LITMUS TESTS, THE BLOOMFIELDIAN COUNTERREVOLUTION,

AND THE CORRESPONDENCE FALLACY*

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1. Litmus tests.

1.1. Introductory remarks. The literature of generative grammar
contains many implicit references to, and explicit discussions of,
tests for various properties of syntactic elements in particular
languages. Most of these tests involve the manipulation of
linguistic examples--substituting, permuting, adding, removing, or
combining elements and noting the grammaticality, paraphrase relation-
ships, appropriateness in context, or some other linguistically
relevant property of the result. Tests for idiomaticity can serve
as typical examples of the sort of thing I am talking about.

Since an idiom is a construct, in some language, whose
meaning is not predictable, on general principles of that language,
from the meanings of its parts, the meaning of an idiom must be
associated with this construct as a whole. It follows that an
idiom should show considerable internal integrity: we would expect
it to be made up of certain parts (not admitting alternatives);
just these parts (not fewer or more); these in specific orders
(without variants, facultative or meaningful, or interrupting

material); and with the whole relating as a unit (not part by part) to other constructs with which it combines. In very clear examples of idiomatic constructions, all of these expectations are satisfied. So, for the idiom take a powder 'leave': substituting alternative words for those in the idiom eliminates the special meaning of the idiom--

      (1) grasp a powder, take the powder, take a dust

adding or subtracting parts also eliminates the special sense--

      (2) take powder, take a large powder

alternative orders, too, eliminate the special sense--

      (3) A powder was taken.
      (4) A powder we are taking.
      (5) A powder is easy to take.

so does interrupting material--

      (6) Quentin took, as we all watched in amazement, a powder.
      (7) Robin will take, we must surely suppose, a powder.

and relating other elements to parts of the idioms will eliminate the special sense as well--

      (8) Shirley took a powder before Tanya took one.
      (9) Uriah took a powder, and then Victor did the same to a tablet.

Then, since so many constructs that are clearly idioms show all of these properties, we can turn matters around and use the properties as tests for idiomaticity: we examine a particular construct by seeing how fixed or 'frozen' it is--whether its parts are invariable, or whether they show the freedom of open syntactic constructions; whether transformations can move some part of the

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construct away from the rest, or move other material into the construct; and whether the construct is an 'anaphoric island' (Postal 1969), in the sense that parts of the construct may not serve as antecedents for anaphoric elements, nor may parts of the construct be replaced by anaphoric elements. These tests for idiomaticity, and some others, are illustrated in Sadock 1972; the interruption test is treated by James 1972:164.

There are also, of course, all sorts of psycholinguistic tests, which use systematic observation or experiment to generate data of various kinds (recall of material, response times, accuracy of judgments, and the like) which can be treated statistically and from which inferences about linguistic structure can be made. My interest here, however, is in the more purely linguistic tests, like those for idiomaticity--tests for which I will use the term litmuses or litmus tests.[1] Perhaps I should say that the tests used in linguistic analysis are in some ways more like medical diagnostic tests or like tests taken by students in a course than they are like chemical litmus tests; the information they give is often less than we would like, or contradictory, or uninterpretable. For tests of idiomaticity, some indication of the size and scope of these difficulties can be gotten from a reading of Fraser 1970a. I consider the general problem in section 3.2 below. In the meantime I continue to use the attractive chemical metaphor, without meaning to suggest that linguistic litmus tests are guaranteed to decide any question that is put to them.

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1.2. Some examples. Linguistic litmus tests have been used by generative grammarians in several different areas. In the following subsections I survey some of these areas, with examples selected from the literature.

1.2.1. Tests for essentially semantic properties. Here I refer to such properties as idiomaticity and ambiguity, which are defined semantically but have significant syntactic reflexes. Several litmuses for idiomaticity have already been mentioned in section 1.1 above. Tests for ambiguity are described at some length in Zwicky and Sadock 1975, with bibliography; I will mention just one type here, that of transformational potential.

The idea behind this type of test is that the two interpretations of an ambiguous sentence may correspond to structures that are differently affected by the application of some transformation, while the single interpretation of an unambiguous sentence necessarily remains through transformational operations. In loose but memorable terms, it is possible for some range of meaning of an ambiguous sentence to 'disappear' when a transformation applies, but the full range of meaning of an unambiguous sentence is untouched. It follows that a disappearance indicates ambiguity, rather than vagueness, generality, or lack of specification with respect to some aspect of meaning. Suppose, for instance, that we are not convinced on other grounds that

(10) They noticed her duck.

is ambiguous, and we wonder if (10) might not merely happen to cover

a broad range of meanings including 'fowl' and 'ducking down'
understandings, just as.

      (11) Paula laughed at the kangaroo.

covers a broad range of meanings including 'recent past' and 'remote

past' understandings. If our suspicion is correct, then the two

understandings of (10) should maintain themselves through all

transformational operations, just as the two understandings of (11)

do. But no. Several transformations, including passivization,

clefting, pseudoclefting, and topicalization, eliminate the 'ducking

down' understanding of (10):

        (12) Her duck was noticed by them.
        (13) It was her duck that they noticed.
        (14) What they noticed was her duck.
        (15) Her duck they noticed.

Compare (12)-(15) with the results of the corresponding operations

on (11):

        (16) The kangaroo was laughed at by Paula.
        (17) It was the kangaroo that Paula laughed at.
        (18) What Paula laughed at was the kangaroo.
        (19) The kangaroo Paula laughed at.

The sentences (16)-(19) all can be understood as referring to the

recent past or the remote past. We conclude from these transforma-

tional potential tests that (10) is indeed ambiguous, but that we

have no evidence from these tests that (11) is.

    1.2.2. <u>Tests to determine boundaries between units</u>. Among

the boundaries to be determined are those between words and those

between constituents.

To determine whether given sequences of morphemes constitute words, linguists have referred to various properties of morphemes within words: the morphemes have a fixed ordering, alternative orders normally being associated with differences in cognitive meaning; they show a high degree of mutual selection, much higher than that between separate words; words as wholes are immune to interruption by parenthetical expressions; words as wholes are anaphoric islands; syntactic rules of movement and deletion do not apply to proper parts of words; some phonological processes (those of 'internal sandhi') apply only within words and not between words, while others (those of 'external sandhi') apply only between words; morphemes with no independent accent are affixes rather than separate words; and bound morphemes, as well as morphemes in construction with bound morphemes, are affixes rather than separate words. A number of these tests are discussed, in connection with some particularly troublesome cases, in Zwicky 1977: secs. 1-3. For a very simple example, consider how we might show that danced in

> (20) They danced.

is a word. We can refer to the fact that the past tense morpheme -ed is a bound morpheme and has no independent accent, hence is an affix; to the fact that the phonological rule of progressive voicing assimilation that applies to this morpheme is a rule of internal sandhi; to the fact that parenthetical interruption is out of the question--

> (21) *They dance, as I supposed you know, -ed.

and to the fact that -ed is immune to the deletion rule that forms
reduced coordinations--

(22)   They danced and played =✗=>   They dance and played.

To determine whether given sequences of words form constituents,
linguists have referred to another set of tests. Most often they
have depended upon the hypotheses that the only sequences of
elements that can be moved (in some cases, deleted) by transformations
are those that form single constituents, and that some deletion rules
are constrained to leave constituents. Both lines of evidence are
used by Fraser 1965: sec. 2.3 to distinguish the constituency of
the two constructions

(23)   a. V Prt NP: [V Prt][NP]   (look up the information)

       b. V Prep NP: [V][Prep NP] (talk about the situation)
Fraser observes that only in (23b) does the postverbal construction
prepose in questions:

       (24)   a. *Up what did he look?
              b. About what did they talk?
and that only in (23b) can the postverbal construction be treated
as a unit for the purposes of reduced coordinations:

       (25)   a. *He looked up the information and over the files.
              b. They talked about the situation and on the
                  issues of the day.
In the first case, what is moved must be a constituent. In the
second, what remains as a reduced conjunct must be a constituent.

