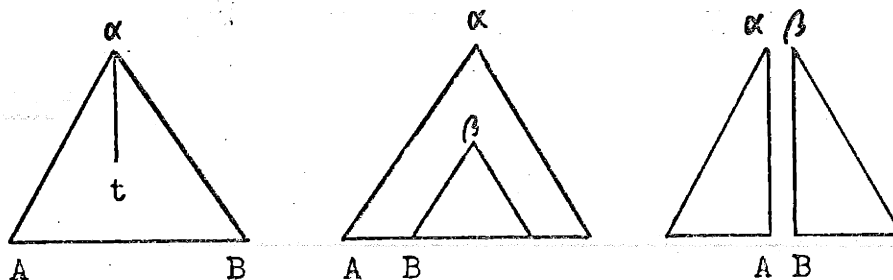
In all three cases, A and B abut no one w-juncture. The first
two have already been encountered. The third is hard to find
examples of in English, because the relevant rules affect
non-lexical categories, always left branches. And unfortu-
nately, I don't believe that be is an exception. Be 'exist'
simply never reduces, any more than will 'desire'. These
are surely verbs.

The desired examples do occur in French, however. See
Part III.

Interesting support for this analysis of auxiliary con-
traction comes from sentences like the following.

(85) *John's, I'm told, shattered by the news.

A theory stating simply that trace blocks auxiliary contrac-
tion has nothing to say about (85), where (one hopes) no ex-
traction has happened. But if the monosyllabic destressing
rule (79), and hence auxiliary contraction, respects w-junc-
ture in the same way as the a/an rule (27), failure of (79)
across parenthesis will parallel that of (27).

(In both cases, I reiterate, the pre-parenthetical
pause is irrelevant:

(86) an...absurd idea

(87) John's...completely shattered

These are perfectly decent, with no special limit on the
number of dots. Once again, all manner of sound effects,
hemming, hawing, throat-clearing, and so forth—anything
without linguistic significance—are permissible during
the pauses of (86), (87). These presumably join the signal
at the last minute and have no structural reflexes at all.)

It should also be true that, in general, destressing
by (79) fails before a parenthetical—not just when de-
stressing conditions contraction. This is also the case.

(88)(a) *You $\begin{cases} [k\overset{o}{æ}n] \\ [kn] \end{cases}$, I hope, explain that away.

(b) You [kǽn] , I hope, explain that away.

(c) You $\begin{cases} [k\overset{o}{æ}n] \\ [kn] \end{cases}$ explain that away, I hope.

(89)(a) *I'm [fr] , unquestionably, wombat leash laws.

(b) I'm [fór], unquestionably, wombat leash laws.

(c) I'm [fr] wombat leash laws, unquestionably.

(90)(a) *I ran into Matthew, Mark, Luke, John, [n],
surprisingly, Ralph, at the store.

(b) I ran into Matthew, Mark, Luke, John, [ǽnd],
surprisingly, Ralph, at the store.

(c) I ran into Matthew, Mark, Luke, John, [n]
Ralph, surprisingly, at the store.

The (a) sentences (of a type noticed first by Bresnan (1971))
seem quite operable upon by (79).

Again, the <u>wanna</u> rule does not apply either, though ap-
plicable, when the verb ends a parenthetical or when <u>to</u> be-

gins one:

(91) (a) *I'll try, if you wanna give you the results

tomorrow.

(b) *I wanna tell the truth, one scoop of vanilla.

These are, of course, really terrible, even when a reason-
ably rapid delivery squeezes out all pauses.

The logically possible sixth case, blockage of the a/an
rule by extraction, is contingently impossible by the rules
of English: Extraction of a nominal obviously cannot get
away with leaving an article behind.

I want to point out that the idea of normality of junc-
ture is quite independent of anyone's idea of word boundary.

Suppose we accept the theory of Selkirk (1972), accor-
ding to which a trace consists of word boundaries. In this
sort of treatment, the structural description of each rule
of trans-word-level phonology or morphology is written so
as to permit the presence of at most a single # between its
terms. Then it will fail across trace.

In connection with failure across parenthesis, we might
have the reasonable suspicion that any $\overline{S}$, even a parentheti-
cal one, will be flanked by ##. This in fact already follows
from SPE conventions.

(92) a--##although I hate to admit it##--

Thus the a/an rule, written

(93) a ⟶ an / ___ (#) V

obligingly fails where it ought to. The immediate objec-
tion is the existence of examples like the following.

(94)(a)   a--idiot that I am--

   (b)   a--in my opinion, anyway-- $\Big\}$ totally unex-

   (c)   a--amazingly--

          pected result

We would have to say that <u>all</u> parenthesized strings, domina-
ted by $\bar{S}$ or not, are flanked by ##. Needless to say, this
has a certain air of the ad hoc, although it could be true.
If we found another environment where the rule failed, there
would be nothing in particular to stop us from putting ##
there too. The symbol ## has at this point descended to the
level of diacritic, more or less without independent function
or significance apart from saying, This is a spot across which
rules are observed not to apply. A theory as weak as this
(which has no adherents, to my knowledge) is not easy to
prove wrong.

     I think that the right question to ask here is whether a
language learner could be counted on to have consistent expo-
sure to sentences in which the three rules discussed in this
Part have not applied across trace or parenthesis--i.e.
whether one <u>learns</u> that each structural description contains
(#), an ordinary contextual term. The answer is evidently no.
Therefore it seems preferable to look for more general proper-
ties of these rules or the structures they apply to from
which their behavior in such utterly marginal cases is pre-
dictable. I'm saying that (#) is <u>not</u> a contextual term--
it simply has to be there. Yet a "boundary" is an item of

vocabulary which one can presumably refer to or not, as one chooses, like any other item of vocabulary.

It is telling that there are no rules applying across full stop. Why is it that there are no rules of discourse phonology, affecting the last segment of a sentence under the influence of the the first segment of the next, or vice versa? It follows from the theory I'm defending, since there are no domination relations among sentences in a discourse. But even supposing we set up outermost sentence boundaries ###, the absence of rules in whose structural descriptions we referred to (###) would be unexplained.

Returning to the parenthetical issue, it is worth pointing out examples like (95),

(95)(a) an I can't tell you
    (b) an I don't know   how silly idea
    (c) an it's incredible

in which the pre-nominal $\overline{\overline{A}}$ contains a sentence, albeit of a distinctly limited type.

(96)(a) *an I'm completely ignorant of
    (b) *a John doesn't know   how silly idea
    (c) *an I know

The analysis we ought to give things like (95) presumably looks like (97).

(97) an $[_{\overline{\overline{A}}}$ $[_{\overline{S}}$ I can't tell you how $[_{\overline{A}}$ silly$]]]$ idea

The constituency relations of the embedded sentence are slightly at issue. Several considerations go to show that a structure like (97) is appropriate. The most obvious is

that I can't tell you (how) is not a parenthetical.

> (98)(a) *a(n), I can't tell you, how silly idea
>
> (b) *a(n), I can't tell you how, silly idea
>
> (c) a, you'll agree with me, silly idea

Further, I can't tell you and how behave like a constituent,
moving (99).(b) and even appearing in the first place (c,d)
together.

> (99)(a) I can't tell you how silly an idea
>
> (b) *I can't tell you a how silly idea
>
> (c) *an I can't tell you very silly idea
>
> (d) *an I can't tell you silly idea

We'd surely like to connect these possibilities with those
for the normal matrix I can't tell you:

> (100)(a) I can't tell you how silly a person he is.
>
> (b) *I can't tell you a how silly person he is.
>
> (c) *I can't tell you really silly person he is.
>
> (d) *I can't tell you silly person he is.

The natural idea is to put how in the complement of I can't
tell you in (99), just as in (100).

I can see very little reason for a boundary theorist to
say that ## does not separate an from the next word in (95).
However, (93) now gives (101):

> (101)(a) *a I can't tell you ⎫
>
> (b) *a I don't know   ⎬ how silly idea
>
> (c) *a it's incredible ⎭

(27), by contrast, gives the good results, since an and the
next word abut a single w-juncture.

Even more telling are examples like (102).

(102)   a what they call adult game

Again, <u>what</u> <u>they</u> <u>call</u> is no parenthetical.

(103)(a) *a, what they call, adult game

(b) a, what's the expression, adult game

The embedded sentence is surely inside the adjective phrase, standing to <u>adult</u> more or less as do <u>really</u>, <u>especially</u>, etc. In fact, the syntactic possibilities for <u>what</u> <u>they</u> <u>call</u> X parallel those for the workaday matrix <u>They</u> <u>call</u> <u>that</u> <u>X</u>.

(104)(a) a what they call adult game

(b) what they call an adult game

(c) *a what they call game

(d) *a what they call very adult game

(e) what they call a very adult game

(105)(a) They call that adult.

(b) They call that an adult game.

(c) *They call that game.

(d) *They call that very adult game.

(e) They call that a very adult game.

We could account for this by embedding (e.g.) precisely (105)(a) in the source for (104)(a).

(106)   a $[_{\bar{A}}$ $[_{\bar{S}}$ they call what $[_{\bar{A}}$ adult$]]]$ game

the necessary derivation by WH-movement shows that the embedded sentence is $\bar{S}$, not just S, as is at least <u>possible</u> in (97). No WH-words are vowel-initial. But there are almost identical examples like (107)

(107)   an as I guess they call those things adult game

where the vowel-initial complementizer shows up. We do not

have a.

The conclusion which this is helping us to draw is that

word boundaries are not only dispensible, but actually in-

correct.

With respect to auxiliary contraction through monosyl-

labic destressing, analogous examples suggest themselves.

(108)   John's $\begin{cases} \text{I can't tell you} \\ \text{I don't know} \\ \text{it's absolutely incredible} \end{cases}$ how

totally devastated.

(109)   John's what I'd call peculiar.

Recall the behavior of true parentheticals. It appears that

no quantity of word boundaries blocks these rules.

The parallel wanna examples unfortunately elude construc-

tion. Of course, if every to clause is a sentence, the wanna

rule involves terms on either side of ## every time it ap-

plies. On the other hand, if every to clause is a sentence,

the deleted (or unfilled) subject should block the rule.

Probably it is better (in general, not just for my purposes)

to assume that to clauses are verb phrases, with interpreted-

in subjects. Such an idea is not novel, occurring in various

connections to Emonds (1970), Schachter (1976), Lasnik and

Fiengo (1974), Brame (1976), Akmajian, Steele, and Wasow

(1977), and Bresnan (1976). In Bresnan (to appear) is a real-

ly thorough working-out of the various justifications and

consequences of this decision.

Another type of interesting phenomenon is represented in (110).

(110)   John shouted out an "I know!"

"I know!" is surely a noun here.   But is it also a sentence? That is, does the quotation have internal structure?

(111) $[_N [_{\bar{S}} [_S [_{\bar{N}} [_{PRO} \text{ I }]][_{\bar{V}} [_{\bar{V}} [_V \text{ know }]]]]]]$

I believe that the answer is no, even though the opposite conclusion appears at first sight to be useful against boundaries, in the same way as (107). Nevertheless, saying that quotations have syntactic analyses on the same order as the ones we'd give normal sentences amounts to saying that a grammar is responsible for generating them, whereas, obviously, any kind of noise, grammatical or not, English or not, human or not, is possible in place of "I know!", which happens to be human, English, and grammatical. If a grammar generated quotations, it would be impossible to say

(112)   "X" is ungrammatical.

where X is in fact ungrammatical.

Partee (1973), against this conclusion, points out that the contents of quotations are accessible to anaphora. An example:

(113)   John said, "Italktoofast," thereby convincing me
          that he did.

I'm not sure what this shows. Note (114):

(114)   John said, "Je parlons francais," thereby convin-
          cing me that he didn't.

Interpretation of the absent $\bar{V}$ in (114) evidently requires

knowledge of matters outside the language, as it does in more

famous (Hankamer and Sag 1976) examples like (115).

(115) Don't!

The interpretive processes which give sense to these kinds of

sentences certainly do not require something labeled $\overline{V}$ else-

where in the sentence or even in the discourse, in particular

not inside the quotes in (110).

The fact is that all true grammatical rules behave as if

quotations had no internal structure.

(116)(a) John can say "who" in Gaa.

(b) *"Who" can John say in Gaa?

(117)(a) It seems "who" is the only thing John can say

in Gaa.

(b) "Who" seems to be the only thing John can say

in Gaa.

In (116), "who" is not a WH-word, merely a noun phrase, and

hence is off-limits to WH-movement. But rules which move noun

phrases around, regardless of their contents, are quite capa-

ble (117) of moving quotations.

Note too examples like

(118) John shouted out an "I know! I know!"

Here we still want to say that the quotation is a noun. It

begins in a vowel, so an appears for underlying a. Now

"I know!" and "I know!", if sentences, must be syntactically

independent, as much as any pair of sentences in a discourse.

This appears to conflict with their both being dominated by N.

Evidently we are forced to conclude that "I know! I know!"

does not contain two adjacent sentences. Further, if
"I know!" were a syntactically independent sentence, like a
parenthetical, then a should regularly appear, as before a
parenthetical. But in fact there is no distinction between
(118) and (110). There is no vowel-initial quotation that
will not get an. John could shout out the entire pentateuch
without its blocking the a/an rule.

> (119)   John, highly agitated, leaped to his feet and
>   blurted out an "In the beginning..."

Likewise, in (120),

> (120)   "A" occurs before consonants.

if "a" were analyzed as a determiner, we'd be forced to say

> (121)   "An" occurs before consonants.

which is undesirable, and also untrue. Note that, because of
examples like (110), the theory that there is a "quotation
boundary" blocking high-level rules fails. Rather, the con-
tents of the quote are extralinguistic. I'll return to this
matter in Part IV.

In the rest of this work I will consistently eschew the
use of boundaries. (The same decision has recently been
made, though for different sorts of reasons, by Selkirk (to
appear).) Still, I will time and time again compare, with
reference to specific problems, boundary solutions and no-
boundary solutions. The running point of these comparisons
will be that boundaries are incapable, by their very nature,
of dealing properly with the real generalizations. But the
theory that is capable of dealing with the real generaliza-

tions can also reproduce the past results of boundary theory, with, it turns out, a more accurate fit to the facts.

It has not escaped my notice that every single one of the rules discussed in this Part involves non-lexical categories. This has a natural explanation. I refer the reader to Part III below.

APPENDIX TO PART I

Boundary Theory: History

I'd like to make a couple of remarks on the historical origins of boundary theory, as it is not immediately clear how linguists got into the habit of thinking of boundaries as palpable, manipulable juncture elements.

"Boundaries," in a sense that will become slightly clearer, were (first, to my knowledge, in Western traditions) recognized by Prague Circle linguists. The best disquisition on the subject is found in Trubetzkoy (1939). The observations in this book are not really oriented towards theory; they consist largely of a fairly harmless, though extensive, catalogue of boundary phenomena in phonology, classified in the usual ingenious fashion. "...jede Sprache...spezielle phonologische Mittel besitzt, die das Vorhandensein oder das Nichtvorhandensein einer Satz-, Wort-, oder Morphemgrenze an einem bestimmten Punkt des kontinuierlichen Schallstromes signalisiert..." These Grenzsignale include all phenomena accounted for by rules which, as rendered by post-SPE phonologists, have sentence, word, or morpheme boundaries in their structural conditions—all phenomena which, to Trubetzkoy's way of thinking, perceptually reveal the limits of these units. "Sie dürfen wohl mit den Verkehrssignalen in den Straßen verglichen werden." The functional explanation is every bit as bald as one begins to suspect: The reason

for the existence (e.g.) of the Greek spiritus asper or

English ŋ, occurring only in word-initial and -final position

respectively, is the demarcation of words. And "die kombi-

natorischen Varianten [allophones] sind nicht bloβ kasuell

bedingt Naturphänomene, sondern teleologisch bedingte Er-

scheinungen, die einen bestimmten Zweck haben und eine be-

stimmte Funktion ausüben. Diese Funktion besteht immer in

der Signalisierung der unmittelbaren Nachbarschaft eines

anderen Sprachelementes..." This we need not accept, and I

will have more to say about it shortly.

(Trubetzkoy classifies Grenzsignale first as phonemische

or aphonemische. The former are distinctive oppositions with

restricted privilege of occurrence (i.e. initial or final on-

ly); these would now be handled by conditions on word or mor-

pheme structure. With his customary unbelievable erudition,

Trubetzkoy notes that "dies ist der Fall z. B. bei den aspir-

ierten Verschluβlauten des schottischen (gaelischen) Dialektes

der Insel Barra, bei den aspirierten und bei den rekursiven

Konsonanten des Ostbengalischen, bei den rekursiven Ver-

schluβlauten und emphatischen-mouillierten Konsonanten des

Tschetschenischen...". The latter is edge-conditioned allo-

phony. "Im Obersorbischen wird der stimmlose gutturale Reibe-

laut x im Morphemanlaut als gutturale Affrikata kx...ge-

sprochen..." Trubetzkoy includes stress in this classifica-

tion.

(Grenzsignale also fall into Einzelsignale, like all the

examples above, or Gruppensignale--special combinations of

elements occurring only at boundaries. The English clusters
θs, ʃz, sθ, zʃ, čt, čs, sš, etc. are examples of what Tru-
betzkoy has in mind. Finally, Grenzsignale may be positive,
like all the examples already cited, or negative, meaning that
they signal the absence of a boundary. "Im Efik kommen h und
r nur im Inlaut vor..." Vowel harmony phenomena go here as
well.

(These three distinctions cross-classify, and Trubetz-
koy provides many examples. I go through this only to trans-
mit a feel for the vast range of phenomena Trubetzkoy consi-
ders.)

Trubetzkoy's picture of boundaries is rather healthy, in
my opinion. Since he has no notation, he is not tempted to
use boundaries notationally, and it certainly does not occur
to him to make phonemes out of them. A boundary, in his
usage, is no more than the left or right edge of any meaning-
ful element, not any sort of formal object of whose proper-
ties one may legitimately speak. I imagine it is similar to,
say, the modern idea of discourse, which certainly exists as
a kind of confluence of true formal objects, but which does
not, at least not so obviously, have non-trivial characteris-
tics of its own that do not follow from those of the smaller
linguistic elements to which it owes its existence. This
point of view I find myself in agreement with. Trubetzkoy
is merely observing that linguistic phenomena (if not rules
yet) are in some sense limited to explicit domains of influ-

ence, which he enumerates. (As we know, however, once this unexceptionable "boundary" gets itself symbolized by rule-writers, the temptation to regard it as a manipulable, operable-upon element like the others becomes irresistible.)

However, the idea that these phenomena are so limited merely in order to demarcate the speech signal is a little offensive, because it does not allow us to hope for interesting and (teleologically speaking) arbitrary properties of the rules behind them. On the subject of stress rules, for instance, Trubetzkoy remarks that they "dienen nur zur Signalisierung der Nähe der Wortgrenze..." He goes on: "Soweit der 'gebundene Akzent' [predictable stress] eine Wortgrenze angibt, hat er eigentlich nur im Satzinnern einen Sinn. In einer Sprache, wo in jedem Worte die letzte Silbe betont und somit die Schluβgrenze des Wortes angegeben ist, sollte eigentlich diese Endbetonung beim letzten Worte eines Satzes unterlassen werden, da in diesem Falle die Schluβ-grenze des Wortes ohnehin durch die Schluβpause des Satzes genügend signalisiert ist. Dies ist auch tatsächlich in vielen Sprachen der Fall. Nach E. D. Polivanov soll im Koreanischen jedes Wort die Schluβsilbe betonen, und nur das letzte Wort im Satze betont die Anfangssilbe..." Following this reasoning, stress rules should also fail in words spoken in isolation, in parenthesis, or with list intonation. Surely the facts he mentions are themselves higher-order boundary phenomena, to be accounted for by sentence-level rules.

I might also cite the obvious existence of example after example of bread-and-butter rules of external sandhi which conspire to destroy Grenzsignale. French liaison, for instance, which effectively inserts a word-final consonant intervocalically, obliterates VV sequences which, left unmolested, would beautifully mark word boundary. Quite routinely, in fact, deletion and insertion rules at word edge (there are examples in almost every Indo-European language) make at least a half-hearted attempt to generalize word-level phonotactics and syllable structure over the sentence. Still more striking is the fact that such effects, when related to speed of utterance, occur during rapid delivery, when decodability and segmentability are presumably at a premium. I really doubt that any broad functional explanation for the existence of boundary phenomena (let alone their properties) is going to get off the ground. I do not offer any kind of explanation myself, because in my opinion there isn't one, any more than there is an explanation for the existence of transformations. I can at least conceive of a phylogenetic explanation for the integration of both of these into the linguistic faculties, but it would surprise me to find anything to explain why they enter into contemporary, already-evolved linguistic systems. (I do have explanations for the curious facts about boundary effects which I mentioned; see Part IV.)

Trubetzkoy himself mentions that "in Bezug auf die Verwendung der Grenzsignale sind die einzelnen Sprachen sehr

verschieden. In den einen Sprache werden hauptsächlich (oder sogar ausschließlich) die Morphemgrenzen signalisiert, in den anderen die Wortgrenzen." This corresponds to the mundane observation that some languages have busier word-level phonologies, while others have busier morphologies. "Es gibt Sprachen, die nicht nur sehr wenig Grenzsignale besitzen, sondern sie auch sehr selten verwenden, so daß nur ein ganz unbedeutender Prozensatz aller Wort- (bezw. Morphem-) grenzen in einem zusammenhangenden Texte 'signalisiert' sind. Zu solchen Sprachen gehört z. B. das Französische, das auf das Abgrenzen der Wörter (bezw. Morpheme) im Satze sehr wenig Wert legt." In these passages Trubetzkoy seems to be doing himself in. This kind of variability in elaboration of Grenzsignale evidently conflicts with his explanation for their existence, assuming (to adopt his idiom) that one language needs to delimit its units as much as another. In my opinion Trubetzkoy underestimates (actually, fails to consider) the contribution of the lexicon to segmentation.

Juncture symbols appeared in American structuralist work at about the same time as the Grundzüge (Sapir and Swadesh 1939, Trager and Bloch 1941). The functionalist slant is of course absent, but essentially similar phenomena are at issue: Asymmetric distribution and allophony respecting unit edge. The boundary symbol is just a way of notating the edge, which Trubetzkoy did not find it necessary to do. But by the time of Harris (1947) and Trager and Smith (1951) junctures had

taken on a really dangerous theoretical status. I will eluci-
date.

Harris sets up junctures whenever he wants to reduce pho-
neme inventories or to express parallel distributions of sets
of phonemes. So, for instance, he observes (drawing from
Trager and Bloch (1941)) the [ay] of minus and the [Ay] of
slyness, "distinguished chiefly by length and type of off-
glide," dislikes the idea of both /ay/ and /Ay/, and accor-
dingly sets up a juncture /-/ which can then be said to con-
dition the distinction: m/ay/nus, sl/ay-/ness. Investiga-
ting further, he notices pl[ey]ful, tr[Ey]full, etc., whose
parallel distinction the same subterfuge will account for.
Likewise wholly different alternations, e.g. the respective
aspiration and nonrelease of the k in market and What a lark!
So far, so good: /-/ is just a way of writing the morpho-
logical edge, although we are not allowed to admit that yet:
"Our arrangement is useful because it will later appear that
whenever the segment [k'] occurs there is a morphological
boundary following it (a boundary which also occurs at utter-
ance end), so that /-/ becomes a mark of that boundary."

In fact "the great importance of junctures lies in the
fact that they can be so placed as to indicate various mor-
phological boundaries." (Harris is using the word juncture
to refer to his formal constructs, boundary to correspond
roughly to Trubetzkoy's Grenze.) "For example, replacing
Swahili V́CV by VCV# is particularly useful because the V

following V́ is regularly the end of an independent morpho-
logical element..." But "things do not always work out so
nicely. In German, we find [t] but not [d] before # ([bunt]
'group', [vort] 'word), while [t] and [d] occur in identical
environments within utterances ([bunde] 'in group', [bunte]
'colored', [vorte] 'in word'). If we insert # after every
[t]and then group [t] and [d] into one phoneme, we would
find that we are writing # in the middle of morphemes (e.g.
/d#ayl/ Teil 'part'). We could still phonemicize [t] as
/d#/, i.e. use the /#/ to indicate that a preceding /d/ rep-
resents the segment [t], but many of the occurrences of this
/#/ would not correlate with morphological boundaries."

In this passage, amazing to modern ears, we realize
what lies within Harris' power. A juncture, according to him,
is just a formal device to allow for simplification of pho-
neme inventories. It may or may not correspond to the po-
sition of any boundary, which at this stage of the analysis
has not been discovered anyway. We might, in a whimsical
frame of mind, start by eliminating English /t/ and replacing
it with /d@/. "By the setting up of junctures, segments which
had previously contrasted may now be associated together into
one phoneme, since they are complementary in respect to the
juncture." We can now write a rule taking /d/ to [t] before
/@/. The entire voiceless obstruent series can be eliminated
with the same juncture, which we might take as a point in its
favor. Given enough juncture symbols, in fact, we could easi-
ly make every English phoneme a positional variant of /ŋ/.

"The fact that one phonemic juncture has been recognized does not preclude the recognition of additional independent phonemic junctures."

The reason that there is nothing in particular to stop such a laughable proliferation of ad hoc junctures is that they are not identified with boundaries (in Harris' and Trubetzkoy's sense), i.e. edges, which are reassuringly exiguous. Harris' method (or the more widespread practice it makes explicit) further discourages any such identification because junctures have to be phonemes, while boundaries are surely not: The environment of a phoneme is defined as the phonemic elements on its left and right. Junctures are also phonemes in Trager and Smith (1951).

