The Syntax-Phonology Boundary and Current Syntactic Theories

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Two important modularity principles are generally regarded as implicit in standard TG theory:

> (I) The phonological component has no access to syntactic information except what is in the surface structure.

> (II) The syntactic component has no access to phonological information.

(I) is the Principle of Superficial Constraints in Phonology (PSCP) discussed by Zwicky (1970). It is implied by the standard view that the phonological component is an interpretive one, performing a transduction from the output of the syntactic component to the level of systematic phonetics. If surface structure is the input representational level for phonology, more abstract levels of the derivation can have no direct influence on phonetic interpretation.

(II) is the Principle of Phonology-Free Syntax (PPFS) explored by Zwicky (1969).

Numerous linguists have discussed phenomena that appear to them to call for either the PSCP or the PPFS to be weakened. Typically, it is argued that global constraints have to be permitted in grammatical descriptions: inaccessible syntactic information has to be made available to the phonological rules, or vice versa. Those who regard global constraints as irredeemably undesirable have attempted reanalyses of various sorts, attempting to utilize already available machinery of the standard theory to handle the facts without breaching the modularity assumpion that keeps the syntactic and phonological components separate.

The past few years have seen the emergence of theories that depart from the standard theory quite radically, in ways that have hitherto unexamined implications for modularity constraints. The most radical are the "monostratal" theories, which posit no syntactic level other than what standard theory would call surface structure. Generalized Phrase Structure (GPSG) is conceptually the purest of these proposals, in that it assigns the whole burden of syntax to a mechanism already admitted in standard theory: the phrase structure (PS) rules. Unlike the standard theory, such a theory necessarily entails both the PSCP and the PPFS in their strongest forms without any fine tuning. The PSCP follows since the surface syntax is the only syntax there is. The PPFS follows because the categorial component of the base operates in terms of categories and formatives and not in terms of any phonological primitives.

In this paper we explore the question of whether a theory that
directly entails the PSCP and the PPFS in unweakened forms should be
regarded as favored for that reason, or whether, in the light of the rather
extensive literature calling for relaxation of the PSCP or the PPFS or
both, a theory like GPSG that cannot admit such weakening should be
regarded as ipso facto suspect. This topic is, of course, a massive one.
We have been studying the corpus of alleged violations of the PSCP and the
PPFS for some time, and the number of relevant descriptive problems we have
encountered in different languages runs into the hundreds. In this paper
we shall call upon just two well known and representative case studies to
illustrate the view we propose to take.

1. The Principle of Superficial Constraints in Phonology

Perhaps the best known example of a problem area in English that
suggests that the PSCP is too strong concerns the phonological reduction of
English auxiliaries when unstressed. This was the main topic of Zwicky
(1970). It was recalled to the attention of linguists by the remarks of
King (1970), rediscovering somewhat more general observations by Sweet
(1908), and was set in the context of a theoretical debate by Lakoff
(1970). It is of interest, however, that in the light of the wide
acceptance of phonologically null surface syntactic constituents with no
phonetically realized effects by virtually all current schools of thought,
the original arguments have lost most of their force.

The phenomena, as is well known, appeared to involve phonological
perturbations--failure of certain unstressed items to assume a normally
sanctioned reduced pronunciation--that were due to the effects of trans-
formations that had moved or deleted material adjacent to the items in
question. A typical contrasting pair of examples is provided by I wonder
whether the party's at Robin's tonight, with contractible is, and *I wonder
where the party's tonight, with uncontractible is. But the advent of
traces, i.e. phonetically null elements appearing in surface locations
where transformations had introduced a 'gap' at an earlier stage, has
definitely altered the situation.

The remarks of Chomsky (1975, 117) concerning the claimed invalidity
of 'excessive power' arguments against his variety of 'trace theory' are
misleading in this conection. It is true that one cannot say that a theory
in which no movement transformations leave traces in any grammar is
inherently more or less powerful than a theory in which all movement
transformations leave traces in all grammars, since neither allows
parochial variation in the matter of whether traces are left by movements.
But given that traces are left by some rules, there is a real difference in
what phenomena can be readily described by rules that mention traces in
their structural descriptions.

