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 assumption 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. Generalised 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 transformations 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 connection. 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 possibility; see e.g. Sag (1978).

The other celebrated problem in English for the PSCP is the formulation 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:

(2) Helen teaches it.

*Helen teaches it.

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

(3) The boy bought some.

*The boy bought some.

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

(4) John knows a woman who excels at karate,

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 unacceptability 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:

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

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 morphological 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 \( A \rightarrow W \), 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

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 V₁ for a feature where the other has
a different value V₂ 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:

\[
\text{NONDISTINCT}(["+\text{anterior}, -\text{coronal}, -\text{continuant}][...]*", \text{FORM}(@)) = \text{TRUE}) \leftrightarrow (\text{VALUE}(\$, F) = +)
\]

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 1..., 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 English 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 *never been pissed
    *the pissedest she's ever been
    never been more pissed
    the most pissed she's ever been

b. The experience { *seemed realer when I took the drug },
    *was the realest I'd ever had

c. The { *scaredest } ones can stay behind.
    *scareder

d. Look for { *a mainer route than this one },
    *the mainest route you can find

e. She { *looks iller than he does }
    *is the illest of all of them

f. I wish I { *felt weller }
    *was the wellest man in the crew

g. Your solution is { *even wronger },
    *the wrongest

h. The laws of the land { *should be juster }
    *are the justest

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 natieest
    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

Inflectable: bitter bitterer bitterest
tender tenderer tenderest
slender slenderer slenderest
Uninflectable: eager *eagerer *eagerest
       proper *properer *properest

(13) Words ending in V:C^1_0:

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 trisyllable adjectives, we cannot say that inflection becomes impossible. Many trisyllable 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 subcategories 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 LRR's of the form "$\varphi \rightarrow \psi $", where $\varphi$ involves a phonological or phonetic predicate and $\psi$ 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 monosyllabic 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
necessity of weakening one or the other of these constraints. Our position is that there are prospects for success in this work, not that the work has already been done.

References