Putting the matter a little more strongly, it is not entirely unreasonable to blame the introduction of this type of string juncture symbol on the American structuralist method itself. Such symbols are necessary precisely because of the inflexibility of the bottom-up analytical procedure. At the level that they first appear, they are merely place-holders for the real "boundaries," which the linguist is betting will show up at the higher levels. But given the possibilities for overzealous reduction which I mentioned, the linguist will sometimes be disappointed.

(In a theory like mine, however, which has no juncture elements, only an all-purpose symbol meaning 'edge of domain,' these problems do not arise, because the edges are where they are, not where we feel like putting them. All "boundary phe-

nomena" are direct reflections of constituency at the various levels of structure. Only as many sets of boundary phenomena--domains of application--may exist as there are levels.)

In SPE theory, the next major development, junctures and boundaries (both in Harris' sense) are one and the same. A juncture is no longer merely a symbol but a terminal element of which one expects as much epistemological significance as one does of any other element. The rationalist underpinnings of the book force this identification. But now that boundaries have formal existence in mental representation, it seems reasonable to want to manipulate them like the others; hence the abuses I mentioned earlier. Boundaries are once again diacritics, but now diacritics with psychological reality.

The reason, incidentally, that boundary symbols are now used only in phonology is that they were inherited directly from American structuralism, which of course had not recognized the exactly parallel "boundary effects" in syntax.

PART II

THE IRISH INITIAL MUTATIONS

As an extended second example of high-level morphology
and what I believe is generally true of it, I want to discuss
the initial mutations of Modern Irish. What's meant by this
term is a series of rather spectacular alternations undergone
by the initial consonants of words, under certain interesting
conditions. The rules responsible for the mutations have
properties and limitations which are remarkably similar, at a
certain depth of analysis, to those of the English rules of
Part I.

Special thanks go to Ken Hale for suggestions and com-
ments on the Irish.

The dialect I cite is "standard" Munster Irish through-
out, with the occasional Galwayism, though all dialects are
substantially the same with respect to the mutations. General
information on the mutations is easily had; see, for instance,
de Bhaldraithe (1953), Dillon and Ó Cróinín (1961), Ostedal
(1962), Mhac an Fhailigh (1968). Initial mutations in some
form are in fact pan-Celtic phenomena, showing up in all of
the several languages of the Celtic group.

There are two major mutations, known among Celticists as
lenition (sometimes "aspiration") and nasalization (sometimes
"eclipsis"). By lenition, extremely roughly speaking, stops
spirantize and spirants either turn to [h] or else delete.

By nasalization, unvoiced consonants voice and voiced ones
nasalize. In the table below I've represented in detail the
effects of the two rules.

			lenited		nasalized	
(1) p	p', p	ph	f', f	bp	b', b
t	t', t	th	h', h	dt	d', d
c	k', k	ch	ç, x	gc	g', g
b	b', b	bh	v, w^	mb	m', m
d	d', d	dh	j^, ɣ	nd	n', n
g	g', g	gh	j^, ɣ	ng	ŋ', ɔ
f	f', f	fh	zero	bhf	v, w^
s	š, s	sh	h', h		
m	m', m	mh	v, w^		

Irish consonants, written in Irish as in the first column,
come in (contrastive) palatalized and velarized varieties;
these are represented phonetically in the second column. I
use the apostrophe to indicate palatalization whenever the re-
quired distinctions are not made by normal IPA symbols, which
is most of the time. In the third column are the written le-
nited consonants: Ch, C any written unlenited consonant.
Next are the pronunciations of the lenited palatalized and le-
nited velarized consonants, respectively. In the fifth column
are the written nasalized consonants: NC, where C is any
written unnasalized consonant and N is the spelling of the re-
sult of nasalizing the consonant C represents. Last are the
pronunciations of the nasalized palatalized and nasalized ve-

larized consonants. [jˆ] is a voiced palatal fricative; [wˆ] is a voiced bilabial fricative with lip rounding.

To a reasonable first approximation, the rules behind these alternations are (2) and (3). Though their phonological details are not my primary concern here, it will be nice to have concrete rules to refer to.

$$(2) \quad \begin{bmatrix} +cons \\ \left\langle \begin{matrix} -vce \\ \kappa cor \\ +cont \end{matrix} \right\rangle \end{bmatrix} \rightarrow \begin{bmatrix} +cont \\ -son \\ -\alpha cons \\ \langle \kappa unit \rangle \end{bmatrix} \Big/ \quad [+L] \underline{\quad}$$

$$(3) \quad \begin{bmatrix} +obst \\ \kappa vce \end{bmatrix} \quad \begin{bmatrix} \kappa nas \\ +vce \end{bmatrix} \Big/ \quad [+N] \underline{\quad}$$

Notice that both rules have a distinctly morphological smack to them: Their applicability turns on the presence in the preceding word of an abstract morphological feature [+L] or [+N], whose sole function is to exact the proper mutation from the following initial. No word is both [+L] and [+N], although many are neither. I assume, then, that [+N] implies [-L], and [+L] [-N], and also that all words which do not cause mutations are [uL, uN]. That is:

$$(4) \quad [\alpha L] \equiv [-\alpha N]$$

$$[uL] \equiv [uN]$$

A reasonable idea at this point is to reduce these to a single binary-valued feature, [±IM], such that [+IM] is equivalent to [+L] and [-IM] is equivalent to [+N]. The implications corresponding to those in (4) now come automatically. We rewrite (2) and (3) as

(2)' .../ [+IM] $\underline{\quad}$

(3)' .../ [-IM] $\underline{\quad}$

These features, as we will see, are lexical properties of words, mostly escaping generalization other than historical. As further support for the morphological character of (2)' and (3)', I observe that there is a collection of more clearly morphological rules which must follow. (I am assuming, of course, some version of the autonomous morphology thesis: See Aronoff (1976), but Carrier (1978), Aronoff (1978).)

An example. The homophonous possessive pronouns a 'his', a 'their', and a 'her' differ only in the mutations they cause.

(5)(a) a bhó   'his cow'

(b) a mbó   'their cow'

(c) a bó   'her cow'

(d) bó   'a cow'

A 'his' is marked [+IM], taking the following [b] to [w^]. A 'their' is [-IM], taking the same [b] to [m]. A 'her' has no marking, and induces no mutation.

Now the preposition ag 'at, by, of' together with a following a (any of the three) surface as a lexically suppletive form á. (The phonology which might produce this would have no generality whatsoever.) The rule is probably a "stupid" one, similar, say, to the rules taking à le to au, in dem to im, and want to to wanna--saying no more than (6):

(6) ag a → á

But á still behaves like three words with respect to the mutations:

(7)(a) á bhó 'his cow's'

   (b) á mbó 'their cow's'

   (c) á bó   'her cow's'

The point is that, to avoid multiplying á's, the mutations

must precede the disappearance of the three a's via (6).

Similarly, the possessive pronoun mo [mʌ] 'my' induces

lenition.

   (8) mo bhó 'my cow'

It also loses its carat before a noun beginning in a vowel.

   (9) m'ainm 'my name'

This elision, like similar rules in a great many languages,

is limited to a small set of specifiers, and is not phonologi-

cal in nature. What happens, we might ask, before f?

   (10)(a) *mo fhear [mʌ æ:r]

   (b) m'fhear [m æ:r] 'my husband'

   (c) fear   [f æ:r] 'a man'

The f lenites--deletes--by (2)', then elision.

It is reasonably easy to conclude from things like this

that the mutations are not phonological rules in the same spi-

rit as, for instance, French liaison or Italian raddoppiamento,

for which see Part III.

Both lenition and nasalization had real phonological con-

ditioning in Common Celtic, the ancestor of the modern Celtic

languages. Lenition originally occurred after word-final vow-

els, nasalization after word-final nasals. But the idea of

reconstructing these segments in the lexical representations

of the modern Irish words is a bad one. First, the attrition

of Common Celtic endings was rather brutal, often wiping out syllables or more. Therefore, for example, proposing a phonotactically reasonable underlying final -N/ for a word in -CI which causes nasalization will entail, in most cases, supplying an entire underlying final syllable -VN/ . It would have to delete unconditionally: There are, to my knowledge, no phonological reflexes of the lost endings apart from the mutations. A further misfortune for this line of thought is the fact that modern Irish words which actually do end in vowels and nasals do not reliably cause anything in particular. Supposing we wanted to make the mutations phonological rules, we would be forced to protect these with underlying finals which also wound up deleting by special rules. This seems moderately artificial. Even on a priori grounds, we can be sure it's wrong. (These remarks are by way of defense of my abstract treatment of the mutations, which, although tolerably natural and easily the least involved, seems on principle to make certain phonologists nervous.)

The collection of phenomena generally referred to as the mutations falls into two classes, only one of which I am really concerned with here. The distinction between the two, not even recognized, to my knowledge, by any writer on the subject, is one I want to clearly draw. In the first, interesting class, a mutation is clearly induced in some way by the word to the left of the affected one. (I will often speak of words as causing and undergoing the mutations.) Mo induces lenition: This is indisputable, though so far without much

theoretical content. Words after mo always lenite, and
the same words unpreceded by mo (or any other known leni-
tion-inducer) do not.

In the other class, it is not in my opinion profitable
to think of the mutation as having anything at all to do with
the preceding word. Rather, it is part and parcel of the
number-, gender-, case-, definiteness-, and tense-marking sys-
tems. Nouns, for instance, when genitive, masculine, singu-
lar, and definite, show up with lenited initials.

(11)(a) capall Shéamais

   horse   Séamas-GEN-M-SG-DEF

   'Seamas' horse'

(b) mac Sheáin

   son Seán-GEN-M-SG-DEF

   'Sean's son'

(c) póca   chóta   na mná

   pocket coat-GEN-M-SG-DEF the woman-GEN

   'the pocket of the woman's coat'

It would be a mistake to imagine that the nouns capall, mac,
póca are in any sense inducing lenition, because the same ef-
fect occurs when an article precedes the genitive noun.

(12)(a) capall an fhir

   horse   the man-GEN-M-SG-DEF

(b) mac an bháicéara

   son the baker-GEN-M-SG-DEF

(c) póca   an chóta

   pocket the coat-GEN-M-SG-DEF

Further, if we merely substitute an adjective for the possessor noun, it remains intact:

(13)(a) capall mór

horse big

(b) mac díomhaoin

son unemployed

(c) póca   beag

pocket small

Yet a <u>following</u> possessor still lenites:

(14)(a) capall mór Shéamais

(b) mac díomhaoin Sheáin

(c) póca beag chóta na mná

Notice too that if the head noun is itself genitive, masculine, singular, and definite, the adjective lenites.

(15)(a) ceann an chapaill   mhóir

head the horse-GEN-M-SG-DEF big-GEN-M-SG-DEF

(b) deartháir an mhic   dhíomhaoin

brother   the son-GEN-M-SG-DEF unemployed-GEN-

M-SG-DEF

(c) tóin   an phóca   bhig

bottom the pocket-GEN-M-SG-DEF small-GEN-M-SG-

DEF

The proper conclusion is surely that the allomorphy triggered by the grammatical marking of the noun ((11), (12), (14), (15)) or the adjective ((15)) includes mutation of its initial, just as it (often) does vowel ablaut and change in quality of final consonant.

(16)(a) an mac [mak]

the son

(b) an mhic [vik']

the son-GEN

The preceding word is irrelevant. To suggest, as investigators usually do, that lenition occurs "after" a possessee noun, or "after" a genitive article, and so on, is in my opinion missing the point of the thing. This point is that the bundle of features [+GEN, +M, +SG, +DEF] is morphologically signaled by a lenited initial.

This kind of treatment explains facts like the following. Adjectives also lenite when qualifying a noun which is feminine, nominative, and singular, i.e. when marked that way themselves. This marking must have occurred, because adjective and noun agree morphologically in respects other than lenition.

(17)(a) an bhean   mhór

the woman-NOM-F-SG big-NOM-F-SG

(b) na mná   móire

the woman-GEN-F-SG big-GEN-F-SG

(c) na mná   móra

the women-NOM-F-PL big-NOM-F-PL

(d) na mban   mór

the women-GEN-F-PL big-GEN-F-PL

Multiple adjectives also lenite, providing a few difficulties for a theory trying to make the immediately preceding noun responsible, i.e. [+IM].

(18)   an bhean mhór shaibhir

rich

But a demonstrative, presumably not marked for gender, number or case since not visibly inflected for them, does not lenite.

(19)   an bhean $\left\{\begin{array}{l} \text{sin} \\ \text{*shin} \end{array}\right\}$

that

'that woman'

Similarly, a predicate adjective is evidently inaccessible to grammatical marking, taking the unmarked NOM-M-SG form regardless of the preceding noun phrase.

(20)   Tá $\left\{\begin{array}{l} \text{an bhean} \\ \text{an fear} \\ \text{na mná} \\ \text{na fir} \end{array}\right\}$ cliste.

be $\left\{\begin{array}{l} \text{the woman} \\ \text{the man} \\ \text{the women} \\ \text{the men} \end{array}\right\}$ smart

The prediction is that lenition will also fail. It does. Compare:

(21)   Tá an bhean cliste.

is the woman smart

'The woman is smart.'

(22)   Tá an bhean chliste saibhir.

rich

'The smart woman is rich.'

(23) Tá an bhean chliste shaibhir brónach.

                                                  unhappy

'The smart rich woman is unhappy.'

(24) Tá an bhean chliste shaibhir bhrónach anseo.

                                                     here

'The smart rich unhappy woman is here.'

Similarly, instead of saying (Dillon and Ó Cróinín 1961, de Bhaldraithe 1953) that lenition occurs "after" a noun in the dual number,

(25) dhá mhuic mhéithe

        two pigs fat

it is less misleading to say that the bundle of features [-SG, -PL], dual, gets inflectionally realized as a lenited initial. Notice, incidentally, that the dual noun itself, mhuic, suffers precisely the same mutation. This, of course, is what we expect. Usually, however, it is accounted for separately, by saying that lenition occurs "after dhá," without which article the dual is not found.

(26) *mhuic mhéithe

       *muic mhéithe

       'fat pigs-DUAL'

As a final objection to the idea, I think probably based on historical considerations, that a mutation must be induced by some other word, I observe that verbs, unpreceded by anything in the normal VSO sentence, lenite spontaneously, without benefit of any possible trigger to the left, as part of the morphological marking of the preterite, the past habitual,

and the conditional. The bare verb stem, for instance, visible in the imperative, lenites to form the "personal" preterite.

(27) Bí ag dul   abhaile.

       be at going home

       'Go home.'

(28) Bhí   sí ag dul abhaile.

       be-PST she

       'She was going home.'

(29) Cuir an leabhar anseo.

       put the book   here

       'Put the book here.'

(30) Chuir   sí an leabhar anseo.

       put-PST

       'She put the book here.'

Optionally present in some dialects is the verbal prefix do-, also marking the preterite: do-chuir, etc. Do- is entirely absent in many areas. It is important to show that, even though historically responsible for the now morphologized lenition, do- is not [+IM]. Note first that lenition in the preterite is areally invariant and obligatory, though do- actually occurs with great inconsistency. Second, Ken Hale points out to me that the (optional) do- now also occurs with the "impersonal" (essentially passive) preterite, where lenition does not. So, for instance,

(31)(a) *(Do-)cuir sí an leabhar anseo.

   (b)   (Do-)chuir sí an leabhar anseo.

(32)(a) (Do-)cuireadh an leabhar anseo.

(b) *(Do-)chuireadh an leabhar anseo.

'"They" put the book here,' i.e. 'The book got put here.'

In the future, we find the stem adorned with the future ending.

(33) Cuirfidh sí an leabhar anseo.

put-FUT

'She'll put the book here.'

Compare the habitual and conditional, which, in addition to taking special endings, also lenite.

(34) Chuireadh sí an leabhar anseo.

put-PST-HAB

'She used to put the book here.'

(35) Chuirfeadh sí an leabhar anseo.

put-COND

'She'd put the book here.'

This kind of example appears to present terminal difficulties for the theory that something must "cause" mutation across w-juncture. Rather, somewhat like strong verb ablaut in English or umlaut in German (marking tense and number respectively), the once contextually-conditioned alternations, detached from all segmental provocation by reason of its historical loss, have been reanalyzed as productive inflectional processes.

There are a good many examples of the preceding type in Irish, involving clusters of grammatical features triggering

one mutation or the other. I will not enumerate them here (they are slightly beside the point), but content myself with noting their existence. Nevertheless, we surely want (2)' and (3)' to be responsible at some level, because the alternations are in every respect identical to those induced by words of the mo sort. I now turn to this problem.

Remaining is a very large residue of mutations not traceable to the inflectional makeup of the word containing the mutating segment. Mo, for instance, cooccurs with lenition of the following word absolutely regardless of any markings it, the following word, may have received.

(36) fear M 'a man'

bó   F 'a cow'

		M	F
NOM	SG	m'fhear	mo bhó
	PL	m'fhir	mo bha
GEN	SG	m'fhir	mo bhó
	PL	m'fhear	mo bhó

It strikes me as a really remarkable fact that every single one of the words which we must conclude are inducing mutations across w-juncture is non-lexical. The possessive pronouns, for instance, almost all induce one mutation or the other.

(37)

		SG			PL	
1		mo [+IM]			ár	[-IM]
2		do [+IM]			bhur	[-IM]
3 F		a			a	[-IM]
M		a [+IM]				

mo chapall	ár gcapall
do chapall	bhur gcapall
a capall	a gcapall
a chapall	

'my horse'	'our horse'
'your-SG horse'	'your-PL horse'
'her horse'	'their horse'
'his horse'	

Clearly a small subgeneralization is available in this instance.

$$(38) \quad \begin{bmatrix} \alpha PL \\ uF \\ mIM \end{bmatrix} \rightarrow [-\alpha IM]$$

Among non-lexical items capable of bearing these features, i.e. inflected nominal specifiers, plurality entails nasalization and singularity lenition. A 'her', alone marked for femininity, causes no mutation. We could, of course, simplify (38) a bit by making a [uIM]; the other possessives, in any case, are simply [mIM].

The quantifier aon 'one' and the associated ordinal céad 'first' both lenite:

(39)   aon chapall

(40)   an chéad chapall

Seacht, acht, naoi, and deich, 'seven', 'eight', 'nine', 'ten' all nasalize the next word:

(41)   seacht gcapaill

(42)   acht gcapaill

(43)   naoi gcapaill

(44) deich gcapaill

Notice the plural gcapaill. Now trí, cheithre, cúig, sé,
'three', 'four', 'five', 'six' all lenite.

(45) trí chapall

(46) cheithre chapall

(47) cúig chapall

(48) sé chapall

Notice the singular chapall. The inescapable conclusion is
that aon, trí, cheithre, cúig, and sé are [mIM, -PL], hence,
by (38), redundantly [+IM]; seacht, acht, naoi, and deich are
[mIM, +PL] and thus [-IM].

Other numerals, e.g. fiche, triocha, daichead, 'twenty',
'thirty', 'forty', cause no mutation.

The nominative singular definite article, an, lenites
feminine nouns, but does nothing to masculine ones.

(49) an bhó

(50) an fear

It appears we must list both an [+NOM, +F, +SG, +IM] and
an [+NOM, +M, +SG, uIM]. Similarly, the dative singular ar-
ticle, yet again an, is [+DAT, +SG, -IM]:

(51)(a) capall 'a horse'

   (b) bean 'a woman'

(52)(a) an capall 'the horse-NOM'

   (b) an bhean 'the woman-NOM'

(53)(a) ag an gcapall 'at the horse-DAT'

   (b) leis an mbean 'with the woman-DAT'

Finally, the genitive plural article, presumably [mIM] and

subject to (38), also nasalizes:

(54)(a) cosa na gcapall 'the legs of the horses-GEN-PL'

(b) deirfiúracha na mban 'the sisters of the wo-

men-GEN-PL'

Note that, unlike the situation in examples such as (18) and (24), in which mutation-by-grammatical-marking affects a noun and all like-inflected hangers-on, here we do not get nasalization of anything more than the noun directly to the right of na.

(55)(a) cosa na gcapall $\begin{Bmatrix} \text{*mbán} \\ \text{bán} \end{Bmatrix}$

white

'the legs of the white horses'

(b) deirfiúracha na mban $\begin{Bmatrix} \text{*gcóir} \\ \text{cóir} \end{Bmatrix}$

just

'the sisters of the just women'

The degree modifiers an 'very' and ró 'too', occurring pre-adjectivally, are [+IM].

(56)(a) maith 'good'

(b) beag 'small'

(57)(a) an-mhaith 'very good'

(b) an-bheag 'very small'

(58)(a) ró-mhaith 'too good'

(b) ró-bheag 'too small'

Quite a few prepositions also induce lenition (assuming no covert cases in Irish), e.g. ar 'on', do 'to', ó 'from', trí 'through', gan 'without', fá 'under'.

(59) ar bhó 'on a cow'

(60) do Sheán 'to Sean'

(61) ó Cheimbridge 'from Cambridge'

(62) trí chorrach 'through a marsh'

(63) gan bhó 'without a cow'

(64) fá chloich 'under a stone'

The one preposition which nasalizes is i 'in'.

(65) i mBoston 'in Boston'

Many miscellaneous verbal particles induce a mutation to their right. Pre-verbal ní 'NEG', má 'if', and the complementizer a lenite:

(66) Creideann sé é.

      believe   he it

      'He believes it.'

(67) Ní chreideann sé é.

      NEG

      'He doesn't believe it.'

(68) Má chreideann sé é, tá sé ar buile.

      if   is he on madness

      'If he believes it, he's crazy.'

(69) Is bréag mheabhlach an scéal a   chreideann sé.

      is lie   malicious the story that

      'The story that he believes is a malicious fabri-

            cation.'

Nasalizing are (e.g.) pre-verbal an 'Q', dá 'if', mara 'un-less', and the complementizer go:

(70) An gcreideann sé é?

Q

'Does he believe it?'

(71) Dá gcreideadh   sé é!

if believe-PST-SUBJUNC

'If only he believed it!'

(72) Mara gcreideann sé é, tá sé cliste.

unless   is he smart

'He's smart if he doesn't believe it.'

(73) Is iad na daoine go gcreideann sé a   scéal.

is they the people that   their story

'They're the ones whose story he believes.'

The copula induces lenition to its right in the past and conditional, which are homophonous in this case.

(74) Ba   dhochtúir Máire.

is-PST/COND doctor

'Maire was/would be a doctor.'

Because just about anything can occur to the right of the copula, as opposed to most of the specifiers in the foregoing examples, the leniting copula is a particularly good illustration of the difference between lenition-by-sentence-morphology and lenition-by-grammatical-marking. We can assume, for instance, that case-marking affects only [+N] items, i.e. the nouns and adjectives. There is no case paradigm for, say, the possessive pronouns (37). (Recall (36).) This explains why we don't get things like

(75) *capall mh'fhir

horse my man-GEN

'my husband's horse'

(where <u>mo</u> would have to be marked [+GEN, +M, +SG, +DEF], like <u>fir</u>), but rather

(76)   capall m'fhir

Non-inflected items <u>never</u> mutate, when the mutation is associated with grammatical marking. But when it isn't,

(77)   Ba shin é.
          that it
       'That was/would be it'

mutation regularly occurs. (Compare (77) with (19) above.)

Drawing all this together, lenition and nasalization are triggered by abstract features [+IM], [-IM], originating either (a) in the preceding word or (b) in the mutating word itself.

When and only when the feature comes from the preceding word, that word is non-lexical.

We must fix up (2)' and (3)' a little, since they account only for case (a); yet precisely the same alternations (1), I repeat, are characteristic of case (b). A reasonable emendation, it seems to me, is as in (2)", (3)",

(2)"   .../ ∅ $\left[\begin{array}{c} \overline{\quad} \\ +IM \end{array} \right]$

(3)"   .../ ∅ $\left[\begin{array}{c} \overline{\quad} \\ -IM \end{array} \right]$

supplemented by (78):

(78)   [αIM] [uIM] → [uIM] [αIM]

(2)" and (3)" mutate only a word bearing the proper feature.

(I assume for now a normal feature percolation convention,
whereby every segment of a word marked [+F] is also marked
[+F].) This feature may arise as a byproduct of number, gen-
der, case, definiteness, and tense marking. A series of re-
dundancy rules marks words carrying certain bundles of fea-
tures as either [+IM] or [-IM]. For instance, as we saw
above ((25)),

(79) $\begin{bmatrix} -SG \\ -PL \end{bmatrix} \rightarrow [+IM]$

dual marking entails lenition.

(78) transfers a mutation feature from the preceding
word, provided it has one. It, the word on the left, must
lose its feature in the process: go creideann, for example,
becomes go gcreideann, not *ngo gcreideann. I am assuming, of
course, that (78) precedes (2)" and (3)". As for the ordering
of (78) with respect to redundancy rules like (79), we have
two choices. The fact is that (78) applies only to markings
[+IM] which do not arise by grammatical rules like (79), i.e.
only to those features which are associated from the start
with particular non-lexical items. Otherwise, we would get

(80)(a) *an bean shin

(b) an bhean sin

the woman that

'that woman'

(81)(a) *Cuir shí an leabhar anseo.

(b) Chuir sí an leabhar anseo.

put she the book here

'She put the book here.'

(82)(a) *Tá an capall Séamais mhór

    (b) Tá an chapall Shéamais mór

       is the horse   Seamas-GEN big

       'Seamas' horse is big.'

and the like. We could order (78) before (79) (etc.). How-
ever, this seems slightly suspicious to me, since I'd like to
see grammatical marking preceding morphological rules like
(78). The alternative is to restrict (78) to non-lexical i-
tems, which are exactly the ones which have $[\pm IM]$ not from
(79).

$$(78)' \quad \begin{bmatrix} \alpha IM \\ -N \\ -V \end{bmatrix} \quad [uIM] \rightarrow \quad [uIM] \quad [\alpha IM]$$

I'll accept this for the time being, though it turns out be-
low that we can simplify things a little. Right now, though,
I'd like to point out that a boundary-strength account of the
same facts is going to be inadequate. Suppose we recast (78)'
as (83):

(83) $[\alpha IM]$ # $[uIM] \rightarrow [uIM]$ # $[\alpha IM]$

This rule requires that $[\pm IM]$ travel across a single word
boundary. But this is not strong enough, as *an bean # shin
and *cuir # shí point up.

