

The Principle of Phonology-Free Syntax:
Introductory Remarks*

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0. **Introduction.** The thesis we examine here is that the phonological rules and the syntactic rules in the grammar of a natural language work independently, and that all the linguists who have proposed some kind of "interpenetration" between the two domains have been in some degree mistaken. We are interested in articulating the strongest version of this informally stated position that can be convincingly maintained, and in conducting a critical examination of essentially all the linguistic literature we are aware of that contains arguments against our position.

Why we think it is important to do this will be explained in the next few sections, but first we must state more exactly what it is that we oppose.

The simplest explication of what the syntactic and phonological parts of a grammar are supposed to do would say that the syntactic component determines the order in which words may be placed in sentences and the grammatical structures associated with particular orders and combinations of words, and the phonological component determines what pronunciations are associated with particular structured sequences of words that the syntax says are well-formed. Although we think this simple explication is basically correct, the trouble with it is that it makes our position sound like a truism, as if no one could possibly disagree with it. But in fact dozens of linguists have disagreed with it, especially since interest in the precise specification of the form of grammars and the interaction of their rules first became a dominant feature of linguistics in the late fifties, and hundreds of descriptive problems have been exhibited whose solutions seem at first to involve violations of our thesis.

The problematic classes of data we are referring to are those that suggest that facts about how a word is pronounced are influencing the operation of the rules that determine grammaticality. More technically, they suggest that certain syntactic rules have to make reference to properties of linguistic units that receive their interpretation in terms of phonetic predicates. We wish to argue that in such cases, appearances are always misleading, and in fact no syntactic rule ever refers to a phonetically defined property.

Even when our view is expressed in these slightly more technical terms, it may sound rather self-evident. Katz and Bever (1976, 28n), for instance, suggest that phonetic conditioning of deletion rules ought to be ruled out in principle as a matter of philosophy of science:

...there should be meta-principles that determine the kinds of empirical events a formal structure in the grammar can be associated with. For example, it might be claimed that semantic correspondence principles can only connect grammatical formalisms to language universal cognitive structures, while phonological correspondence principles must often link grammatical formalisms to language specific articulatory configurations. Further, there ought to be constraints that determine conditions under which the same formal structures can be referred to by different kinds of correspondence principles. For instance, it would clearly be absurd if one principle connects ellipsis to the operation of erasure transformations as in most discussion, but another principle allowed mapping rules that correlate phonetic structures with physiological properties of the vocal tract to apply prior to such erasures.

Yet it is not the case that such "clearly absurd" practices as assigning phonetic structure like intonation contours at a derivational stage that precedes the application of deletion transformations have seemed absurd to everyone. Bresnan (1971), Langacker (1970), and Pope (1971) all argue for analyses that involve exactly this kind of interpenetration of syntax and phonology, while maintaining that deletion is a purely syntactic matter. It is our intention to examine the arguments and evidence that lead to such analyses, and to formulate the closest empirically supportable approach to the view that Katz and Bever take on aprioristic grounds.

In this article we make our main proposals explicit, locate them within the larger fabric of linguistic theory and metatheory, and sketch our strategy for defending them. Section 1 specifies that we are investigating the theory of grammars, not a theory of the linguistic activities of individuals or groups; the enterprise is linguistic in a narrow sense, not psycholinguistic or sociolinguistic. Section 2 introduces the *interface program* for the theory of grammar, within which our specific proposals about the interaction of syntax and phonology are situated. According to the interface program, a grammar is composed of a number of autonomous components, interacting with one another in limited ways. In the course of this discussion we state both the hypothesis that is our main object of interest, the Principle of Phonology-Free Syntax (PPFS), and also an important related hypothesis, the Principle of Superficial Constraints in Phonology.

Section 3 examines putative counterexamples to the PPFS. We first sketch the set of components we assume in addition to syntax and phonology. Fleshing out the idea that syntax and phonology are independent of one another leads us to propose a fairly rich collection of components that can be said to lie "between" syntax and phonology. And finally we provide a taxonomy of apparent violations of the PPFS.

1. **Languages, grammars, and speakers.** For the most part, we are following a large corpus of assumptions about linguistic analysis and theory that has emerged from a quarter century of generative grammatical work and the long tradition of language study preceding it, and we have little to say about metatheoretical issues like the relations between linguistics and the psychology of language. However, we are clearly

presupposing a great deal already in speaking of "grammars," "languages," "rules," and so on; we therefore make a few remarks of a methodological nature here.

To begin with, we must make it clear that we do not regard the study of human language as identical or reducible to the study of the organism that uses it. A straightforward reading of some of the statements of generative grammarians who have talked about "the rules of mental computation" (Chomsky and Lasnik 1977) and "the biology of grammars" (Lightfoot 1982) might suggest that not everyone makes such a distinction, but we believe that in practice virtually every serious student of linguistics does and must make it.

It is commonplace to agree that properties of the human organism such as its striking limitations of short-term memory or its susceptibility to speech production errors must be omitted from the idealized model of human behavior relative to which we evaluate linguistic theories. A theory of English grammar and semantics that characterizes (1) as grammatical, and true of the sequence "85327," is not generally taken to be false simply by virtue of the fact that English speakers appear bewildered when they are confronted with (1). This, if it is accepted, is enough to establish that there can be facts about what speakers do with their languages that are not facts about those languages themselves.

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It is, of course, quite difficult to tell from raw data of acceptability whether we have hit upon an indication that some rule of grammar is in effect or whether we have discovered some regularity in what the speakers of the language actually do in certain situations. But if we keep very clearly in mind what we mean by the terms "language," "grammar," and "speaker," though there will still be occasions on which we are uncertain about matters of fact, we will at least not be tempted into the incoherence that results from failing to distinguish the study of languages from the study of language users.

A language is a specified collection of objects (sentences), each of which is a structured sequence of other objects (words) and each of which has associated phonological and semantic properties. A grammar is some algebraic construct interpretable as a definition of such a collection. A grammar, in the broadest sense, constitutes a theory of the logic of the relation between the meanings and the syntactic and phonological forms of the expressions of the language. Like any theory, a grammar is not something that can be located in time or space. And this is all the more true of a language, since it exists only as the collection whose membership is defined by a particular grammar.

Speakers, however, exist in space and time. They have lifespans, sizes, birthdays, whims, beliefs, and imperfections. It ought to be very clear, therefore, when we have stopped talking about the abstract system of a language or a grammar and begun talking about the speakers of a language and what they do. But in fact many proposals have been advanced in linguistics that involve a failure to make this distinction. Perhaps

the most notable is the development of the theory of "variable rules" by Labov (1970, sec. 2). If a speaker of a language in which the copula is optionally omissible chooses to omit it in 63% of cases when talking to a stranger and in 95% of cases when talking to friends, that is a potentially interesting fact about the speaker's interaction patterns, but it is not a fact about the language or the grammar in our terms, and cannot be. It concerns a speaker who interacts with other people at specifiable times in definite locations and chooses one alternant rather than another with some determinate frequency.

Bickerton (1971) and Gazdar (1976), among others, have correctly criticized the theory of "variable rules" for building such notions into the grammar. The point here is *not* that linguistic variation lacks system or structure, or that theories of variation are impossible; rather, the claim is that several distinct, but interlocking, theories are needed if we are to make sense of what happens when people talk.

The reason we stress the distinction between grammars and speakers at this point is that important decisions made below will depend on it. In particular, we will need to draw a distinction between *rules of grammar* and *tendencies* in dealing with a wide variety of sets of facts; and we will need to distinguish *rules of grammar* from other regularities in the *linguistic practices* of speakers.