A related test (Levin 1976: sec. III) uses the fact that
fragment answers to information questions must be syntactic

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constituents. So, the question-answer sequence

> (26) Q: What did you throw out?
> A: The French toast.

is fine, but

> (27) Q: What happened to the French toast?
> A: I threw out.

is bad, because the answer consists of an NP and a V that do not
together form a constituent.

Still other tests for constituency are distributional in
nature, using generalizations about syntactic structure--using, for
instance, the hypothesis that the only categories involved in the
strict subcategorization of some element are those in coconstituency
with that element (Chomsky 1965: sec. 2.3.4 of ch. 2); or using the
observation that most constituent types occur in positions that can
be filled by single words from some syntactic category (an observation
made much of by Harris 1946).

Next, there are tests for the closeness of construction between
constituents, using various hypotheses about the consequences of
relative closeness--that relatively close constituents cannot be
separated by intonational marks, by substantial pauses, by parenthe-
tical material of various sorts, or even by modifying phrases, so that
positions where such separation is possible are locations of major
constituent breaks. Fraser 1965: sec. 2.3 uses two tests of this sort
to distinguish the constituency of the two constructions in (23)
above: he observes that the postverbal construction in (23a) can
be separated by parenthetical material, while the postverbal

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construction in (23b) cannot--

(28) a. He looked up, without a reply, the information.
       b. *They talked about, without a reply, the situation.

and that the verb cannot be separated from the postverbal construction
by a manner adverbial in (23a), though it can in (23b)--

(29) a. *He looked quickly up the information.
       b. They talked loudly about the situation.

The data in (28) indicate that the postverbal construction in (23a)
is not a close one, while the corresponding construction in (23b) is.
The data in (29) indicate that the verb and following word are
closely constructed in (23a), but not in (23b).

   Finally, there are tests for constituency that appeal to
phonological phenomena, in particular to the lack of accent and
phonological reductions characteristic of words that are cliticized
to other words. Given the general principles governing cliticization
and accent placement in a language, these phonological phenomena can
be used to argue that particular words form a constituent with the
words to which they are cliticized. On grounds of this sort, Postal
1974: sec. 4.4 argues that it forms a constituent with the preceding
verb in

(30) Max believes it to be true.

but not in

(31) Max believes it is true.

and Fraser 1976: sec. 1.1 argues that off forms a constituent with
the preceding verb in

(32) She ran off the pamphlets.

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but with the following noun phrase in

    (33) She ran off the stage.

    1.2.3. <u>Tests for relationships between parts of units</u>. The units in question may be words, phrases, or sentences, and the relationships in question are various. Under this general heading I include such morphological matters as the distinction between derivational and inflectional affixes, which stand in different relationships to the roots with which they are combined (for a recent brief discussion of the distinction, see Aronoff 1976: 2-3; more extensive lists of tests differentiating the two sorts of affixes are to be found in Nida 1949: 99-100 and Bazell 1953: ch. VI), as well as a variety of syntactic relationships, a few of which I will mention briefly.

    First, there are tests to distinguish between several types of coordination with <u>and</u>, types that differ in the way the individual conjuncts are related to their verbs (see the discussion in Stockwell, Schachter, and Partee 1972: 298-324). For example, there are tests designed to separate 'phrasal conjunction', in which coordinate NPs are understood as a unit--

    (34) Amelia and Belinda are similar.
from 'sentential conjunction', in which coordinate NPs are understood as separately related to the verb--

    (35) Carlo and Dennis are foolish.
Among these litmuses are the possibility of two-sentence paraphrases--

(36) a. Amelia is similar, and Belinda is similar.
          [≠ (34)]
      b. Carlo is foolish, and Dennis is foolish.
          [= (35)]

the possibility of a paraphrase with one conjunct appearing in a
sentence-final prepositional phrase--

(37) a. Amelia is similar to Belinda. [= (34)]
      b. Carlo is foolish to Dennis. [≠ (35)]

and the occurrence of quantifiers like both and each--

(38) a. *Both Amelia and Belinda are similar.
      b. Both Carlo and Dennis are foolish.

(39) a. *Amelia and Belinda are each similar.
      b. Carlo and Dennis are each foolish.

    Second, there are tests to distinguish between coordination and
subordination as the relationship holding between two constituents
of similar type within one sentence. Li and Thompson 1973, for
example, consider the serial verb construction in Mandarin Chinese
(sentences with a subject NP followed by two predicates, each of
the form V(NP)) and ask whether the second verb is subordinated to
the first or coordinate with it. They observe that Ross constraints
on movement (Ross 1967) prohibit individual conjuncts from being
moved by transformations of a certain type, while at least some
subordinate clauses can be moved; mobility is therefore a litmus
for subordinate status. As it happens, Li and Thompson conclude that
the serial verb construction is ambiguous, with both coordinate
and subordinate (purpose) structures.

    Third, there are tests to determine which of the parts of a
construction is the head, and which the dependents or modifiers.

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This distinction has played a fundamental role in many diverse
theories of grammar, and is not without significance even in
transformational grammar; Robinson 1970 surveys the matter with
examples from writers working in a variety of frameworks, including
Harris 1946, Ross 1967, and Chomsky 1970. Among the tests for
headship are independence (an element like wallabies in the piquant
wallabies, which can stand alone as a constituent, is the head of
the construction), characterizing function (an element like raced
in raced across the freeway, the properties of which essentially
determine the external distribution of the constituent, is its
head), and selectional determination (an element like asked in
has asked Mark if pigs can fly is the head of the construction
because the choice of verb determines which auxiliaries and objects
can occur with it rather than the reverse[2]); for some recent
discussion, with an additional transformational test, see Akmajian
and Lehrer 1976.

As a final illustration of tests for relationships between parts
of syntactic units, I offer the litmuses for grammatical subjects.
These have been enumerated and discussed at some length by Keenan
1976, and would require no further comment except that Keenan
appears to view the properties he discusses as part of a definition
of the notion 'subject of', rather than as external evidences of
subjecthood (see the criticism in Johnson ms. 1976: sec. 0.3 on
just this point). But we may interpret Keenan's list as a set of
litmuses, each providing some evidence as to whether a particular NP

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shows the typical signs of a grammatical subject--lack of case

marking, control of verb agreement, serving as the antecedent for

anaphoric constructions, position as the leftmost NP in a sentence,

and so on.

1.2.4. Tests to identify the syntactic category of particular

constituents. In this group fall tests to distinguish N from NP,

VP from Pred, NP from S--or simply to determine if some constituent

is an NP, a VP, an S, and so on. The literature on generative

grammar is rich in such tests, among them the Lakoff-Ross 1966 do so

test for VPs, Chomsky's 1970 arguments that 'derived nominals' are

NPs rather than Ss, and Perlmutter's 1970b demonstrations that

expletive there is an NP. I give now a few comments on these three

illustrations.

G. Lakoff and Ross 1966 maintain that do so is a proform that

stands for VPs and for VPs only.[3] Thus, it may stand for a VP--

> (40) I approached the kangaroo after Margaret did so.
> (41) The kangaroo yawned, and then the dingo did so, too.

(the underlined portion is the antecedent for do so) but not for

less than a VP--

> (42) *I gave them a dollar, and she did so a pound.
> (43) *I fell off the bed before the baby did so out of
>   the crib.

while certain adverbials may act as if they are outside the VP--

> (44) Ellen left because the talks were so boring, but
>   I did so because I had to go to the bathroom.
> (45) Francis sliced a salami with a ripsaw while I was
>   doing so with a meat cleaver.