Now (78)' is a high-level morphological rule. It ap-
plies across w-juncture. By its very nature, it applies only
across w-juncture: We have no reason yet to imagine that the
structural description of (78)' could ever be met word-inter-

nally, i.e. that the feature [+IM] is less than word-sized. See, however, below.

By hypothesis, (78)' ought to behave like other rules of high-level morphology, for instance the ones discussed in Part I, in the characteristic respects. In fact it is true that the mutations apply across pause.

(84) Ba...dhochtúir í.

was doctor her

'She was...a doctor.'

Parentheticals, unfortunately, are impossible after non-lexical items, including all mutation-inducers.

(85)(a) *Ba, deir sé, dochtuir í.

(b) *Ba, dheir sé, dochtuir í.

(c) *Ba, deir sé, dhochtuir í.

says he

'She was, he says, a doctor.'

On the other hand, a parenthetical is (slightly marginally) possible after the head of a genitive construction. Since, according to the theory, the mutation of the possessor noun is due to a word-internal feature, not one which travels a-cross w-juncture by (78)', the preceding parenthetical should not affect it. This is true. To the extent that the paren-thetical is syntactically natural, the genitive morphology is absolutely obligatory.

(86) Shílim gurbh é col ceathar, nó

I-thought that-be-PST him first cousin or

```
        b'fheidir gaol      eigin eile,   Sheáin
        maybe      relative some   other  Sean-GEN
```

Uí Ríogáin.

O Reagan-GEN

'I thought he was a first cousin, or maybe some

other relative, of Sean O Reagan.'

As for trace, examples in the relevant places are a lit-

tle hard to come by. I do have a couple of good ones, how-

ever. Recall that the pre-verbal particle ní 'NEG' lenites:

(87) Déanann sí é sin.

      does   she it that

      'She does that.'

(88) Ní dhéanann sí é sin.

      'She doesn't do that.'

When ni precedes the copula, however, the copula deletes.

(89) Is dochtúir í.

      'She's a doctor.'
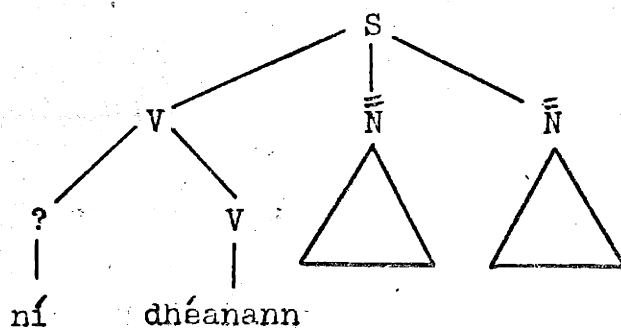
(90)(a) *Ní is dochtuir í.
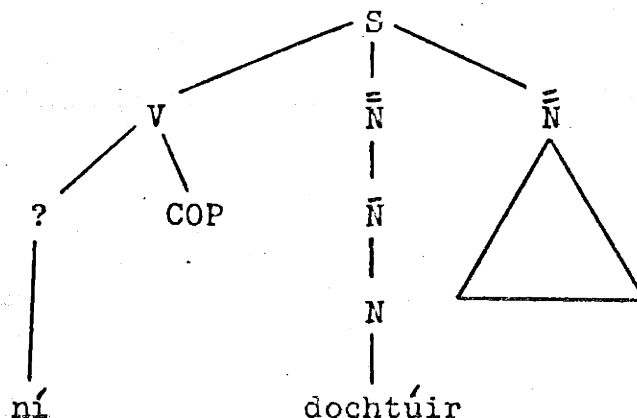
   (b) *Ní dhochtuir í.

   (c) Ní dochtuir í.

      'She's not a doctor.'

Lenition is now impossible. Presumably, then, is left its

pre-terminal structure behind when it went:

(88)'

```
                    S
          V        N̄̄        N̄̄
      ?       V   /\       /\
      |       |  /  \     /  \
      ní   dhéanann
```

(90)'

```
                    S
          V         N̄̄        N̄̄
      ?      COP    N̄        /\
      |             |       /  \
      |             N̄      /    \
      |             |
      ní            N
                    |
                    dochtúir
```

Now <u>ní</u> and <u>dochtúir</u> abut no one w-juncture, (78)' fails, and <u>dochtúir</u> is saved from lenition, as desired.

Note that a trace-as-word-boundary theory gives the wrong results here. The prediction of such a theory is that only removal of words belonging to lexical categories will end up producing extraction-site effects, since only they will have # on either side to leave behind. But here the deleted item is the copula, which is not lexical.

Non-lexicalness could, in principle, be something of a wild card. In this case, however, we have independent reasons not to think of the copula as a [+V] verb: Unlike a verb, it is impossible alone, stressless ("proclitic"), and in many circumstances deletable altogether. The very fact

that <u>ba</u> induces a mutation makes us want to class it with the unquestionably non-lexical items, which often do, rather than with the verbs, which never do.

The behavior of <u>ní</u> is no fluke. The interrogative <u>an</u>, which nasalizes, affords parallel examples.

(91) An bhfeiceann tu é sin?

      Q see   you it that

     'Do you see that?'

The copula also deletes after <u>an</u>, as after all other pre-verbal particles:

(92)(a) *An is feirmeoir í?

     (b) *An bhfeirmeoir í?

     (c) An feirmeoir í?

       'Is she a farmer?'

But now <u>an</u> no longer nasalizes.

There is a small list of such particles. The negative interrogative <u>nach</u>, to take a final example, also belongs on it. <u>Nach</u> nasalizes.

(93) Nach mbeannaigh sé thú?

     NEG-Q salute   he you

     'Doesn't he salute you?'

(94)(a) *Nach is báicéir í?

     (b) *Nach mbáicéir í?

     (c) Nach báicéir í?

       'Isn't she a baker?'

We must make sure, incidentally, that when (78)' fails in cases like (94), the [+IM] word loses its feature anyway.

The reasoning is slightly involved. Note that neither ní, an, nor nach could possibly be affected by either of the mutations I've mentioned. However, there is one I haven't mentioned, also set off by the feature [-IM]: Before vowels, n appears.

$$(95) \quad \emptyset \rightarrow n \; / \; \underline{} \; \begin{bmatrix} V \\ -IM \end{bmatrix}$$

E.g.:

> (96)   Nach n-ólann tú portar?
>
> NEG-Q   drink you porter
>
> 'Don't you drink porter?'

Note also:

> (97)(a) *Nach (n-)is Albanach é?
>
> (b) *Nach n-Albanach é?
>
> (c) Nach Albanach é?
>
> 'Isn't he a Scotsman?'

But now recall (92). When (78)' fails across trace, we do not end up with

> (98) *N-an feirmeoir í?

which is what (95) would give unless we do something. We could prop up (78)' with a fairly trivial parenthesization:

$$(78)'' \quad \begin{bmatrix} \alpha IM \\ -N \\ -V \end{bmatrix} \; ([uIM]) \; \rightarrow \; [uIM] \; [\alpha IM]$$

However, I prefer to think of facts like (98) as evidence that the feature [±IM] is not in fact word-sized, but rather segment-sized, occurring at word edge. An, for instance, is listed as (99), not (100).

> (99) [ən[-IM]]

(100) $\left[\begin{array}{c} \text{an} \\ \text{[-IM]} \end{array}\right]$

(2)" and (3)" become (2)''' and (3)''', and (95) becomes
(95)':

(2)''' ... / [+IM] ___

(3)''' ... / [-IM] ___

(95)' ... / [-IM] ___ V

Notice now that (78)' is completely superfluous, on the
assumption that the rules spelling out grammatical marking
place [±IM] at left word edge. By giving [±IM] stringwise
position, we avoid (98) into the bargain. The behavior of
(2)''', (3)''', and (95)' across w-juncture (i.e. when [±IM]
is not placed by grammatical spell-out) still follows from
the same theory which predicted the behavior of (78)'. The
fact that these rules do not breach w-juncture if the word
on the left is lexical we can now account for by equipping
only the non-lexical items as in (99). Lexical items never
have mutation features at their right edges; thus we do not
get (80), (81), (82).

The situation, then, is as follows. There are two pos-
sible positions of mutation-inducing features with respect
to the initials they mutate: They end the preceding word, or
else they begin the same word.

(101)(a) [±IM]][___

(b) [[±IM] ___

Both of these cases are covered by (2)''', (3)''', and (95)',
which do not even recognize the distinction. However, just
in case (a), where the terms of the structural description

turn out to correspond to factors on either side of a w-juncture, the predictable juncture effects show up.

Now I'm suggesting that it's inappropriate to write these rules with parenthesized boundaries. As it developed, writing

(102) ... / [$\pm$ IM] (#) ___

is actually wrong. But suppose it were right. Even then it would be undesirable, because the notation implies that parenthesizing some other configuration (say the strongest occurring series) of boundaries would yield a possible output--one in which the juncture effects are not in evidence. It also implies that (2)''', (3)''', and (95)' could differ from one another with respect to their associated juncture effects. These, I believe, are not the right implications.

I'm going to conclude, then, that the mutations are respectful of w-juncture in the same way as the English rules of the first Part. Also in the same way, they are triggered only by non-lexical items. In the next Part I'll have a little more to say about this last matter.

PART III

SYNTACTIC CONDITIONING OF PHONOLOGICAL RULES

The English and Irish rules just discussed belong to a
much larger class of phonological rules that allow a bundle of
features on one side of a w-juncture to influence another on
the other. We may consider, assuming that there are no boun-
daries, that these feature bundles are adjacent in a quite
literal sense, and that therefore we're talking about a
special class of local processes. Now a given rule of this
class is either set off by some particular non-lexical item
or set of items, as in the examples of Parts I and II, or
else it isn't, and the rule is lexically productive--also
a quite widespread phenomenon.

Most often a rule of this last sort will not produce
sandhi between just any two words, supposing the phonologi-
cal conditions on its application to be right. This selec-
tive failure is the problem to which I'm going to devote this
Part. By way of explaining it, I cannot make the reasonable
and somewhat standard assumption that some aspect of the in-
tervening juncture, expressed in the quality or quantity of
the terminal symbols separating the features involved, must
have something to do with the further conditions that we'll
evidently have to lay on the rule. Even so, I will constant-
ly be pointing out in what follows that such an assumption
is by its very nature incapable of dealing properly with the

facts.

Let me make parenthetically a quick and somewhat trite remark on the idea of terminality. Any feature of pre-terminal syntactic structure can in principle be persuaded to induce a special symbol @ in the terminal string, given appropriate procedures for the positioning and interpretation of @. We might contemplate doing this if, for example, we made up our minds that this aspect of pre-terminal structural information actually had to be made available to phonological rules, and if we held that phonological rules referred only to terminal symbols. Still, this kind of expedient would not be especially believable unless it could be demonstrated that @ had independent business in the terminal string, as does, for instance, #, in standard phonological theory. Selkirk (1972, 1974) wants to do precisely this for French, where, in her account (below), sandhi fails only across double word boundary.

In spite of what we might think of @, the reality of the information it carries, however graphically represented, is not really in question. We find such a symbol vaguely offensive only because it is redundant and because it blurs the distinction between phonological and structural information, otherwise nicely portrayed as terminal and pre-terminal information. This distinction is the one actually at issue. Is it in fact the truth that phonological rules have no purely syntactic conditioning?

I want to say here, to begin with, that some rules of

"selective" external sandhi ignore all independently apparent terminal properties of the junctures they breach--i.e. the properties traditionally represented by phonological boundaries--in favor of certain pre-terminal ones. I exclude the trivial sense in which @ is terminal; what I am really saying is that sandhi is not always conditioned simply by phonological features, and I want to present a theory of how I find it is conditioned. The notion w-juncture once more turns out to be crucially apropos.

In a recent series of studies (Rotenberg 1975, 1976, Kaisse 1977, Napoli and Nespor 1977, Clements, to appear), the point has been repeatedly made that SPE-style word boundaries can be as immaterial to phonological rules operating at gross phrasal levels as morphological boundaries can be to rules operating at the level of the word, or as both can be to rules operating at the level of the syllable (see Kahn 1976). There is a very real sense in which, for example, a pair of segments related by a phonological rule are adjacent even though a morpheme edge may happen to intervene. This in itself makes us begin to wonder in what sense a + boundary representing it actually exists. (The accepted convention for this sort of case is, of course to forgo writing + inside a pair of parentheses.) In a theory without #, the same goes for segments at word edge. Nevertheless, we must at some point correctly delineate the conditions that hold of rules relating them. The question of course arises, what do these conditions look like, anyway?

Surely we would like to say something interesting and falsi-
fiable about exactly what sorts of information phonological
rules are privy to.

In a very broad way, the key idea involved in such con-
ditions is constituency. I'll show below that precisely the
same idea is useful in a lot of other places in the grammar.
Let us say, quite generally, that

(1) in a labeled bracketing $\mathcal{A}$, a sequence A of elements
$a_1 a_2 \ldots a_n$ such that

    (i) every a_i is a (prime member of the same natural
class $\underline{a}$ of units (these units specified by uni-
versal grammar and presumably including the lexi-
cal and prosodic ones of Part I), and

    (ii) $d_a(\mathcal{A}) = A$, where

        (a) $d_a(\gamma) = d_a(\psi) \, d_a(\varphi)$ if $\gamma = \psi\varphi$, ψ and φ non-null,

        (b) $d_a(\gamma) = a_k$ if $\gamma = a_k$, for any k, and
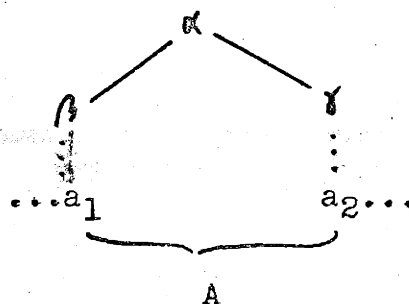
        (c) $d_a(\gamma) = e$ (the null string) otherwise

    is a constituent α if and only if there is a well-
formed labeled bracketing $\omega = [_\alpha A]$.

We'll say that A is a α , and write $(_\alpha A)$.

The idea of (1) is not difficult, more or less the stan-
dard is a. It allows us to say that a sequence A of elements
$a_1 a_2 \ldots a_n$ is a constituent purely by virtue of its being ulti-
mately and exhaustively dominated by a node α. We find every
a_i under α, ignore everything else, and can now speak of each
a_i and a_{i+1} as being both adjacent and co-constituent.

Say, for instance, we had a structure as in (2).

(2)

Then
$$\alpha A = [_\beta \ldots a_1 \ldots] \; [_\gamma \ldots a_2 \ldots]$$
$$d_a(_\alpha A) = A = \quad a_1 \qquad\qquad a_2$$
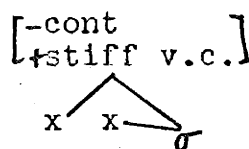$$\omega = [_\alpha A] = [_\alpha \quad a_1 \qquad\qquad a_2 \quad]$$

and we write $(_\alpha a_1 a_2)$.

(1) is probably quite a handy notion. The important question being begged by it is the question, what sorts of labeled bracketings are there? Equivalently, what sorts of nodes α are there? I imagine that the constituency predicate given in (1) will have to be employed in some form by phonological rules operating over any domain of linguistic structure where α is defined, where there are in fact nodes.

Note that the theory of levels (Part I) becomes a special case of (1), where α is from the set of levels L. That is, (1) effectively allows us to say that a rule R is an α-level rule, where α is any node, not just one at the root of a tree at some level of representation. At most levels of representation, however, there is no distinction, since the trees are fairly flat. The node at the root of the tree is labeled with the same label as any other nodes that there might be. Phrase structure, of course, is the notable exception, and it is in the case of phrase structure that (1) is especially useful.

A first example. In the work of Kahn (1976), we can see
that this generalized notion of constituency is precisely the
relevant primitive. Kahn offers a powerful demonstration that
syllable structure directly conditions rules of phonology.
In his account of the allophones of American English /t/, for
instance, Kahn shows that voiceless stops (including /t/) are
aspirated when syllable-initial and non-syllable-final; that
/t/ is glottalized when non-syllable-initial and directly
following a non-consonant; and that /t/ is flapped when ambi-
syllabic, i.e. dominated by two adjacent nodes σ. In Kahn's
notation, the structural conditions of the rules distributing
this allophonic variation are (3), (4), and (5).

(3)
$$\begin{bmatrix} -\text{cont} \\ +\text{stiff v.c.} \end{bmatrix}$$
x   x — σ

(4)
[-cons] $\begin{bmatrix} -\text{cont} \\ +\text{stiff v.c.} \\ +\text{cor} \end{bmatrix}$
σ x

(5)
[-cons] $\begin{bmatrix} -\text{cont} \\ +\text{stiff v.c.} \\ +\text{spread glottis} \\ +\text{cor} \end{bmatrix}$ [+syll]
σ σ
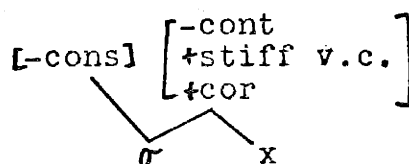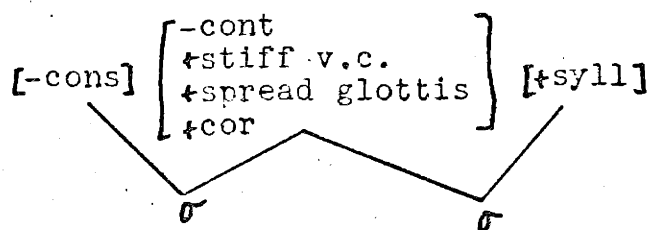
(An x at the end of a line excludes syllabic association. So,
for instance, the segment in (3) neither follows anything in
its syllable nor belongs to a preceding syllable.) These are
equivalent to (3)', (4)', and (5)'.

$$(3)' \quad (_\sigma \begin{bmatrix} -\text{cont} \\ +\text{stiff v.c.} \end{bmatrix} X)$$

$$(4)' \quad (_\sigma X [-\text{cons}] \begin{bmatrix} -\text{cont} \\ +\text{stiff v.c.} \\ +\text{cor} \end{bmatrix} Y)$$

$$(5)' \quad (_\sigma X [-\text{cons}] (_\sigma \begin{bmatrix} -\text{cont} \\ +\text{stiff v.c.} \\ +\text{spread glottis} \\ +\text{cor} \end{bmatrix}) Y)$$
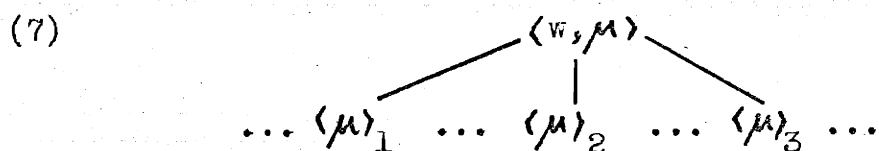
In these examples, $a_1 a_2 \ldots a_n$, of which constituency in the sense of (1) is being predicated, are segments. X and Y are variables, standing for any number of adjacent a_i's, and allowing in this case for exhaustive analysis of syllables. Thus we stipulate that stops must be syllable-initial in (5)' by omitting a variable after the left parenthesis. α has the value σ, the only label allowed nodes at this level of representation. (See, however, Duncan (to appear).)(For another example of $\alpha = \sigma$, see McCarthy (to appear).)

I draw the reader's attention to the fact that a_i and a_{i+1} may logically be separated by more than brackets. Suppose a and b were mutually exclusive natural classes. Then d_a would operate on, say, $[a_1] [b_1] [b_2] [a_2] [b_3]$, leaving $a_1 a_2$. There are, in fact, theories of discontinuous "non-linear" phonology (Clements 1976, Vergnaud 1977) in which it is convenient to build word-internal trees over segments belonging to the same natural class and in which segments a_1 and a_2 above might end up being a constituent, as in (6):

(6)
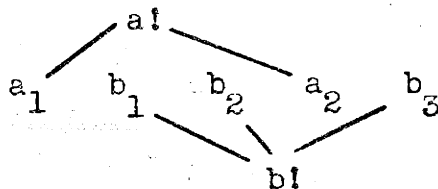
For example, Vernaud (1977 and to appear) makes the fol-
lowing ingenious proposal (here grossly simplified) for elimi-
nating variables in word-level phonology. Let every member μ
of a natural class $\underline{\mu}$ in a terminal string $\mathcal{T}$ = w (i.e. the word)
be dominated by a node $\langle \mu \rangle$. Let the maximal sequence
$\langle \mu \rangle_1 \langle \mu \rangle_2 \ldots \langle \mu \rangle_n$ in turn be dominated by a node $\langle w, \mu \rangle$. Now
Vergnaud is in a position to define an apparently discontinu-
ous harmony process, for instance, over the node $\langle w, \mu \rangle$.

(7)

$$
\langle w, \mu \rangle \\
\ldots \langle \mu \rangle_1 \ldots \langle \mu \rangle_2 \ldots \langle \mu \rangle_3 \ldots
$$

$$
\begin{aligned}
\text{Here} \quad \mathcal{A} \; &= \; \ldots \langle \mu \rangle_1 \ldots \langle \mu \rangle_2 \ldots \langle \mu \rangle_3 \ldots \\
d_\mu(\mathcal{A}) \; &= \; \quad \langle \mu \rangle_1 \quad \langle \mu \rangle_2 \quad \langle \mu \rangle_3 \\
\omega \; &= \; [_{\langle w, \mu \rangle} \langle \mu \rangle_1 \quad \langle \mu \rangle_2 \quad \langle \mu \rangle_3 \;]
\end{aligned}
$$

and we write $(_{\langle w, \mu \rangle} \langle \mu \rangle_1 \langle \mu \rangle_2 \langle \mu \rangle_3)$. In this instance a has the
value μ, since we want $d_a(\mathcal{A})$ to give the sequence
$\langle \mu \rangle_1 \langle \mu \rangle_2 \ldots \langle \mu \rangle_n$. $\kappa = \langle w, \mu \rangle$.

In Nez Perce, to give a more concrete illustration (see
Aoki (1966), SPE (p. 377)), the diacritic feature $[+F]$, origi-
nating from any V_i that happens to possess it, infects every
other V_j under $\langle w, V \rangle$, where it consummates the complex alter-
nation characteristic of harmony in this language.

$$
\langle w, V \rangle \\
\langle V \rangle_1 \ldots \langle V \rangle_i \ldots \langle V \rangle_n \\
C_o \; V \; \ldots \; V \; \ldots \; V \; C_o \\
[+F]
$$
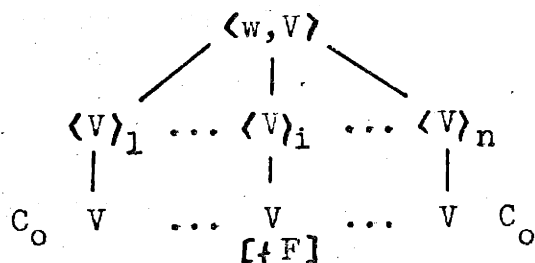
α has the value $\langle w,V \rangle$ and a has the value V. We can write the domain of the rule as below.

(8) $(\langle w,V \rangle X \underset{[+F]}{V} Y)$

This says merely that the sequence inside the parentheses must _be a_ $\langle w,V \rangle$.

My point is that tree geometry conditions phonological rules at this level of structure precisely as it does at the level of the syllable.

It now seems a fair bet that the nodes provided by phrase structure could define the domains of higher-level rules in analogous ways. One would be surprised and disappointed if they alone among nodes, broadly considered, could not.

I believe that they can. I do not wish to say, however, that any kind of syntactic conditioning should necessarily be written into the rules subject to it. Even if the junctures involved could be characterized in terms of independently necessary boundaries, it would be a mistake to put such a characterization into the rules affecting the flanking segments. This is because quite often there is a list of rules all subject to the same condition. (Selkirk, to appear.) In Anlo-Ewe, for instance (Clements, to appear), there are (at least) three tone sandhi rules, all of whose domains of application are defined in terms of the same structural configuration. Pretty clearly, writing the same condition n times in n rules amounts to admitting that its repeated occurrence is an accident, whereas coming up with a way to factor it out and predicate it of all n says that its repeated occurrence is

part of a larger generalization.

This point is in fact already a point against supposing that terminal juncture symbols, like word boundary, could be the real factors conditioning selective sandhi, in any way at all. Word boundaries obviously show up in normal phonological rules, because, in the standard theory, they're phonological units. But then, if we let them condition high-level sandhi, what stops us from referring to their presence repeatedly and identically in a list of rules? (See the French case, below.) On the other hand, if the real conditioning factors have to do solely with pre-terminal structural information, not much needs explaining, since this we have no independent reason to want to terminally encode. If phonological and syntactic conditions cannot be mixed, the syntactic conditions cannot be specific to each phonological rule.

The form of these conditions is simply that of the rules we have already seen, taken to one remove of abstraction. Let us imagine a φ-juncture J, φ from the set of lexical units U (see Part I), which a rule or list of rules at a level higher than γ is going to relate features on either side of. (To breach a morphological juncture, for instance, we need at least a word-level rule.) I'll symbolize ξ the two immediate ψ-units, ψ from the set of levels L, on either side of J and containing at their far right and far left segments bearing these features. γ is not necessarily the same as ψ: It turns out (below) that we'd like, for instance, to be able to set ψ equal to f, though there is presumably no f-juncture

as distinct from the w-juncture.