The statement of a rule of grammar will be algebraic, not statistical, and will concern the definition of abstract objects that have no temporal or spatial existence. The statement of a tendency will have the exactly converse properties, being a statistical statement about actual language users in real situations. The statement of a linguistic practice might be statistical or algebraic, but in either case it requires essential reference to situations of utterance or to the intentions of interactants, usually to both; see Morgan (1978) and Green (1982) for arguments that statements about linguistic practices should be distinguished from rules of grammar.

Consider, to illustrate these points further, the influence of the unlikely category of chronological age on grammatical agreement. Consider first the case of Hindi. As the age of a person increases, it gets more and more likely that others will refer to that person by means of the plural pronoun *wee* rather than the singular pronoun *woo* in Hindi. But this turns out to be because chronological age is (in general) correlated with respect, in the sense that greater respect (in the special linguistically relevant sense at least) is shown to the old than the young within the Hindi-speaking culture. The grammatical device of using plural concord features is exploited in Hindi as a kind of surrogate honorific system.

We would say that the correlation between the age of a person and reference to that person by means of a plural pronoun represents a tendency, not a rule (because the regularity is statistical), and a linguistic practice, not a rule of grammar (because a contextual factor, the age of the person referred to, is a term in the regularity, and because an intention on the part of the speaker, to express respect for the person referred to, is a condition on the choice of pronoun). The rules of grammar involved, for example, the rule of subject-verb

agreement, are concerned with definite, nonstatistical matters like whether the features on the verb match those on the subject NP, and nobody's age affects them at all. The grammar of Hindi specifies only that the two pronouns are among those available in the language, and that particular pronouns require agreeing forms elsewhere in sentences.

It is instructive to compare this situation with a related one in Achenese (spoken in Indonesia; see Lawler 1977). In Achenese, a verb takes a subject agreement inflection determined partly by whether the referent of the subject NP in question is younger or older than the speaker. While the determination of a person's chronological age is an empirical matter, of course, the grammar of Achenese is perfectly clear and definite: if the subject NP in a clause is a pronoun from the "older than speaker" set, only the "older than speaker" verb agreement inflection is grammatical, and correspondingly for the "younger than speaker" set. With nonpronominal subject NPs, the verbal inflection used is the one appropriate to the pronoun one would use to refer to the same entity (a choice that a communicatively competent speaker will base on chronological age insofar as the requisite data is available). But there is no tendency for age of a person to affect verbal inflection in sentences mentioning that person. Instead, the grammatical rule of subject-verb agreement, which is precise and nonstatistical, refers to a grammatical category which, as a matter of linguistic practice, is employed in a way that relates directly to chronological age.

2. **The interface program.**¹ Our model of syntax-phonology relations derives from a set of more general assumptions about the nature of language structure (and therefore also about the nature of grammars) that are familiar from the work of Chomsky. Hale, Jeanne, and Platero (1977) provide a convenient summary of what they term the Autonomous Systems view of language structure: "According to this view, language consists of a number of distinct systems, each possessing inherent principles of organization that are essentially independent of factors relating to any other linguistic system or to extralinguistic considerations" (379). We perceive three distinguishable assumptions here, and although Hale et al. speak about "language," the assumptions seem to us to be about the form grammars must have if they are not to misrepresent the complexity of language.

The first assumption is of *modularity*: a grammar consists of a number of modules; we shall refer to these as *components*.

The second assumption is that these components are *nonuniform*, distinct from one another in the sense that the representations and internal organization appropriate for one component in a grammar will in general be different from the representations and internal organization appropriate for another. We will make the standard assumption that each component functions to relate a small number of types (usually two) of linguistic representations, which we will call its *terminal representations*.

The third assumption is that each component is *autonomous*, independent of all the others, in the sense that aspects of one component will not depend upon factors appropriate only to another. What this assumption means for grammars is that rules in one component of a grammar

cannot be contingent upon representations, rules, or rule operations available only in another component. Still, the components must be related to one another in some fashion. Assuming that the components are autonomous restricts these interactions between (or among) components to mediation via their terminal representations, which then serve as interfaces between components.

The assumptions of modularity, nonuniformity, and autonomy characterize what we will term the *interface program* (IP) for grammatical theory. As a research program, the IP has implications extending well beyond the question of syntax-phonology interactions. It should not in our view be regarded as highly controversial. Much theorizing about the nature of grammar in the past half-century has implicitly incorporated some version of the IP.

2.1. **Rejections of the IP.** We should make it clear, however, that the application of the IP in the area of syntax-phonology interactions has been challenged on many occasions by linguists working in a wide range of different frameworks. Baker and Brame (1972, 54) raise the matter rather tentatively:

It is conceivable that the Aspects theory is incorrect in maintaining a strict separation between syntactic rules on the one hand vs. morphological and phonological on the other.²

Hall and Hall (1970, 49) regard it as clear that as early as 1970 linguists had found empirical reasons for questioning the separability of syntax and phonology (though they cite no references):

In generative-transformational grammar, morphophonemic rules are considered low level rules which are applied only after the entire cycle of syntactic derivation. However, recently, problems in the handling of data from various languages have required solutions which call into question the strict hierarchical ordering of rules.

Hudson (1976, 115) goes yet further, implying that regularities in grammar whose statement violates the autonomy of the syntactic and phonological components are quite commonplace:

Interlevel regularities are very easy to find, and can link any pair of levels.

And other writers have gone much further, rejecting the IP quite categorically. Thus Hetzron (1972, 251-2) summarizes the main thesis of his paper in these terms:

...there are syntactic rules which must apply *after* some phonological information has become available. ...[T]here is no clearcut boundary between syntax and phonology. There exists a certain amount of osmosis between the two domains.

And similarly, Awbery (1975, 24) argues that

The interface model [is] inadequate. An interlocking model is required which will allow phonological rules to reach further back in the derivation and mix with purely syntactic rules. The syntactic and phonological components cannot, on this view be neatly separated out. Instead there is a transition zone in the derivation where both syntactic and phonological rules are relevant.

Tegey (1975, 571) is just as explicit in his claims:

...a strict separation of phonological from syntactic processes is not possible. In fact,...the usual assumptions of current linguistic theory that phonological processes apply after syntactic ones and that syntactic (transformational) rules need make no reference to phonological information cannot be maintained.

Rivero and Walker (1976, 100) speak in strikingly similar terms:

Standard generative approaches to the structure of a grammar require that all syntactic operations take place before the application of any phonological rules and that phonological considerations do not constrain transformations. Evidence has accumulated, however, to show that this is too strong a restriction on syntactic rules. Syntax utilizes information created by the rules of the phonological component.

An even more extreme claim is that of R. Lakoff (1974, XVIII-40). She asserts that in the theory she calls "generative semantics":

Very simply, there is no separation of levels: a single, highly abstract, underlying structure underlies the semantics, the syntax, and the phonology, and further, syntactic information may be used in the statement of phonological or semantic rules, and conversely.

And this undifferentiated theory of rule interaction has by no means seemed as undesirable to everyone as it does to us. Traugott (1977, 90), for instance, appears to regard the position Lakoff takes as both desirable and uncontroversially established as correct:

Particularly promising for pidgin and creole studies is the fact that, in keeping with some of the most recent work in linguistics, dynamic wave theory is based on a theory of language that insists that the structures of language can themselves not be forced into totally discrete categories. Just as no absolute boundaries exist between a trade jargon and a pidgin, none exist between semantics, syntax, phonology, and lexicon.

2.2. **Expressive power.** Views in which no demarcation at all is accepted between (say) the syntactic and phonological components of a grammar are relatively far from the center of the spectrum (though not by any means uncommon; we can quote many others in connection with specific issues relating to the syntax-phonology interface). In general, however, it is clear enough why there have been moves toward abandoning autonomy assumptions. Autonomy assumptions are a widely accepted way of restricting the expressive power of grammars (a topic we return to in

section 2.4.3 below), and thus natural candidates for being relaxed when ways are sought to extend such expressive power in the face of empirical evidence that cannot be otherwise accommodated.