The generalization that an auxiliary followed at one stage by a
constituent which is later moved or deleted cannot undergo a certain
phonological rule P (which is reminiscent of what seems to be going on in
English contraction, though it is not a fully accurate description) seemed
essentially uncapturable in the unvarnished standard theory. But once the
relevant locations in the surface tree are identifiable by a marker of any
sort that consists of syntactically or phonologically mentionable material,

the statement of such rules is straightforward, even if the resulting
statements are not notably explanatory. And the way is open for a somewhat
more explanatory formulation to be developed along similar lines (see
Selkirk 1972). Notice that it is not the case that Chomsky has in practice
eschewed language-particular rules that mention traces. One may be seen in
Chomsky and Lasnik (1977, 478, example 154), for example. Nor have others
overlooked this possibiity; see e.g. Sag (1978).

The other celebrated problem in English for the PSCP is the formula-
tion of the syntactic environment for the English Nuclear Stress Rule
(NSR). Bresnan's (1971) analysis crucially involves a rule of stress
assignment applying to representations that are (in some cases) present
only during the syntactic cycle on a given clause, so it can hardly be
claimed to be compatible with the PSCP. However, it seems to us that, for
a number of reasons, B's account must be rejected anyway.

To begin with, we think that there is a fundamental confusion inherent
in the remarks about 'normal stress' that permeate Bresnan's paper. Recall
that the NSR places a heavy accent on the final primary word-stress in the
sentence. Bresnan claims:

> This is, in general, the 'normal' intonation for an
> English sentence. There are, however, well-known
> classes of exceptions to this pattern. Final anaphoric
> pronouns do not normally receive primary stress:

$$\overset{1}{}$$
(2) Helen teaches it.

$$\overset{1}{}$$
*Helen teaches it.

> ('Normally' means 'excluding emphatic or contrastive
> stress'.) Nor do final indefinite pronouns normally
> receive primary stress:

$$\overset{1}{}$$
(3) The boy bought some.

$$\overset{1}{}$$
*The boy bought some.

Other anaphoric items, even when grammatically definite,
receive no 1-stress:

(4) John knows a woman who excels at karate,
$$\overset{1}{}$$
and he avoids the woman.

In what follows I will assume that, by some means or
other, anaphoric and indefinite elements are not assigned
primary stress, and generally I will ignore the stressing
of items which are not relevant to the point at issue.

The confusion we are pointing to is to think that there could be 'some means' by which anaphoric constituents could be identified and exempted from the operation of a stress rule. Lakoff (1972, 291) is quite right to point out that 'Anaphora...is not a lexical property. It is a syntactic-semantic phenomenon which can, and must, be specified independently of lexical idiosyncracies.' To see the difficulty, consider (1).

> (1)   Lord Threshingham has been singularly careless in his liaisons with servant-girls. What can we do about the bastard?

There is no way a stress rule could determine on the basis of the syntactic or semantic structure of the second sentence in (1) whether the bastard was anaphoric. We obtain a well-formed sentence whether we place heavy accent on bastard or on do (to mention only two possibilities). If bastard is accented heavily, the utterance will be interpreted by the hearer to suggest that the bastard refers to some entity not referred to in the earlier part of the sentence: an illegitimate son (presumably of Lord Threshingham's), to be precise. Or it can convey extraordinary exasperation with Lord Threshingham, in which case the bastard refers to Lord Threshingham. If bastard is not heavily accented, the bastard would be interpreted by the hearer to be anaphoric, i.e. to refer to an entity already introduced into the discourse. This could be an illegitimate son if one had been mentioned earlier in the discourse, or it could be Lord Threshingham, or anyone else recently mentioned and still salient. There is no finite limit on what we might need to know about the discourse of which (1) is assumed to be part in order for us to be able to predict whether the phrase the bastard should be read with low stress or not. (Such decisions are difficult enough that experienced actors often fail to see enough of the structure in their script, and read a line with a stress pattern that cannot possibly be correct given the full context.)