Now what we want to do is predicate constituency, in the sense of (1), of strings containing $\varepsilon\varepsilon$ by way of constraint on the rule or rules affecting them. For instance:

(9) X ($_\alpha$ ε ε) Y

X and Y are variables, standing for any number of ψ-units. By using the variables, I intend (9) to be an exhaustive analysis of a k-sized string ($k \in \underline{L}$ and higher than ψ), i.e. it's a k-level condition.

According to (9), $\varepsilon\varepsilon$ must $\underline{be}$ $\underline{a}$ α. Suppose $\alpha = f$ (the set of phrasal nodes $\begin{bmatrix} \beta N \\ \gamma V \end{bmatrix}^b$, $b > 0$) and $\varepsilon = w$. Then we'll interpret (9) as saying that a pair of adjacent words must $\underline{be}$ $\underline{a}$ syntactic constituent in order for some rule(s) to apply. As I'll show below, (9) is now precisely what is needed for the Hebrew case.

(Note that there is presumably $\underline{no}$ node ultimately dominating segments separated by full stop. The theory is predicting, then, correctly to my knowledge, that selective sandhi between successive sentences in an utterance will not exist.)

THE FRENCH EXAMPLE

My first example is French liaison. The term liaison refers to the pronunciation, under certain syntactic conditions, of word-final consonants before word-initial vowels. The usual assumption is that underlying final consonants delete elsewhere--when a vowel does not follow or when the syntactic conditions are not met. It is these conditions which get most of my attention below, and not the phonological rule they govern. Surprisingly, though, the theory I'll develop to handle the former has a few interesting things to say about the formal nature of the latter.

The example of liaison is unfortunately not entirely happy, because the proper boundaries of the phenomenon are not so easy to discern through the sociological fluff which surrounds it. Generally, the less casual the conversation, the more final consonants are audible. Usually people who think about it respond to this fact by speaking of "styles" of liaison, all of which a given French person is in command of and chooses among depending on social context. Fouché (1959), for example, precipitates a vast range of liaison phenomena into three discrete styles--la conversation courante (Style I), la conversation sérieuse et soignée (Style II), and le style soutenu (Style III), named in order of increasing number of syntactic liaison contexts. (This classification is accepted by Selkirk (1972).) The set of liaison contexts char-

acteristic of style n is a proper subset of the set of liai-
son contexts characteristic of style n+1, which is to say that
no increment in formality results in the <u>loss</u> of any liaison
context. On the contrary, a core of pan-Gallic, general-pur-
pose liaison is successively added to, yielding the more rare-
fied styles. (Remember, though, that this effect is achieved,
in most accounts, by allowing a rule of final consonant de-
letion to apply <u>less</u> frequently.)

Now speakers of each style have a certain amount of lati-
tude in rendering it. For a given style k, liaison in a syn-
tactic context C is either obligatory, optional to various de-
grees, or totally out of the question. Liaison in C may be
required or prohibited because it is required or prohibited
by <u>all</u> styles, i.e. by French; or it may be required or pro-
hibited because of the stipulated characteristics of style k.
Now if liaison in C is merely <u>allowed</u>, with more or less en-
thusiasm, there always exists a higher style in which it is
obligatory and/or a lower style in which it is forbidden.
It would be entirely appropriate, once we've decided to i-
dealize discrete styles in which liaison is not optional, to
eliminate the remaining optionality in C by replacing k with
k', like k save that liaison in C is prohibited, and by set-
ting up a higher style k", like k' save that liaison in C is
obligatory. However, in view of the fact that infinitely
many styles would be required to eliminate optionality in all
contexts, it would also be absurd. But now we begin to won-
der about the idealization to the first three styles.

I am not pushing for a squish, at least not a linguistic one. My point is that any discrete stylistic treatment of liaison is unfair. There is a set of liaison contexts which it would be un-French not to observe. This set ("Style I") is the real linguistic phenomenon, the common denominator of which is unconsciously known in the form of a rule (to be brought to consciousness below) by every speaker. In addition to this piece of knowledge, every speaker is also aware of a certain social pressure to make more liaison in stuffier social contexts. To what extent and how consistently it is actually performed depends mainly on the speaker. There is some absorption through normative institutions (e.g. the schools) of ideas about "correct" liaison in the formal styles. So, for instance, educated speakers may have been explicitly taught that, for poetry reading and such events, liaison is supposed to be made before a post-nominal adjective: des idée[z] absurdes. This "rule" is very much like a rule of spelling. Both are explicitly taught to some fraction of the population; both may or may not be learned, or if learned remembered, or if remembered observed; and both have zero linguistic significance.

Liaison, however, is not fortunate to have a well-established, consultable norm, doing for it what the dictionary does for spelling. Consequently it is the case that there is almost no agreement about the facts of liaison in non-conversational styles. Beyond such simple cases as the post-nominal adjective, very few people have much idea about where to

put their liaison, simply because there are no real, productive rules. They know, "I'm supposed to be making more liaison," and they know this with more certainty as the situation gains in formality. Some socially conscious speakers develop their own elaborate routines for non-conversational liaison, which they may stick to quite consistently; but seldom do they have genuine intuitions extending to unusual syntactic contexts, and their routines are strictly their own. I mention as an analogue the amazingly involved and varied private routines that many literate speakers of English have developed for avoiding, or flaunting, sexist choices of unmarked anaphoric pronouns. It is not clear in what sense this kind of linguistic behavior, existing solely for social reasons, represents something that is part of the language. The rules responsible are far from trivial, but, I would say, not interesting to a linguist qua linguist.

In sum, then, I think that the non-conversational styles, to the extent that they exist, are artificial and normative. To idealize to discrete styles is misleading, just as it is misleading to say that there is a discrete style in English in which people don't use contractions, don't strand prepositions, or don't palatalize final dentals (see Part IV). Rather, the rule-governed compulsion to drop the final consonant, contract, strand, and palatalize is variably offset by the variably effective social sanctions against it. Not sharing this view are Fouché (1959), Selkirk (1972, 1974), Rotenberg (1975, 1976), and Postal and Pullum (1977).

I'm going to go on now and review what has been said a-
bout the syntactic conditioning of conversational liaison.

There is only one clear idea in the literature on this
subject. It enjoys wide currency and is, to my knowledge, un-
challenged in print. Due originally to Milner (1967), it is
accepted by Schane (1968), Dell (1970), and Selkirk (1972):

> (10) "...the phonological phenomena characteristic of
>   liaison operate when just one word boundary, #,
>   separates one word from the next."   (Selkirk,
>   p. 208.)

These two words are spoken of as "being in a liaison context."

This is an interesting, powerful, and elegant theory,
worth examining closely. Let us in fact examine it closely.
The idea is that there is a rule on the order of (11). I
temporarily simplify.

> (11) $C \rightarrow \emptyset / \underline{\quad} \# C$

Suspending for the time being our opinion of rules like this,
we should ask exactly where a single # is allowed to appear.
Recall, first of all, the SPE conventions on boundary place-
ment which I outlined in the introduction. Given these, we
are in a position to find out in what syntactic circumstances
a sole # may stand. Let us imagine a pair of adjacent words
A and B.

1. Suppose A and B both belong to lexical categories.
Then A will be [# A #] and B will be [# B #]. We have

> (12) [# A #] ... [# B #]

where the dots stand for possible intervening brackets.

There is no chance of redemption by the boundary-telescoping convention BC II; two word boundaries permanently separate A from B.

2. Suppose A is non-lexical and B is lexical. Then A is [A] and B is [# B #].

i. Now suppose A is dominated by some node α that does not also dominate B. Then we have:

(13) $[_\alpha \# \ldots [A] \#][\# B \#]$

Once more ## separates A from B.

ii. If B is dominated by some node α not also dominating A, BC I gives

(14) $[A] \ldots [_\alpha \# [\# B \#] \ldots \#]$

which BC II improves to

(15) $[A] \ldots [_\alpha \# [\quad B \quad] \ldots \#]$

But A must be dominated by something, β; as the preceding shows, any β dominating A must also dominate B if β's # is not going to appear to the right of A.

(16) $[_\beta \# \ldots [A] [_\alpha \# [B] \ldots \#] \ldots \#]$

Put another way, a single # separates A and B if and only if the first node dominating A dominates B. This relation between A and B is similar to the idea of C-command (Reinhart 1976): A C-commands B if and only if the first branching node dominating A dominates B. For convenience in the following, I will invent a relation IC-commands (immediately C-commands).

3. Suppose A is lexical and B is non-lexical. Then, by

identical reasoning, if and only if B IC-commands A, a single # intervenes.

4. Suppose A and B are both non-lexical. Then A is [A] and B is [B].

i. If A is dominated by a node α not dominating B, we have

(17) $[_\alpha$ # ... [A] #] ... [B]

If any β dominates B without dominating A, another # appears before B. Therefore a single # separates A and B if and only if B IC-commands A.

ii. Similarly, if B is dominated by a node β not dominating A, one # intrudes between A and B if and only if A IC-commands B.

iii. If some γ immediately dominates both A and B,

(18) $[_\delta$ # [A] [B] #]

no #'s appear between them. Note that A and B IC-command each other.

What (10) says, then, is that two adjacent words are in a liaison context when and only when one, a non-lexical item, IC-commands---is immediately dominated by a node dominating--- the other. We may now look and see if this is true.

I briefly survey the basic liaison contexts. Final con-sonants are heard between:

(19)(a)   a pre-nominal adjective and its noun

(b)   a pre-adjectival adverb and its adjective

(c)   an auxiliary and its verb

(d)   a copula and its object

(e) a determiner and what follows

(f) a pronoun ("clitic") and what follows

(g) a preposition and what follows

E.g.:

(19)(a)' un grand⁀orang-outang

(b)' vraiment⁀idiot

(c)' ont⁀été

(d)' est⁀absurde

(e)' un⁀homme

(f)' en⁀est

(g)' en⁀été

I use without comment from now on the ligature "⁀" to indicate liaison, and the solidus "/" to indicate no liaison. Often, when it is irrelevant, I will indicate neither.

The first thing to notice is that the contexts in (19) fall into two classes. (i) There are words--prepositions, determiners, and pronouns--which always make liaison. This is to say that for a given preposition, article, or pronoun of the form /XVC/, one _always_ finds [XVC] V, never [XV] V. (There are highly significant exceptions, to which I return.) (ii) In all the other cases, [XVC] V alternates with pre-vocalic [XV] V. (I assume that words which always appear as [XV] in fact possess a final consonant at no level of representation.)

(20)(a) un bon/et gros oignon

(b) vraiment/idiot et ridicule

(c) ont$^?$/astucieusement avoué

(d) est$^?$/absolument absurde

I put off discussion of the first class for a while. Pro-
visionally observe, however, before leaving the subject, that
no syntactic generalization covering these cases is going to
be possible, or, for that matter, necessary: We could very
easily conclude that these words themselves are _inducing_ the
the liaison, much as Irish a_ 'his', as opposed to a_ 'her',
induces lenition; and that (ii) the categories to whose right
liaison unstoppably occurs are all non-lexical. (i) and (ii)
are not unrelated.

As for the second class, it seems obvious that the word
on the left _need_ not belong to a non-lexical category. I
might approach an even stronger statement by noting that it is
not in the least clear that what I informally refer to as the
auxiliary and the copula must in fact be represented in French
as belonging to formal categories distinct from V. See, on
this, Emonds (to appear). The word on the right, due to the
right-nuclear phrase structure of French, is (at least in the
simple examples in (19)) never non-lexical. Therefore it
seems to be a mistake to require, as the boundary theory does,
that at least one of the words in a liaison context be non-
lexical.

(Selkirk (1972) is quite alive to the problems posed by
(19)(a) and (b), on the assumption that adjectives and adverbs,
but not auxiliaries and copulas, belong to lexical categories.
To deal with (b), it is assumed that adverbs are really non-

lexical. To deal with (a), a thornier problem, a special
rule deletes a word boundary.

(It is interesting to observe the general philosophy
of stylistic liaison in this work. The idea is that (10) is
always true, holds good for all styles (of which I am only
considering one). But the styles are obviously different.
Therefore the boundaries must be different. And in fact, to
give the good results, style-specific readjustment rules
work to insert and delete the respective wanted and unwanted
boundaries, starting with the basic installation given by
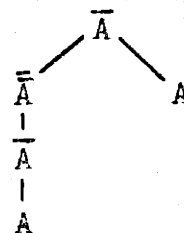SPE conventions.

(One begins to fear for the content of (10).)

Now it is undeniable that one word IC-commands the other
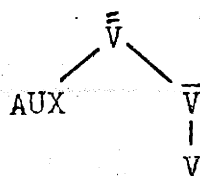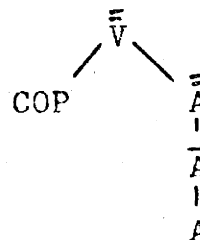in all cases.

(19) (a) "
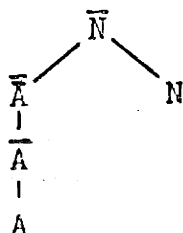
(b) "

(c) "

(d) "

But note that, in these simplest of examples, the two words
also C-command each other, and each C-commands only the other,
which is merely an involved way of saying that the two of
them exhaust some constituent. We might easily entertain sus-

picions that these facts are trying to tell us that something

like (21) is actually governing liaison.

(21)   X ($_f$ w w) Y

(21), a condition on sentence-level rules, says simply that

the two phonological domains flanking the w-juncture across

which a rule might apply must <u>be</u> <u>a</u> constituent unto them-

selves.

To say, as we are now doing, that a liaison consonant

is heard at the end of a word if and only if (phonological

conditions being favorable) it and the next <u>are</u> <u>a</u> constitu-

ent is, however, too strong.   What if, in (19)(a)"-(d)", we

try expanding the one expandable sister?

(22)(a) $\bar{N}$ (b) $\bar{A}$

(c) $\bar{\bar{V}}$ (d) $\bar{\bar{V}}$
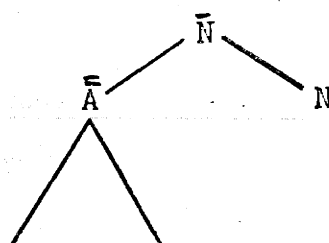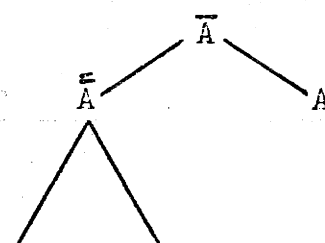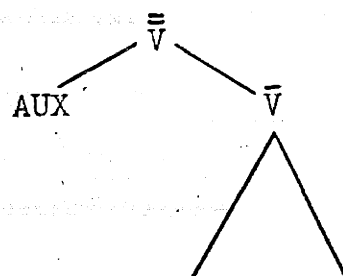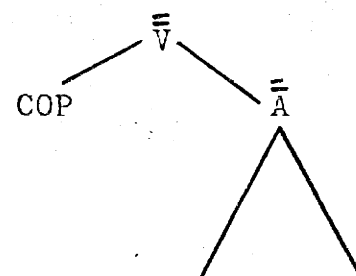
(a)'   un beau et gros⌢oignon

(b)'   complétᵉment, vraiment⌢absurde

(c)'   ont?/astucieusement avoué

(d)'   est?/absolument absurde

What happens is that liaison fails only when the com-

plexity is introduced on the right--i.e. when the word to the
right of the liaison consonant does not end the constituent.
Notice, too, examples like the following.

(23)(a) un gros/et bel oignon —special liaison form of 'beau'

(b) Il est influent/avocat et législateur à la fois.

(I assume Ross' (1967) account of coordinate structures, ac-

cording to which the conjunction and the second conjunct are themselves a constituent. (See Part I.)) But:

(24)(a) un gros⌢oignon

(b) un très gros⌢oignon

(c) un absolument, incroyablement gros⌢oignon

And:

(25)(a) trop/ou pas assez intelligent

(b) à la fois trop/influent et vieux

But:

(26)(a) trop⌢absurde

(b) vraiment trop⌢absurde

(c) sans doute indéniablement trop⌢absurde

In an example like (27),

(27) Jean est très intelligent et modeste.

liaison appears before intelligent only if the bracketing is as in (27)'(a), not (27)'(b).

(27)'(a) [très intelligent] et modeste

(b) ? très [intelligent et modeste]

In the verb phrase, parallel effects obtain, though for some reason they are never nearly as clear-cut. Liaison becomes a good deal less natural before a complex verbal complement.

(28)(a) Nous allons⌢atterrir.

(b) Nous allons?/atterrir à Paris dans un instant.

The theory I'm going to stick with is formalized in (29).

(29) X ($_f$ Y w w) Z

(29) says only that the unit to the right of the w-juncture

breached by liaison must finish a constituent that also con-
tains the unit to the left of it.   This handles everything
I've mentioned.   Notice, incidentally, that a boundary theory
predicts exactly the wrong results in (22)(a)'-(d)', and is
incapable of distinguishing (23) from (24), (25) from (26),
(27)'(a) from (27)'(b), or (28)(a) from (28)(b).

The rule of liaison subject to (29) must evidently be
something like (30).

(30)   C $\longrightarrow$ [+unit] / ___ V

In (30) I make use of two formal gimmicks to get the right
results.   The first, the feature [± unit], which opposes all
units ([+unit]) to zero ([-unit]), is due to Morris Halle.
The second is conventional elsewhere-case alpha-switching
(Kiparsky 1973):   Every rule

(31)   A $\longrightarrow$ [α F] / in some environment

actually abbreviates

(31)'   A $\longrightarrow$ $\begin{cases} [\alpha F] / \text{ in some environment} \\ [-\alpha F] / \text{ elsewhere} \end{cases}$

What I intend by (30), then, is that consonants become [-unit]
--delete--in environments other than the specified one, i.e.
before C or zero, or before V if (29) is not met too.   The
result, observe, is that the rule (30) saving liaison conso-
nants from deletion applies vacuously where the liaison conso-
nant actually surfaces.   Speaker intuition evidently agrees
that something seems to be "happening" in liaison environ-
ments (i.e. not elsewhere), even though the audible consonant
presumably ascended straight from the deepest representations.

(30), final consonant saving, is consistent with the feeling that deletion, not no deletion, is the unmarked case.

I think we would do well at this point to take quite seriously the formal awkwardness of (30). Why is it that we are forced into such an oddly contorted solution? The reason is twofold. (i) It turns out to be formally easy, given the theory I'm working with, to describe the environment where liaison is heard. The environment where liaison is not heard is best described simply as the complement of (29). (ii) I've assumed that liaison consonants start in the deepest representations and are deleted wherever they are not present on the surface. From (i) and (ii) it follows that we must arrange for deletion not to apply in the easy-to-describe liaison contexts.

(i) I will not question. It is simply a fact, though about a theory I wish to maintain. (ii), however, is a questionable assumption. True, it has the weight of tradition behind it. Nevertheless, there is a quite competitive account according to which liaison consonants are inserted: See Klausenburger (1974, 1978). I urge the reader to set aside any accumulated theoretical prejudice and consider it. At first glance it is difficult to imagine what sorts of empirical evidence there might be to distinguish the insertion theory from the deletion theory. It does exist. Unfortunately, the considerations adduced by Klausenburger, a natural generativist, are identical in spirit and nearly point-by-point to those cited by Vennemann in favor of underlying a plus an

a → an rule (recall Part I): developmental priority, fre-
quency of occurrence of alternating forms, and so on. These
seem weak to me.

Note first that on the insertion theory we can simply
insert the liaison consonant in the liaison context (29).
The mechanics of this insertion are fairly trivial—basically
an a → an rule on a grander scale. We might imagine that
each word ϕ which makes liaison has associated with it a
liaison consonant L (henceforth L-consonant), just as a has
associated with it what amounts to a liaison consonant n.
So we might write:

(32) $\emptyset$ → L / ϕ ___ V

This rule is subject to (29) in a straightforward way.

As a second and probably less equivocal example of the
mechanism I have in mind, consider the familiar problem of
strong verb ablaut in English.

I'm simply going to assume that SPE is wrong in trying
to push the strong verbs through various parts of the vowel
shift. I assume further that no independently necessary rule
or rules derives or derive sang, wove, took, etc. from sing,
weave, take, etc., and that no phonological generalization
uniting the considerable range of alternations is even pos-
sible. (See, however, Halle 1977.) On the other hand, we
evidently don't want to say that sang is merely suppletive
for singD, and so on, because we miss thereby the fairly good
generalization that the verb stem, apart from the varying
vowel, is constant in the present, past, and also participial

forms. In some fashion, then, we clearly want to make just the _vowel_ suppletive.

A natural idea is to list, in the lexical entry for each verb, the alternating vowels, and to invent labels for them:

(33) /siŋ/

$$V_{pst}: [æ]$$
$$V_{ppl}: [\Lambda]$$

Having done this for all the strong verbs, we can of course write general morphological rules using the labels V_{pst} and V_{ppl}.

(34) $V \rightarrow V_{pst} / \underline{\quad} C_o D$

(35) $V \rightarrow V_{ppl} / \underline{\quad} C_o EN$

Something along these lines seems necessary in this case, as far as I can see. Similar examples of partial suppletion will occur to the reader in the languages that she knows. Here it is obviously absurd to say that the underlying form /siŋ/ in any sense contains the alternating vowels $[æ, \Lambda]$.

What I'm suggesting is that the same kind of lexical structure is appropriate for French. Each word ϕ which participates in liaison has listed with it an associated liaison consonant, labeled L, which we can then insert by rule.

(36) /gro/

L: [z]

It is undesirable to list both /gro/ and /groz/ as free variants, and then rule out /groz/ before a consonant, for the

same sort of reason that it is undesirable to list /siŋ/,
/sæŋ/, and /sʌŋ/: We'd be losing the generalization that in
every case the prevocalic form differs from the preconsonantal
one only in being one consonant longer. Note that here, un-
like the English case just cited, it is not _absurd_ to say that
the underlying form /gro/ also includes the final /z/; but
it is not equivalent, since the good consequences (above and
below) of the insertion rule do not follow. In this respect
the situation is precisely analogous to the a/an case of
Part I.

As a case intermediate in lexical saturation between the
English a/an example and the French example I might mention
the Classical Greek movable consonants. I take no stand here
on whether these are inserted or deleted; my point is just
that these alternations, while not merely suppletive, are not
phonological in nature either and must be lexically governed,
in spite of the fair number of morphemes involved.

There exists a small list, whose contents vary slightly
depending on dialect, of morphemes of which prevocalic vari-
ants having final nu or sigma (two of the three final conso-
nants generally possible in Greek) occur. These must simply
be learned: No phonological generalization picks out the
morphemes to which the rule carrying out the alternation ap-
plies, nor is one to be expected. A true phonological gen-
eralization specifies the environment of the movable conso-
nant--namely, everywhere but before another consonant, in-
cluding sentence-finally. In this it differs from liaison.

Also, there is no evidence that movability of sigma or nu
is at all affected by constituency.

Movable nu appears (i) after estí 'is' (éstin állos
'there is another', ésti theós 'there is a god'); (ii) after
the third person singular in -e (élegen ekêinon 'he said
that', élege lógon 'he said a word'); and (iii) after most
words in -si. -si generally is or ends an inflectional mor-
pheme of one sort or another, marking (e.g.) the dative
plural (pâ:sin élegen 'he spoke to everybody', pâ:si légo:
'I speak to everybody'), the locative (athé:ne:sin ê:san
'they were at Athens', athé:ne:si polemôusin 'they fight at
Athens') or the third person plural (légousin emoí 'they
speak to me', légousi soí 'they speak to you'); thus the
list of word-final morphemes in -si is actually small.

Movable sigma occurs (i) after ek 'out of' (eks agorá:s
'out of the marketplace', ek póleo:s 'out of the city'), and
(ii) after hoúto: 'thus' (hoúto:s epoiei 'he acted thus',
hoúto: poiêi 'he acts thus').

For the Greek case a series of lexical variants must
surely be listed in some way. This conclusion in itself
does not allow us to choose between an insertion account and
a deletion account of the relation between the variants.
Formally speaking, the two are merely restatements of one
another; a complexity issue does not arise. In the same way,
note that the arrangement in (32) is neither simpler nor more
complex than a deletion rule operating on ad hoc underlying
consonants. Fairly patently, the necessary machinery is even
between deletion and insertion.

However, the number of exceptions to the deletion rule
is absolutely enormous, and there is nothing un-French about
them: avec, tous, oeuf, six, gaz, baleine, mec, homme, auto-
bus, etc., etc. These words are neither borrowed, odd-sound-
ing, nor especially recent additions to the language. They
possess no other exceptional features which might be con-
strued as concomitant. They are never mistakenly subjected
to the loss of their final consonants.

If there is no deletion rule, of course, they are not
exceptions, any more than banana is an exception to the
a/an rule. We need not deal with them by giving them nega-
tive rule features or underlying protective final schwas, as
have been done. On the contrary, it is the words which have
genuinely movable finals that are exceptional. I noted that
the proposed deletion rule shows no sign of losing its excep-
tions. By contrast, the exceptional insertions are definitely
losing in both strength and numbers. Klausenburger notes
that "optional liaison" (on which more below) is now heard
less than fifty per cent of the time from many speakers, and
the L-consonants of many words are increasingly finding
younger speakers who do not recognize them at all: devant,
trop, ont, etc.

Another important fact connected with L-consonants is
their syllabic membership. Note first the related fact that
pause does not block liaison.

    (37)(a) *[me] yeux

       (b) [mez] yeux

       (c) *[me]...yeux

If it were true merely that pause blocked liaison, (37)(c) ought to be acceptable. The real generalization is that the L-consonant must begin a syllable. This is the well-known enchaînement.

(38)(a) *[mez]...yeux

(b) ?[me]...[z]yeux

(38)(b) is, surprisingly, not so bad. This too would be difficult to understand if liaison paid no attention to pause. It is wrong, however, to think that all final consonants must begin syllables. The "exceptions" to liaison survive quite well before pause.

(39) Avec...(un) quoi?