A simple example of a move of this sort is the relaxing of assumptions about the syntactic and semantic components that introduced the variety of transformational generative grammatical theory known as the Extended Standard Theory. Active-passive pairs like *Many arrows didn't hit the target* and *The target wasn't hit by many arrows* were perceived to have different understandings. The basic semantic roles of *arrows* and *target* were unchanged, but the relative scope of the quantifier and the negation element seemed to be different. Therefore, relaxing the then-current autonomy assumption that permitted only the deep structure to be input to the semantic rules, it was proposed that the deep structure and the surface structure should both be scanned by semantic rules (see Chomsky 1972a, 103-6). The analogous relaxing of autonomy within generative semantics was the introduction of derivational constraints sensitive to both deep and surface structure (G. Lakoff 1971, sec. 2). In both cases, no a priori undesirable weakening of linguistic theory would attend the relaxing of autonomy assumptions if the access to different levels possessed by different rules could be shown to be prescribed on a universal basis; but in fact both Chomsky and Lakoff based their arguments entirely on English facts, so in practice, though not in principle, their proposals are examples of theory-weakening through the relaxation of autonomy assumptions.³

Given the possibility of increasing expressive power by removing autonomy restrictions, it is possible to regard permeability of the syntax-phonology dividing line as an advantage of any theory that exhibits it. And, indeed, we find Huddleston (1973, 353) criticizing stratificational grammar, which adheres to the IP and entails a very rigid dividing line between syntax and phonology, and citing the ability of TG to handle interlevel generalizations as a positive point:

Within a TG framework [interlevel] generalizations can be expressed by means of redundancy rules in the dictionary, but [stratificational grammar] does not allow for their expression... Examples of this kind 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.

Examples Huddleston cites include the selection of *more* and *-er* as the comparative marker in English according to the number of syllables in the compared adjective and the phonological reduction of auxiliary verbs in English when they are not followed by a movement or deletion site. We do not regard these cases as constituting empirical justification for relaxing autonomy assumptions; see our discussion in Pullum and Zwicky (1984). We see it as essential to permit such relaxations only to the minimum degree possible. Our strategy, therefore, will be to search for potential counterexamples to claims inherent in the IP, but also to examine such potential counterexamples with care and in detail. What we hope to be able to show is that in all the hundreds of cases of putative counterevidence that can be found, the

best analysis of the facts is actually compatible with the IP. To the extent that this cannot be shown in particular areas, it will be our goal to amend the model as conservatively as possible, so as to extend expressive power only as far as the facts force us to, rather than accepting the counsel of some of the quotes given above and allowing interlevel relationships of any sort whatever to be stated by grammatical rules.

2.3. **The PFFS and PSCP.** The specific proposal within the IP that we are defending is that the theory of grammar prescribes that syntax and phonology constitute autonomous components. This is a claim about the grammars of all languages.

But how can a claim of autonomy be defended or attacked? According to the discussion in Zwicky (1984), which we will not reproduce here in detail, if two components are autonomous, then we expect no forward interactions between them, no backward interactions between them, and no duplication of principles between them. And we expect them to be nonuniform.

For the syntactic and phonological components, the hypothesis that there are no forward interactions is the Principle of Phonology-Free Syntax (PFFS) of Zwicky (1969):

- (2) PFFS: No syntactic rule can be subject to language-particular phonological conditions or constraints.

Although a great many potential counterexamples to the PFFS have been put forward, it is our thesis that the PFFS can be maintained in its strongest form, which has guided most research on grammatical theory (in a variety of theoretical frameworks) during this century and is in fact *required* by other assumptions within certain of these frameworks (as we argued in Pullum and Zwicky 1984).

For the syntactic and phonological components, the hypothesis that there is no backward interaction is the Principle of Superficial Constraints in Phonology (PSCP) of Zwicky (1970):

- (3) PSCP: The only syntactic conditions or constraints on phonological rules are those referring to surface structure.

Like the PFFS, the PSCP has been subject to many challenges; and like the PFFS, the PSCP has guided grammatical research in a variety of theoretical frameworks, in some of which it is a necessary consequence of other assumptions (again see Pullum and Zwicky 1984). Here too we maintain that the autonomy of syntax and phonology can be defended against apparent counterexamples; Kaisse (1985) examines a number of these.

Now consider the degree to which syntax and phonology exhibit nonuniformity. Several types of principles that are not obviously part of syntax--notably, those concerned with the placement of clitics and with the internal organization of morphemes within words--seem "syntactic" in character; in particular, they often assign hierarchical structures analogous to constituent structures in syntax (hence the title, *The*

Syntax of Words, of Selkirk's 1982 book on morphology). On the other hand, these very same types of principles also seem to have a phonological cast to them: word formation rules can be constrained to apply only to morphemes of certain phonological shapes, and (in the case of infixation and reduplication at least) they can perform phonological operations; cliticization rules are often claimed to be subject to at least one sort of phonological constraint (lack of stress), and there are phenomena that at first glance look like "endoclitics" (Zwicky 1977), parallel to infixes. It is clear, then, that word formation rules have some properties of syntax and some of phonology, and it is at least arguable that cliticization rules do too. One might be tempted to conclude, therefore, that syntax and phonology are not totally nonuniform.

Word formation and cliticization also seem to be interactionally intermediate between syntax and phonology. Although many problematic cases have been put forward, there is at least some support for all of the following generalizations, which locate both word formation and cliticization after syntax and before phonology:

- (4) Cliticization after syntax: No syntactic rule must crucially apply after some cliticization rule.
- (5) Word formation after syntax: No syntactic rule must crucially apply after some word formation rule.
- (6) Phonology after cliticization: No cliticization rule must crucially apply after some phonological rule.
- (7) Phonology after word formation: No word formation rule must crucially apply after some phonological rule.

Like the PPFs and PSCP, these generalizations are by no means universally accepted (though we would propose that when (4)-(7) are properly formulated, they are correct). Proposal (4) is implicitly rejected in many analyses of clitic phenomena. Proposal (5), the Generalized Lexical Hypothesis of Lapointe (1980)—"No syntactic rule can refer to an element of morphological structure"—was conspicuously rejected in early transformational grammar. Proposal (6) goes against standard assumptions about phonology and cliticization, which often have cliticization contingent on lack of stress. Proposal (7), which prohibits word formation rules that are conditioned or constrained by derived phonological representations, is the orthodox assumption of generative grammar, though it is relaxed in level-ordered phonology (Kiparsky 1982).

2.4. Metaconsiderations. We now collect our metatheoretical reasons for favoring the IP, and for defending the PSCP and PPFs in particular: because these assumptions are valuable as part of a research strategy (section 2.4.1), because they fit with proposals about the nature of the syntactic component (2.4.2), because they can contribute to limiting the expressive power of grammatical theory (2.4.3), and because they are compatible with hypotheses about modularity in domains other than grammar (2.4.4).

2.4.1 **The IP as a research strategy.** A research strategy built on the supposition that a grammar consists of a number of components interfacing with one another in limited ways gives rise to a series of expectations about the way languages work, and so to a series of predictions about the correct analysis of phenomena in particular languages, while a research strategy built on the supposition that there are few components, or that components can freely interface with one another, or both, generates few such expectations and predictions. The IP then permits us to entertain, and to test, a variety of hypotheses about the components of grammar, most of which would simply be invisible given a less differentiated framework. Even the search for negative results about component divisions, as pursued by Anderson (e.g., 1975) among others, demands that these divisions first be entertained as serious possibilities.

