CHAPTER SIX

Some Choices in the Theory of Morphology
Arnold M. Zwicky

BACKGROUND

A theory of morphology specifies what sorts of systems can associate the meanings of words to their pronunciations. The overall architecture of such a system hinges on a series of choices, seventeen of which I will discuss here. This paper has both a descriptive aim, to survey what I see as the major options, and a prescriptive one: to recommend (some variant of) one option in each case, and so to say why it is I pursue the particular variety of morphological theory that I do.*

The theorist's customary exclusions

I should make it clear at the outset that what I am engaging in here is linguistic theorizing, and not either psycholinguistic modelling or computational modelling. My concern is to find a logic connecting semantics and phonology at the word level. This logic should be linguistically appropriate, that is, its instances should be interpretable as claims about what particular people know about (certain aspects of) their languages. And it should capture generalizations; phenomena a linguist would recognize as unitary should not be treated as unconnected to one another.

But I make no claims about how the processing of language proceeds in real time, using real memories. My concern is with abstract inventories of expression types, properties of expressions, and generalizations about those properties; it makes no sense to say that such generalizations “take time.” I also make no promises that this logic will fit well with existing abstract theories of computation, or of course with the computational hardware and software that happen to
be available at the moment. Linguistic theorists are well advised to attend to psycholinguistics and the theory of computation—good ideas about symbolic systems are not easy to come by—but they cannot expect that questions about the way languages are organized will be reducible to problems already solved in one of these other domains. It also might well be that the current state of understanding about morphological theory would never have been reached without the spur provided by computational linguists, who find it an occupational necessity to make claims precise and get systems that run, but that wouldn't make morphological theory a branch of computational linguistics.

I will also confine myself to the theory of grammar. "Not every regularity in the use of language is a matter of grammar," Pullum and Zwicky (1987:330) observe in an article that calls for treating certain types of "morphology" (for instance, the celebrated expletive infixation in abso-goddam-lutely) as belonging to a domain separate from grammar—a domain of conventional regularities in language use, to be sure, and one certainly within the linguist's province, but one comprising principles that are significantly different in character from the rules of "plain morphology."

Programs, frameworks, and theories

Strictly speaking, I will not even be talking about theories here. I make a rough distinction between programs, frameworks, and theories, according to which my subject is frameworks, not theories.

A program comprises a few leading ideas. What I have called the Interface Program (Zwicky 1984) is indeed a program, then, since it is characterized by nothing more than the proposal that grammars are highly modular, with interactions between modules limited to certain principles of precedence. The proposal that systems of context-free phrase structure rules are sufficient for the framing of generalizations about syntactic organization constitutes a program for syntax. The proposal that the morphological rules of a language can be partitioned into a small finite number of rule-sets distinguishable on the basis of their phonological effects and the linear position of the affixes they describe constitutes a program for morphology.

To get from a program to a framework, you need a conceptual apparatus and a network of assumptions framed in terms of this apparatus. Usually when linguists talk about "theories," they're actually talking about frameworks. To get a theory, you need to fill out a framework in two ways: the set of assumptions has to be filled out so as to be complete with respect to the domain at issue, and descriptive
mechanisms (that is, an interpreted formalism) must be provided. This ideal has only occasionally been approached in linguistics – so far as I know, never in morphology (as opposed to semantics, syntax, and phonology). Whenever it is approached, however, an unfortunate ambiguity can arise, as when the publication of Gazdar et al. (1985) caused the phrase “generalized phrase structure grammar” to become ambiguous as between reference to the GPSG framework and to the syntactic theory laid out in the book. (For that matter, there are probably people who think it refers to the program of phrase structure syntax in general.)

To any program there correspond an enormous number of frameworks carrying out that program, and to any framework there correspond an enormous number of theories filling out that framework. It is undoubtedly no accident that so much “theoretical” discourse is about frameworks, since these constitute a more or less manageable middle level of analysis. It is hard to say anything substantial enough about programs to allow for discussion of relevant matters of fact, and it is hard to get intellectually aroused about the details of a theory when an astronomical number of plausible alternatives come quickly to mind. In any event, my own discussion is on this middle level – not at the level of mechanisms, but at the level of properties that mechanisms must have. If you really care about the details, you’re going to find it pretty thin gruel.

**AGONY ABOUT “WORDS”**

To reach this middle level, I’m going to have to make a rather large number of conceptual distinctions. The point is to avoid making fundamental decisions about a framework inadvertently, through the sloppy use of technical terms like “word” (and “lexical,” for complaints on which see Aronoff 1989 and Zwicky 1989a, 1990b).

(Along with a rather large number of conceptual distinctions go a rather large number of pieces of terminology. I am not particularly good at devising memorable technical terms, especially by the dozen or the gross, and I have made a number of different tries at this particular set over the years. I don’t suppose that the current choices are much better than their predecessors, but I do want to stress that what is at issue here is not terminology – rotten, boring, or striking – but conceptual distinctions.)