Therefore the fact that L-consonants must begin syllables is not part of a larger truth about French syllable structure. (I don't believe that these exceptions are to be blamed on underlying final schwas, as in Dell (1970); but even if they were, syllabification would undoubtedly take place after their disappearance.) Likewise, we could not do a general resyllabification, à la Kahn, of finals, whereby they would end up tautosyllabic with the next vowel, and then condition liaison on it:

(40) *Ave...[k]un quoi?

(I am assuming, of course, that one thing which does not admit of lexical exceptions is syllabification. However, even if we could list avec as an exception to it, we would be wrong. A normal pronunciation of avec un quoi has syllable-initial [k].

On the other hand, once the theory has distinguished

L-consonants, which are inserted, from run-of-the-mill finals, which are not, we are in a position to do what we like with the inserted consonants with respect to their syllabification. Two possibilities occur to me. (i) We insert L actually at left word edge.

(41) $\emptyset \rightarrow L\ /\ \emptyset \underline{\quad} V$

(ii) We install a filter, much like the one we needed for an in Part I.

(42)

$$\sigma$$
$$\diagup \diagdown$$
$$*L \qquad x$$

For no very good reason, I will choose (42) and keep (32). I will continue to accept an insertion theory of liaison in what follows, though it is not absolutely crucial to my own. It does, however, make things extremely simple. Note that the deletion theory, together with the assumption that an L-consonant heard in one of the higher "styles" must be present in all underlying representations, makes for a good deal of labor in getting ordinary conversational French rid of it entirely. Consider, for instance, the problem of post-nominal liaison (des idées absurdes). This problem vanishes, on the insertion account: There simply is no L-consonant [z] associated with the zero plural morpheme attached to nouns. I continue to assume that its availability in higher styles is strictly artificial. A special insertion rule to cover it seems entirely appropriate.

This last fact raises an interesting question. Given an insertion analysis, making liaison essentially morphopho-

nemic, we might expect to find L-consonants associated with morphemes, as in the Greek case, rather than with words. This may actually be the right conclusion. Selkirk (1972) catches liaison in the act of evidently applying word-internally, across (in boundary-theoretic terms) what must be +, as illustrated by the alternations [de]~[dez], [me]~[mez], [su]~[suz] in the (derivational) prefixes dés-, més-, sous-:

(43)

débloquer	désassembler
décroiser	désinfecter
défavoriser	désorienter
méfait	mésaventure
mécompte	mésestimer
médire	mésintelligence
souligner	sousestimer
soulever	sous-alimenté
soutenir	sous-officier

(Selkirk's examples, p. 304.) Also across word-internal #, though the evidence seems remarkably weak: vit~vivons, dort~dormons, etc. (See Schane 1968.) Facts like these call for an emendation of the deletion rule in (11):

$$(44) \quad C \rightarrow \emptyset\ /\ \underline{\quad} \left\{ \begin{matrix} \# \\ + \end{matrix} \right\} C$$

But we need not do anything like this. The conclusion is simply that the units φ which have associated L-consonants are morphemes.

Another kind of example bearing on the same issue is the adverb in -ment. Some speakers pronounce the [t] in liaison contexts; others have [mã] everywhere. But there are no speakers who say, e.g., probable[mãt] V, but spéciale[mã] V. If L-consonants were associated with words,

we might expect variation like this.

To press the same point a little, note that liaison does not apply across zero, i.e. no "boundary." Suppose we accept a deletion account. The relevant examples are then piston, actuel, agnostique, exiger, hebdomadaire. Suppose we accept an insertion account. The relevant examples are nuage, Saône, naïf, douane, créance. Even though, in all ten cases, a syllable break presumably intervenes, we do not find CC becoming ØC by deletion or VV becoming VCV by insertion. On the first theory, this state of affairs is slightly puz-zling. Since it follows from nothing in particular, we must deploy specified boundaries: (44). But on the second theory, it follows right away from the fact that a syllable is not a unit which can be listed in the lexicon.

At this point I should remind the reader of the advan-tages gained by freeing the abstract structural conditioning (29) from the nuts and bolts of the actual rule of liaison (32). First, remember that the conditioning is purely syn-tactic. (32) applies word-internally unconstrained by it. Thus, writing something like

(45) $\emptyset \rightarrow L / X (_f Y \epsilon_1 \underline{\quad} \epsilon_2) Z, \epsilon_1 = \mathcal{V}, \epsilon_2 = VX$

crucially misrepresents the nature of the process. It is simply not true that liaison applies only across w-juncture.

Second, there is no other way to subject a list of rules to the same condition, at least without making its appearance in rule after rule look like some sort of amazing accident. It is in fact repeatedly true (Selkirk, to appear)

that configurational sandhi contexts have some generality.
For this reason we'd like to be able to factor the context
out of individual structural descriptions.

Somewhat unfortunately for my purposes, French does not,
despite Selkirk (1972 and to appear), constitute an illustra-
tion of this necessity. It is the case (Selkirk 1972) that
several processes other than liaison seem to apply in the
same context expressed by (29). However, on closer considera-
tion, it turns out that liaison simply feeds them, in a quite
straightforward manner. It is even unnecessary to have them
apply across w-juncture at all. I'm going to go into it here
because I think it's an interesting piece of phonology.

The second process to evidently respect (29) is nasaliza-
tion. Nasal vowels in French appear before consonants or word
edge, e.g. bon, bonté [bõ, bõte]. Nasal vowels often partici-
pate in alternations with sequences of non-nasal vowel plus
nasal consonant: bonheur, bonne [bɔnær, bɔn]. Writing a
nasalization rule to account for this sort of thing is a
traditionally attractive idea (but see Tranel 1977). The
rule looks something like (46).

(46)   V N ⇒ $\underset{[+nas]}{1}$ ∅

(46) allows us to purge underlying representations of all
nasal vowels, which is supposed to be an important economical
measure.

The case for an active rule, though not necessarily (46),
seems strengthened by the existence of facts like the follow-

ing.

    (47)(a)   C'est un b[õ] copain.

       (b)   C'est un b[ɔn] ami.

       (c)   Est-ce b[õ] ou est-ce mauvais?

       (d)   C'est b[õ].

Although lexical correspondences could handle morphological
alternations like

       (e)   b[õ]té

       (f)   b[ɔn]heur

they clearly are not up to facts like those in (a)-(d),
which show that the context is infinitely variable. See Part
IV.

    The drift of (47) is that nasalization fails exactly
when it would delete a consonant that liaison inserts. Clear-
ly we want to say that the [n] of <u>bon</u> is an L-consonant. Note
(48).

    (48)(a)   C'est un très très très b[ɔn] ami.

       (b)   Ce sera b[õ] après demain.

       (c)   C'est b[õ] en été.

To make this idea work, however, we have to make /bõ/ the
underlying form. Liaison produces [bõn], and what we are
forced to conclude is a rule of de-nasalization gives [bɔn].
The result, which seems quite desirable to me, is that de-
nasalization is simply a kind of by-product of liaison. I
for one find I have no intuitions at all about what ought to
be the directionality of rules like this. It seems to me
that no one should be especially astonished by a de-nasali-

zation rule, except when prepared by previous nasalization accounts of the same facts. The rule takes the form (49). It is a word-level rule.

(49) $V \rightarrow$ [-nas] / ___ [+nas]

(32) and (49) work together to produce the examples of (43) as follows.

(47)'	(a)	(b)	(c)	(d)	(e)	(f)
Underlying:	Ṽ C	Ṽ͡V	Ṽ/V	Ṽ.	Ṽ V	Ṽ C
Liaison:		N			N	
De-nasalization:	V				V	
	Ṽ C	VNV	Ṽ V	Ṽ	VNV	Ṽ C

(a) [bɔ̃] : word edge, preconsonantal

(b) [bɔn] : word edge, prevocalic, liaison context

(c) [bɔ̃] : word edge, prevocalic, no liaison context

(d) [bɔ̃] : full stop

(e) [bɔɴ] : morpheme edge, prevocalic

(f) [bɔ̃] : morpheme edge, preconsonantal

One point about this analysis. Although extremely simple, it requires underlying nasals alongside of the plain ones. Is this bad? I think we ought to seriously question the idea that the optimality of a grammar has much to do with the size of the required phoneme inventory. I'm afraid I don't understand in what sense "eliminating" phonemes by writing phonological rules in itself constitutes a saving. It seems to me that optimality more clearly resides in the form of the rules of the grammar. The trend of much recent research is toward showing precisely this: We can, and in fact should in

specific cases, buy a sought-after impoverishment and sim-
plification of the grammar at the price of an enrichment
(though reasonably well circumscribed) of the lexicon.

I know of no indication that anybody learning French
should want to resist setting up underlying nasals on uni-
versal grounds. On the contrary, any realistic alternation
condition (Kiparsky 1974) will prevent a free-ride derivation
by nasalization of words like dindon, quinze, oncle. Thus
evidently we'll need underlying nasals anyway.

There are sometimes supposed to be distributional vir-
tues to traditional deletion analyses of liaison and nasa-
lization. Once we've postulated a level of structure at
which French has its finals, it ought to be possible, as in
so many cases, to regularize the distributional statements
which we must at some point make by making them about this
level of structure, whose regularity would be disturbed only
in the ensuing derivation.

However, in the French case such advantages are not
obvious. Note that on both insertion and deletion analyses,
French must have final consonants and final vowels at all
levels: avec, ami. Note that on both insertion and dele-
tion analyses, nasal vowels must (accepting the alternation
condition) occur initially, medially, and finally at all
levels: onde, donc, chignon. As must nasal consonants,
even when postvocalic: mot, emmener, rhum. It would be
quite odd, of course, if the L-consonant associated with
every liaison-making word ending in a nasal vowel turned

out to be a nasal consonant. On an insertion analysis, this
would have to be an accident. But it is not true: grand,
long, dans.

One more rule apparently applying in liaison contexts
is what Selkirk (1972) refers to as ER-Conversion. This rule
is responsible for [-ɛr] ~ [-e] alternations in every respect
analogous to the -VN ~ -V alternations above.

(50)(a)   C'est un singuli[e] fromage.

(b)   C'est un singuli[ɛr] ami.

(c)   C'est singuli[e] en eté.

(d)   C'est singuli[e].

(e)   singuli[ar]ité

(f)   (no example)

(a)   word edge, preconsonantal

(b)   word edge, prevocalic, liaison context

(c)   word edge, prevocalic, no liaison context

(e)   full stop

(f)   morpheme edge, prevocalic

(g)   morpheme edge, preconsonantal

Following the same thinking, we want to make that [r] an L-
consonant. Having inserted it, we readjust the underlying /e/
to [ɛ], as in Schane (1968):

(51)   e → ɛ / ___ r

(Another rule backing ɛ before consonant plus high vowel is
supposed to be responsible for the [a] of singularité (Sel-
kirk, p. 348); this I accept for the purposes of my discus-
sion. It must, of course, apply after (51).) (51) is a

word-level rule. Derivations:

(50)ʼ	(a)	(b)	(c)	(d)	(e)
Underlying:	e C	e͡ V	e/V	e.	ə V
Liaison:		r			r
E-lowering:		ε			ε
	e C	εrV	e V	e	εrV

Below I assemble the three rules whose effects are

visible in liaison contexts. (52)(a) precedes and feeds (b)

and (c).

(52)(a) Liaison. $\emptyset \rightarrow$ L / Ψ ___ V

   (b) De-nasalization. V $\rightarrow$ [-nas] / ___ [+ nas]

   (c) E-lowering.   e $\rightarrow \varepsilon$ / ___ r

I stress, at the risk of overinsistence, that in (52) I use

no boundaries. In this practice I differ from other people

(Schane 1968, Dell 1970, Selkirk 1972, 1974) who have thought

about these processes. To instance the most recent treatment,

Selkirk's (1972) assumptions force her to write the following

rules:

(53)(a) Liaison.

$$C \rightarrow \emptyset / [\# X \underline{\quad} C_0 \left(\begin{Bmatrix} r \\ \# \end{Bmatrix} C X \right) \#]$$

   (b) Nasalization.

$$[\underline{\# Y} \; V \; N \; \underbrace{(\; (\#) \; C \; X \;) \; \#]}{} $$
$$\quad 1 \; 2 \; 3 \qquad 4 \qquad \Rightarrow \; 1 \; \underset{[+nas]}{2} \; \emptyset \; 4$$

   (d) ER-Conversion.

$$[\underline{\# Y} \; \varepsilon \; r \; \underbrace{(\; \# \; C \; X \;) \; \#]}{}$$
$$\quad 1 \; 2 \; 3 \qquad 4 \qquad \Rightarrow \; 1 \; \underset{[-low]}{2} \; \emptyset \; 4$$

It serves no special purpose to discuss these in detail,
especially since Selkirk no longer adheres to them. Still,
it is interesting to consider the metatheoretical background
whence comes the remarkable garishness of these rules. I
note that it is necessary to repeat three times (i) the syn-
tactic conditioning, (ii) part of the phonological condition-
ing, and (iii) part of the structural change. In each rule
we find (i) nearly identical stipulated boundaries and brac-
kets, (ii) the factor (C X), and (iii) a deletion of a fi-
nal consonant. (i) is necessary because the syntactic con-
ditioning is encoded phonologically, in terms of terminal sym-
bols, and because all three rules must be subject to it.
This in turn is necessary because of (ii) and (iii). (ii)
and (iii) follow because final nasal consonants and r's are
not analyzed as L-consonants. They can't be, on a deletion
theory, because of the ordering paradox which would arise.
Suppose we allowed liaison (a) to remove final nasals and
r's. Then there is no way to (non-globally) get (b) and (c)
to apply in the right spots, i.e. if and only if (a) applied
(or is going to apply, given the other order) to a final na-
sal consonant or r, respectively. Hence (a) must effective-
ly be repeated in (b) and (c). None of these problems a-
rises, given the rules in (52).

By the repeated factor ___ (# C X) # in (53) is meant
the complement of (52)(a)'s ___ V, for these rules not only
delete but apply non-vacuously in the non-elsewhere cases.
The distinction

(a) ...($\left\{ \begin{matrix} + \\ \# \end{matrix} \right\}$ C X) #

(b) ...((#) C X) #

(c) ...(# C X) #

is spurious, as far as I can tell. Apparent morpheme-internal nasalization (b) (dindon) is ruled out, even on a deletion analysis, by the alternation condition. On the insertion analysis, morpheme-internal de-nasalization can never happen because liaison can never insert a nasal consonant there. However, we do get alternations across +: bonté, bonheur. As for the required # in (c), it is based on examples like perdre, which does not get ER-converted to [pedrə]. On the insertion account, this shows exactly zero. None of our rules can do anything to /pɛrdrə/. But even on the deletion account, the alternation condition rules out such a derivation. Finally, there are no examples (opposed to particularité, etc.) making it necessary to stop ER-Conversion across +. On the insertion account, of course, particularité shows that application across + is necessary.

My point is that, given the assumptions underlying them, (53)(a)-(c) should really appear as

(a) ...($\left\{ \begin{matrix} + \\ \# \end{matrix} \right\}$ C X) #

(b) ...($\left\{ \begin{matrix} + \\ \overline{\#} \end{matrix} \right\}$ C X) #

(c) ...($\left\{ \begin{matrix} + \\ \overline{\#} \end{matrix} \right\}$ C X) #

This result shows that the stipulated boundaries, which could in principle vary like any stipulated term, are in fact absolutely identical in the three rules. Note that the array

of boundary phenomena which (53)(a)-(c) happen to account for
must follow given the treatment in (52)(a)-(c), which do not
refer to boundaries at all and hence cannot repeat then. We
do not have the option of playing with boundaries in our rules
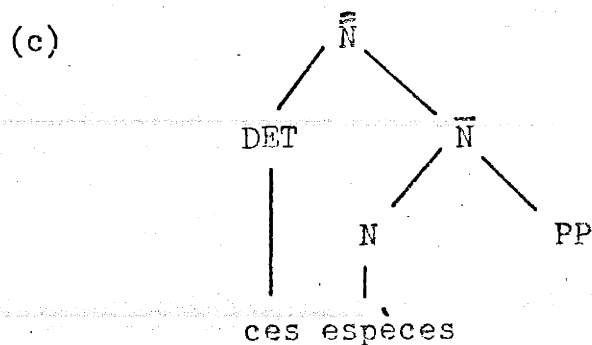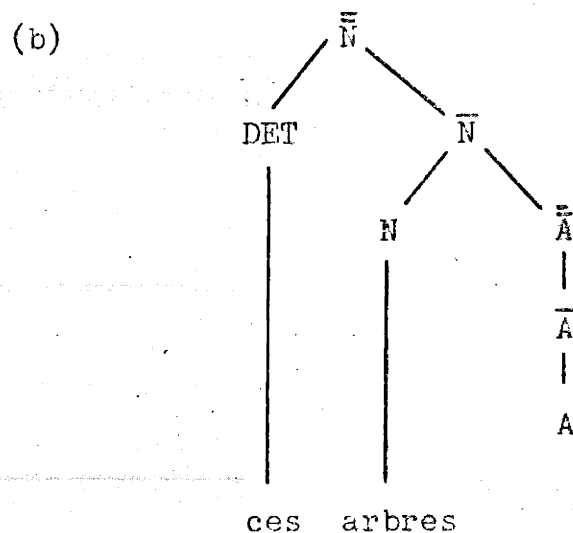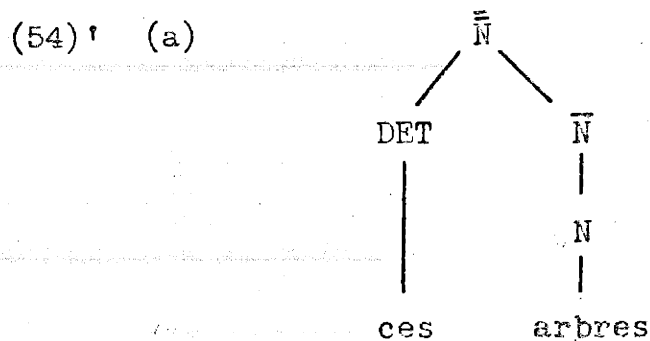in order to get some other output.

Notice, too, that liaison as in (53)(a) is written so as
to delete a consonant before another, word-final consonant,
as well as deleting the final consonant before a word-initial
consonant. Thus it is designed to collapse what have often
been thought of as "truncation" and "final consonant deletion,"
respectively. The relevant facts are alternations like
gran[t] homme, but grand[z] hommes. I've been restricting my
attention to "final consonant deletion." But, as Klausenbur-
ger (1977) points out, on the insertion account there is no
distinction in the first place. The L-consonant associated
with grand can be inserted only before a vowel, not, for in-
stance, before the plural morpheme Z. The L-consonant associ-
ated with Z is inserted under identical conditions.

Klausenburger also observes that this treatment entails
insertion of the L-consonant to mark the feminine. With re-
spect to the le ~ l' alternation and the few other lexically-
governed examples of final V alternating with zero, I see no
clear implications for the necessary machinery.

I return finally to the matter of the residue left by
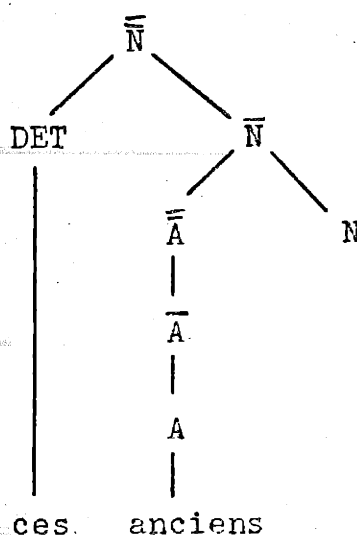(29). There are, as we know, words which are **never** found not
making liaison before vowels. Determiners, for instance:

(54)(a) ces⌒arbres

   (b) ces⌒arbres énormes

   (c) ces⌒espèces d'arbre
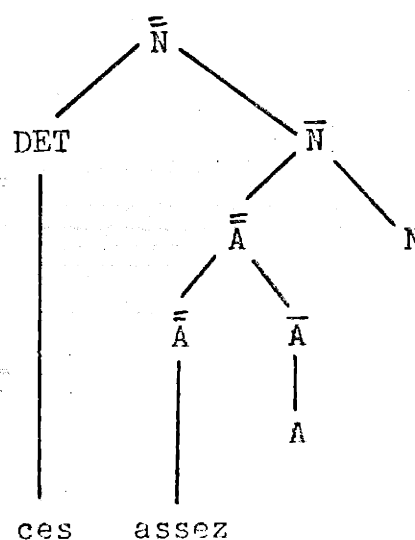
   (d) ces⌒anciens arbres

   (e) ces⌒assez gros arbres

I assume that corresponding to these strings are structures more or less like those in (54)'.

(54)' (a)

(b)

(c)

(d)

$\overline{\overline{N}}$ — DET, $\overline{N}$ — ($\overline{\overline{A}}$, N); $\overline{\overline{A}}$ — $\overline{A}$ — A

ces   anciens

(e)

$\overline{\overline{N}}$ — DET, $\overline{N}$ — ($\overline{\overline{A}}$, N); $\overline{\overline{A}}$ — ($\overline{\overline{A}}$, $\overline{A}$ — A)

ces   assez

I see no reason to doubt that, at the point when liaison ap-
plies, determiners are syntactically just where the phrase
structure rules put them, apart from the peculiar fact that
they consistently make liaison. In all but the first exam-
ple, of course, this conflicts with (29). It strikes me as
an extraordinarily unattractive conclusion that _ces_ must be
re-adjoined more closely to the word immediately on the
right: As far as I can make out, the only thing apparently
making such an operation necessary is (29). Evidently, then,
bearing the appropriate constituency relations is sufficient

for an item to be able to trigger liaison, but, for all non-lexical items, not necessary.

The class of unilateral liaison-makers includes specifiers of all kinds--articles (un, les, des, etc.), demonstratives (ces, cet), possessives (mon, ton, mes, tes), degree modifiers (très, bien, tout)--prepositions (en, chez, sans), and pronouns ("clitics")(en, les, nous).

With respect to this last case, pronouns, there is often supposed to be reason to believe that a pronoun defensibly moved from somewhere else ends up being closely adjoined in some fashion to the next word. In French, for example, the sequence clitic-verb exhibits a list of interesting properties, all seeming to lead to the conclusion that the two are sisters under a node V: (i) Nothing can intervene between a clitic and a verb; (ii) a clitic cannot occur independently of a verb; (iii) a clitic cannot be modified; (iv) clitics occur in a strict order. (From Kayne 1975, pp. 81 ff.)

(55)(a) Jean toujours soupçonne Marie.

(b) *Jean la toujours soupçonne.

(c) Jean la soupçonne toujours.

(56)(a) Qui est-ce qu'il soupçonne?

(b) *La.

(c) Elle.

(57)   *Jean vous trois soupçonne.

(58)(a) Jean le lui donnera.

(b) *Jean lui le donnera.

Thus we might like, with Kayne, to say that clitic and verb
are co-constituent, both making up another V, which explains
why the clitic behaves in the relevant respects like verbal
morphology. Saying such a thing would, it is true, account
for the fact that liaison between clitic and verb is obliga-
tory.

(59)   Jean $\begin{Bmatrix} \text{les} \\ \text{vous} \\ \text{nous} \end{Bmatrix}$ abomine.

However, it seems to me that properties (i) through (iii)
clearly follow from the existence of a late rule moving
clitics to pre-verbal position. I confess I don't really
see why property (iv), clitic ordering, says anything in
particular about their relationship to the following verb.
After all, a noun phrase before a verb has internal ordering
of constituents too, but that does not speak for its being
proclitic. In any case, it is hardly overwhelmingly obvious
that movable pronouns end up adjoined in any fancy way. I
will assume, in fact, that there is no such adjunction opera-
tion, any more than in the cases of specifiers and preposi-
tions.

The same four tests, incidentally, when applied to
specifiers and prepositions, fail utterly, presumably under-
scoring the distinction moved/not moved rather than the dis-
tinction "cliticized"/not "cliticized": (i) There is no
category that stands to a specifier or preposition as a
verb stands to a clitic. In (54), for instance, the noun

is clearly not it, because various sorts of lexical material can comfortably intervene; (ii) specifiers and prepositions assuredly do not occur alone, but are like other non-lexical categories in this respect; (iii) specifiers and prepositions cannot be modified, but this, of course, is virtually the definition of non-lexicalness--categories which take modifiers do not lie outside the bar system; (iv) specifiers and prepositions occur only singly, hence obviating ordering problems.

The immediate point is that the special status of the non-lexical categories is essentially aconfigurational--it has nothing to do with their position with respect to the surrounding structure. It is simply true of them, in some way, that they make liaison. I take this opportunity to point out that, without boundaries, we also are powerless to define their exceptionality in terms of boundary weakness --i.e., fewer than two #'s.

I believe that this impotence is in fact appropriate. There are cases far more striking than (54). For instance, contraction in Modern Athenian Greek occurs to the right of a verbal clitic even though the verb is on the left. (From Kaisse 1977, pp. 125 ff.)

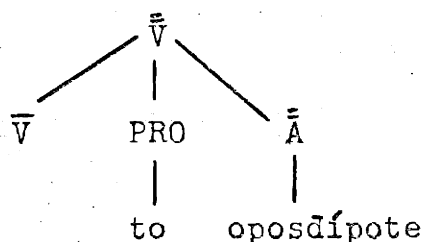(60) diàvasé to oposdípote → ...t'oposdípote

     read   it anyway

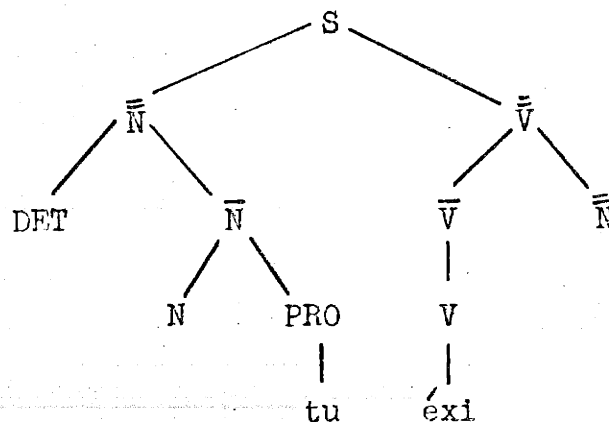   'Read it anyway.'