The IP can then serve as a powerful generator of hypotheses at several levels in linguistic analysis. It is virtually certain that some of these hypotheses will turn out to be incorrect, but we believe that it is at least as valuable to have a clear sense of why certain plausible hypotheses *cannot* be maintained as it is to have hit upon a collection of proposals that seem at the moment to be supported by the known evidence.

2.4.2 **The PPFs, the PSCP, and theories of syntax.** It is difficult to conceive of a theory of syntax for natural languages which would not allow for an analog of the Principle of Phonology-Free Syntax to be stated in its terms. Hence the positions we are taking should have a very high degree of generality and applicability; if they can be convincingly defended, then they will have consequences for any imaginable theory of syntax.

The range of different syntactic theories that have been seriously argued for in recent linguistic work is remarkably wide; if it is true that science is at its healthiest when numerous contending theoretical positions are being pursued simultaneously, then the field of syntax is visibly healthy. The upshot is that any work that claims to have truly general relevance must take account of the possibly distinct claims of theories as different as the pure phrase structure syntax advocated by Gazdar (1982), the more powerful two-level lexical-functional grammar (LFG) of Bresnan (1982), and the complexly modular recent versions of TG that go under the name of government-binding theory (GB; see e.g. Chomsky 1981), as well as many other varieties of syntactic theory with smaller numbers of adherents.

It is worth keeping in mind that there is a lot these theories share. For example, in all of them some notion of "surface structure" is present and is centrally important. For phrase structure theories it is the only syntactic structure there is. For LFG it is "c-structure," one of the two significant levels of representation for sentences. In GB, it is either the output level of the transformational component of the syntax or a level derived from this by certain operations such as deletion rules (the literature has not been particularly explicit about the details). Even in such a strikingly novel syntactic theory as Arc Pair Grammar (see Johnson and Postal (1980) for a detailed presentation) it has a direct analog, the concept of S-graph. Moreover, many standard

labels for nodes, names of uncontroversially accepted lexical and phrasal categories like NP (Noun Phrase), VP (Verb Phrase), S (Sentence), and so on, are in common use in virtually all theories, so at the very least the diversity of notions and notations is not total.

Within certain syntactic theories, the PSCP and PPFS have a special status, in that they are consequences of other theoretical assumptions and not additional conditions on grammars. Consider "monostratal" theories, those positing no syntactic level other than what standard TG would call surface structure. Phrase structure approaches like the generalized phrase structure grammar (GPSG) of Gazdar, Klein, Pullum and Sag (1985) are conceptually the purest of these proposals, in that they assign the whole burden of syntax to a mechanism already admitted in standard TG, the phrase structure rules. Unlike standard TG, a theory like GPSG entails both the PSCP and the PPFS in their strongest forms. 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 the vocabulary of phrase structure (i.e. terminal and nonterminal symbols) and offers no possible role for phonological primitives.

It follows that an argument for abandoning either the PSCP or the PPFS is also an argument against GPSG and similar monostratal theories. Proponents of GPSG consequently have a compelling motive, in addition to general metatheoretical considerations favoring the IP, for supporting the PSCP and PPFS. And to the extent that the PSCP and PPFS hold, they can be seen as arguments for a theoretical framework (like GPSG, and unlike standard TG) in which these principles are necessarily valid. There is then an intimate relationship between the interfacing principles and the choice of a monostratal vs. a transformational syntactic theory.

2.4.3 Issues of expressive power. A theory with a number of components, interfacing with one another in limited ways, is potentially more falsifiable than one with few components, free interfacing, or both. This is because the former is potentially consistent with a narrower range of languages than the latter. To the extent that the IP restricts the set of possible languages, it is to be preferred to less modular frameworks for grammatical theory.

We must qualify these claims with "potentially" because whether a genuine restriction in the set of possible languages accrues from the IP depends on what components there are and what they are like internally. There are two caveats to be made here: the general observation (stressed by Wasow 1978) that restricting the set of possible grammars does not necessarily reduce the expressive power of a theory of grammar, that is, the set of languages that can be (weakly or strongly) generated under that theory; and a warning specific to the syntax-phonology interface, namely that the empirical consequences of proposals like the PSCP and the PPFS depend very much on the character of the syntactic and phonological components.

Does the PPFS actually restrict the expressive power of grammatical theory? There are certainly some syntactic theories in which imposing the PPFS has no empirical effect at all. Peters and Ritchie (1973) formalize essentially the theory of Chomsky (1965) and show that there are no

recursively enumerable sets of strings whatever that do not have transformational grammars as defined by this formalism. (The technique is to use transformations to mimic the computations of a Turing machine; see Bach and Marsh (1978) for a simpler proof of the same result using a rather different technique.) What this means in its starkest form is that every language, attested or imaginary, is a language with a transformational grammar of the sort described in Chomsky (1965) or Lasnik and Kupin (1977); abandoning some, or even all, autonomy assumptions could not possibly lead to a wider class of languages being describable.⁴

The imposition of the PPFs will generally make no difference to this situation, for the proofs of "transformational omnipotence"⁵ generally trade on the option of having as many arbitrary new terminals or nonterminals as necessary and on the power of deletion transformations. If we take any grammar containing a rule that clearly violates the PPFs, we can construct another that generates the same language (weakly and strongly) but does not violate the PPFs.

In fact, a stronger result is easy to prove: if the phonology defines a recursive mapping, and the syntactic part of the theory can provide a grammar for any recursive set, then for every grammar that violates the PPFs by virtue of making a reference in the syntax to some recursive phonetic property of constituents, there is an equivalent grammar that does not violate it. The proof is straightforward, and we merely sketch it.

Let C be the set of constituents generated by the syntactic component of some grammar G , and let P be the set of phonological representations of constituents in C . Without loss of generality, we will assume the mapping M from C to P is a function. Suppose there is some recursive subset P' of P whose members meet a particular phonetically defined condition. Then there is some subset of C , call it C' , that is mapped onto P' by a submapping of M . Since P' is recursive, we can decide membership in it. Since the phonological component defines a recursive mapping, we can decide for an arbitrary member of C whether its image is in P' . This is equivalent to deciding whether it is in C' , hence C' is recursive. But in that case, since the theory provides a grammar for every recursive set, we can give a syntax for C' directly. Hence for any syntactic rule conditioned by a reference to the property of being in P' , we can give an equivalent purely syntactic account that makes no mention of P' .

An example may help for readers who prefer not to view things so abstractly. Consider the case of a grammar containing a movement transformation that obligatorily moves to the beginning of the sentence the highest constituent that begins *phonetically* with a bilabial consonant. Imagine that the language has an optional rule of vocalic prothesis, so that knowing a constituent has a bilabial-initial word as its first word at the underlying phonological level is not sufficient to determine whether it should be moved. This would be a paradigm case of a PPFs violation. Yet by what we have just established, a syntax can be given for this language within any theory that provides grammars for all the recursive sets, in terms that do not mention the phonetic property of bilabiality, provided only that (i) it is decidable for arbitrary

syntactically represented constituents what phonetic representations they are assigned by the phonological rules, and (ii) it is decidable for arbitrary phonetically represented constituents whether they begin with a bilabial consonant. One can hardly imagine a theory of phonology that did not guarantee (i) and (ii).

It might be charged, then, that the PPFs is without consequences and so is of no theoretical interest, at least within sufficiently powerful theories (for example, any transformational or other theory capable of providing grammars for arbitrary recursive sets). One response to this charge invokes the "strong mentalist" position on the nature of grammars: that the grammars defined by linguistic theory are actually identical to a component of the mind of a speaker of the language, so that any claim restricting the number of permissible grammars has empirical consequences in cognitive psychology and ultimately in brain neurophysiology.