Chomsky 1970 confronts the category status of 'derived

nominals' while comparing them to gerundive nominals; constructions

like Steiner's analysis of translation are seen in contrast to

Steiner's analyzing translation. Chomsky says that only derived

nominals

> (46) have the internal structure of noun phrases. Thus
> we can have such expressions as the proof of the theorem
> (*the proving the theorem, with a gerundive nominal),
> John's unmotivated criticism of the book (*John's
> unmotivated criticizing the book), and so on.
> Correspondingly, the derived nominals cannot contain
> aspect; there is no derived nominal analogous to John's
> having criticized the book. Furthermore, many derived
> nominals pluralize and occur with the full range of
> determiners (John's three proofs of the theorem, several
> of John's proofs of the theorem, etc.). And derived
> nominals, in fact, can appear freely in the full range of
> noun phrase structures. For example, the sentence John
> gave Bill advice is just like any other indirect object
> structure in that it has the double passive (advice was
> given (to) Bill, Bill was given advice). It is difficult
> to see how a transformational approach to derived
> nominals can account for the fact that the structures in
> which they appear as well as their internal structure and,
> often, morphological properties, are those of ordinary
> noun phrases.
>
> (Chomsky 1972: 20)

Continuing the discussion later in the same article, Chomsky uses the

failure of certain transformations (for instance, the movement of

particles over object NPs--Chomsky 1972: 26-7) to apply to derived

nominals as an argument for their being NPs rather than Ss. Finally,

he notes

> (47) that although gerundive nominalization applies
> freely to sentences with verb phrase adjuncts, this is
> not true of the rules for forming derived nominals.
> Thus we have (15) but not (16):

(15) his criticizing the book before he read it
(because of its failure to go deeply into
the matter, etc.)
(16) *his criticism of the book before he read it
(because of its failure to go deeply into
the matter, etc.)

(Chomsky 1972: 27)

In his discussion of expletive there, Perlmutter 1970b observes
that this word

(48) behaves like an NP with respect to transformational
rules in that it inverts in questions (Was there a
commotion?), shows up in tag questions (There was a
commotion, wasn't there?), shows up with so (Joe said
there would be a commotion, and so there was), undergoes
it-replacement [what is now known as raising--AMZ]
(We expected there to be a commotion), and undergoes
the passive transformation (There was expected to be a
commotion).

(Perlmutter 1970b: 116,
fn. 6)[4]

1.2.5. Tests for properties (subcategorizations and super-
categorizations) of particular constituents. Here the collection
of litmus tests that have been used by generative grammarians is
enormous, and I can do no more than give a wide sample of citations,
with a few illustrations. It is convenient to group these by the
constituent concerned; I will mention properties of S, NP, N, VP,
V, Quant, and Prt, though this list does not cover all the constituent
types, or putative constituent types, that have been treated in the
literature.[5]

1.2.5.1. S. Among the properties of S for which there are
tests are the distinctions between negative and positive sentences
(Klima 1964, Baker 1970a), between free relative clauses and embedded

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interrogative clauses (Baker 1970b), and between interrogative and
exclamatory clauses (N. McCawley 1973, Elliott 1974). Thus, Elliott
notes that the complement constructions in examples like

(49) a. I wonder how Joe saves money.
b. It's marvelous how Joe saves money.

behave differently with respect to a number of tests, including
occurrence with the adverb ever--

(50) a. I wonder how Joe ever saves money.
b. *It's marvelous how Joe ever saves money.

and introduction by the wh word whether--

(51) a. I wonder whether Joe saves money.
b. *It's marvelous whether Joe saves money.

The complement type in (49a) is interrogative, the one in (49b)
exclamatory.

1.2.5.2. NP. Most prominent of the tests for subcategorizations
of the constituent NP have been those to distinguish various sorts
of nominal constructions, in particular the gerundive (or factive)
nominal versus the action nominal versus the 'derived nominal' versus
the concrete nominal (see Lees 1960, Fraser 1970b, Chomsky 1970, and
many others). In the previous section we saw some litmuses
separating gerundive from derived nominals; among the tests to
distinguish gerundive from action nominals, as in

(52) a. Jacob's designing (the igloo) startled us.
b. Jacob's designing (of the igloo) took only
three days.

respectively, are the absence of of following the nominal in (52a)
versus its presence in (52b); the requirement that the nominal in

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(52b) describe an activity, while the nominal in (52a) need not--

(53) a. the soda's containing saccharin
      b. *the soda's containing of saccharin

and the possibility of an alternative to (52b) in which the prenominal

possessive NP appears in a postnominal by-phrase, while there is no

such alternative to (52a)--

(54) a. *The designing the igloo by Jacob startled us.
      b. The designing of the igloo by Jacob took only
          three days.

Other subcategorizations of NP have been tested in the literature,

as when Hankamer 1973: 33-4 (disputed by Channon 1974) attempts to

distinguish 'titular NPs' from true NPs, as in

(55) a. We elected Karen head of the board of trustees.
      b. We introduced Lars to the head of the board of
          trustees.

respectively, and when Postal 1966 (variously amended and criticized

by Thorne 1972, Sommerstein 1972, Morgan 1972, Grannis 1974, Thorne

1974, and Milsark 1977) proposes to separate definite from indefinite

NPs by means of such tests as occurrence in the existential there

construction--

(56) a. *There was Melanie on the front porch.
      b. There was a friend of mine on the front porch.

1.2.5.3. N. Probably the most often cited litmuses for

subcategorizations of the category N are those distinguishing mass

from count nouns, as in this convenient summary from Quirk et al.

1972: 128--

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	count	mass
(57) a. zero (or no) article:	*bottle	bread
b. indefinite article:	a bottle	*a bread
c. indefinite quantifier:	*some bottle	some bread
d. plural:	bottles	*breads

There are also tests for the subclass of N comprising pronouns, or for particular types of pronouns, illustrated in Sadock's 1975: 26-7 discussion of why the English personal pronouns do indeed constitute a class of words with similar behavior, and in Stahlke's 1976: sec. 2 arguments that the relative that does not share signi-ficant properties with the (clearly pronominal) wh words, hence is not a pronoun.

1.2.5.4. VP. By far the subcategorizations of the category VP that have been most pursued in the literature on generative grammar are the distinction between active and stative predicates (G. Lakoff 1966, further developed in Lee 1971, Valesio 1971, Ross 1972a, Cruse 1973, Sag 1973, and Dowty 1975, among others) and the elaborated distinction between predicates of state, activity, accomplishment, and achievement (based on Vendler 1957; see the survey in Dowty 1972: ch. 2). Some of the tests for these distinctions are clearly semantic; thus, the fact that

(58) Norbert walked for an hour.

entails

(59) Norbert walked at each time during an hour.

while

(60) Olive painted a picture for an hour.

does not entail

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(61) Olive painted a picture at each time during an hour.

indicates that the predicate walk is an 'activity' predicate (in

Vendler's terms), while paint a picture is an 'accomplishment'

predicate. Other tests are (at least apparently) syntactic; so,

the fact that ask questions can occur in the progressive in English--

(62) Three people are asking questions.

while constitute a trio cannot--

(63) *Three people are constituting a trio.

indicates that ask questions is an 'active' predicate, while constitute

a trio is a 'stative' predicate.

1.2.5.5. V. Among the tests for subcategories of V are those

for verbs taking different types of complements (Kiparsky and Kiparsky

1970, Bresnan 1970, and Hooper 1975, and many other sources), those for

verbs taking different combinations of Fillmorean case relations (for

instance, the verbs treated in Fillmore 1970), and an assortment of

tests for other verb classes (see Zwicky 1971 on manner-of-speaking

verbs and Zwicky 1973 on connection-of-ideas verbs). As an

illustration, we can take some of the Kiparskys' litmuses for

factivity, including both semantic tests, like the fact that

(64) They regret that Paula likes eels.

entails

(65) Paula likes eels.

and syntactic tests, like the obligatory nature of that in (64)--

(66) *They regret Paula likes eels.

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and by the immunity of NPs within complements of verbs like regret
to extraction--

     (67) *What do they regret that Paula likes?

To (64)-(67), compare examples with think instead of regret--

     (68) They think that Paula likes eels.

does not entail (65); and both

     (69) They think Paula likes eels.
     (70) What do they think that Paula likes?

are substantially better than (66) and (67).

    Another important set of tests for subcategories of V comes in
separating 'true' verbs from modals and other auxiliaries, a project
that has been pursued by many writers and is nicely summarized in
Pullum and Wilson (to appear), who note seven criteria distinguishing
the two subclasses of verbal elements (while maintaining that they
belong to the same large syntactic class, despite Jackendoff's
1972: 100 claim that modals and true verbs show 'totally different
syntactic behavior').