(61) í mána tu éxi dío pedyá → ...tu'xi

    the mama his has two children

    'His mama has two children.'

(60)'

$$\overline{\overline{\overline{V}}}$$

```
        V̿̿
       /|\
      /  |  \
     V̄  PRO  Ā̄
         |    |
        to  oposdípote
```

(61)'

```
                    S
                  /   \
                /       \
              N̿̿         V̿̿
             / | \       /  \
           DET N̄  \    V̄    N̿̿
              / \   |
             N  PRO V
             |   |   |
            tu  éxi
```

Contraction always happens between a clitic and a word to
the right, supposing phonological conditions to be favor-
able. Note that no syntactic intimacy seems to be required.
As Kaisse shows, pointing to the lack of word boundaries a-
round clitics will not do either, since, in cases like (61),
the N dominating the clitic will have its own #, ultimately
leaving two between clitic and verb. Thus a contraction
rule demanding the presence of a single word boundary will
not be able to distinguish a sequence [N PRO] V , as in
(61), from a sequence [N][V], where no contraction occurs.

Now since (29) concerns itself solely with constituent
structure, defining non-lexicalness as (merely) a function
of boundary weakness will be ineffective, at least if we want
the non-lexical items to participate in the same rule of li-
aison. Examples like the preceding show that it is actually

wrong (in Greek, anyway). But, as there is no indication
that we should syntactically re-adjoin them by special rule,
we are left with the problem of how to represent their re-
lationship to the next word.

Words usually considered to be clitics are notorious
for behaving only partly as if they were affixes of the host
word; if they behaved in every respect like affixes, there
would be very little reason to think that they were not. On
the other hand, they do not behave like completely autono-
mous words, either--witness the special phonology I've been
talking about. Clearly we need some way of marking these
special items as special. As I've suggested, it cannot be
close phonological association (in terms of boundaries),
and it cannot be close syntactic association.

Let us consider once again the French case. The fact,
stated most baldly, is that liaison is always made by non-
lexical categories. We clearly want to say in some way that
making liaison goes along with belonging to a non-lexical
category. The reader should be struck by the similarity of
this as yet vague conclusion to the one reached with respect
to the English and Irish rules of Parts I and II.

In all these cases, a rule always applies across a
w-juncture if a non-lexical item is on the left.

I drive home this fact by noting that liaison, like
the other examples, fails at high levels when w-junc-
ture does not intervene--across trace, parenthesis, and
outermost sentence edge.

(62)(a) Il y en a beaucoup t / à rencontrer.

    (b) Il y a beaucoup à écrire.

(63) Il y a beaucoup,/ à ce qu'on dit, à écrire.

(64)(a) C'est nous./ Allons-y...

    (b) Nous allons.

(With respect to beaucoup, the appropriate category is not obvious, as with English lots. I'll just assume, with Selkirk (1972), that it belongs to a non-lexical category, called there Q.)

I think it's clear that we want to say in some fashion that a sequence of non-lexical item(s) plus lexical item $\begin{bmatrix} -N \\ -V \end{bmatrix}^*$-w, separated by w-juncture(s), is a unit, call it a clitic group, intermediate between w and f. Although this last statement is not going to be universall true--it does not allow for enclisis--we will nevertheless be able to give a universal definition of the clitic group, henceforth abbreviated c. Having done this, we could reasonably omit the specification $\begin{bmatrix} -N \\ -V \end{bmatrix}$ in the left terms of rules like English monosyllabic destressing (Part I). We would simply say that they are c-level rules. Similarly, the Irish initial mutations are triggered by features belonging only to non-lexical items because these rules are morphologizations of older c-level rules.

Likewise liaison would be a c-level rule.

As for defining c properly, suppose we said that clitics are distinguished from other items merely in being marked [+P] or [+E], i.e. 'proclitic' or 'enclitic'. For

French, it appears that we can get away with saying that the non-lexical categories are all redundantly [+P].

(65) $\begin{bmatrix} -N \\ -V \end{bmatrix} \rightarrow [+P]$

Now, by a convention which is presumably universal,

(66) a clitic group c is a maximal sequence of terminal

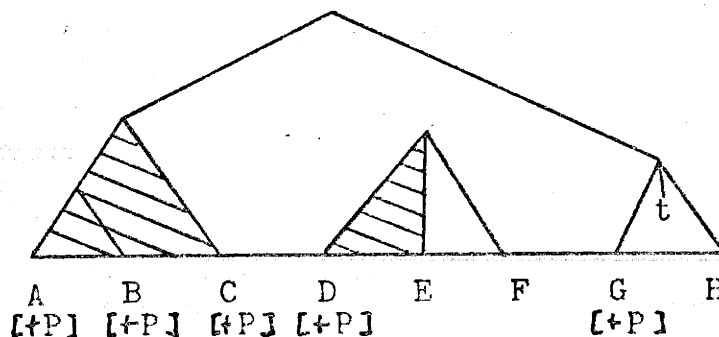$A_1 \ldots A_n$ such that for every pair $A_i A_{i+1}$

(i) there are nodes $\alpha_1 \ldots \alpha_n$ such that the figure

$\alpha_1 \ldots \alpha_n A_i A_{i+1}$ bounds a w-juncture and

(ii) A_i is [+P] or A_{i+1} is [+E].

How this works might become clearer with an example.

(67)

A through H are words. $\overline{ABC}$ and $\overline{DE}$ are clitic groups, shaded in the figure. Since $\overline{CD}$, $\overline{FG}$, and $\overline{GH}$ are not sides of curves bounding any w-juncture and since E is not [+P], CD, EF, FG, and GH are not members of clitic groups. However, by the definition, F, G, and H, though words, are also clitic groups which happen to contain no clitics.

Thus homme, un⌢homme, chez⌢un de ces⌢hommes are all clitic groups. Beaucoup/ t à rencontrer, beaucoup/à ce, nous/allons ((62)(a), (63), (64)(a)) are not clitic groups.

Now recall the conclusion we reached with respect to phrase-level liaison. We said that liaison applies before a right branch (29). Note that this formulation, accounting for f-level liaison, is totally distinct from what we just said about c-level liaison.

I believe that this distinction is exactly what's appropriate. It is a truly remarkable fact that it is precisely c-level liaison that is absolutely obligatory, and shows no sign of attrition (save via sporadic item-by-item loss of L-consonants), even in the most radical urban dialects. By contrast, f-level liaison has a growing optionality in conversational French, and is definitely on the way out.

In sum, liaison is an obligatory c-level rule, i.e. (68) is true of it,

$$(68) \quad W \; (_c \; X \; w \; w \; Y \;) \; Z$$

and also an optional f-level rule under the familiar conditions.

$$(69) \quad X \; (_f \; Y \; c \; c \;) \; Z$$

(69) is shaky to varying degrees in modern dialects. Note that, having defined c, I now state (69) in terms of it. The reason is that there are contrasts like the following.

(70)(a) Il est̑ honnête.

    (b) Il est̑ assez honnête.

    (c) Il est/évidemment honnête.

(71)(a) Il est̑ avocat.

    (b) C'est̑ un avocat.

(both)

   (c) Il est/avocat et législateur à la fois.

Assez honnête, un avocat are clitic groups. Évidemment hon-
nête, avocat et législateur à la fois are not.

Why a sequence CONJ X, as in (23)(a), resists being a
clitic group is a mystery which I have not succeeded in pene-
trating.

A concluding remark. It seems clear that every theory
has to have some way of representing a word and its associated
"clitics" as a unit. All a clitic is is a non-lexical item
which behaves with respect to some grammatical process or
processes (originally stress rules) as if it were included
in a lexical item to its immediate left or right. What I've
done, first, is to propose that there is such a unit, namely
c, and that the processes usually thought to be diagnostic
of an operation of cliticization are really rules at level c.
Rather than performing some structural (syntactic or phono-
logical) deformation to express cliticization, it is easier
and less artificial to reduce clitichood to a lexical redun-
dancy and to define c, in turn, in terms of it. In English,
French, and Irish, the required redundancy rule is evidently
very simple: All non-lexical items are [+P]. In languages
with more complicated clitic systems, of course, we'll have
to say special things about individual non-lexical items in
the lexicon, or wherever it is that idiosyncratic information
about non-lexical items goes.

THE HEBREW EXAMPLE

As a second illustration I cite (though not at nearly as great length) the accentuation of the Hebrew Old Testament. I am indebted to John McCarthy for drawing my attention to Wickes (1881) and for a lot of help with the Hebrew.

There is a traditional system of diacritics accompanying the Old Testament, devised by unknown hands, which is designed to represent orthographically the details of the even more ancient declamation of the books. Actually there are several systems; I restrict my attention here to the one used in standard Tiberian Hebrew, since it is both the least degenerate in its details and the best documented. The cantillation was propagated orally until some time between the fifth and ninth centuries, when someone thought to actually write it into the texts, using a fantastically complex system of signs --the "accents." By means of these accents three aspects of the traditional Hebrew oratory are diacritically represented: the pronunciation, the music, and the intonation. (The vowel signs, also diacritic of pronunciation, were probably introduced at about the same time. See, however, Dotan (1964, 1972).) The accentuation takes the form of signs written above, below, before, or after (depending on the particular accent) the tonic syllable of each word, thus incidentally indicating primary stress. So much for the phonetic value of the accents. As for their original musical value, it is

almost totally lost. They probably stand for things like
'trill', 'sustain', etc. But--this is the interesting part--
the intonational value turns out to be deducible from the
texts themselves, together with a grammar of the language
they're written in. I mean to say that there exists an al-
gorithm for distributing the accents over any text. (This
makes them--or at least the linguistic information borne by
them, as we'll see--ultimately redundant, in much the same
way as, for instance, question marks.)

It is very likely the case that the <u>principal</u> value of
the accents is intonational, each accent mainly carrying one
of two possible pieces of information: 'pause' or 'no pause'.
As in many religious traditions, the melodic side of the
scriptural reading is what is called "logogenic," i.e. more
or less servile to and reflective of the syntactic and rhe-
torical organization of the text, though far from fully pre-
dictable from it. In fact the accentuation is known as
<u>pissuk te'amim</u>, the division by the accents, evidently re-
ferring to their essentially pausal value.

Now my point is going to be that this same syntactic
and rhetorical organization determines how the pauses are
distributed, and in an interesting way. I don't claim to
have discovered this interesting way; its discovery is
largely the work of Christian accentuologists of the seven-
teenth and eighteenth centuries. See Florinus (1667),
Spitzner (1786). More recent versions of the same idea are
in Wickes (1881) and Spanier (1927).

The accents fall into two classes, the so-called dis-
junctive and conjunctive, having, along with an overlay of
musical significance, the respective meanings 'pause follows'
and 'pause does not follow'. (The absence of the one could
not be allowed to stand for the other, because the melody
still had to be notated.) We deduce that these pauses are
more than merely part of a narrow religious tradition from
the fact that they appear to condition authentic phonologi-
cal processes. There is, for instance, a rule spirantizing
initial stops after a word-final vowel. It applies only
in the vowel-final word bears a conjunctive accent.

(70)   wayyillāhămû ⌢ bǝnê-yǝhûḏā

       and-waged-war sons-of-Judah

       'and the sons of Judah waged war' (Jud. i 8)

(71)   wayyiš?ălû/bǝnê   yiśrā?ēl

       and-asked   sons-of Israel

       'and the sons of Israel asked' (Jud. i 1)

(Spirantization is notated in the usual transliteration with
the underline. I continue my notational practices in indi-
cating conjunction with ⌢, disjunction with /.)

There is an astonishingly involved set of rules (non-
linguistic in nature) governing which accent out of one class
or the other is actually used in the text. I will not dis-
cuss these. The system applied to the "Poetical Books" (Job,
Psalms, and Proverbs) is slightly different from the one ap-
plied to the "Prose Books"; the reason is that a different,

more florid and artificial sort of melody accompanies the
shorter verses of the Poetical Books. This matter I will not
discuss either.

It seems that the Hebrew accentuators proceeded, either
unconsciously or by way of conscious formalization of un-
conscious processes, as follows. (It's quite possible, inci-
dentally, thay they could have had some commerce with expo-
nents of the quite active Hebrew grammatical tradition.) A
binary branching is imposed on each verse, the highest branch
being at its caesura. (The prose books are arbitrarily
broken up into verse-length chunks for this purpose.) This
process of unlabeled bracketing, called the "continuous
dichotomy" by the Christian accentuologists, is repeated,
top down, until no more three-word stretches are left
unexamined.
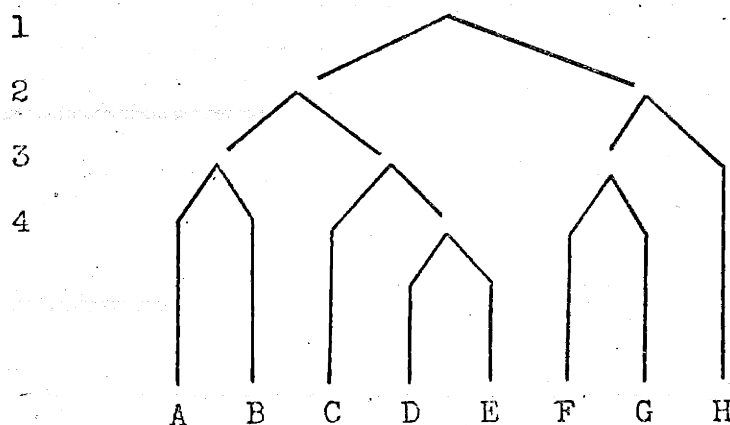
Put differently, only two-word strings may stand undivided:
AB, DE, and FG in the example. Words A, D, and F receive con-
junctive accents. B, C, E, G, and H receive disjunctive ac-
cents. Which among the twenty-odd signs is actually found on

a word depends in part on which dichotomy (the numbers in the example) finally results in its marriage or separation-- i.e. its distance, in number of nodes, from the root of the tree. But there is no reason to think that this orthographic distinction, with the aid of which an entire tree is recon- structible, was reflected in the spoken Hebrew by graded lengths of pause or no-pause. (See Wickes, II 14-15.)

This bracketing fairly clearly corresponds to the syn- tactic structure of the verse.

There are a few qualifications I must interject. The first is that the correspondence is fairly sloppy at gross levels of structure, because it is precisely here that bi- nary syntactic branchings begin to become indefensible. I'm speaking of several kinds of lack of defense. One, the kind exhibited by adjacent sentences--separated by full stop-- which often occupy a single verse and end up separated by the dichotomy; presumably no syntactic branching at all corres- ponds to this division. (Every word has to have an accent. There is no third possibility after conjunction and disjunc- tion. In this last case, of course, the intonational value of the disjunctive, pause, is quite appropriate.) Two, the ternary branching of S immediately necessary for a VSO language such as Hebrew. Three, the workaday n-ary branchings made necessary at phrasal levels by multiple adverbs and PP's. In all these cases the (real, we're assuming) n-ary branching shows up in the texts as a nesting of n-1 fairly arbitrary binary ones. There are somewhat artificial rules for carry-

ing out this subordination, guaranteeing, in the main, left-branching structures.

The second qualification is that the dichotomy is easily bullied to one spot or another by the rhetorical purpose of the particular verse. Words which the accentuators (or the speakers they were transcribing) thought worthy of emphasis, even when they would otherwise (see below) receive conjunctive accents, show up set off by disjunctions. This of course is fairly natural on the assumption that a disjunction corresponds to a pause, in this case pregnant.

(72) hēn gôr   yāgûr   ʔepes/mē?ôtî

       if   strife one-strives not   by-my-order

     'If anyone stir up strife, it's not my fault.'

      (Is. liv 15)

Similarly, though more amusingly, the accentuators were somewhat nervous about conjoining the name of the Lord to questionable objects. In such cases the holy titles often get one of the first disjunctions, even though the syntax may not happen to correspond.

Explainable exceptions aside, observe the results of the continuous dichotomy. Between any pair of words ultimately and exhaustively dominated by some node there is no pause. Between all other pairs of successive words some sort of pause is at least allowed.

The rule distributing no-pause at word edge is (73).

(73) $\emptyset \rightarrow$ no-pause $/ \begin{cases} \emptyset \underline{\quad\quad} \\ \underline{\quad\quad} \emptyset \end{cases}$

This we might collapse into (74), using mirror-image notation.

(74) $\emptyset \rightarrow$ no-pause $// \emptyset$

('Before or after a word, a pause is not allowed.' That this
generalization is actually syllabic is a possibility which I
have no way to investigate. Slightly unfortunately for this
idea, C͡C is found just as often as V͡C.) (74), as written
a word-level rule, has a condition on it, obvious enough from
the way I've been describing the problem:

(75) X ($_f$ w w) Y

I'll give some examples of sole sisters held together by
the accents and more distant relations separated by them.

A Hebrew conjunction is literally part of the word to its
immediate right. There is evidently no phonological test
enabling one to say that there is a word edge between the two.
(I've been notating this synthetic arrangement with a hyphen
in the gloss.) Therefore, unlike French and English, Hebrew
cannot and does not practice Rossian subordination of the
second conjunct, and it must be a sister of the first. In
fact the two conjuncts consistently show up accentually glued
together--

(76) wəlimšōl   bayyôm ⌢ uḇallaylā(h)
      and-to-govern over-the-day and-over-the-night
      (Gen. i 18)

(77) kî-ᶜāśîtā   mišpaṭî ⌢ wəḏînî
      for-you-have-performed my-judgment and-my-law
      (Ps. ix 5)
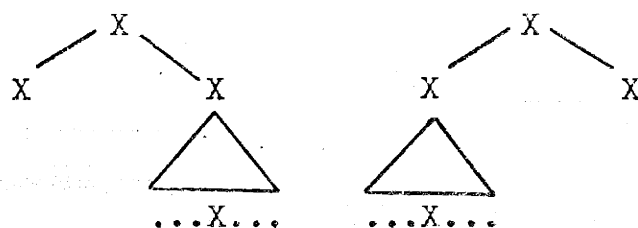
(78) yəhaləlûhû   šāmayim^wā?āres

will-praise-him heavens and-the-earth (Ps. lxix 35)

--just as in the occasional asyndetic compound.

(79) sinnîm^paḥîm

arms   (and-)swords (Prov. xxii 5)

However, as soon as either conjunct consists of more than
a single word, the two zero-bar categories of the coordinate
construction no longer exhaust it,

and a dichotomy is made.

(80) yammîm/wəkōl- rōmēś   bam

seas   and-all crawling-things in-them (Ps. lxix 35)

(81) yahweh ṣaddîq   yibḥan wərāšāᶜ/ wə?ohēḇ^

God   righteous watches and-evil and-lover

ḥāmās

of-violence

'God watches the righteous and the evil and the

lover of violence' (Ps. xi 5)

Conjoined verbs behave identically to the nouns in the
foregoing examples:

(82) ?et-mî hēraptā ^ wəgiddaptā

whom   you-dreaded and-you-feared (Is. xxxvii 23)

(83) šuḇ ^ qaḥ-ləkā məgillāh ?aḥeret

do-again take   scroll   another

'take again another scroll' (Jer. xxxvii 23)

Again, a complex conjunct induces a disjunction:

(84) yizkərû / wəyāšubu ⌢ ?el-yahweh kol

will-remember and will-turn to-God all

?apse-?āreṣ

ends-of-the-earth

'all the ends of the earth will remember and turn
to God' (Ps. xxii 27)

(85) lō ⌢ yādə͜ʿû / wəlō ⌢ yābînû

they do-not-know and-they do-not-understand

(Ps. lxxxii 5)

Likewise conjoined adjectives, also sometimes asyndetic:

(86) yahweh/ʿizzûz⌢wəgibbôr

lord strong and-mighty (Ps. xxiv 8)

(87) kî-yahweh/ʿelyôn⌢nōrā

lord high terrible (Ps. xlvii 3)

Nouns show up conjoined with all manner of single-word
complements--adjectives, objects of word-internal prepositions,
etc.

(88) ?ēl⌢gādol

God great (Ps. xcv 3)

(89) wəšōhad⌢ʿal-nāqî

bribe against-the-innocent (Ps. xv 5)

(90) śimhā(h)⌢lā?îš

joy to-a-man (Prov. xv 23)

But when any qualifying expression is more than two words
long, a disjunctive accent falls on the preceding nominal.

Cf. (86), (87) and:

(91) kə‵ēs/sātûl   ‵al-palgê māyim

tree planted at-water-brooks (Ps. i 3)

(92) hā?ēl / hannôṯēn   nəqāmôṯ   lî

the-God the-one-giving vengeances to-me

'the God avenging me' (Ps. xviii 48)

(This last is a standard relative clause.)

An interesting case is the pseudo-appositive (very like-
ly another example of asyndeton): King David, the-Lord God,
etc., consistently conjoined by the accents. Compare the
true appositive, just as consistently disjoined:

(93) ?eṯ-?oyəḇêḵem / ?et-mô?āḇ

ACC-your-enemies ACC-Moab

'your enemies, the Moabites' (Jud. iii 28)

(94) ?et-bərîṯî   / šālôm

ACC-my-covenant peace

'my covenant, peace' (Num. xxv 12)

(Note the intonational contrast even in the corresponding
English.) Parentheticals, in general, have disjunctions on
both sides:

(95) wayyišmā9 ⌒ pašhur/ben-immēr ⌒ hakkōhēn/

and-listened-to Pashur son-of-Immer the-priest

wəhû-pāqîḏ   nāḡîḏ

and-he-was-entrusted with-an-office

bəḇêṯ yahweh /?eṯ-yirməyāhû

in-the-temple ACC-Jeremiah

'Pashur, son of Immer the priest (and he was en-

trusted with an office in the temple) listened
to Jeremiah'   (Jer. xx 1)

Unfortunately, examples like this prove very little with
respect to the possible failure of (74) across parenthesis,
because we already know that a disjunction appears where
any kind of pause occurs, even the kind that we can assume
comes with Hebrew parentheticals.   I also know of no signi-
ficant examples of what one might assume is a trace blocking
(74).

THE ITALIAN EXAMPLE

As a final illustration of high-level phonology influenced by constituency relations among the words it involves, I'm going to briefly discuss a single rule of Italian, known as raddoppiamento (sometimes rafforzamento) sintattico 'syntactic doubling' ('strengthening'), which geminates an initial consonant under dialectally varying phonological conditions and under either of two pan-dialectal syntactic conditions. According to Napoli and Nespor (1977), "RS [the abbreviation I'll adopt from here on in] can apply between a word a and a following word b, where a is immediately dominated by the pre-terminal category symbol A and b by B,...if A is the left branch of the first node that dominates both A and B." This is the same as saying that RS applies between a and b if a begins a constituent to which b also belongs. In our terms, RS is subject to (96).

(96) X ($_f$ w w Y) Z

Also, "RS can apply on S between the first constituent and the second constituent..." That is,

(97) ($_f$ X w) w Y

For instance:
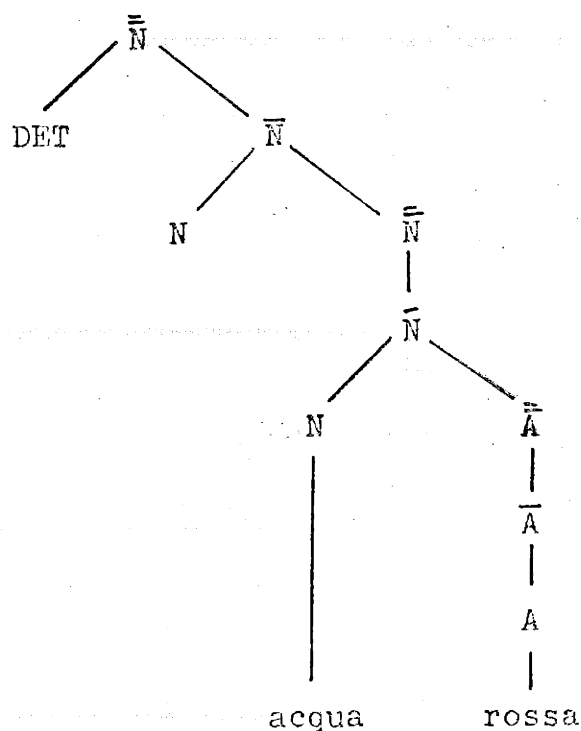
(98)   Voglio una brocca d'acqua rossa.

(a) 'I want a pitcher of red water.'

(b) 'I want a red pitcher of water.'

(Napoli and Nespor's examples throughout.) This sentence is

ambiguous. However, if RS doubles the initial of <u>rossa</u>, only the first reading turns out to be possible. That this should be so follows from (96).

(98)(a)'

```
              N̿
            /   \
         DET     N̄
               /   \
              N     N̿
                    |
                    N̄
                  /   \
                 N      Ā̿
                 |      |
                 |      Ā̄
                 |      |
                 |      A
                 |      |
              acqua    rossa
```

(99)(b)'

```
              N̿
            /   \
         DET     N̄
               /    \
              N̄      Ā̿
            /   \     |
           N     N̿    Ā̄
                 |     |
                 N̄     A
                 |     |
                 N     |
                 |     |
              acqua  rossa
```
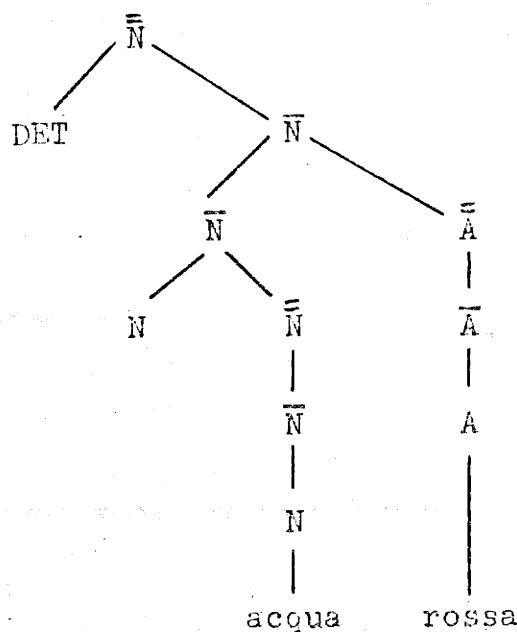
Similarly, "old men and women" examples like (99).