Though textbooks (like Matthews 1974) and foundational treatises (like Mel’cuk 1982) take some care to distinguish various concepts often lumped together under the label “word,” in my experience most linguists have little patience with these discussions, which they view
as mere philosophical quibbling. To my mind this is exactly the wrong response, roughly like rejecting the distinction between “mass” and “weight” as pointless sophistry in physics. Theoretical advances require some serious splitting, not thoughtless lumping. After the dust of splitting settles some, we of course expect to understand better the relationships between the pieces that have been split off; that is the point of the exercise.

I will be distinguishing various sorts of elementary linguistic expressions, where I understand an expression to be a pairing of semantic interpretation with phonological substance. Some of these distinctions (treated in the following section) are syntagmatic, having to do with properties of expression tokens. Others (treated in the remaining sections) are paradigmatic, having to do with properties of expression types.

Properties of expression tokens

Syntactic groupings: \( W, W_{\text{MAX}}, W_{\text{MIN}} \)

I begin with various properties of subexpressions, in particular the syntactic properties that I will label “rank” and “depth” (as in Zwicky 1990b, sec. 2). Syntactic theory requires a distinction between (at least) subexpressions of clause rank (rank C), phrase rank (rank P), and word rank (rank W).¹ Subexpressions of rank W are sometimes said to have the property of belonging to a “0-bar” or (alas) “lexical” category.

Just as subexpressions of rank P can include or be included in other subexpressions of rank P (as in the nested VPs of must have been watching penguins), so subexpressions of rank W can include or be included in other subexpressions of rank W (as in the nested NWs of savings bank location information). In such circumstances I will distinguish the most inclusive, or maximal, subexpression of a given rank from those included within it, and the least inclusive, or minimal, subexpression from those including it. Here we have one source of our inability to give a single answer to the question of how many “words” are in an expression like savings bank location information. There is one \( W_{\text{MAX}} \) (the whole expression), but four \( W_{\text{MIN}} \)s (savings, bank, location, and information) and seven \( W \)s, that is, subexpressions of rank W (the five already listed, plus savings bank and savings bank location).

Syntactic representations (hereafter, “synreps”) and syntactic rules (hereafter, “synrules”) are mutually characterizing. The synrep for an expression provides information about the grouping of material into
constituents, which are subexpressions relevant for synrules; about
the category, rank, depth, and other relevant properties of individual
constituents; and about ordering, grammatical relations, and other
relevant relations between pairs of constituents. A synrule is then a
stipulation as to how assemblages of such properties of expressions
are paired with semantic interpretation. Constituents of rank W are
the building blocks for the purposes of synrules.

Prosodic groupings: w

It is well known that the grouping of linguistic material into prosodic
domains, which are stretches relevant for phonological rules, is not
necessarily the same as the grouping into constituents, though there
are intimate relationships between the two. At least one prosodic
domain, the "phonological word" \( w \) of Nespor and Vogel (1986) (they
use the Greek letter omega to refer to this domain) and earlier works,
is approximately co-extensive with the Ws of syntax.

The relationship between the units of morphosyntax – whether
these are constituents, as in the current subsection, or tactemes, as in
the next subsection – and the prosodic domains of phonology is ex-
pressed in a set of prosodic domain formation rules, which give gen-
eral matchings of units (W to \( w \), for instance) as well as matchings
specific to particular items or classes of items. See Volume 4 of the
Phonology Yearbook for further discussion of prosodic domain for-
amation.

Properties of expression types

Inventory elements: lexemes

In Syntactic groupings: \( W, W_{\text{MAX}}, W_{\text{MIN}} \) I considered the grammatical
analogue of the atom; now consider the grammatical analogue of the
chemical element: an expression type of a language that occurs free
and is an elementary object in the inventory of free expression types,
in the sense that it is not a composite of other free expression types.

Like everything else I have to say here about fundamental concepts,
this is a characterization, not a definition. What we mean when we
say that an elementary expression type occurs free might well be as
complex as what we mean when we say that a chemical element, like
technetium (atomic number 43), samarium (62), or californium (98),
occurs free; the properties of these elements are such as to make them
extraordinarily rare in occurrence or extraordinarily short-lived or
extraordinarily likely to occur only in combination with other ele-
ments, so that they are in principle free, but in practice very hard to
find loose on the hoof. Lexemes are also in principle free, but their freedom is hemmed in by all sorts of considerations: a lexeme might be confined to occurrence in only a few syntactic constructions, even just one; it might occur only in a single idiom; its semantics or pragmatic values might severely restrict its distribution; it might be subject to prosodic or other phonological conditions that greatly limit where it can occur.

Indeed, similar remarks hold for being elementary. An atom of helium could be viewed as an atom of hydrogen with an electron tacked on, but that doesn't cause us to abandon the classification of helium as a chemical element. The fact that a lexeme like HAPPILY can be viewed as an instance of the lexeme HAPPY with an affix tacked on shouldn't cause us to abandon the classification of HAPPILY as a lexeme. (Later, I will consider "superlexemes," like the unit instantiated as cat's in The cat's hungry.)