    There are finally, supercategorizations of V, properties that
verbs share with other syntactic categories. The relationship
between 'true' verbs and auxiliaries can be seen in this light,
but the supercategorization of V that has attracted the most
attention in the literature is the relationship between verbs and
adjectives, following G. Lakoff's 1970: app. A discussion of
properties shared by the two.

1.2.5.6. Quant. I devote this subsection and the next to
citing a few studies in which properties of minor, closed syntactic
categories have been investigated. Among these is Jackendoff's 1968
division of English quantifier words into two classes--

> (71) a. some, each, few, which, all, both
> b. a few, many, one, three

primarily on the basis of whether the quantifiers permit articles
in front of them--

> (72) a. *the some/each/all men
> b.   the many/three men

1.2.5.7. Prt. In Emonds 1972a it is argued that the English
minor syntactic categories Prt and Prep belong to a single super-
category (as Emonds puts it, particles are 'intransitive prepositions'),
on the basis that particles and prepositional phrases share many
properties, including modification by the emphatic word right--

> (73) We put the spices right on the meat.
> (74) Let's look it right up.

and preposability--

> (75) Into the house he ran.
> (76) In he ran.

1.2.6. Tests for markedness of categories. One final collection
of litmuses is designed to determine which of two opposed morpho-
logical or syntactic categories is marked and which unmarked, the
marked category being, in an intuitive sense, the more special,
less normal, more complex of the two. These tests have been treated
at some length in several places (notably by Greenberg 1966: esp.
ch. 3, Schane 1970, and Comrie 1976: ch. 6), so that an extended

discussion is not necessary here. We expect the marked category to be realized by morphological material, while the corresponding unmarked category need not be (thus, the noun plural in English is realized as a suffix, while there is no affix for the singular); instances of the marked category are normally less frequent in texts than instances of the unmarked category; and it is the unmarked category that appears in positions of neutralization (hence, the interrogative word who, which can be used indifferently for individuals or for groups, has singular number agreement).

1.3. The source of litmus tests. It should be noted that one might come across a litmus test in either of two ways--by prediction from general, theoretical considerations, or by happy accident, by noting that one sort of linguistic behavior correlates with another. In the preceding sections I have pointed out a number of tests that follow from other principles; the transformational potential tests for ambiguity in section 1.2.1 are of this sort, as are Li and Thompson's use of a Ross constraint to distinguish coordination from subordination (section 1.2.3) and Perlmutter's arguments for the NP status of expletive there (section 1.2.4). But others, like Emonds' use of emphatic right (section 1.2.5.7) to lump together the categories Prt and Prep, surely are not. There is something of an analogy here in the determination of numerical values for various chemical properties: there are so-called 'theoretical values', calculated from theoretical formulas, and 'empirical values', obtained by actual measurements.

It could even happen that the very property being tested for might have become of interest only through the happy accident that someone noticed a correlation between two properties. But we would expect such discoveries to be rare, since most properties are of interest because they arise from semantic categorizations (like negativity, factivity, and ambiguity) or from surface categorizations like constituency, which are relevant for perception and for phonological organization. I return to these matters in section 3.4 below.

2. The Bloomfieldian counterrevolution.

2.1. Introductory remarks. The utility of linguistic litmus tests has been questioned by Dougherty 1974, who claims that the fact that they refer to properties of strings in surface structure indicates that they belong to the methodology of a 'pregenerative view' of linguistic research. Consequently, according to Dougherty, the use of these tests by adherents of generative semantics signifies a 'Bloomfieldian counterrevolution', a return to the segmentation-and-classification linguistics that was overturned by Syntactic Structures.

Dougherty's attacks on litmus tests are directed against Postal's 1966 discussion of definiteness in NPs; against Fillmore's 1968, 1969 case proposals, which include the delineation of a number of verb classes; against the G. Lakoff and Peters 1966 distinction between phrasal and sentential conjunction; and against the treatment of coordination in J. McCawley 1968.[6] There have been

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several responses to this work of Dougherty's and to Dougherty's other work directed against generative semantics: Botha 1976a and 1976b, and now the very long treatment in Botha 1977, all of which take up issues other than those I want to focus on in this article; and J. McCawley 1975, a reply specifically to Dougherty 1974, and a paper that makes several of the points I will put forth below. In addition, a metatheoretical discussion closely related to Dougherty's has now appeared in Culicover 1977, which is a critical treatment of argumentation in Postal 1966 (again) and Sommerstein 1972 on pronouns and articles, Ross 1969 on adjectives and noun phrases, and Carden 1970 on quantifiers and adjectives.

Before turning to a discussion of Dougherty and Culicover on litmus tests, I must say a few words about the conclusions drawn and analyses advocated by Postal and the other authors examined by Dougherty. Postal, as part of a larger attempt to argue that English personal pronouns are, at a more remote level, definite articles, gives a number of diagnostic environments for indefinite NPs and for definite NPs. Now, I am not defending Postal's larger claim-- indeed, I would agree with Thorne 1972 that it is more likely that the is an unstressed form of deictic that (parallel to Perlmutter's 1970a derivation of a(n) as a unstressed form of the number word one[7]) than that it represents more remote it-one, parallel to the one; Sommerstein 1972 suggests a related analysis, with definite articles deriving from remote pronouns, rather than the other way around. Nor is it necessary for me to uphold Fillmore's case

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grammar in all its details, or to believe that the distinction G.

Lakoff and Peters make applies to the examples they cite and for

the reasons they give,[8] or to accept any of the analyses of coordinate

structures advanced by J. McCawley. All that I am defending is the

utility of litmus tests in arriving at and arguing for analyses of

particular languages.

2.2. Discovery procedures. Dougherty explains his objections

to the Postal, Fillmore, Lakoff-Peters, and J. McCawley articles as

follows--

> (77) In a discussion of discovery procedures, Chomsky
> 1957 indicates that procedures of segmentation and
> classification, i.e. slot filler tests, diagnostic
> environments, etc., are useless if our goal is to
> construct a generative grammar. Chomsky argues, quite
> persuasively to my mind, that the linguist must seek
> to evaluate grammars which generate the sentences of a
> language and not attempt to construct substitution tests
> to discover facts about the surface structure
> distributional similarities of strings.
>   (Dougherty 1974: 263-4)

But what Chomsky says, in Syntactic Structures, Current Issues in

Linguistic Theory, and elsewhere, is not really that procedures and

classification are useless if our goal is to construct a generative

grammar (though to be fair to Dougherty, Chomsky's early writings on

this question are spirited to the point of overstatement, so that

it would be easy to mistake his intent). Rather, his object is to

repudiate the view that linguistic theory is identical with the

specification of a discovery procedure for grammars (a view in which

theory is equated with practice' (Teeter 1964: 204)), as in this

quotation from Chomsky 1966 (which Dougherty himself cites on page

266)--

(78) [the] fundamental assumption [of structural
linguistics] is that procedures of segmentation and
classification, applied to data in a systematic way,
can isolate and identify all types of elements that
function in a particular language along with the
constraints that they obey. A catalogue of these
elements, their relations, and their restrictions of
'distribution' would, in most structural views,
constitute a full grammar of the language.
                                (Chomsky in Reibel and Schane 1969: 6)

To reject the identification of theory with method is not to
reject particular pieces of method. Chomsky's criticism is
appropriate when directed towards views of linguistic litmus tests
that take them to be definitional for properties and to be embedded
within a discovery procedure, but not necessarily when it is directed
towards other views. It can even be argued that the structuralists
attacked by Chomsky had a much less stringent notion of the connection
between theory and method than Chomsky attributes to them (just as I
want to argue that the generative semanticists attacked by Dougherty
have a much less stringent notion of this connection than he
attributes to them). Miller 1973: 123, in fact, notes that 'in the
work of [Bloomfield, Harris, Wells, and Hockett] a distinction is
carefully drawn between the actual process of discovering the
structure of a language and the business of describing a structure
which has already been discovered'--though it must be granted that
the theoretical pronouncements and practice of these linguists are
sometimes divergent, and their theoretical work is not entirely
consistent, so that their intent is not always clear.