Under the strong mentalist interpretation of the subject matter of linguistics, clearly, it is not difficult in principle to specify the consequences of assuming the PPFs (though it may be in practice very difficult to identify them experimentally). There may be much to be said for the idea that grammatical constraints have psycholinguistic implications. In particular, we suspect that there may be very significant consequences of the PPFs in the domain of parsing: to know that the phonetic complexities of speech processing will not be implicated in the syntax at arbitrary points in unpredictable ways must surely take some potential complexity out of grammatical parsing. But we regard this connection as conjectural, and regard the strong mentalist interpretation of grammatical theory as somewhat implausible. Linguistic theory is surely of some psychological relevance, but to equate the specification of grammars for natural languages with the investigation of the brains of speakers strikes us as falling into the trap of confusing grammars and speakers, which we have warned against above (cf. Soames 1984 for discussion).

Another response to the omnipotence argument to which we do *not* wish to subscribe appeals to a division of the theory of grammar into

two parts: a universal grammar UG that determines the class of possible grammars and the way they operate, and a system of evaluation that ranks potential grammars in terms of "optimality" or "simplicity"...To attain explanatory adequacy the theory T must be sufficiently restricted so that relatively few grammars are available, given a reasonable amount of experience E, to be submitted to evaluation; otherwise, the burden on the evaluation procedures is intolerable. A reasonable project for linguistic theory, then, is to attempt to constrain UG so that potential grammars are "scattered" in terms of a measure of optimality; only a few grammars need be considered, given experience. (Chomsky and Lasnik 1977, 427)

In this proposal the evaluation metric would bear a great part of the burden of explaining why we find languages with certain sorts of structure and do not find languages with other sorts; both classes of languages might be consistent with universal grammar, but those in the second class would have grammars that score badly on the measure of

optimality. Exactly this proposal is put forth by Sampson 1973, who supposes that an evaluation metric will sort out the languages in the second class as (relatively) "unnatural" because of the complexity of their grammars, as measured by evaluation procedures.

We wish to reject this line of reasoning on at least two grounds, both stemming from the sort of "constructivist" universal grammar that Chomsky and Sampson have in mind in these discussions. In the now-standard view, universal grammar supplies a collection of pieces of formalism from which individual grammars can be constructed, along with a set of restrictions on their combination. The (also universal) evaluation metric assigns values to the individual pieces and (by regular formulas) to combinations of these pieces. The ultimate function of the evaluation procedures is to assign a metric of complexity to the grammar as a whole.

The first difficulty arises directly from the latter fact. The metric measures the (un)naturalness of the whole grammar; subparts of the grammar are assigned measures, but these measures are of no systematic importance. As a result, there is no way to speak of an individual rule as being either natural or unnatural. Within the constructivist framework there is no way to say that a syntactic rule containing 1000 symbols is highly unnatural (indeed, one might want to say it is impossible). If a rule containing 1000 symbols were one of only one or two rules comprising the entire grammar, the grammar as a whole might be evaluated as no more complex than grammars of quite familiar languages with their dozens or hundreds of rules that can be stated fairly briefly. Similarly, the framework does not make it possible to say that a rule mentioning the arbitrary list { Article, [NP, +Pro, +Acc], [Adv, Manner], *remonstrate*, *S'*, } is unnatural (or impossible). The difference between a set of items that must be listed and one that can be picked out by reference to some motivated syntactic feature is, from the point of view of an evaluation metric, a very small difference--less than the complexity contributed by most single rules--so that a *grammar* with a rule that includes reference to an unmotivated list will not necessarily be rated as particularly complex or unnatural.

A second difficulty arises from the fact that in the constructivist framework there is no intrinsic connection between the parts of a rule; anything constructible from the elementary formal units according to the principles of combination is a possible rule of grammar. To see the sorts of predictions about possible rules and possible grammars that are thereby made, take any carefully stated version of a reasonably uncontroversial syntactic or phonological rule and construct from this original a collection of other possible rules by replacing bits of the original by alternatives, and by altering the order of the original parts, and by eliminating some of the original parts. The results will in most cases be nonsense from the point of view of the grammars of genuine human languages, even though they are, strictly speaking, well-formed rules. Moreover, they are rules of comparable complexity/naturalness to the original, given an evaluation metric along the lines suggested in the literature on generative grammar.

We take the view, therefore, that it will not do to adhere to a constructivist view of universal grammar and so to rely on the evaluation

metric to sort out the available grammars and possible languages from the unavailable grammars and impossible languages.

However, it is not necessary for us to adopt the strong mentalist interpretation of linguistic theory or to embrace the evaluation metric in order to explicate a sense in which the PPFs can have consequences. What is required is a genuine limitation on universal grammar, achieved either by restricting its formalism (as advocated, for instance, by Peters (1973) and by GPSG in general) or by restricting its substance (as advocated, for instance, by Bach (1965) and by relational grammar in general). In particular, it would be desirable to find a principled way of imposing restrictions on the nonterminal vocabulary of the grammar (that is, on the set of syntactic categories) which were both formal and substantive: a finite bound on the nonterminal vocabulary of universal grammar, and a requirement that every syntactic category be subject to substantive constraints as to the role it can play in syntactic rules. What this would bar is the ad hoc construction of syntactic categories to surmount descriptive obstacles, or the ad hoc formulation of syntactic rules, using motivated categories, for the same purpose. If such uses of syntactic categories are excluded, then assuming the PPFs makes certain languages undecidable.

Consider an analogy between grammars and Turing machines. The power of Turing machines can be seen as arising from two sources, the lack of a limit on the number of symbols a machine can work with and the lack of a limit on the number of machine states. It is known that limiting Turing machines to only two auxiliary states (plus one 'accepting state', but with no limit on the number of auxiliary symbols) does not reduce their generative capacity (Shannon 1956), nor does limiting them to only two auxiliary symbols (but with no limit on the number of states) (Hopcroft and Ullman 1969, 100, citing Wang 1957); a reduction in generative capacity can be achieved only by limiting both the stock of states and the stock of symbols, so that, speaking intuitively, a Turing machine no longer has the unlimited capacity to do scratch work. The excessive power of standard TG has analogous sources; transformational grammars can do their scratch work either by using some special nonterminal symbols or by applying special rules to a fixed nonterminal vocabulary (then deleting blocks of symbols used for scratch work), and a reduction in generative capacity can be achieved only by limiting both the stock of nonterminals and the operations that can be performed on whatever nonterminals there are.

We are suggesting that formal limitations should be imposed, and that in addition the limits should be linguistically motivated. If this is done, then the PPFs will do just the work we intended it to do. No grammar could then have the effect of making reference to a constituent whose first word begins with a bilabial: the PPFs would bar direct reference to such constituents, and the constraints on nonterminal symbols would bar indirect reference, since the class of constituents whose first words begin with bilabials is surely not a syntactic category that universal grammar would make available on other grounds.

2.4.4 Other kinds of modularity. The issue of modularity in the theory of grammar—that is, the issue of whether the logic of the relationship between sound and meaning in language supports the division

of the rules describing this relationship into a number of distinct components—must be kept distinct from issues of modularity in two other domains, psycholinguistics and cognition in general. Nevertheless, there are connections between grammatical modularity and these other types of modularity: psycholinguistic modularity could add substantial support to the IP, and the IP in turn presupposes general cognitive modularity.

Consider first the question of modularity in psycholinguistics, that is, in language processing (production, comprehension, and memory). It is widely assumed that language processing is modular, and Garrett and his colleagues have vigorously defended the assumption of a close connection between psycholinguistic modules and the components in a grammar; Garrett and Kean (1980), in fact, propose that the levels of representation in processing and the interface representations in grammar are identical. (Note that they do *not* claim any special relationship between the internal organization of a psycholinguistic module and the corresponding grammatical component.) Verification of this proposal would give considerable support to specific models within the general IP, although the IP in no way depends upon the existence of psycholinguistic modularity.