(99)   le ragazze e i ragazzi tedeschi

(a)   'the German [boys and girls]'

(b)   'the [German boys] and girls'

RS after _ragazzi_, by (96), forces the (b) interpretation.

In the next example,

(100) Maria verrà/sicuramente.

'Maria will come for sure.'

RS is impossible after _verrà_ because _sicuramente_ is a sentence
adverb. Thus S branches ternarily, and _verrà_ and _sicuramente_
are the first words of no constituent. But suppose (100)
lacked a subject noun phrase:

(101) Verrà sicuramente.

'She'll come for sure.'

RS appears, by (96). (I do not take any position here on
whether the subject in (101) went via a rule of subject pro-
noun drop or was simply absent to begin with. Recall that w
is terminal, so that a dangling node will fail to be factored
by either (96) or (97).)

Quite generally, in fact, in multiply branching struc-
tures $[_\alpha \underline{a} \underline{b} \underline{c} \dots \underline{n}]$ (α of arbitrary category and every $\underline{k}$ a
word) the result is $\underline{a} \underline{b}/\underline{c}/\dots/\underline{n}$.

(102)(a)   È $[_{\bar{A}}$ bagnato di vino.

(b)   È $[_{\bar{A}}$ bagnato di vino/da capo a piedi.

(c)   È $[_{\bar{A}}$ bagnato da capo a piedi.

(a) 'He's wet with wine.'

(b) 'He's wet with wine from head to foot.'

(c) 'He's wet from head to foot.'

(103)(a) Ho visto tre [$_{\bar{N}}$ grandi⌢cani.

(b) Ho visto tre [$_{\bar{N}}$ grandi⌢cani/pastori.

(c) Ho visto tre [$_{\bar{N}}$ cani⌢pastori.

(a) 'I saw three big dogs.'

(b) 'I saw three big German shepherds.'

(c) 'I saw three German shepherds.'

(104)(a) Ha [$_{\bar{V}}$ già⌢parlato.

(b) Ha [$_{\bar{V}}$ già⌢visto/Carla.

(c) Ha [$_{\bar{V}}$ visto⌢Carla.

(a) 'He's already spoken.'

(b) 'He's already seen Carla.'

(c) 'He's seen Carla.'

(97) is responsible for RS in cases like (105)-(107).

(105) La religione che pratica⌢purtroppo perde fideli.

'The religion he practices unfortunately is losing

followers.'

(106) Dopo martedì Maria non viene più.

'After Tuesday Maria isn't coming anymore.'

(107) Mangiò i fagioli⌢Mario sono sicura.

'Mario ate the beans, I'm sure.'

I would like to point out that the existence of a phenom-
enon like the one accounted for by (97) poses a serious prob-
lem for any theory trying to do the same job by characterizing
in some way the terminal symbols across which RS applies.
There is no arrangement of word boundaries which RS, governed
by (97), will fail to breach. The reason is that it is not

the <u>words</u> on either side of the ligature in examples like
(105)-(107) that must stand to each other in a stipulated
structural relationship, but the phrases they belong to.
In structures $[_\alpha$ A B C ... N] (α = S and every K $\in$ f), the
result, by (97), is A⌢B/C/.../N. So:

(100)　$\bar{\bar{N}}\,\bar{\bar{V}}/\bar{\bar{A}}$　Maria verrà/sicuramente.

(108)(a)　$\bar{\bar{N}}\,\bar{\bar{A}}/\bar{\bar{V}}$　Maria⌢quando vorrà/verrà.

　　　　'Maria when she wants will come.'

(b)　$\bar{\bar{V}}\,\bar{\bar{N}}/\bar{\bar{A}}$　Verrà⌢Maria/di sicuro?

　　　　'Will Maria really come?'

(c)　$\bar{\bar{V}}\,\bar{\bar{A}}/\bar{\bar{N}}$　Verrano⌢di sicuro/quei fratelli?

　　　　'Will those brothers really come?'

(d)　$\bar{\bar{A}}\,\bar{\bar{N}}/\bar{\bar{V}}$　Sicuramente⌢Maria/verrà.

　　　　'Surely Maria will come.'

(e)　$\bar{\bar{A}}\,\bar{\bar{V}}/\bar{\bar{N}}$　Di sicuro⌢verrano/quei fratelli.

　　　　'Surely those brothers will come.'

I might note that (96) and (97) are not really as dis-
similar as they appear. The import of both is that in
$[_\alpha$ A B C ... N] only the left branch of α, A, makes RS with
what follows. If α = S, A $\in$ f. If $\alpha \in$ f, A = w. So we might
think of writing (109).

(109)　X ($_\alpha \epsilon$ ϵ Y) Z, where $\alpha \in \{f,S\}$ and $\epsilon \in \{w,f\}$

It is interesting that there appears to be no evidence
for c in Italian, at least not from RS. The reason is that
Italian, like French, is right-nuclear. A specifier or pre-
position is never found as anything but A in $[_\alpha$ A B C...],
which is always covered by (109).

I ought to mention, finally, that RS appears to also have syllabic conditioning. It is essential to know, before going into this, just what the rule actually does. In the speech of Saltarelli (1970) and Vogel (1977), essentially "standard" Tuscan Italian, a lone initial consonant doubles following a stressed short vowel.

$$(110) \quad [+\text{cons}] \rightarrow [+\text{long}] \ / \ \begin{bmatrix} -\text{cons} \\ -\text{long} \\ +\text{str} \end{bmatrix} \_\_\_ V$$

Final vowels are always short in Italian. However, medial vowels may be long. Level theory has us predicting word-internal RS after ones that are not. In fact, it occurs. Compare, for instance, casa [ká:sa] 'house' and cassa [kás:a] 'box', which Saltarelli gets from /ka:sa/ and /kasa/ respectively. However, these pronunciations are obligatory, whereas RS at word edge is blocked by pause.

Now this kind of thing is the usual story for syllabic rules. Pause excludes a syllabic linkage. There are no pauses word-internally. Hence the facts, if we say that the operation of our rule is contingent upon a syllabic connection having been established. (Compare, for example, the case of English palatalization. We have obligatory pic[č]ure but optional pick[č]your. Presumably the same obligatory rule does both of these, but requires ambisyllabicity of the dental, which is optional at word edge. More on this in Part IV.) Vogel (1977) in fact justifies, to my mind totally convincingly, a rule of left capture of initial consonants for Italian.

She ends up saying, in essence, that only ambisyllabic conso-
nants geminate. The rule becomes (111):

(111)

$$\sigma \qquad \sigma$$

V   C

[+long]
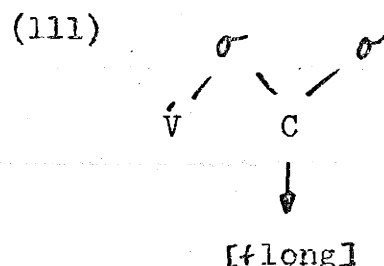
In this (in my opinion) peculiar situation, where (111)
has both prosodic and non-prosodic structural conditioning,
I suppose we ought to expect the diagnostics of the latter to
defer to those of the former. Syllabic structure is built
without any regard to the existing syntactic edifice, inclu-
ding w-juncture. (I refer the reader to the next Part for
some justification of this statement.) Hence trace and paren-
thesis should not block RS.

This appears to be true. Napoli and Nespor cite exam-
ples like (112), for instance.

(112) Chi è che i bambini immaginano t grandi?

'Who is it that the children imagine big?'
It is especially interesting that RS is possible into a
parenthetical, but not out of one.

(113) Non ne posso più sai/di Giorgio.

'I can't take any more, you know, of George.'
Note that it is still true that immaginano and più are left
branches. Sai is not. So (109) is still appropriate. (1),
incidentally, correctly allows [α ... [parenthetical] ...]
to be a α, as examples like (113) evidently require that

(109) permit, since, even given our conclusions (Part I)
about the structural independence of the parenthetical, (1)
is defined in terms of bracketing, rather than domination.

I'd like to make a concluding remark. It seems to me
that the potential value of the theory of configurationally-
conditioned sandhi which I developed and illustrated in this
Part is great. I chose my examples in order to avoid prob-
lems of debatable constituent structure; the essentials of
the structures which I assumed, without defense, were
moderately clear for independent reasons. But there are,
needless to say, many remaining problems in the area of
derived syntactic structure and many constructions whose
details or even gross features have not been worked out.
Now given a theory of syntax-sensitive sandhi for some lan-
guage, it should in principle be possible to learn about
poorly-understood constituency relations from the observable
phonological facts. Note that French, Italian, and Hebrew
offer diagnostics for right branch, left branch, and sole
sisterhood, respectively. The possibilities are enormous.
Here, I think, is the theory's most useful and welcome
application.

PART IV

ENGLISH PALATALIZATION

The information in a labeled bracketing is of two obvious sorts: The configurational, carried by the bracketing, and the categorial, carried by the labels. I've been preoccupied so far with configurational conditioning of high-level phonological and morphological rules--with rules which involve segments in adjacent words A and B provided A and B stand in some particular geometric relationship to one another, perhaps within some particular domain. The formalism I use does not, the reader will notice, allow me to stipulate that A or B be of some particular category. This is because I believe that there are no such rules.

In this Part I'm going to take a close look at a phonological rule of English which, to my knowledge, has never received one before. This is the rule of final dental palatalization, henceforth FDP. My purpose is first to illustrate a rule of high-level phonology which is totally obtuse to the configurational properties of lexical structure. Several subsidiary points will be made in the course of this illustration. It strikes me as immensely significant that FDP, as well as not being configuration-sensitive, contrasts in every major aspect of its behavior with what we've come to expect of rules which are. This fact, which I will not fail to demonstrate, supports the causal relationship I proposed be-

tween configuration-sensitivity and these diagnostics--
all of which follow because the w-juncture is as it were the
medium of such rules. To the extent that a rule cannot be
influenced by both lexical and prosodic structure (the Italian
case (Part III) suggests that such an occurrence is not com-
pletely out of the question), FDP, not exhibiting w-juncture
effects, could not be sensitive to constituency relations a-
mong lexical units.

FDP does exhibit a number of oddities connected with
its syntactic conditioning. These at first glance make it
look like some combination of categorial and configurational
information must be available to the rule. Considered more
carefully, however, it seems inescapable that all of FDP's
apparent eccentricity is to be removed from the phonological
rule itself and blamed on pragmatic factors. The conclusion
is strong: There is actually no logical way for these factors
to directly influence the phonology. Rather, what they must
influence is the output of blind rules. FDP turns out to be
a case something like the sociologically "variable rules" of
Labov (1972), except that FDP, invariable, grinds out surface
forms with variable social sanction. Whether it is _necessary_
to think of the variability in Labov's examples as residing
in the rules is an interesting question. I am ignorant of the
relevant facts.

Let me begin by briefly discussing the phonological ef-
fects of FDP, first addressed in Selkirk (1972). FDP is a
casual speech rule. This means that palatalization occurs

only in some people's speech, and, even in theirs, only some-
times, as long as distinctness of utterance is not (conscious-
ly or otherwise) very highly valued. It's the familiar rule
which takes any word-final dental and palatalizes it, evident-
ly optionally, if yod immediately follows. To give some exam-
ples of the simplest type:

(1)(a) could you   [d] ~ [ǰ]

(b) can't you   [t] ~ [č̌]

(c) as you   [z] ~ [ž̌]

(d) unless you [s] ~ [š̌]

(e) can you   [n] ~ [ñ̌]

For the time being we may think of FDP as in (2).

(2) $\begin{bmatrix} +cor \\ +ant \end{bmatrix} \rightarrow \begin{bmatrix} +high \\ -ant \end{bmatrix} / \underline{\qquad} [y$

(2) is definitely wrong. The left bracket after the focus
bar, especially, has no theoretical status whatsoever. I
want the reader to understand thereby only that the yod is
supposed (by all investigators to date) to begin the next
word. This is not, in point of fact, true, and the matter
gets more attention below.

The yod itself generally lenites, often to the point of
disappearing altogether, after triggering FDP. This occur-
rence varies somewhat from speaker to speaker, all other
things, in particular the applicability of FDP, being equal.
The weakening of the yod is an independent event, brought
about by an independent rule. Observe, for instance, that
it occurs even if the palatal preceding the weakened yod

was never a dental.

$$(3) \quad \text{Don't let them} \begin{Bmatrix} \text{(a) misjudge} \\ \text{(b) touch} \\ \text{(c) camouflage} \\ \text{(d) push} \end{Bmatrix} \text{you.}$$

$$\begin{Bmatrix} [\check{j}] \\ [\check{c}] \\ [\check{z}] \\ [\check{s}] \end{Bmatrix} \quad \begin{Bmatrix} [^{y}uw] \\ [uw] \\ [^{y}\partial] \\ ?[\partial] \end{Bmatrix}$$

Consequently, in my examination of palatalization I will carefully distinguish this process, henceforth treated and referred to as y-deletion, from FDP. Also, starting with the last example, I will neglect to consider palatalization of [n]. My reason is that it is not easy to hear when it happens—evidently because it results in a segment still belonging to the same phoneme, something not true of the other four satisfyingly blatant changes. Clear-cut data will become essential later. In general, I will end up ignoring a great many equivocal facts associated with this basically fuzzy phenomenon.

There is a related rule, final dental assimilation, which for similar reasons I will mainly fail to consider. FDA assimilates a word-final dental in point of articulation to a word-initial palatal consonant.

$$(4) \quad \begin{bmatrix} +\text{cor} \\ +\text{ant} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{high} \\ -\text{ant} \end{bmatrix} / \underline{\quad} \begin{bmatrix} +\text{cons} \\ +\text{high} \\ -\text{ant} \end{bmatrix}$$

E.g.:

> (5)(a) a bad   [ǰǰ]
>
>   the worst   [čǰ]
>
>   whose   } joke   ?[žǰ]
>
>   an even worse   ?[šǰ]
>
> (b). a bad   [ǰč]
>
>   the worst   [čč]
>
>   whose   } choice   ?[žč]
>
>   an even worse   ?[šč]
>
> (c) bad   [ǰš]
>
>   the worst   [čš]
>
>   whose   } shape   [žš]
>
>   even worse   [šš]

(There are, of course, no word-initial [ž].) Already these facts are moderately murky. It is not so easy to distinguish dental from palatal underneath the following consonant, still less to decide which is grammatical. Even though one hopes that FDP and FDA are in fact special cases of a single rule, obtained by omitting the specification [+cons] in (4), I will mainly forget about the portion of its effects which is not really distinctly audible. The reader will soon come to appreciate this decision.

To return to (2), the next thing that should be observed is that stress, either before or after the consonant to be palatalized, does not prevent application of the rule.

> (6)(a) You couldn't, coul[ǰ] you?
>
>   You can, can'[č] you?

Did that surprí[ž] you?

I see that doesn't impré[š] you.

(b)   coul[ǰ] yóu guys

nobody bu[č] yóu

as smart a[ž] yóu

unle[š] yóu do

Though Cooper, Egido, and Paccia (1977) demonstrate that FDP
is somewhat statistically less likely in examples like (7)
than in examples like (8),

(7)   John'll lend yóu his orgone accumulator.

(8)   John'll lend yo̊u his orgone accumulator.

I want to drive home the fact, almost too obvious to mention,
that intended emphasis goes hand in hand with clarity of enun-
ciation, and that some slight hiatus tends to precede a con-
trastively or emphatically stressed initial syllable. (Re-
call the Hebrew example, in this connection.) Both of these
will fight FDP. FDP obviously fails if obscurity of the pal-
atalized dental and subsequently lost yod conflicts with the
high information load of the emphasized word. Indeed, the
essence of FDP's status as a "casual speech rule" is that its
output is judged to be an obscuration--thus tolerated, for the
most part, only in moments of high redundancy. Moments of
high redundancy, in turn, are characteristic of informal so-
cial give-and-take. As for pause,

(9)   *No[č]...you!

FDP's two terms must evidently be on the same side of it.
My point is that the stress effect, though real, is not the

result of some stipulation in (2).

Now why should it be true that FDP, in contrast to the other rules I've discussed, fails across pause? The answer, I believe, is that FDP has direct syllabic conditioning. Kahn (1976), addressing himself to the problem of syllabic linkages established across word edge, proposes (10) below (his Rule V).

(10) In C V , associate C and σ.
         |
         σ-

This C, unassociated by Kahn's other rules of syllabification (see Part I above), must be a word-final one. Kahn notes that (10) applies only in "connected," i.e. pauseless, speech. It is precisely in pauseless speech that FDP applies, followed by y-deletion. Put another way, wherever FDP or y-deletion has applied, (10) has also:
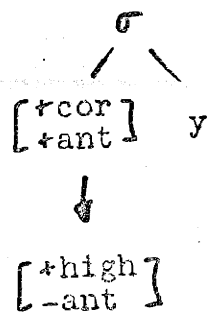
(11)(a)   I'll ge[č] (*...) [yuw] later.   (FDP)

(b)   I'll ge[č] (*...) [uw] later.   (FDP > y-deletion)

(c)   I'll cat[č](*...) [uw] later   (y-deletion)

I infer that both rules are in fact conditioned by the tauto-syllabicity of the final consonant and the following vowel. Failure of either one across any sort of pause is a consequence. I am assuming, of course, that "V" in Kahn's formulation includes a possible [y] on-glide. It plainly must: Compare the f's of few, nephew, if you.
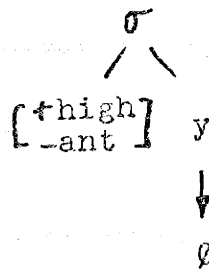
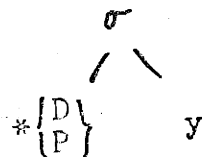The new version of FDP is as in (12) on the next page.

(12)

$$\sigma$$

$$\left[\begin{array}{c} \text{+cor} \\ \text{+ant} \end{array}\right] \quad y$$

$$\left[\begin{array}{c} \text{+high} \\ \text{-ant} \end{array}\right]$$

Y-deletion we can write as (13).

(13)

$$\sigma$$

$$\left[\begin{array}{c} \text{+high} \\ \text{-ant} \end{array}\right] \quad y$$

$$\emptyset$$

These two rules, which we can make obligatory, but conditioned by the optional (10), conspire to rid the language of all final dentals and palatals

(14)

$$\sigma$$

$$*\left\{\begin{array}{c} D \\ P \end{array}\right\} \quad y$$

before tautosyllabic yod.

The next point to establish is that the word whose final dental suffers FDP may belong to any category.

(15) N   the nigh[č] you left

A   how ridiculou[š] you are

V   I misle[ǰ] you

ADV but instea[ǰ] you

AUX coul[ǰ] you

P   besi[ǰ] you

CONJ an[ǰ] you

COMP tha[č] you

PRO tho[ž] you respect

It may stand in any syntactic relation to the following [y]-initial word.

(16)(a)   John like[č] you.

(b)   John like[č] you guys.

(c)   John like[č] you and your dentist.

(17)(a)   John really like[č] you.

(b)   John liked and respecte[ǰ] you.

(c)   John really liked and respecte[ǰ] you.

(18)   John really liked and respecte[ǰ] you and your

dentist.

(19)(a)   the god[ž] you worship

(b)   the minor pagan god[ž] you worship

(c)   the minor pagan gods and goddesse[ž] you worship

(20)   the minor pagan gods and goddesse[ž] you and

your dentist worship

(21)(a)   John said tha[č] you'd be here.

(b)   John sai[ǰ] you'd be here.

(22)(a)   ?Whatever he sai[ǰ] you laughed uproariously at.

(b)   ?After that outburst[č] you sure looked shocked.

(c)   ?He's so sozzle[ǰ] you'd better take him home.

In particular, the complexity of the constituents containing either of FDP's two terms varies independently of FDP's applicability, as does their syntactic distance, measured (say) in number of intervening brackets. Examples like (18) effec-

tively dampen the notion, tentatively conceived in Selkirk (1972), that more than one word boundary blocks FDP:

(18)(a)' John [[#liked#][you]]

(b)' John [[#liked#][#you guys#]]

(c)' John [[#liked#][#you and your dentist#]]

FDP once again becomes slightly questionable (22) precisely where the absence of a pause drops in acceptability.

Let us now ask ourselves what sorts of [y]-initial words may follow the palatalized consonant, as it turns out a more interesting question. First, look at (23).

(23)(a) all excep[č] you

(b) all excep[č] yourself

(c) all excep[č] your brother

(d) ?all excep[č] yours

(23)(d) is mysteriously inferior for some speakers. I have nothing very strong to say about this at the moment. Generally, FDP before a pronoun seems moderately successful. As for the other categories, most of them do not fare nearly as well. I find that my speech allows the following:

(24) PRO   Don't ki[ǰ] yourself.

Jus[č] you wait.

Who'[ž] your shrink?

What'[š] your number?

COMP (no examples)

CONJ (no examples)

P   (no examples)

AUX   (no examples)

ADV   I just got can[ǰ] yesterday.

Aren't you finish[č] yet?

I did tho[ž] yesterday.

That'[s] yet another problem.

V   I'[ǰ] use another example.

?Jus[č] use your head.

?*John'[ž] yodeling upstairs.

?*Thi[s] yields two dozen, it says.

A   *Today's my dreade[ǰ] yearly checkup.

*I'm not tha[č] young anymore.

*These are from John'[ž] yellow period.

*Thi[s] urinary infection's getting me down.

N   *He's my thir[ǰ] urologist in as many months.

*It's jus[č] yogurt, it won't kill you.

*John'[ž] Yiddish is really appalling.

*How much is thi[s] ukelele?

There are speakers who dislike V, as much as A or N, follow-
ing an FDP-derived consonant, or accept any category but N.
(There are no complementizers, conjunctions, prepositions, or
auxiliaries beginning in [y].) The first generalization we
might make is that the more categorial features a word shares
with nouns, the more difficult it becomes to palatalize a
preceding dental. It is clearly the category of the [y]-ini-
tial word that matters, and not that of any phrase it begins.
(18)(a-c) are identical with respect to FDP.

A first description of the facts is the nouniness squish
in (26).

(26)   In D] $[_\alpha$ y, D a dental to be palatalized,

$\alpha = \{$ N < A < V < everything else $\}$

(" < " means "less grammatically than." Note that we cannot
say simply that α must be a non-lexical category: Verbs are
often permissible. It seems to me that contrasts like (27)
(a-d), for instance, are quite real.

(27)(a) My brother[ž] use Breem regularly.

(b) I noticed my brother[ž] using Breem.

(c) *I noticed my brother[ž]' used Breem.

(d) *My brother[ž]' use of Breem is getting compulsive.

(26) must be wrong. I'd like to nip in the bud the idea
that it or anything like it is directly conditioning FDP. I
think on the contrary that word frequency, not category mem-
bership, is the true conditioning factor: The more frequent-
ly occurring a word is--the more characteristic of normal con-
versation--the better it sounds following a palatal from FDP.
This effect should follow in an obvious way from FDP's status
as a casual speech rule, though it is still curious that the
frequency of the word actually affected by FDP is irrelevant.

Frequency (currency in everyday conversation) gives
roughly the same ranking of categories as (26). The roughness,
as we'll see, is crucial. It is of course true that in normal
speech one finds a small number of frequently-occurring func-
tion words, a larger number of lexical predicates, and quite
a considerable number of relatively rarely used lexical argu-
ments to put in them. I find speaking of word frequency more
attractive and likely to be accurate than speaking of nouni-

ness for several reasons. The first is that there are a
good many perturbations in the gross ranking in (26), most of
them traceable to clear frequency effects. On the nouniness
account, though, they are exceptional. For instance, FDP
before verbs, not marvelous to begin with, is definitely en-
feebled as the verb becomes exotic.

(28)(a)   I'[ǰ] use

(b)   ?I'[ǰ] utilize

(c) ?*I'[ǰ] euphuize

Yon and yonder, infrequent to the point of nonoccurrence in
most people's speech, sound very bad after FDP, though they
presumably belong to non-lexical categories.

(29) *hither an[ǰ] yon

(30) *that For[ǰ] yonder

Universal, usually alone among adjectives, allows FDP to a
certain extent in the speech of many linguists, among whom it
has exceptional currency.

(31)(a) tho[z̆] universal rules

Compare, on the one hand,

(b) *that rule'[z̆] universality

and

(c) *tho[z̆] unicellular organisms

on the other, neither one of which is at all good. One pre-
sumes that (c) would be in, and (a) out, for a microbiologist,
for instance.

It is also interesting that adverbs, when de-adjectival
or de-nominal, are no better in the eyes of FDP than the ad-

jectives or nouns inside: <u>youthfully</u>, <u>usefully</u>, <u>yellowly</u>,
<u>usually</u>. (Compare the underived <u>yet</u>, <u>yesterday</u>.) This lack
of improvement seems to show that category is not a factor.
Similarly the verb <u>yo-yo</u>, de-nominal, is treated by FDP just
like the noun, as (26) leads us not to expect.