At any rate, there is no reason to see in references to litmus

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tests an abandonment of the Chomskyan revolution in linguistics.[9] It

is a misinterpretation of the articles Dougherty cites to see in them

the sort of definitional discovery-oriented criteria that characterize

works like Fries' Structure of English (Fries 1952). As J.

McCawley puts it,

> (79) Dougherty's objections [to Postal 1966] seem to
> be in the realm not of methodology but of etiquette:
> he is objecting not to the content of Postal's treatment
> but to Postal's having chosen a mode of presentation
> that reminds hyper-sensitive transformational grammarians
> of a linguistic school that they find offensive.
> (J. McCawley 1975: 152)

Indeed, reading Postal the way Dougherty would have us read him

should oblige us to read Chomsky's 'Remarks on Nominalizations' the

same way. But we have already seen, in the quotation in (46) above,

that Chomsky 1970 uses a variety of differences in internal structure,

external distribution, and applicability of transformations to

distinguish between gerundive and 'derived' nominals and to argue

that the latter are NPs. An uncharitable reading would take these

remarks by Chomsky to constitute a purely 'structuralist' definition

of NP by reference to internal structure, external distribution, and

transformability. Similarly, reading Dougherty 1970a in this spirit

should lead us to conclude that he had reverted to pregenerative

methods--

> (80) In the PSR Hypothesis, the interpretation of a
> coordinate conjunction is determined by features
> assigned to the coordinated node. Two of these features,
> [±individual] and [±totality], are defined in terms of
> properties of the distributive quantifiers each, all,
> and both. These features have (roughly) the following
> interpretation:

[+individual]: The adverbs <u>alone</u>, <u>singly</u>, <u>indi-
vidually</u>, and <u>independently</u> can occur <u>with a quantifier
having this feature</u>. This feature emphasizes the inde-
pendent, individual action of each of the constituents of
the conjunction, and suggests that a conjoined sentence
paraphrase exists.

[-individual]: The adverbs <u>alone</u>, <u>singly</u>, <u>indi-
vidually</u>, and <u>independently</u> cannot occur <u>with a quantifier
having this feature</u>. No conjoined sentence paraphrase
exists. The elements of the conjunction are like the
elements in a series bound together by plus signs. Often
<u>and</u> and <u>plus</u> are interchangeable conjunctions with
<u>[-individual]</u> quantifiers: <u>Two plus/and two is four</u>.

[+totality]: The adverbs <u>together</u>, <u>simultaneously</u>,
<u>en masse</u>, <u>at once</u>, <u>in chorus</u>, etc. can occur <u>with a
quantifier bearing this feature</u>. The surface-structure
quantifier is <u>all</u>. The feature emphasizes that the
conjunction is <u>to</u> be considered as a unity; the conjuncts
act mutually.

[-totality]: The adverbs <u>simultaneously</u>, <u>en masse</u>,
etc. do not occur with a <u>quantifier having this feature</u>.
The conjunction is not to be considered as a unity; the
conjuncts do not act mutually.

<div align="right">(Dougherty 1970a: 868)</div>

But surely this would be to mistake Dougherty's intent, and in fact

he adds to the discussion in (80) the note that his 'definitions are

merely suggestive, since the real content of the features comes from

the role they play in the grammar'.

2.3. <u>Constituency test arguments</u>. Culicover 1977 considers a

collection of litmus tests that have been used to argue that two

superficially distinct categories (pronouns and articles, adjectives

and noun phrases, quantifiers and adjectives) are 'really the same',

that is, belong to a supercategory. Culicover sees these <u>constituency</u>

<u>test arguments</u> (his term) as taking the form

(81) Classes A and B display the same distribution of
grammaticality judgments with respect to some test T.
Therefore, in the structures in which T is applicable,
A and B are the same type of underlying constituent, and
are distinguished by some feature if necessary.
<div align="right">(Culicover 1977: 66-7)</div>

and says that

> (82) Users of [(81)] appear to be of the opinion that
> the greater the number of tests satisfied by classes A
> and B, the better the argument that A and B are the same
> type of constituent...
> If classes A and B fail to satisfy a particular test
> this does not necessarily lead to the conclusion that
> A and B cannot be the same type of constituent for
> someone who employs [(81)].
>
> (Culicover 1977:67)

In addition, he maintains that (81) is a (covert) part of the
evaluation metric assumed by syntacticians like G. Lakoff, Postal,
and Ross. This last point I would like to dismiss fairly quickly,
since here Culicover seems to be assuming that the reasons linguists
give to other linguists for selecting one analysis over plausible
alternatives necessarily represent pieces of an evaluation metric.
I see no reason to think that evidential questions should find direct
reflection in theoretical assumptions like those embodied in an
evaluation metric, even for linguists who subscribe whole-heartedly
to the idea of such a metric (as many of those criticized by Culicover
do not).

Otherwise, the burden of Culicover's discussion is that the
arguments he considers for lumping categories together are often
weak (a point made very cogently by Schachter 1973), in particular
that there are in most cases items that do not pass the tests, as
when various adjectives fail Ross' 1969 tests--

> (83) a. Peter is smart, but he doesn't look it.
>   b. *Peter has become careful, but Susan will never
>   become it.

and that the writers criticized sometimes miss generalizations in a

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way that undercuts the conclusions they would like to reach. With these criticisms I have no quarrel of principle. But they in no way show that linguistic litmus tests are valueless--only that, as tools, they must be carefully chosen and carefully used. On the question of counterexamples, I shall have more to say in sections 3.2. and 3.3 below.

2.4. Support for litmus tests. In my remarks this far I have maintained a distinction between methods or techniques of arriving at linguistic descriptions, on the one hand, and the content of descriptions and theories, on the other--a distinction I have explored in a number of articles (especially Zwicky 1975). Now I wish to defend the claim that such points of methodology as litmus tests are worth serious investigation. Perhaps I should say that my own interest in the subject comes from observing that the decisions linguists make between alternative analyses are often not dictated by theoretical considerations, but rather reflect certain methodo-logical biases on the part of the investigator. Beyond this, there are at least three reasons why litmus tests should be taken seriously.

First, a general point. The working linguist, like any working scientist, needs orderly methods for attacking his tasks and some accepted forms for presenting and defending his results. The phenomena to be described are so complex that we cannot rely entirely on unanalyzable lucky guessing as the source of hypotheses, and the amount of material to be communicated is so great that we cannot expect writers to construct a new mode of organization for each argument. This is not to deny that great advances require

brilliant strokes.

What Dougherty says on this point is that a generative linguist

> (84) must 'invent' new grammars and test them against
> existing grammars. Theory construction and hypothesis
> testing are the main tools to achieve rational progress.
> Hypothesis testing is a general method employed to
> choose the superior grammar from a field of alternative
> grammars which make conflicting claims about a given
> range of data. The method requires the alternative
> grammars to be compared with each other and with the
> data.
>
> (Dougherty 1974: 272)

But the invention of new grammars is not a totally random process.

Hypothesis testing is, of course, a standard procedure in generative

grammar (and in structuralist approaches too, to judge from works

like Harris 1960). However, unless 'theory construction' and 'hypo-

thesis testing' are understood in a very loose and inclusive manner,

they cannot serve as the only methods in grammar construction and

validation.

Next, the working linguist needs ways of establishing the content

of superficial syntactic representations, semantic representations,

and (in some theories) deep syntactic representations. Hypothesis

testing alone will not suffice here, since these are the things the

hypothesized grammars are supposed to generate or relate; they are

the givens of analysis. Though linguists have been inclined to take

for granted their own assertions that a sentence has such-and-such

as its constituent structure and that it has such-and-such as its

semantic representation, both these 'external' aspects of grammatical

organization are in fact highly abstract, arrived at only by complex

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chains of inference from actual observations, and difficult to
validate in detail. Hence, there are tests for these 'givens', as
in sections 1.2.1 and 1.2.2, and frequent doubts as to whether these
constructs are real and, if real, whether they are determinate (as
in Haas' 1973 complaints about surface structure, or many philosophers'
worries about the idea of semantic representation).