Next consider the question of general cognitive modularity, of the degree to which there is an autonomous grammar module among other such modules (as maintained by Chomsky in many places and treated at length by Fodor (1983)).⁶ A commitment to general cognitive modularity carries with it no investment in the IP. But those who support modularity in grammar will also champion general cognitive modularity, for obvious reasons: If there is no distinguishable grammar module, how can we discuss whether it has autonomous components? With other advocates of the IP, then, we assume that grammar is distinct from various extragrammatical domains. Some pieces of discourse in a language will be bizarre in meaning, pointless in context, lacking in grace, hard to comprehend, rude in tone, hard to pronounce, metrically regular, devious in intent, previously encountered, novel in form, frequently uttered, or open to multiple interpretations, to mention just a few factors assignable to domains distinct from (though related to) the domain of grammar. But the operation of rules in any component of grammar will not depend on whether sentences that the rules describe have such properties. There are many aspects of the study of language that are distinct from the study of grammar; they include studies of the purposive use of language, speech perception, speech production, the social "meanings" of linguistic forms, discourse organization, stylistics, and poetics.

3. **Putative counterexamples to the PPFS.** We now sketch our strategy for defending the Principle of Phonology-Free Syntax (PPFS).

The PPFS makes a specific technical claim about grammars, in the sense of *grammar* we introduced in section 1 above. The claim is that none of the rules in the syntactic component of a grammar refer to constructs drawn from the phonological, as opposed to the morphosyntactic, subset of the constructs made available by the overall theory of grammar. But the PPFS says nothing about rules in other components of a grammar. We must therefore be specific about what other components there are and how they interface with syntax and phonology; indeed, we must be specific about the components falling under the

headings "syntax" and "phonology." These matters are the subject of section 3.1. Then (in section 3.2) we provide a typology of apparent violations of the PFFS.

3.1. **The components of grammar.** The grammatical components of interest to us include both syntactic components, describing the combination and ordering of words in phrases and sentences; and phonological components, describing the realization of morphosyntactic units in terms of phonological units.

3.1.1. **Syntactic components.** There is much that we can leave open concerning the structure of the purely syntactic components of a grammar. But what we can be explicit about includes the fact that the syntactic components provide (1) a set of representations we shall call *preterminal structures* which contain at least categorial, constituency, and linear precedence information, but not the content of particular lexical entries; and, derived directly from them, (2) a set of representations called *terminal structures* which contain all the above information plus an indication of which particular lexical items occur in the represented sentence.

Thus, to take a very simple case, if the syntax is assumed to involve just a phrase structure grammar as in Gazdar (1982), the preterminal structures are trees with immediately preterminal nodes--and thus most of the feature detail associated with the items in the sentence, though not the information distinguishing between words belonging to the same syntactic category--and the terminal structures are similar trees, but have indices or names of particular syntactic words added under the preterminal nodes. Thus *Birds eat* and *Birds drink* might have identical preterminal structures but distinct terminal structures. This amounts to a claim that the difference between *eat* and *drink* is not a syntactic difference.

By distinguishing between preterminal and terminal structures, we avoid reproducing much of the content of the lexicon of a language in its syntax. Preterminal structures are, speaking strictly, the output of the syntactic component; the syntax then is not responsible for providing features to distinguish between every pair of words the language happens to have in its lexicon. However, we also need a level of "syntactic" representation at which (for instance) *Birds eat* and *Birds drink* are not the same. Terminal structures differ from preterminal structures *only* in this respect; they can be viewed as preterminal structures with pointers to words entered in the lexicon, but of course without any of the content of those lexical entries.

One further distinction needs to be mentioned here because it leads to a series of apportionment problems involving syntax and the lexicon. This is the distinction, in what we shall call "classical TG" (cf. Jakobson and Pullum (1982, Editorial Introduction)), between *cyclic* and *postcyclic* syntactic transformations. Classical TG assumes a component division here, both because of limited interactions (no cyclic rule applies after a postcyclic rule) and also because of nonuniformity (cyclic rules are bounded, potentially lexically governed rules making reference to grammatical relations but not to linear order and applying in cyclic fashion, while postcyclic rules are potentially unbounded,

lexically ungoverned rules making reference to linear order but not to grammatical relations and applying in a single pass; cf. Pullum (1979, chs. 2 and 4)). Consequently, in standard TG there is an interface representation—sometimes called *shallow structure*—between the two components, which is presumed to be a grammatically significant level of representation.

Virtually all current versions of generative grammar (whether transformational or not) make some distinction reminiscent of the cyclic/postcyclic distinction, but they differ in just where the line is drawn, how the difference is represented, and what significance is attributed to the interface representation (if any is defined). This is not the place to conduct a detailed review of the matter, but we have to consider one possibility, namely that (some or all of) the traditional cyclic rules are to be replaced by "lexical" analyses.

Consider a rule like the Dative Movement of classical TG, which has been regarded as cyclic. There are two quite different proposals for a lexical alternative to such a rule. The first denies that there is any generalization to be made about the relationship of two classes of structures (for instance, transitive VPs containing a *to*-dative and ditransitive VPs). Instead, particular words are subcategorized according to which frames they occur in, that is, according to the points in preterminal structures at which their indices can be inserted. A Dative Movement verb, on this analysis, is one that has two independent lexical features, one indicating that the verb occurs in a VP frame with a NP and a PP with *to*, the other indicating that the verb occurs in a VP frame with two NPs. A second lexical treatment of a cyclic rule posits the same subcategorization features in the lexicon as the first treatment, but declares that these features are not independent of one another—that there are principles predicting some features on the basis of others—and that these principles belong in the lexicon rather than in the syntactic component. On this analysis, a Dative Movement verb has both of the subcategorization features, the occurrence of one of them (let us say the ditransitivity feature) being predictable from the occurrence of the other.

The general class of principles relating features within lexical entries we refer to as *lexical implication principles*, or LIPs; they are known as *lexical redundancy rules* in the TG literature. In general, the existence of such principles does not depend on how the traditional cyclic rules are to be treated. That is, there is a component of LIPs (which we can think of for the moment as being "in the lexicon"), and it might be that some, or all, of the traditional cyclic rules can be eliminated in favor of principles in this component.

In standard TG a particular class of phenomena involving lexical government might be best described by independent subcategorization features in lexical entries, by an LIP distributing subcategorization features in lexical entries, or by a cyclic transformational rule triggered by a rule feature in lexical entries. A similar apportionment problem arises in GPSG, where the role of a cyclic transformational rule can sometimes be filled by a metarule (a principle predicting one class of phrase structure rules on the basis of another) and where both independent subcategorization features and LIPs are available. That is,

in standard TG and in GPSG, both syntactic and (two kinds of) lexical analyses can be framed for certain phenomena that might on first acquaintance be viewed as "syntactic". Finally, in more stringently lexicalist frameworks, such as LFG, only the two types of lexical analyses are permissible in such cases, and no syntactic component can be appealed to for an account of the facts.

The PPFS bars any syntactic rule in which items subject to the rule are picked out by phonological predicates. In TG terms, it prohibits phonological determination both in phrase structure rules and in transformational rules; neither the class of verbs subcategorized for the frame [___ NP pp[*to* NP]] nor the class of verbs subject to Dative Movement can be picked out phonologically. In GPSG terms, phonological determination is impossible both in rules and in metarules. But the PPFS is silent on the question of whether principles in components of a grammar other than the syntactic component can refer to phonological predicates, and unfortunately (as we point out in Pullum and Zwicky 1984) the existence of a component of LIPs opens the door to analyses that use phonological reference in the LIP component to achieve the effect of phonological determination in a lexically governed syntactic rule. As a result, the increased reliance on a rich set of LIPs (versus transformations or metarules) in lexicalist approaches to syntax is not unproblematic. We believe, however, that the spirit as well as the letter of the PPFS can be maintained here.