Bresnan's approach is essentially to identify a kernel class of sentences in which the stress is 'normal' and for which the rules of grammar to determine it operate without special circumstances obscuring them. We regard this approach as completely mistaken in principle.

But there are empirical difficulties with the rule system she advocates as well. Consider the following examples.

> (2)   a. I've already GIVen it to him.
>   b. #I've already given it TO him.
>
> (3)   a. You've already given it to WHOM?
>   b.##You've already given it TO whom?
>   c.##You've already GIVen to whom?
>
> (4)   a. Who have you GIVen it to?
>   b. Who have you given it TO?
>   c. Who have you GIVen to?

The capitalization indicates stress. Example (2a) is quite natural, while (2b), with a stressed preposition, is unnatural. In (3a) the only natural stress is on the wh-pronoun whom, the other possibilities in (3b) and (3c)

being extremely unnatural. From a source like the natural (2a), her analysis predicts that under wh-movement we would get the stress pattern seen in (4a). This is well and good. But it also predicts that the pattern in (4b) will have the same unacceptability as the completely unnatural (3b), and that the pattern in (4c) will have the same unaccept-ability as the completely unnatural (3c), and both predictions are quite incorrect. The hypothesis that stress patterns are preserved through transformational derivations is not supported by such cases.

Let us now turn to the cases on which Bresnan originally based her hypothesis about the ordering of the NSR, namely the cases discussed by Newman (1946), and analogous examples. The typical contrast is one like (5).

> (5) a. George has plans to LEAVE.
>   b. George has PLANS to leave.

Newman noted that where the stress is as indicated, the verb leave is read as intransitive (i.e. as 'depart') in (5a), but as transitive (i.e. as 'deposit, drop off, abandon') in (5b). We shall refer to this as the Newman effect. Bresnan's explanation for it is, in essence, that stress is placed on the final constituent of the VP in both (5a) and (5b), but in (5b) the stressed constituent is a wh-phrase (the object of leave) that is moved and then deleted by the rule that derives infinitival relative clauses.

An important example of a generalization missed by Bresnan (but pointed out to us by Ivan Sag) is that the Newman effect operates in (7) as well as (6):

> (6) Stacy has a proPOSal to incorporate.
> (7) Stacy has a proPOSal to be incorporated.

Both imply that a proposal will be incorporated into something. But if inCORporate(d) bears the sentence accent, the meaning changes (Stacy proposes to become a corporation):

> (8) Stacy has a proposal to inCORporate.
> (9) Stacy has a proposal to be iNCORporated.

For (6), Bresnan's theory postulates a postverbal NP in cyclic structure that absorbs nuclear stress. But the passive analog (7) is treated in a completely different way (see Bresnan 1972:328-9, essentially acceding to the point made by Berman and Szamosi 1972:307). Hence Bresnan's account does not seem optimal (a welcome conclusion for Bresnan, who now advocates a theory with no syntactic cycle; cf. Bresnan 1982). It is encouraging that accounts are now being advanced--see in particular Culicover and Rochemont (1983)--in which sentence stress is not predicted directly from syntactic structure.

2.   The Principle of Phonology-Free Syntax

Whether the PPFS is implicit in standard TG is a matter that depends on the rather confusing question of how exactly lexical insertion is supposed to operate in TG. It is probably assumed by many linguists that the PPFS is entailed by the definition of transformational rules, since transformations are assumed to be able to refer only to categories (like NP or V) and formatives (like you in Imperative Subject Deletion or there in There-Insertion), but not to details of the internal phonological composition of formatives.