    (32)(a) *tho[ž] yo-yos

       (b) *tho[ž] yo-yo like crazy if you're not careful

The one noun which does not fairly unequivocally block
FDP is <u>year</u>: <u>thi[š] year</u>, <u>las[č] year</u>, <u>nex[č] year</u>, <u>firs[č]-</u>
<u>year</u>, <u>secon[ǰ]-year</u>, <u>thir[ǰ]-year</u>. Not coincidentally, <u>year</u>
leads by far all other [y]-initial nouns in frequency of oc-
currence. Alone among them, in fact, it regularly appears in
compounds, at least some of which are surely lexical, like the
preceding six. <u>Year</u> also permits FDP, though slightly less
naturally, when not part of a compound.

    (33)(a) my firs[č] year here

       (b) my firs[č]-year students

(33)(a) seems marginally worse than (b), though not bad at
all. FDP is quite productive before <u>year</u>:

    (34)(a) twenty-eigh[č] years ago

       (b) those waste[ǰ] years

       (c) the wors[č] year by far

It cannot be that anything about the phonological makeup
of <u>year</u> is distinguishing it: Compare <u>yeast</u> and <u>yield</u>, for
example. <u>Thi[š] yeast</u>, <u>thi[š] yield</u> seem impossible to my
ear.

I ought to establish, more generally, that the phonologi-

cal properties of the word of category α are irrelevant to the applicability of FDP. Let us first consider the initial vowel nuclei found in words successfully following an FDP-derived initial.

(35)   úw, uw

      $\acute{\varepsilon}, \varepsilon$ ə   ów

              $\wedge$

Examples:

(35)'   yóu, you

      yesterday, yet; yourself; yours

            ya

Conversely, the initial nuclei of the unsuccessful words:

(36) ${}'\acute{\imath}$ íy   úw, uw

     $\acute{\iota}, \varepsilon$ éy   ə   ó   ów

             $\acute{\wedge}, \wedge$

       $\acute{æ}$ á

As:

(36)'   Yiddish, yeast;   euphony, euphonic

      yellow, Yehudi;   Yale; Uranus; yod; yo-yo

         yucca, Yahoo

    yam   yacht

Every initial nucleus (35) offered by the poor collection of [y]-initial words in (35)' is also found among the ones in (36). Notice also that number of syllables is irrelevant, as is stress: Recall (7), on the one hand, and compare *bu[č] euphony, *qui[č] euphonic on the other.

     The theory, then, says so far that FDP becomes more dif-

ficult as the following [y]-initial word drops in frequency.
This kind of thing is, of course, the usual story with casual
speech rules. Observe, for instance, the possibility, pro-
gressively smaller top to bottom, of reducing the final [ow]
in the following words,

(37)   tomato

       potato

       borrow

       wheelbarrow

       shadow

       sorrow

       kimono

       Plato

or, a slightly weaker effect, of reducing to, have, after
various verbs.

(38)   seem to

       try to

       attempt to

       strive to

       endeavor to

(39)   could have

       might have

       can have

       will have

       shall have

In all four cases the possibility of performing the rule
varies directly as frequency of locution. (This type of

phenomenon is extensively documented in Fidelholtz (1975).)
All of this is moderately understandable. There are two
reasons, as far as I can tell, for effects like these. First,
the more removed from the vernacular a word is, the more
fastidiously it will tend to be pronounced, since its infor-
mation load is correspondingly greater. Second, a style of
speech going in for arcane vocabulary is not generally a
style to tend to allow reduction by casual speech rules in
the first place; these two styles will obviously tend to ex-
clude one another. Reasons one and two are not the same:
Compare (37) with (38) and (39).

Conversely, frequent usage can drive a word into the
purview of a rule which otherwise excludes it. The classic
case (due to Fidelholtz) is the anomalously frequent reduc-
tion of _bassoon_ to [bəsún] in the speech of bassoonists. Non-
tense weakly-stressed vowels normally resist reduction in
closed syllables C_o__C_2 (SPE, p. 121): Compare _monstrosity_,
demonstrate; _sensation_, _insensate_. (To close the initial
syllable of _bassoon_ [bæsún] we probably have to supply an un-
derlying geminate _s_.) This is obviously very like the _uni-
versal_ example above.

Note now the following interesting contrast.

(40) Bu[č] you have three lemons.

(41) *Bu[č] "you" has three letters.

I invite the reader to control for stress and pause.

A quotation is a noun. This is evident enough from the
fact that it takes nominal determiners and complements.

(42) $[_{\bar{\bar{N}}} [_{DET}$That$] [_{\bar{N}} [_N$"you"$] [_{PP}$in the last line$]]]$

is just about illegible.

Note also that "you", in addition to taking modifiers, governs agreement like a singular noun, not like a second-person pronoun, and will of course continue to do so regardless of the contents of the inverted commas.

(43)(a) *But "you" have three letters.

(b) *That "you" are illegible.

A quotation can obviously be anything at all--ungrammatical English, grammatical strings in other languages, ungrammatical strings in other languages, noises significant in no language--without affecting the grammaticality of the outer English sentence.

(44) "Q" is not English.

A grammar, as I mentioned in Part I, cannot be held responsible for specifying Q; it is an absolutely arbitrary vocalization. In (43) it happens to correspond to an English word, $[_{PRO}$you$]$. My point is that "you" stands to you as, say, "oui" stands to we: "you" is not structurally a pronoun. At the very least, there is no test according to which it does not behave only as a noun. If "you" were a pronoun, (45) would be bad.

(45) "You is" shows faulty agreement.

I mean to suggest that "you" is to be represented as $[_N$you$]$, not $[_N [_{PRO}$you$]]$.

Therefore we may say that a quotation is a word--i.e. something dominated by a terminal category--of frequency so

low that it is not even listed in the lexicon. Indeed, it can't be, since it's arbitrary. This explains why FDP fails before "you", just as before any other [y]-initial but linguistically insignificant noise.

I want to quickly destroy the idea that some sort of quotation effect insulates what's inside the quotes phonologically in cases like (41). Pause, first of all, is clearly not the culprit, since a pauseless rendering of (41) improves matters for FDP not one bit. Second, note that it is the frequency only of the [y]-initial word that affects FDP. Quotations, like all [y]-initial nouns but year, block it. The frequency of the dental-final word is irrelevant. One imagines, then, that a quotation should allow its final dental to palatalize. So it is:

(46) That's the last "plea[ž]" you'll have to say.

Note also:

(47)(a) Have you read Kidnapp[č] yet?

(b) Have you been kidnapp[č] yet?

(48)(a) I read "The Frog[ž]" yesterday.

(b) I dissected the frog[ž] yesterday.

By contrast:

(49)(a) *Bu[č] You Can't Go Home Again is fairly self-indulgent writing, to my mind.

(b) Bu[č] you can't go home again.

As I mentioned in Part I, there are dialects of Australian English which palatalize everywhere a dental and a yod pauselessly meet, totally regardless of any non-phono-

logical information, in particular about word frequency. In Australia, then, FDP is not a casual speech rule: tha[č] yo-yo, John know[ž] Yerkish, qui[č] yelling, etc. We leap to predict that, for speakers of these dialects, examples like (41) should go through with FDP, since the fact that a quotation is a low-frequency noun will be true but irrelevant. This is actually the case. (Thanks to David Nash.)

The theory that it is FDP that is affected by word frequency is, however, incorrect. It seems to me slightly bizarre to begin with that only the frequency of the [y]-initial word should be of relevance. Notice also that FDA (4) is not at all sensitive to word frequency, as (5) shows. Therefore we will not be successful in collapsing FDA and FDP, even though this result is of course desirable.

In what respect to FDA and FDP differ? Y-deletion applies obligatorily to the output of FDP, but not to the output of FDA. Investigating one step further, it turns out that y-deletion alone is bad exactly where FDP followed obligatorily by y-deletion is.

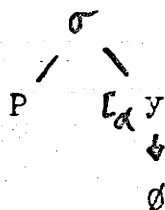(50)(a) *I taugh[čɪ]ddish nights.

(b) *I tea[čɪ]ddish nights.

(51)(a) *Tha[čow]deling is against the regulations.

(b) *Su[čow]deling is against the regulations.

Evidently y-deletion is the rule subject to our frequency effect. Nevertheless, it must be wrong to write y-deletion as (52),

(52)

$$\sigma$$

P [$_\alpha$ y
↓
∅

with some condition on the frequency of word α, because the result of not applying y-deletion is also bad when and only when FDP has already applied.

(53) *I taugh[čy]iddish nights.

(54) I tea[čy]iddish nights.

In (54), obviously, Kahn's Rule V ((10) above) has not applied, and therefore y-deletion has no chance to. But in (53), (10) must have applied, since FDP has. It appears that y-deletion must remain absolutely obligatory.

The responsibility for the phenomenon must, I think, be pinned on an output condition, sociolinguistic in nature: The less frequent a word--the less characteristic of ordinary conversation--the more stigmatized its derivation when it includes the loss of its initial segment. This simple idea explains every fact we've considered.

Y-deletion is virtually the only rule in English which deletes initial segments. A second example which comes to mind, h-deletion before unstressed vowels, is also constrained by word frequency. Compare:

(55) herself

historical

hysterical

heraldic

Hieronymus

hendiadys

The only other example I can think of in English is the some-
what childish clipping of initial unstressed non-tense vowels
which one sometimes hears: 'parently, 'merican. This too is
subject to a frequency effect. Clipping seems progressively
more difficult in the words in (56).

(56)   apartment

effectively

abusive

epenthesis

effulgence

agouti

(On this account, incidentally, we can see why an urolo-
gist (see Part I) is going to be bad for palatalizers. Since
the n of the article must be ambisyllabic (recall (25)), it
palatalizes and the y drops. But this is not allowed for
words like urologist. And unlike this urologist, an urolo-
gist cannot simply surface without the syllabic connection
having been established. The only option is not to have in-
serted the n in the first place. There is no English word
in [yuw-] which is both frequent enough to allow y-deletion
and possible after the article.)

Nevertheless, it is surely a mistake to think, as does
Allen (1962) that high information load at word onset uni-
versally militates against rule-governed violence to initial
segments. Allen's (pp. 17 ff.) underlying assumption, that

intelligibility of output cannot be reduced by rule, is
plainly unjustified: Any rule which takes A to B in some en-
vironment is giving the hearer an additional task in recovering
A. (Surely commonplace reductions like [čədún] 'what are
you doing' are not performed in the interests of intelligibil-
ity.) The prediction, too, is false, as attested, for in-
stance, by the Celtic case (above) and the Awįtim case (Hale
1976), in which initials are regularly and massively affec-
ted. No, the explanation for the weakness of word-initial
deletion in English lies in its relation to the rest of
standard English phonology--these are the only rules affec-
ting initials, and not one is at all well-entrenched. Hence
the opacity of derivations in which they apply, unless aided
by informational redundancy. Note too that none of the three
is restricted to word-initial application. We also have h-
deletion, elision, and palatalization medially: prohibition,
potato, textual.

This last assertion needs further commentary. We have
successfully removed all conditions but the syllabic one from
FDP. (12), taken at face value, leads us to expect palataliza-
tion word-internally. And in fact there is a well-known rule
of palatalization (SPE, p. 230) doing just this.

$$(57) \quad \begin{bmatrix} -son \\ +cor \end{bmatrix} \rightarrow \begin{bmatrix} -ant \\ +strid \end{bmatrix} / \underline{\quad} \begin{bmatrix} -cons \\ -voc \\ -back \end{bmatrix} \begin{bmatrix} -cons \\ -stress \end{bmatrix}$$

(57) is the rule responsible for node~nodule, fact~factual,
seize~seizure, sex~sexual. It is part of the word-level pho-
nology, because in SPE theory an optional word boundary must

appear in structural descriptions as (#). Apart from this,
(57) is virtually identical to FDP sans syllabic conditioning.
And the syllabic conditioning, I observe, is always met word-
internally. The only thing stopping us from saying that (57)
is just a special case of (12) is the stipulation in (57) to
the effect that the vowel following the yod must be unstressed.

However, this stipulation is wrong. It is based on exam-
ples like ensue, resume, perpetuity ([-súw, -zúwm, -túwɾtiy]),
which I really see no reason to give underlying /y/, fated to
delete, at all. What we need is a good example of an indepen-
dently justifiable [XD] clearly alternating with [XP] yv́.
Such examples are nearly nonexistent, but I offer, along with
grains of salt, credulity [krɛǰúwlɪtiy] (for many speakers),
the persuasiveness of which depends on its derivation from
/kred/ (cf. credibility, etc.); and individuity [ɪndəvɪǰúwɾtiy]
(divide).

Actually, examples of real words are slightly irrelevant
at this point. A small gedankenexperiment will tell us all we
need to know. Imagine a word zeft, derived from which there
is an adjective in -urious. The result is clearly
[zɛfčíyəs], not [zɛftɾíyəs]. Or imagine a word gand, also
appearing inside a noun in -ulity: [gǽnǰúwlɪtiy], not
[gǽndúwlɪtiy].

The point I am making is this. One repeatedly finds
high-level phonological rules which are merely less punchy
versions of independently necessary word-level ones. (See,
e.g., Harris (1968), Cheng (1973), Rudes (1976), Bolozky
(1977).) The wider rule does the same thing as the narrower

one, but applies with nothing like the same regularity, and only in connected speech. In every such case, I predict, there is in fact only one rule, an utterance-level one sensitive to prosodic structure. Since prosodic structure changes with faster speech, generalizing itself over progressively larger domains, so will the applicability of the rule. There may also be various types of social sanctions against its application: Hence "casual speech rules," which are often also fast speech rules.

Finally, I want to assemble several significant facts about FDP, now more accurately referred to as DP. DP is an utterance-level rule whose two terms often lie on either side of a w-juncture. However, unlike all the rules of Part I,

i) DP is blocked by pause.

ii) DP ignores phrase structure, as long as it is not reflected by pause. It is false, in particular, that DP is clause-bound, as suggested by Postal (1974). His examples, like (58) and (59),

(58) *I met Mary, who is sitting on the ma[č], yesterday.

(59) *Shaking the po[č] yields few benefits.

disallow DP (or y-deletion) for independent reasons---namely the presence of pause (58) and of a low-frequency verb (59) to the right of the palatalized consonant. Compare (60) and (61), which have no subordinate clauses.

(60) *I met Mary, your bo[š], yesterday.

(61) *Tha[č] yields few benefits.

On the other hand, note (62), (63), and (64), which do:

(62) I gue[š] you'll be glad to hear I got a job.

(63) This prov[ž] you're mistaken.

(64) They never got the letter[z] you sai[j] you sent.

iii) DP is not blocked by parenthesis, to the extent that pause does not accompany it.

(65) I coul[j] (,) y'see, but I won't.

(66) I had thi[s] (,) y'know (,) like really insane idea.

(67) Quie[č] (,) you guys, I'm trying to sleep.

Conversely, when the interpolated material ends in a dental:

(68) Quiet, you guy[ž] (,) you're driving me bananas.

(69) "Come on," he say[ž] (,) "you're putting me on."

(70) This is, I gue[s] (,) your last chance.

iv) DP is not blocked by trace, again to the extent that pause does not accompany it.

(71) I didn't like the apartment I looked a[č] t yester-
        day.

(72) I can't understand what it i[ž] t you're saying.

(73) I said I woul[j] t (,) your honor.

(74) John says he's your friend more than he i[ž] t
        your lover.

(As in Part I, I do not necessarily assume that all of these are derived by movement. A "trace," in my usage here, is the pre-terminal structural residue of any extraction. In each case, we know it is there because the non-lexical item at, is, would, is preceding the gap is not allowed to destress or reduce. The usual distinction between movement (leaves a trace) and deletion (evidently no trace) useful in syntax is actually, I assume (following Chomsky 1975), the distinction

between the presence of a semantic variable and its ab-
sence. For a justification of an underlying quantifier re-
moved from site t in sentences like (74), see Bresnan (1975).)
Note also the lack of contrast between (75), for those who
can say it at all, and (76) or (77).

(75) Who'd you sen[ǰ] t your resumé?

(76) Who'd you sen[ǰ] your resumé to t?

(77) Where'd you sen[ǰ] your resumé t?

In a study of DP across the site of verb gapping, Cooper,
Egido, and Paccia (1977) conclude from contrasts like

(78)(a) The bus driver will take your brother and then
guide your sister.

   (b) The bus driver will take your brother and the
guide your sister.

(79)(a) The porter took your bags and weighed your
luggage.

   (b) The porter took your bags and Wade your luggage.

(their (9) and (6)) that an extraction site tends to block the
rule. Note the natural pause in the (b) examples. The easi-
ly conceived idea that some more abstract state of affairs--
like the presence of a trace--is responsible both for the
pause and for blockage of DP is quite hard to seriously main-
tain. First, an extraction site is not always accompanied by
a natural pause: (71)-(77). Second, DP is not inhibited pre-
cisely here. Third, DP is inhibited by all sorts of pauses,
pauses which surely are not due to trace.

(80)(a) They've weighed...your luggage.

(b) It's all weighed. Your luggage is over there.

(c) Wade, your luggage is over there.

(d) After it was weighed, your luggage blew up.

Hesitations, ends of sentences, vocatives, and preposed ad-
verbials naturally require pauses, and also make DP difficult.
Since it is so clearly true that pause blocks DP, due to the
failure of (10), it seems counterproductive to say that ex-
traction sites also block DP (or even _y_-deletion), particular-
ly when this theory is borne out only to the extent that pause
is required as well.

Cooper _et al._ also point out that an actual _silence_ of
any length is not measurable in many instances of blocked DP.
However, _silence_ is not to be identified with _pause_. A pause
is a thing of some abstraction, often surfacing as a lengthen-
ing of the pre-pausal syllable. Note:

(81) *John telephone.

This is, of course, bad without a pause after the vocative.

(82) John, telephone.

But a _silence_ of significant duration is not necessary. In
rendering the comma of (82), it is possible to utter either
(83)(a) or (83)(b).

(83)(a) John...telephone.

(b) Jo-ohn telephone.

The hiatus in phonation after _Jo-ohn_ need not be large, or,
it seems to me, even present at all. Note that lengthening in
(78)(b) and (79)(b) seems virtually obligatory before the gap.
According to Carmen Egido, of Cooper _et al._ (personal communi-

cation), their data in fact consistently show it.

v)   DP is not even blocked between adjacent sentences in
the same utterance.

(84)   Try i[č] (,) you'll live.

I assume that (84) is structurally no different from (85),

(85)   Try it. You'll live.

in which the two sentences separated by the period are pre-
sumably not a syntactic constituent. The comma in (84) is
merely an orthographic indication of their _prosodic_ intimacy,
which is exactly what DP requires.

In every single one of these properties DP distinguishes
itself from the rules I've been worrying about in the rest of
this work. There is no a priori reason why the absence of
syntactic conditioning (ii) should entail (iii)-(v), or why
the presence of syntactic conditioning should entail the con-
tradictory propositions in case after case. I stress one more
time that, according to my theory,

(86) (a)   α   (b)   α   (c)   α   (d)   α β

A   B   A   B   A   B   A   B
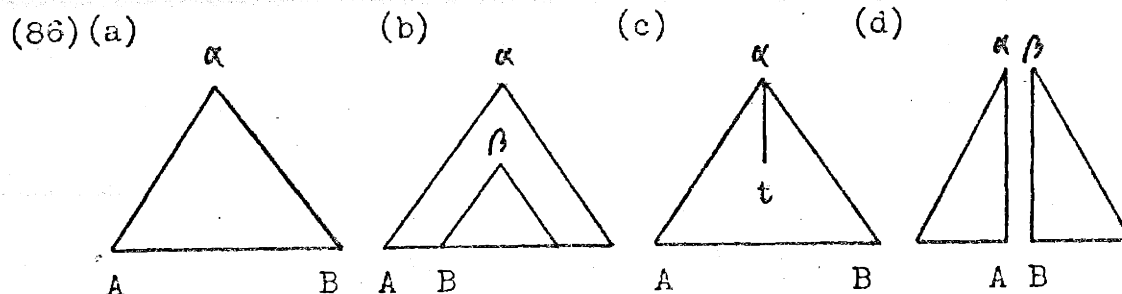
syntactic conditioning of a rule involving words A and B is of
necessity dependent on their being separated by w-juncture,
e.g. the one bounded by αAB in (86)(a). But parenthesis (b),
trace (c), and full stop (d) all have structural reflexes
ruling out the possibility of any w-juncture inside a curve
including A and B. On the other hand, pause (i), a prosodic

phenomenon, does not affect the normality of junctures, though it interferes with prosodic rules. DP turns out to be a prosodic rule, hence is predictably blocked by pause. To the extent that prosodic rules are not also affected by non-prosodic configurational conditioning (ii) (but recall the Italian example), (i) and (iii)-(v) will tend to exclude each other. One also imagines that there is no third case, i.e. that high-level phonology is sensitive either to prosodic structure or to non-prosodic, lexical structure. Hence a demonstration that a rule is not tied in to lexical structure should make us look for its prosodic conditioning, and vice versa. Note that even rules like the a/an rule, evidently without abstract limitations in the manner of Part III, are always conditioned by lexical structure in the sense that they are unproductive to an extreme degree. It ought to be impossible to lay such restrictions on a prosodic rule like DP.

I should mention that prosodic rules do not always have the superficial, phonetic air that DP has. An otherwise similar example is Finnish gemination, which appears to be conditioned by abstract segments.

(87)   Mene [p:]ois!

       go   away

(88)   Pane [p:]uita uuniin.

       put   wood   in-the-stove

The words which cause this gemination belong to a list which one simply has to learn. Unlike the Irish case, a morphologi-

cal treatment is unattractive, since special syllable-final
phonology also cooccurs with the ability to cause gemination.
Nevertheless, the rule of gemination is defensibly conditioned
by the analogue of Kahn's Rule V. It applies only in connec-
ted speech.

(89) *Pane...[p:]uita uuniin.

Now note (90) and (91):

(90) Pane [k:]uule [p:]uita uuniin.

listen

'Put, hey, wood in the stove.'

(91) Mitä pitäisi   panne t [p:]äällimmäiseksi?

what should-one put   on-top

(Thanks to Paul Kiparsky.)

A word on the "psychological reality" of phonological
rules. Looking naively at the vowel shift, for example, it is
not the least bit clear that the set of examples constituting
this phenomenon exhibit more than merely the sorts of fortu-
itous correspondences one would expect to find in any large
body of inherited vocabulary. It is not obviously true that
an authentic phonological rule synchronically accounts for
them. The vowel shift has many exceptions, is not productive,
is not supported by intuitions, etc. It is still possible to
have an opinion about the nature of the vowel shift, ranging
from the orthodox phonologism of SPE through various sorts of
lexical correspondence theories (Vennemann 1972, Leben and
Robinson 1977) to the extreme position that inane~inanity has
about the same status as Cambridge~Cantabrigian. Now a

trans-word-level phonological rule is a phonological rule
par excellence, because its productivity makes it difficult
or impossible to think of as a lexical correspondence. DP,
for example, is responsible for a completely productive alter-
nation [XD]~[XP]. It is powerfully absurd to think that both
of these are listed in the lexicon. Still needed is a means
of specifying where [XP] actually occurs. If this means is
a rule, the rule is DP, which makes /XP/'s lexical presence
superfluous. But unlike the [ɪnæn]ity case, here it is not
a serious alternative to list every [XP][yY]. Put another
way, while one can think of inane~inanity as either (92)(a)
or (92)(b), (93)(b) is simply ridiculous.

(92)(a) /ɪnæn/ : /ɪnæn/ity, plus VS

   (b) /ɪneyn/ : /ɪnænɪtiy/

(93)(a) /bʌt/ : /bʌt/ you, plus DP

   (b) /bʌt/ : /bʌčuw/

The position on the vowel shift that [ɪnænɪtiy] is a monolithic
word, without a constituent underlying inane, doesn't even
have a counterpart on DP. We simply cannot get away without
analyzing but in [bʌčuw].

Now when we write the rule, which apparently we must, ac-
counting for this palatalization of final dentals, we find
that already it accounts for word-internal seize~seizure, etc.
It's not so clear, just looking at it, that seize~seizure is
a "real" alternation. But the fact that DP is general enough
to account for it makes any lexical correspondence redundant.
Hence DP constitutes a kind of evidence for word-internal

phonology.

An especially serious theory of lexical phonology is pro-
posed in Leben and Robinson (1977). In this highly interes-
ting and attractive system, surface forms are listed as is in
the lexicon. /ɪnænɪtiy/, for example, is then related to
/ɪneyn/ by interpretive phonological rules, essentially the
inverses of the standard ones. Attempting to match /ɪneyn/
with the stem /ɪnæn-/, we apply the inverse of the vowel
shift, getting [ɪnæyn]. (The complete derivation will also
include the inverses of diphthongization and laxing.) This
theory correctly predicts that the vowel shift will not be
productive, because /ɪnænɪtiy/ has to already be in the lexi-
con for any rule to apply to it. Now imagine an analogous
treatment of DP.

    (94)   /ɪneyn/ : /ɪnænɪtiy/, plus $\overline{VS}$

    (95)   /bʌt/ : /bʌčuw/, plus $\overline{DP}$

<u>Bu[č] you</u>, <u>bu[č] your</u>, <u>bu[č] yet</u>, <u>bu[č] yesterday</u>, <u>coul[ǰ] you</u>,
and every grammatical example of DP must be listed and submit-
ted to rules first eliminating the word to the right of the
palatalized consonant and then, via the inverse of DP, re-
covering <u>but</u>, <u>could</u>, etc., also listed. Absurd as this is,
it also predicts that productive palatalization will not oc-
cur, which is not true. Unlike the vowel shift, DP quite
naturally applies to borrowed, nonce-, and spontaneously
made up words.

    (96)(a) the fe[ž] you're wearing

        (b) the skateboar[ǰ] you're riding

(c)   the gan[ʲ] you ate

Therefore DP is in the active, rightside-up phonology. But
then, by the same transitive reasoning above, so is DP when
it applies word-internally.

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