3.1.2. **Phonological components.** We assume that phonology itself is articulated, comprising principles in a number of distinct components. However, as far as the PPFS is concerned, phonology could well be a single homogeneous component. The PPFS rules out phonological predicates, of any sort, in syntactic rules.

Nevertheless, we cannot discuss examples in a theoretical vacuum. It is also conceivable (though, in our view, unlikely) that the PPFS could not be maintained in its full generality, in which case we would not want to admit phonological predicates of all sorts in syntactic rules, but would search for restrictions on the types of phonological representations that could play a role in syntax; an articulated phonology would serve as a natural source of potential restrictions.

In any event, we follow Dressler (1985) in distinguishing *allomorphy rules*, involving phonological operations as concomitants of morphological rules (whether derivational or inflectional), from *morphonological rules*, in which general phonological operations apply in morphosyntactic domains, and these in turn from (*purely*) *phonological rules*, in which general phonological operations apply in purely phonological, or "prosodic", domains. In addition, following Zwicky (1986), we distinguish a set of *shape conditions* that override allomorphy rules and precede morphonological rules; among the shape conditions are those governing the well-formedness of clitic groups.

Though all the details of this proposal are important, in the present context what is most significant is that these component distinctions impose a sharp division of "phrase"-phonological rules into two types, a prosodically sensitive group and a morphosyntactically sensitive group. This is essentially the division advocated by Rotenberg

(1978) and Hasegawa (1979) and defended in some detail by Kaisse (1985), who uses the terminology "rules of fast speech" (for automatic, prosodically sensitive rules of phrase phonology) versus "rules of connected speech" (for nonautomatic, morphosyntactically sensitive rules of phrase phonology).

3.2. Analyzing apparent violations of the PPFs. A genuine violation of the PPFs would be a generalization about a language which is correctly expressed as a syntactic rule referring to phonological constructs. An apparent violation could then fail to be genuine on any of the following grounds: the generalization might be spurious (section 3.2.1); a real generalization might involve not a rule, but rather a preference or tendency (3.2.2); a real generalization might involve a rule not of grammar, but rather of some extragrammatical domain (3.2.3); a rule of grammar might be located not in the syntactic component, but rather in one of the other components discussed in section 3.1 (3.2.4); or a rule of grammar might be subject to a phonological condition or constraint that is universal, and therefore is not to be stated as part of the rule (3.2.5).

3.2.1. Spurious generalizations. Occasionally in the literature it has been claimed that some syntactic rule is subject to a constraint involving the phonological properties of some morpheme, word, or constituent—but on closer inspection it turns out that there is no real phonological conditioning whatsoever, that when the constraint is correctly described, it can be seen to arise from some essentially nonphonological basis. The generalization involving phonology is spurious. Sometimes a putative generalization vanishes completely under scrutiny.

Particularly susceptible to reanalysis in nonphonological terms, or to outright rejection, are "functional" accounts of syntactic and morphological phenomena. It is sometimes maintained, for instance, that some forms take the shape they do *in order to* achieve a one-to-one association between morphosyntactic categories and their phonological realizations—that is, in order to avoid ambiguity and redundancy—and that this teleological statement involving phonology constitutes a sufficient description of the morphosyntactic facts. Both linguists (Durrell 1979) and language teachers (Eltzner and Radenhausen 1930, 22-3) have espoused versions of this proposal for the three adjective "declensions" in German. We sketch the facts briefly here; for a full treatment, see Zwicky (to appear, sec. 3.1).

There are three paradigms for adjective inflection in German, traditionally called "strong," "weak," and "mixed." The choice among them is governed by the determiner preceding the adjective. Indeclinable determiners (including the zero determiner) govern the strong declension, in which most of the 16 case/gender/number combinations are realized by distinct endings. Determiners in a second group (with nearly the same paradigm as the strong declension of adjectives) govern the weak declension, in which there is massive levelling in favor of only two endings, *-e* and *-en*. Determiners in a third group (with zero endings for some combinations) govern the mixed declension, which has some endings from the strong declension and some from the weak. The paradigm for the mixed declension can be roughly viewed as an trade-off

in information about case, gender, and number: if the determiner has an ending, the adjective doesn't need to supply any information, and so has a weak-declension ending; but if the determiner lacks an ending, the adjective must supply information, and so has a strong-declension ending. The indefinite article *ein* governs the mixed declension, so that when it has an ending, as in the dative singular masculine *einem*, a following adjective has a nondescript ending (-*en*); but when it lacks an ending, as in the nominative singular masculine *ein*, a following adjective has an informative ending (-*er*).

The question is what the grammar of German says about these facts. Zwicky (to appear) formulates several versions of a principle requiring unambiguous and unredundant phonological expression of case, gender, and number within German NPs and supplies counterexamples to all of them. Zwicky further observes that even if one of these versions had been free of counterexamples, it would still have been far too weak to predict the actual paradigms that German has and so would have no place as a rule in any component of grammar. If we lower our sights and try to describe only the mixed declension, with the other two declensions as givens, it is possible to formulate a rule of allomorphy much as in the preceding paragraph, which will cover this narrow range of facts but doesn't mention ambiguity or redundancy: The ending of an adjective in the mixed declension is chosen from the strong paradigm if the preceding determiner has a zero ending, otherwise from the weak paradigm. It then turns out that the reference to the makeup of adjacent words and to (phonological) zero in this allomorphy rule are both dispensable. The following allomorphy rule covers the facts equally well: The ending of an adjective in the mixed declension is chosen from the strong paradigm in the nonfeminine nominative singular, otherwise from the weak paradigm.

The fate of putative syntactic generalizations employing functional notions like ambiguity and redundancy is, in our experience, uniformly grim. (We considered another case in Zwicky and Pullum 1983, on Somali.) Those who advance such proposals are attempting to make rules of grammar perform a task they are not equipped for: not only to describe some aspect of the sound-meaning pairing in a language, but also to encode directly their extragrammatical reasons for being, to (so to speak) wear these reasons on their sleeves. This is to insist that form should not merely follow function, it should *be* function. It makes sense that grammars should contain rules that, individually or in concert, help make sentences pronounceable, parsable, informative, reasonably brief, and the like, but there is no reason to think that we can tell what a rule is good for by looking at it wrenched from its grammar, and we believe it is always a mistake to formulate a rule explicitly in terms of its functions.

Cases of spurious generalization are often complex. In some, there is a correlation between a phonological property and the applicability of a rule, but this correlation is weak, constituting at best a tendency (see section 3.2.2). In some, the constraint not only is nonphonological but also applies to a rule that belongs in some component other than syntax (see section 3.2.4). On occasion, there are dialect differences, with one dialect failing to present a counterexample to the PPFs because a phonological generalization is spurious and a second dialect failing to present a counterexample to the PPFs for a different reason.

Two cases from English, one involving ditransitive verbs and the other combinations of verbs with particles, illustrate some of this complexity. These are treated in Zwicky and Pullum (1986).

3.2.2. **Preferences and tendencies.** As we said in section 1.1, there are (at least) two ways in which a real generalization about linguistic events can fail to constitute a rule of grammar and so cannot possibly be a candidate for a phonological constraint on a syntactic rule. The first of these is that the generalization describes a *preference* (if we look at matters from the point of view of speakers) or a *tendency* (if we take a more neutral viewpoint). For instance, given two alternative expressions differing in length, speakers might prefer to use the shorter in most circumstances, thus following a principle of least effort, both for themselves and for their addressees. Or given two alternative expressions, one alliterative and the other not, speakers might tend not to use the alliterative version, thereby avoiding material that is difficult to pronounce.