The matter is obscured by an error in Chomsky (1965). The lexical insertion algorithm Chomsky gives (1965, 84) reads as follows:

If Q is a complex symbol of a preterminal string and (D, C) is a lexical entry, where C is not distinct from Q, then Q can be replaced by D.

This formulation substitutes phonological matrices for complexes of syntactic and semantic features at deep structure, with the result that transformations have access to the phonological shape of formatives but not access to syntactic features or even categories (and the semantic component has no access to semantic properties of lexical items). This is apparently a mistake, as was pointed out by both Brekle and Luelsdorff (1975, 376) and Hudson (1976, 90). As Hudson observes, we can safely assume that the way the standard theory is supposed to work is that the phonological shape D is appended to the syntactic/semantic feature complex C, and that although phonological shapes of formatives are henceforth present in syntactic representations, they are rendered inaccessible to the operations of transformations, which are permitted to analyze only the syntactic information contained in the complex symbols that label the nodes.

Hudson (1976) argues quite sensibly that a modification should be introduced that has only syntactic and semantic information inserted at deep structure, phonological and morphologcal details being added at surface structure. This might seem to be sailing dangerously close to the generative semantic wind, in that it makes lexical decomposition in the syntax much easier to handle. But later we find Chomsky and Lasnik (1977) proposing 'lexical insertion at surface structure' anyway, so Hudson's idea cannot have been totally heretical even from Chomsky's standpoint. Provided something like Hudson's revision is adopted, or that transformational rules are simply blinkered by stipulation to make phonological representations invisible to them, the PPFS will be entailed by standard TG.

While it would be possible, through only slight tampering with standard TG, to permit transformations to inspect details of phonological representations attached to nodes (and thus to formulate, e.g., a rule to front phrases that begin with a bilabial stop), the definition of PS rules excludes such a possibility. A PS rule of the form A --> W, where A is a syntactic category label and W is a string of terminals and/or nonterminals, can pick out an individual formative that happens to begin with a bilabial stop and stipulate that it be the first element of W, but it cannot quantify over the entire stock of such formatives. If a terminal is

mentioned first in W, only that item will be picked up, while if a non-terminal is mentioned, all members of that category will be picked up regardless of their phonological composition. Even a list of rules that included one for each lexical item beginning with a bilabial stop would not achieve the effect of fronting all [p]-initial and [b]-initial constituents once we consider the fact that the lexicon is in effect open (e.g. there is no limit to the number of possible proper names beginning with [b]). The list approach would not embody the claim that all newly coined names beginning with [b] would also determine fronting. And the various schemata and other devices for capturing syntactic generalizations in GPSG merely have the effect of stating sets of ordinary PS rules more compactly. They do not alter the character of the operations that can be performed by PS rules.

However, there is a possibility inherent in TG that is inherent in exactly the same way in GPSG. Given the availability of syntactic features and the possibility of lexical redundancy rules (LRR's) being conditioned by phonological properties, there would be legal analyses capable of obtaining the result that all phrases beginning with bilabial stops appear together (as a group) at the beginning of their clauses. A simple statement of such an analysis can be devised using the ID/LP format of Gazdar and Pullum (1981).

We first state an LRR to assign a feature [+F] to all and only those lexical items that begin with a bilabial stop. It is not too hard to develop an explicit statement of the LRR. Let FORM be a function of one argument that applies to a lexical item and returns its phonological representation (a string of feature matrices). Let NONDISTINCT be a function of two arguments (both quoted strings of feature matrices) that returns TRUE if its first argument is nondistinct from its second argument in the usual sense: two feature matrices (not necessarily fully specified) are nondistinct if neither has a value V_1 for a feature where the other has a different value V_2 for that feature. Let VALUE be a function of two arguments returning the value that its first argument (an item) has for its second argument (a feature). The LRR could then be stated as follows:

(NONDISTINCT("[+anterior, -coronal, -continuant][...]*", FORM(@)) = TRUE) <--> (VALUE(@, F) = +)

Second, we state a feature-percolation convention that requires the feature [+F] to be present on any node that has a [+F] daughter constituent. The feature [+F] will then percolate from a lexical item with this feature all the way up to the root node. Third, we assume an LP statement in the grammar that says "@[+F] < $[-F]", where @ and $ are universally quantified variables ranging over the nonterminal vocabulary. Regardless of what ID rules we have for stating what constituents can appear in S, the only linearizations that the LP statement just mentioned will admit are those that put [+F] constituents leftmost.