Finally, we would like to make scientific hypotheses as fallible
in detail, in addition to as a set, as we can--in the linguistic case,
to ask that grammars be as vulnerable in parte, in addition to in
toto, as we can make them. Using litmus tests can serve this end for
us, by encouraging us to predict connections between properties,
rather than merely generating the correct surface structures or
achieving the correct pairing between surface structures and semantic
representations. Sometimes such predictions will turn out to be
wrong, of course; I mention a few cases in the next section.

Note that litmus tests are used not only by operationalist
structuralists and generative semanticists, but also by transformational
grammarians in work antedating intimations of the split between
generative and interpretive semantics (for instance, Chomsky's 1965:
101 arguments for the ambiguity of He decided on the boat), by
transformational grammarians in work intended to be neutral to this
controversy (Baker 1970b, Elliott 1974), and by clear interpretivists
(Chomsky 1970, Dougherty 1970a). The historical source for this
approach to analysis is undoubtedly traditional grammar; in most
cases, the properties under examination have been believed to be

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grammatically relevant for centuries, and it is easy to find argumentation in the compendious grammarians (sometimes there are extensive lists of criteria, as in Buyssen's 1959 study of negative elements in English, cited by Klima 1964 and Baker 1970a). Particular litmus tests might be badly chosen or badly applied, but surely the widespread occurrence of litmus tests in linguistic investigations indicates an equally widespread belief in their utility. It is difficult, in fact, to see how we could do without them, as heuristics and in public argumentation.

3. The correspondence fallacy.

3.1. Introductory remarks. A more interesting criticism of litmus tests (not made by Dougherty but certainly intended by Culicover) is that no two tests for the same property give quite the same results. Indeed, there is now a sizable body of work on the failure of perfect correspondence between different tests used by transformational grammarians--for instance, R. Lakoff's 1973: 689-93 discussion of Kiparsky and Kiparsky 1970 and Karttunen 1970, 1971a, 1971b on factivity, Ross 1972b on the distinction between nouns, verbs, and adjectives, Ross 1973a on NPs, and Ross 1973b on NPs and Ss.

However, to conclude from this that tests should not be used, or that different tests always detect different properties, would be to fall into one form of the correspondence fallacy discussed by C. E. Bazell 1952, whose article deserves quotation at length--

> (85) The prototype of these fallacies is the assumption
> that two distinct (sets of) criteria will necessarily lead
> to isomorphous analyses...

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But it is of course no fallacy to assume (at least
as a working principle) that two units coincide more
often than not. Nor is it a fallacy to assume that it
is possible to devise, a posteriori, two sets of criteria
which will lead to very similar results. It may be
possible to gear the articulatory criteria [for phonemic
analysis] to an acoustic analysis, or distributional
criteria [for morphemic analysis] to a semantic analysis,
providing a good deal is known of what these analyses
would be. The fallacy lies in assuming a one-one
relation between the results of criteria which have not
been selected with this end in view.
    The complementary fallacy consists in the supposition
that, since different criteria lead to different results,
these criteria have necessarily to be taken separately.
In determining the phoneme-inventory one must use arti-
culatory criteria alone (earlier Bloch), or acoustic
criteria alone (later Bloch), but never both together.
Morphemes may be determined on a distributional basis or
(perhaps) on a semantic basis; but in absence of
correspondence these are simply different sorts of units,
maybe with a certain correlation (C. F. Hockett). The
recognition of the inevitability of incomplete correspondence
leads to the misrecognition of the value (which is not
"merely heuristic") of probable correspondence, in
determining the units.
    But there are fallacies deriving more directly from
the prototype. Such is the fallacy of supposing that
some single necessary and sufficient criterion will lead,
not to a unit fixed by some other criterion, but rather
to some unit already recognized either·by traditional
grammar, or by the "linguistic conscience", or by
linguists engaged in field-work who have set up the unit
for purely practical purposes. Definitions have been
sought for the "word" on this assumption (A. Martinet,
K. Togeby). Sometimes (most often in America) a complex,
necessary and sufficient, set of criteria replaces the
single criterion. The complementary fallacy is sheer
scepticism: the unit cannot be defined at all (Daniel
Jones for the phoneme, A. S. C. Ross for the morpheme).
                                (Bazell 1966: 271-2)

The version of the correspondence fallacy that I am concerned with

here is, of course, the complementary fallacy mentioned by Bazell.
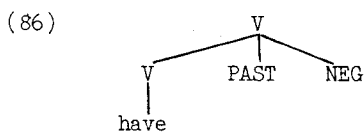
    Now it may very well turn out that different tests do select

classes that are without question distinct (but happen to overlap).

Thus, Lee 1971 and Sag 1973, among others, have argued that the progressivity test for stative predicates is distinct from the remaining ('activity') tests. As a matter of fact, I doubt that the various tests for definiteness in NPs select anything like the same class.
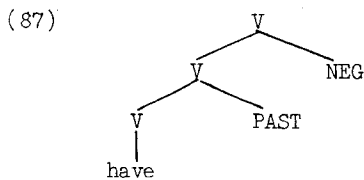
Aside from this possibility, there are various difficulties that can arise in applying particular tests. I now survey these briefly before returning to the difficulty referred to by Bazell, the failure of perfect correspondence.

3.2. Difficulties in using litmuses.

3.2.1. Limited domain. The domain of applicability of a test will have some limitations, and examples in which we have a special interest might not fall within this domain. For example, I have occasionally worried about whether the constituent structure of hadn't and similar contracted auxiliaries was ternary--

(86)

```
              V
         _____|_____
        /     |     \
       V     PAST    NEG
       |
     have
```

or binary--

(87)

```
              V
          ____|____
         /         \
        V          NEG
     ___|___
    /       \
   V       PAST
   |
 have
```

but I know of no test for constituent boundaries that will decide the question. This difficulty arises frequently when we try to

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determine constituent structure below the level of the word.

Another example: Zwicky and Sadock 1975: 23-5, 31-4 point out that tests for syntactic ambiguity are, by and large, inapplicable when the meaning of one putative structure is included within the meaning of another, as in the intentional and nonintentional (better: not necessarily intentional) understandings of

(88) Chuck dropped the tea tray.
(89) Doris slid down the stairs.

3.2.2. Fuzzy results. The sharpness of results varies from case to case, primarily because informants find it difficult to make clear judgments about some examples. I am myself unsure about a number of examples in which constituents have been extracted from factive complements, for instance

(90) Who does Johann know Max killed?

Sentences like (90) ought to be ungrammatical, given the Kiparskys' analysis.

In the same vein, many informants find it hard to judge whether examples like

(91) It wasn't a dog Linda saw, it was a bitch.

are acceptable or not. If dog is ambiguous between 'canine' and 'male canine', then (91) ought to be acceptable, according to a contradiction test for ambiguity (discussed by Zwicky and Sadock 1975: 7-8).

There is, of course, now a considerable literature on grammaticality judgments and their attendant problems, much of it surveyed in Householder 1973.

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3.2.3. <u>Exceptions</u>. Exceptional elements may give anomalous results. For instance, although singular proper nouns normally lack articles in English, there are a few that require the article <u>the</u> (<u>The Hague</u>, <u>The Ginza</u>, etc.). We can still use lack of an article as a test for proper nouns (in the singular), but we must note that there is a list of idioms that violate an otherwise useful generalization about English. Asyntactic idioms, like <u>by and large</u> and <u>trip the light fantastic</u>, will frustrate nearly any generalization, in fact.

3.2.4. <u>Interfering factors</u>. Interference of various other kinds will sometimes prevent tests from giving usable results. It can happen in the laboratory that the acid dissolves the litmus paper. In the linguistic case, what happens most often is that although a test predicts that some sequence should not occur, it might never- theless occur as the output of a rule, or the exponent of some construction, other than that for which the test was devised. Thus, singular proper nouns <u>do</u> occur with articles in constructions like

(92) That's not the Chomsky I know.
(93) I know two Chomskys.