It is not necessary for a tendency to be explicable by reference to language production or comprehension, as these two (not entirely hypothetical) examples are. A statistical tendency favoring one class of forms over another in certain circumstances can be a remnant of linguistic history, subject to diachronic but not synchronic explanation; see our discussion of Dative Movement verbs in Zwicky and Pullum (1986). We argue there that if there were any tendency for these verbs to be either monosyllables or initially stressed disyllables, such a tendency would be sufficiently explained by reference to the historical sources of the verbs. From a synchronic point of view, any such tendency would be an accident. There would be no reason to think that it played a role in language production or comprehension, and certainly no reason to think that it should be expressed in a rule of grammar.

In section 1.1 we mentioned a third source of tendencies favoring one class of expressions over another: structured variability in language use. The first lesson of quantitative sociolinguistics is that linguistic variables are often correlated (in the statistical sense) with social, situational, and personality variables, as well as with one another. Particular groups of speakers can then be characterized sociolinguistically by their base settings on certain linguistic variables (expressed as estimated probabilities) plus their pattern of correlations among variables (expressed as a system of formulas each relating the probabilities for several variables); see Weiner and Labov (1983) for an illustration of the method applied to agentless passives in English. What interests us here is the occurrence of correlations between linguistic variables. We take particular note of the possibility that the applicability of a syntactic rule might be correlated with some phonological variable--that, say, topicalization might be favored for polysyllabic NPs over monosyllabic NPs.

There are actually two ways in which this correlation might be established. The first is that each of the linguistic variables might be dependent on some nonlinguistic variable and covary as a result. For instance, in some group increasing age might predict higher frequencies for both topicalization and polysyllabicity. There might then be a

tendency for polysyllabic NPs to topicalize and/or a tendency for topicalization to affect polysyllabic NPs. But a sociolinguistic description of language use would have no reason to mention any such tendencies in the speech of this group, and neither of course would a grammar for their dialect.

The second possibility is that the correlation between variables might be irreducible and so require explicit representation in a sociolinguistic description of language use, as a statement relating polysyllabicity, topicalizability, and other factors. A tendency would then have found expression as a principle in an extragrammatical domain.

We do not know whether there are any real-life instantiations of this possibility. For one thing, the methods of quantitative sociolinguistics are not designed to distinguish causes and effects within a set of variables; a probability formula merely describes a mathematical relationship among many factors, both linguistic and nonlinguistic, and it cannot be taken seriously as a principle in a sociolinguistic description of language use. Such a principle should describe a linguistic practice, should describe what speakers know about how and when to use some element of linguistic form. It should say, for example, what speakers know about using the word *steed* or what they know about using topicalized sentences. But to our knowledge, no precise, unified, and comprehensive theory of such principles exists (in the way that precise, unified, and comprehensive theories of syntax exist). As a result, there is no sensible way to address the question of what some subset of these principles might be like. We do not rule out the possibility that one of these principles says that some syntactic construction is especially favored when it has certain phonological properties. Needless to say, we cannot rely on this possibility in reanalyzing putative violations of the PPFS.

We have uncovered at least three sources of statistical tendencies in linguistic behavior: speaker preferences based on extragrammatical considerations, including production, comprehension, and style; residues of linguistic history; and structured sociolinguistic variability. In all three cases, explanations for the tendencies are to be sought *outside grammar*, in accounts of language use or diachronic change.

We use this fact when we classify some phenomenon as a tendency or preference rather than a rule or a condition on a rule; we intend that every such classification should be backed by a reference to an extragrammatical consideration that can provide a sufficient explanation for the phenomenon at hand. We do not claim that every statistical tendency in linguistic behavior has a discoverable extragrammatical explanation. But we are not willing to dispose of putative counterexamples to the PPFS (and other interfacing assumptions) by facile references to "mere tendencies."

The problem arises when we have to distinguish a (statistical) tendency from a rule with exceptions that must be characterized by the grammar. Suppose we are confronted with the observation that certain instances of a construction do not occur (in speech or in texts), or that informants find them unacceptable. There are three possible interpretations: either the unacceptable data are to be treated as

ungrammatical, and they are to be described as systematic exceptions to the rule describing the construction, via a condition on the rule; or the unacceptable data are to be treated as ungrammatical, but the exceptions are to be listed, and any similarities among them reflect mere tendencies; or the data are unacceptable because they are victims of some tendency favoring alternative expressions. The second interpretation is the one we use in our discussion of Dative Movement and verbs taking particles. The third interpretation is the one we propose to appeal to in, for instance, discussions of the unacceptability of adverbs like *friendlyly* and sentences like *They gave a fight that now seemed to them utterly without hope of success up*. In both interpretations, the appeal to tendencies must be backed by a sketch of relevant extragrammatical considerations.

3.2.3. **Extragrammatical generalizations.** A real generalization about linguistic events can fail to constitute a rule of grammar because it describes a preference or tendency. It can also fail, as we noted in section 1.1, because it describes a linguistic practice (however rule-governed) in some domain other than grammar. As in the case of preferences and tendencies, if the generalization is not a rule of grammar, *a fortiori* it is not a possible candidate for a phonological constraint on a syntactic rule. The extragrammatical domain that has most often been confounded with grammar is the realm of verbal play and verbal art. There are principles in this domain which do in fact refer to phonological properties of morphological and syntactic units--the "rules" of language games (like Pig Latin) and poetic forms (like the sonnet). These phenomena have considerable import for a theory of phonology and perhaps for theories of other components of grammar as well. But they have nothing to do with the PPFS or the other interfacing assumptions, since they are not rules of grammar.

3.2.4. **Nonsyntactic rules.** Even if a generalization genuinely involves phonology, and even if it is to be formulated as a rule of grammar, rather than as a tendency or as a regularity in some extragrammatical domain, it might still be beside the point in an examination of the PPFS because it is not a rule of *syntax*, but belongs instead in some other component of grammar. It might, for instance, be a "phonological" rule (of one sort or another) with a syntactic constraint on it, rather than the reverse, or it might be a rule of morphology or a shape condition. In such a case the existence of a phonological condition on the rule has no bearing on the PPFS.

3.2.5. **Universals.** A final possibility is that there is a phonological condition on some syntactic rule, but that the condition is supplied by universal grammar, not stipulated parochially. Individual grammars have no choice in the matter. We are willing to entertain such circumscribed phonological constraints on syntactic rules because (unlike parochial constraints of this sort) they involve no increase in the expressive power of grammars. As it happens, universal constraints like this are more than a hypothetical possibility; see the discussion in Pullum and Zwicky (in press) of a universal condition on coordinate structures which refers to phonological identity.

Notes

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¹See Zwicky (1984) for an extended discussion of the ideas in this section.

²We do not in fact think it is correct to say that the theory of Chomsky (1965) entails this strict separation, though it was probably intended to; see section 2 of Pullum and Zwicky (1984).

³It seems likely that it was also an unnecessary weakening; see Katz (1980) for a critique of Chomsky's treatment of the syntax-semantics interface.

⁴It is therefore surprising to us that Chomsky and Lasnik (1977, 427) say

Even this extremely rich theory [of Peters and Ritchie (1973)--GKP/AMZ] does not encompass such devices as structure-building rules, global rules, transderivational constraints, and others that have been proposed. Any enrichment of linguistic theory that extends the class of possible grammars requires strong empirical motivation. We feel that this is lacking in the case of devices that exceed the framework of Chomsky (1955), Peters and Ritchie (1973), and comparable work...

But the point is that there can in principle be no empirical motivation: no facts about the class of languages could speak either for or against a proposed extension of the class of grammars these theories define.

⁵The term is due to Sampson (1973).

⁶The terminology in the literature on grammatical modularity and cognitive modularity is confused, with the words *modularity* and *autonomy* used in different ways by different authors in their discussions of grammars and of cognitive models. We advocate using both terms in both contexts, intending thereby a distinction between the existence of modules and their distinctness.

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