We are therefore able to construct, even in phrase structure terms, an analysis that positions a constituent syntactically according to whether its initial lexical item begins with a bilabial stop or not--a paradigm case of a PPFS violation. And clearly we could construct such an analysis within TG as well, even within a version of TG that was set up to deny

transformations access to phonological form; an obligatory fronting transformation would be stated in terms sensitive to the syntactic feature [+F]. Two questions arise: whether we should forbid such analyses, and whether we can.

We take the position that an analysis along the lines just sketched should indeed be excluded. We shall argue that linguistic theory should not permit any LRR to predict a syntactic property on the basis of a phonological one. However, this raises the second question: Is such a restriction too strong? Are there any sets of facts that clearly and uncontroversially call for analysis in terms of an LRR of the type we plan to prohibit?

Although many cases from different languages could be discussed in this connection, we shall again take just a familiar case from English: inflectional versus periphrastic degree marking in adjectives. There is a traditionally recognized and apparently phonology-related generalization distinguishing the adjectives like nice, which accept the -er and -est suffixes (nicer, nicest), and those like gorgeous, which do not (*gorgeouser, *gorgeousest) and therefore have to take the periphrastic comparative and superlative markers (more gorgeous, most gorgeous). To put it very roughly, the adjectives in the former class are shorter and those in the latter class are longer, and length of words is assessed in terms of phonological rather than syntactic units. Here is the account of the generalization offered in slightly more precise terms by Jespersen (1933, 222).

> Comparatives in -er and superlatives in -est are formed freely from monosyllables and from words of two syllables ending in a vocalic sound (e.g. pretty, narrow, clever) or in a syllabic l..., or else having the stress on the last syllable (polite, severe)...But with all longer words, especially if ending in a hard group of consonants, these endings are avoided, and comparison is effected by means of preposed more and most...

Not only does this (slightly abridged) summary make it look as if phonological considerations are playing a role in the syntax of comparatives and superlatives, the facts have actually been cited as evidence that a theory that allows for some flexibility in the matter of syntax-phonology relations is ipso facto favored over more stringent alternative theories. Huddleston (1973, 353) criticizes stratificational grammar for being too restrictive in this domain:

> ...in English we shall need to distinguish in the lexotactics and/or morphotactics between adjectives like tall which take the comparative suffix -er, and those like beautiful which take more: within the SG framework the classes are entirely arbitrary at these grammatical strata, for the theory does not allow any references to phonological syllable structure at this point. Examples of this sort seem to me to present quite compelling evidence against the stratificational hypothesis: the theory is based on an assumption of a much greater independence of semantic, grammatical (or syntactic) and phonological phenomena than can be empirically justified.

We disagree with Huddleston. We believe that the rigidity of strati-
ficational grammar on this point ought to count in its favor, and likewise
for other frameworks that do not countenance the statement of correlations
in the phonology-to-syntax direction. We will argue that the traditional
phonological generalization does not hold up under scrutiny. There will be
some variation between individual speakers in the data we cite, but we
believe it is straightforward to show for any idiolect of Enlgish that
purely phonological conditioning is not operative.