And, although occurrence in existential constructions is a litmus for indefinite (as opposed to definite) NPs--

(94) There had been a gorilla on the porch.
(95) *There had been the gorilla on the porch.

there <u>is</u> a 'list' construction in which sequences like (95) occur--

(96) What a morning! she thought. There had been the
gorilla on the porch, the narcotics bust, the
winning lottery ticket, and the fire in the
aviary.

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These are cases in which something we expect to be unacceptable turns out to be possible in another context.[10] There are also cases in which the examples are indeed unacceptable, but for some reason other than the one being considered; this is one of the main lines of criticism Bolinger 1971 advances against the analysis of the verb remind in Postal 1970. For instance, Bolinger 1971: 537 examines a putative constraint on coreference in certain complements to verbs of saying or thinking. According to Postal it is such a constraint that accounts for differences like

> (97) John says Lucille impressed him as being similar to
>   Mary in that she was very emotional.
> (98) *John says Lucille impressed me as being similar to
>   Mary in that she was very emotional.

Bolinger agrees with the judgments, but maintains that the problem with (98) is the use of say in a situation where the speaker is better qualified to give the information in the complement than the subject of the sentence is, not a matter of a coreference constraint at all. Consequently, the restriction illustrated in (97) and (98) could not be used as a test for experiencer NPs in complement constructions, as it could be if there was the sort of constraint Postal posits.

Factors not strictly part of grammar may also interfere. For instance, although occurrence in subject position is a test for being an NP, that S sequences are sometimes unacceptable as embedded subjects--

> (99) *For that Marcia came to surprise Martin would
>   astonish me.

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We might conclude from this, as Emonds 1972b: sec. 1.5 does, that that S sequences simply are not NPs. But many have seen the problem with (99) not as a grammatical restriction, but rather as unacceptability resulting from a perceptual difficulty manifested by (99); so Ross 1967; sec. 3.1.1.3.1 describes the construction as one 'excluded on the basis of [a] very general output condition on performance', and Langendoen 1970: 99 sees it as a case of 'sentence unacceptability arising from internal complexity'.

3.2.5. <u>Variously significant results</u>. The interpretation of negative results is different for different tests. Some tests for the occurrence of property y are of the form <u>if x then y</u>, others of the form <u>if x then not y</u>, and a few of the form <u>x if and only if y</u>. Negative results are significant only in the last case. Otherwise, we can conclude nothing from the fact that property x does not occur. Thus, from the fact that factive verbs don't permit the raising of negatives--

(100) I didn't regret Hugh was at the party. ≠
(101) I regretted Hugh wasn't at the party.

and the fact that <u>say</u> doesn't permit this raising

(102) I didn't say Irene was the winner. ≠
(103) I said Irene wasn't the winner.

we learn nothing about whether or not <u>say</u> is factive.

Similarly, we observed in section 1.2.1 that transformational potential tests indicate that

(10) They noticed her duck.

is ambiguous, because one of the understandings of (10) disappears

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when various transformations are applied, but that from the fact
that

    (11)   Paula laughed at the kangaroo.

maintains both its 'recent past' and 'remote past' understandings
through the application of these transformations we conclude merely
that we have no evidence that (11) is ambiguous (not that (11) is
definitely unambiguous with respect to the two types of past tense).
Even if we could somehow know that no transformation of English
affected the two understandings of (11), we could still not conclude
that (11) is unambiguous (though the discovery would certainly
support that conclusion); the way this litmus works is that if
application of a transformation eliminates an understanding then
the sentence is ambiguous.

    3.3. Center and margin. Let us return now to Bazell's
discussion of the correspondence fallacy. Bazell's response to the
failure of perfect correspondence is to say that for each category
or property there is a central class of elements, and various classes
that include this central class plus some marginal elements. Here
is his discussion of the morpheme--

> (104) ...it is merely the narrow Harris-like sense of
> the term morpheme that gives rise to this necessity
> [for a variety of terminological distinctions]. The
> boundaries of the unit having been fixed once and for all,
> the word is not any longer available, although the new
> unit has precisely the same nuclear members. And the
> many other units having the same nucleus will similarly
> claim special terms.
>   The solution is easy. We maintain the term
> morpheme for all units having the same nucleus.
> "Productive morpheme" can be used to describe the

category of units defined by the criterion of productivity, "distributional morpheme" for the category defined by distributional criteria, and so on. Similarly morpheme-alternants may be described as alternating morphemes. For this the term morph may be considered a useful abbreviation. But it will not be wrong to use the term morpheme for any of these units, providing the context is clear.

(Bazell 1966: 274)

And in reference to an attempt by Martinet to define langue, Bazell

maintains that 'it is surely enough to define the centre of the

linguist's interests. As for the marginal cases, the linguist

may occupy himself with one or another of them as he likes' (283).

Bazell's use of the center/margin distinction here is

especially interesting in connection with recent proposals for the

hierarchic arrangement of linguistic elements (among them Quirk

1965, Fraser 1970a, DeCamp 1971, Bailey 1973, the Labov papers

collected in Labov 1972a, 1972b, and the Ross papers cited in

section 3.1 above). Since Ross' squishes are perhaps the easiest

of these proposals to relate to the center/margin distinction, I

will discuss them in some detail.

A squish is a two-dimensional matrix which is itself the

product of two linear orderings—one ordering of linguistic elements

with respect to some property, and one ordering of tests for this

property. I will consider Ross' 'fake NP squish' (Ross 1973a) for

a concrete example. The property in question is being an NP; the

elements examined are, in order,

(105) animates (Harpo), forces of nature (the heat) and concretes (the rock), events (the concert), abstracts (his evasiveness), tack in take Det tack, headway in

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make headway on, 'weather it' with predicate adjectives, the dummy it with extraposed clauses, 'weather it' with verbs, expletive there, tabs in keep tabs on, heed in take heed of.

and the nineteen tests applied include, in order,

(106) occurrence in tag questions, as the head of a relative clause, with conjoined VPs, with the possessive suffix, as the antecedent for subject deletion in adverbial gerundive clauses (Before leaving, Herbert uttered many foul oaths), and in double raising-to-subject constructions (Irene is likely to be shown to have known all along).

The intersection of these two hierarchies is a matrix of the form

(107)   item$_1$

item$_2$

item$_3$

	test$_1$	test$_2$	test$_3$	...
item$_1$				
item$_2$				
item$_3$				

in which the cells are filled with symbols indicating whether or not

a particular test applies to a particular item. If the hierarchies

are arranged like those in (105) and (106) above, then the northwest

corner of the matrix will be the 'positive corner', the southeast

corner will be the 'negative corner', and the dividing line between

the positive and negative parts of the matrix will be approximately

the diagonal running from southwest to northeast.[11] And the way the

items and tests are arranged in the hierarchies is so that, matrix

will show just this pattern--if the data will submit to such an

arrangement (not all will, of course). What this means is that the

items being tested are close to the positive corner--are more central

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members of the class being tested--according as they pass more of the tests, and the tests being applied to the items are close to the positive corner--are more central tests--according as they select more items. Animates are very central NPs, heed a very marginal one. Occurrence with tag questions is a very central test, double raising-to-subject a very marginal one.

Bazell's account, however, is consistent with there being many distinct sorts of marginal elements with the same center, so that the elements are not necessarily arrangeable in a hierarchy. Indeed, Bazell's treatment of the morpheme concept in (104) above makes it clear that this is his view, that elements are arranged around a center not in implicational, hierarchical, or squish fashion--

(108)

but rather in a multiple overlap pattern--

(109)

And, in fact, there is a good reason to think that the typical pattern for items and tests is more like (109) than (108). Ross' squishes have a relatively large number of cells with marks that do not fit the expected pattern, which suggests that the data he

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is examining simply are not the product of two hierarchies like
(108); see the extended criticism in Hindle and Sag 1975: sec. 2.

The pattern in (109) is now a familiar one from studies of
word meanings (see particularly Rosch 1973, Labov 1973, and
Fillmore 1975), where the referents of a word cluster around
prototypical cases and diverge in various dimensions from these
prototypes. I will pursue this analogy a bit in the next section,
after reconsidering the question of where litmus tests come from.