First, it is not true that monosyllabicity is a sufficient condition
for inflectability in adjectives. We find the following examples all
ungrammatical:

(10) a. God is coming; and She's $\left\{ \begin{array}{l} \text{*never been pisseder} \\ \text{*the pissedest she's ever been} \\ \text{never been more pissed} \\ \text{the most pissed she's ever been} \end{array} \right\}$.

   b. The experience $\left\{ \begin{array}{l} \text{*seemed realer when I took the drug} \\ \text{*was the realest I'd ever had} \end{array} \right\}$.

   c. The $\left\{ \begin{array}{l} \text{*scaredest} \\ \text{*scareder} \end{array} \right\}$ ones can stay behind.

   d. Look for $\left\{ \begin{array}{l} \text{*a mainer route than this one} \\ \text{*the mainest route you can find} \end{array} \right\}$.

   e. She $\left\{ \begin{array}{l} \text{*looks iller than he does} \\ \text{*is the illest of all of them} \end{array} \right\}$.

   f. I wish I $\left\{ \begin{array}{l} \text{*felt weller} \\ \text{*was the wellest man in the crew} \end{array} \right\}$.

   g. Your solution is $\left\{ \begin{array}{l} \text{*even wronger} \\ \text{*the wrongest} \end{array} \right\}$.

   h. The laws of the land $\left\{ \begin{array}{l} \text{*should be juster} \\ \text{*are the justest} \end{array} \right\}$.

Second, it is not of course true that monosyllabicity is a necessary
condition for inflectability. We find hundreds of forms such as those in
(11).

(11)   nasty   nastier   natiest
        obscure   obscurer   obscurest
        stupid   stupider   stupidest
        noble   nobler   noblest
        severe   severer   severest

Sweet (1891: 326-327) suggests a number of generalizations governing
which adjectives inflect and which do not, but they are not watertight.
The problem is that for each of the subclasses he refers to we can find
both members that inflect and members that do not. Some examples follow.

(12)   Words ending in $C_0 V(\underline{r})$

        Inflectable: bitter   bitterer   bitterest
                        tender   tenderer   tenderest
                        slender   slenderer   slenderest

Uninflectable:	eager	*eagerer	*eagerest
	proper	*properer	*properest

(13)   Words ending in $V:C_0^1$:

Inflectable:	obscure	obscurer	obscurest
	polite	politer	politest
Uninflectable:	afraid	*afraider	*afraidest
	unreal	*unrealer	*unrealest
	alone	*aloner	*alonest
	unkempt	*unkempter	*unkemptest

Even when we move to trisyllabic adjectives, we cannot say that
inflection becomes impossible. Many trisyllabic adjectives with the
negative prefix un- take adjectival inflection; but again, there are others
that do not:

(14)   Trisyllabic adjectives

Inflectable:	unlikely	unlikelier	unlikeliest
	unwieldy	unwieldier	unwieldiest
Uninflectable:	uncertain	*uncertainer	*uncertainest
	unlawful	*unlawfuller	*unlawfullest

Thus the division of adjectives into inflecting and periphrastic subcate-
gories turns out to be a matter of arbitrary lexical conditioning. The
tendency for one subcategory to contain shorter stems than the other is
explicable historically and is not grammatically relevant.

We have found that this sort of situation is typical of the various
putative phonologically constrained LRR's that have been suggested for
English or other languages. We are therefore inclined to think that LLR's
of the form "$\varphi \supset \psi$", where φ involves a phonological or phonetic predicate
and ψ a syntactic one, should be disallowed in principle. This would mean
that descriptions of languages with (for example) a productive preposing of
phrases beginning with [p] or [b] would be completely excluded if grammars
were phrase structure grammars. We think this is the right result.

3. Conclusion

Our conclusion from this brief review of two familiar descriptive
problems in English is that a monostratal syntactic theory like GPSG might
well be formalized in such a way that it entailed both the PSCP and PPFS in
their strongest forms, and that on presently available evidence this must
be regarded as a point in favor of such theories. It should go without
saying, however, that there is a large amount of work to be done in
developing adequate GPSG analyses of the kind of phenomena at the syntax-
phonology interface that have been held to provide evidence for the

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