3.4. On the motivation of litmus tests. Even when a set of
data does fit well into the squish paradigm, the resulting arrange-
ment of items and tests is only an organization of the facts and
not an explanation of them. If the nearness of an item or test
to the positive corner of the squish is to correspond to its
centrality, then we must ask why these elements are arranged as
they are. Presumably the most central tests are those intrinsically
connected with the property in question. But what of the others?
Ross himself has articulated the problem quite clearly--

> (110) ...so much of [the Fake NP Squish] is unexplained.
> The ordering from top to bottom is not such a problem:
> in Navaho, such hierarchies as that among the first
> four rows [animates, forces of nature and concretes,
> events, and abstracts] are well documented, and the fact
> that some idioms are more frozen than others is also
> well known. But why should the syntactic processes
> that head the columns be arranged in just the way they
> are? Why, for example, should get-passives be
> choosier than be-passives, rather than vice versa?
> Why should the pronouns that appear in tags be less
> choosy about the noun-phrasiness of their antecedents
> than other pronouns? Why should Being DELETION be so
> much choosier than To Be DELETION, when they often

produce identical output sequences? And why should
PROMOTION be so much choosier than RAISING, when they
seem to be so similar in function?
(Ross 1973a: 128)

Thus, in some cases, like the first four classes of items in

(105), there seems to be a principled relationship between the degree

of centrality of a class and its behavior with respect to litmus

tests. For the first four classes of items in (105), their centrality

is related to their semantics (with animate beings, especially human

beings, as the 'best' or prototypical things for referring expressions

to refer to, and other things less good according as they have fewer

of the salient properties of human beings--sentience, intentionality,

activity, concreteness, and so on). Systematic relationships like

this are what led traditional grammarians to see the parts of speech

and other syntactic categorizations as 'notional'   or semantic at

root, an idea revived by Lyons 1966 in reaction to structuralist

and early generative assumptions that syntactic categorizations

were autonomous of semantics, and an idea pursued by L. Anderson

1974 in connection with squishes; L. Anderson 1974:54 speaks of

'quasi-universal tendencies for related meanings to be expressed

by words of the same "grammatical category"' and looks on a

category as 'a "family" of words with "core" members, and

"peripheral" members where meaning-relations produce conflicting

tendencies'.

In other cases, however, differences in the centrality of

items and tests look quite arbitrary. The relative frozenness of

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idioms is an accident of history, and it would be silly to search for deep explanations for their order in a sequence like (105). But there may well be many intermediate or mixed cases, motivated to some degree but not entirely. In this vein there is the proposal by Newmeyer 1972, 1974 to predict some aspects of the behavior of idioms from their semantic structure, to predict, for instance, that kick the bucket 'die' fails to undergo passivization on the ground that the idiom is semantically intransitive, with no object argument in its semantic structure to correspond to the bucket; hence

> (111) The bucket has been kicked by many careless
>   mountain climbers.

lacks the fatal sense of kick the bucket.

These matters are further complicated by the fact that the properties being tested for are themselves of different sorts-- from semantic categorizations to superficial ones, with perhaps many possible intermediate types. On the one hand, there are tests for properties like semantic ambiguity; on the other, tests for surface constituency. But for many syntactic categories and properties, it is not entirely clear where they fall. Certainly, it must be possible for categories and features to be introduced by rule, as Schachter notes in his comments on syntactic categories and their relevance to the Universal Base Hypothesis--

> (112) ...Chomsky attributes potential universality
>   only to categories of the base: that is, only to
>   categories which are found in pre-transformational
>   structures. Thus the possibility is left open that

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> various syntactic categories needed for the character-
> ization of the surface structures of particular
> languages need not be included in a universal
> vocabulary of category symbols at all. For example,
> the category relative pronoun must evidently be
> distinguished from other syntactic categories in an
> analysis of surface structures in English and certain
> other languages. But if, as is generally believed,
> relative pronouns have no place in base structures
> and arise only as the result of transformations, they
> need not concern those seeking to establish a universal
> set of syntactic categories.
>
> (Schachter 1973: 139)

Finally, recall that it is possible to come across a good test

for some property without knowing why it works. I noticed some years

ago, for example, that the idiom be to 'visit, see, go' is confined

in standard English to occurrence in the perfect--

> (113) I have never been to Australia.
> (114) We had been to Chicago many times before we
>   realized it was not the state capital.
> (115) Having been to Santa Cruz only once, I naturally
>   would like to return.
> (116) *Right now I {$\begin{matrix} am\ to \\ am\ being\ to \end{matrix}$} Columbus.
> (117) *We were to Vienna last year.
> (118) *She will be to Essex soon.

and therefore can serve as a test for whether a form of have is a

perfect; the grammaticality of (115) shows that the have there

represents the perfect, although have in adverbial gerundive

constructions does not always do so--

> (119) Having seen Norman yesterday, I doubt that we
>   can trust him for the job.
> (120) *I have seen Norman yesterday.

I see no deep reason why this be to should occur only in the perfect.

The semantically very similar visit, see, and go have no such

restrictions on them; compare (117) with

$$(121) \quad \text{We} \left\{ \begin{array}{l} \text{visited} \\ \text{saw} \\ \text{went to} \end{array} \right\} \text{Vienna last year.}$$

So the constraint appears to be an arbitrary syntactic condition on this idiom, and therefore of a different character from other tests for the perfectivity of have, which are semantic in nature (for instance, the use of occurrence with adverbials like since Monday as a litmus for perfectivity). The be to test was discovered by chance and has no motivation I can see.

Central tests of unknown motivation illustrate how hard it can be to give an account of the intrinsic connection between tests and the properties they are criterial for, even though we can sometimes derive tests from theoretical principles or from generalizations about the grammatical structure of a language, so that the motivation of the tests is entirely clear.

Notes

1. I have heard this use of the term attributed to Zellig Harris in his classroom presentations, but I have not found it in his published writings, where the relatively neutral terms tests and criteria appear.

2. Though showing the direction of determination in such cases is not itself always an easy matter.

3. Significant amendments to this statement have been made by several writers, especially Bouton 1970 and S. Anderson 1971.

4. There is a similar list of tests in Ross 1973a: 96.

5. For lexical categories like N, V, and Prt, the properties in question have ordinarily been described by the use of features, or by decomposition into primes of some sort, whereas for construction types like NP, VP, and S, the properties have been described structurally, by some characteristic configuration of the constituents or by the presence of a special marker. Despite this difference in the mode of description, the two cases are similar (they are treated in exactly parallel fashion by Hudson 1976: ch. 2, building on the sort of systemic grammar outlined in Hudson 1971), and the same sorts of tests have been suggested for both.

6. Fillmore 1968 and J. McCawley 1968 both appear in Bach and Harms 1968, which is reviewed in Dougherty 1970b, and Postal 1966, G. Lakoff and Peters 1966, and Fillmore 1969 all appear in Reibel and Schane 1969, which is reviewed in Dougherty 1973, so that Dougherty 1974 is in some ways a return to familiar arenas.

7. This, too, has been disputed; see Yasui 1975.

8. In fact, I think several of their arguments are faulty. See Zwicky and Sadock 1975: 15 for a discussion of an invalid transformational potential argument in the Lakoff-Peters article. The Lakoff-Peters arguments that depend on disambiguation with quantifiers like each and both probably fail as well, in which case they would be cases of invalid special distribution arguments (Zwicky and Sadock 1975: 12-4).

9. And I share with J. McCawley a grave uneasiness about this use of the word 'revolution' as applied to the history of linguistics, at least insofar as revolutions in linguistics are supposed to be the sorts of things one is faithful to, abandons, leads counterrevolutionary movements against, and so on.

10. Bolinger 1968 lists a variety of examples in which context beyond the sentence is relevant to judgments of grammaticality.

11. As it happens, Ross arranges his hierarchies so that the northeast corner is the positive one, but this difference in mode of presentation affects no matter of substance.

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