Some terminology will be necessary here; as in Zwicky (1989a, 1990a), I will engage in a certain amount of deliberate coining and stipulation of meaning. Call any free expression type in a language a "tacteme," any elementary tacteme a "lexeme" (note that nothing I have said so far would ensure that each lexeme occurs only as a single W, or that each W is an occurrence of a single lexeme), and call any other tacteme a "syntacteme." Let the (infinite) inventory of tactemes in a language be its "tacticon," partitioned into a "syntacticon" (also infinite) of syntactemes and a "morphicon" (possibly also infinite) of lexemes. Generalizations about the content of the syntacticon are expressed by a special type of synrule, specifically devoted to the description of free expression types. Other types of synrules concern themselves with syntactic valency (or dependency) and with the form of syntactic constituents, and nothing guarantees that a valency set can make a constituent on its own (the subject and its predicate complement in I am happy and I am in Ohio and I am a professor cannot do so), or that a constituent can occur free (they be here in I insist they be here cannot do so), or that a syntacteme is a single constituent (Flowers to the judge can occur free, as an answer to a question, but it is demonstrably not a single constituent). Generalizations about the content of the morphicon are expressed by what I will call "morrules"—terminology intended to suggest "morphological," but without dragging along whatever further assumptions people might have about what morphology is.

It is an important factual, rather than terminological, observation that while it is reasonably common for morrules to be contingent on phonological properties of lexemes, synrules never seem to be. The latter observation, elevated to a theoretical assumption, is the Princi-
ple of Phonology-Free Syntax of Zwicky (1969) and Pullum and Zwicky (1988). I will assume it here, which means that morrules in a sense constitute the initial entry point of phonology within the grammar.

Each lexeme can be viewed as a set of properties, which will in some sense be present in all occurrences of the lexeme. These crucially include some semantic properties, some phonological properties (which I will assume can be organized into one or more “phonological representations,” or phonreps, each of which is a coherent structured assemblage of phonological features), and some syntactic properties. Morrules then fall into two large classes: “cross-lexeme” rules, those having to do with generalizations relating (the properties of) distinct lexemes, and “within-lexeme” rules, those having to do with generalizations relating different properties within lexemes. There will of course be subtypes of each. For instance, garden-variety derivational rules (DRs) constitute a subtype of the cross-lexeme rules, and garden-variety inflectional rules (IRs) a subtype of the within-lexeme rules.

**Forms, v-forms, p-forms, m-forms**

In a very simple world, each lexeme would have constant phonological content; its pronunciation might vary according to general phonological rules (applying within domains that might be syntactic, prosodic, or even morphological), but phonological stipulation would be limited to a single phonrep for each lexeme. We live very far from this simple world.

To begin with, many lexemes in many languages have alternative phonreps (often called “forms”) according to the syntactic properties of the Ws instantiating them. That is, phonologically distinct versions of a lexeme are often available to convey information about agreement ([3, SG, PRS] jumps and [1, SG, PRS] jump for the verb lexeme JUMP in English) and government ([PSP] jumped and [PRP] jumping for JUMP). The relevant syntactic properties I will call “i-features” (so as to suggest “inflectional features”). Synrules constrain the distribution of i-features on constituents of various sizes, including those of rank W. We can then consider each constituent of rank W as possessing some full coherent combination of i-features, what I will call a value set, or “v-set.” Such a rank-W constituent instantiates some particular lexeme, and we can ask what phonrep corresponds to this v-set for this lexeme.

The properties of a lexeme, then, include a collection of “entries,” each entry being a pairing of a v-set with a phonrep. (In the next section I will revise this characterization of the phonological proper-
ties of a form.) Note that there might be gaps in this collection, that is, there might be v-sets for which no phonrep is provided (as is the case for the English modal lexemes and all v-sets containing the i-feature [-FIN]); that there might be more than one entry for a given v-set (as is the case for the English lexeme DREAM and all v-sets containing the i-feature [PST], given the alternatives dreamed and dreamt); and that there might be more than one entry for a given phonrep (as is the case for DREAM and the v-sets containing the i-features [PRS, 1, SG] and [PRS, 3, PL], both of which correspond to dream).

If we are to be careful, there are in fact four distinct notions here, and linguists are not always clear as to which one they mean when they use locutions like “the (inflected) forms of a word.” I would prefer to keep the unadorned term “form” for an entry, as I have already done. But we might want to refer to entries by means of their v-sets, to refer to “v-forms,” so that there is a single v-form for dreamed and dreamt. And we might want to refer to entries by means of their phonreps, to refer to “p-forms,” so that there is a single p-form for run, corresponding to v-sets containing either [PSP] or [PRS], but distinct p-forms for dream and dreamt.

And we certainly want to group entries into sets by virtue of phonological identities predicted by the morphs of the language, to treat [PSP] jumped and [PST] jumped as somehow the same, despite their irreconcilable difference in i-features, and to treat [PRS, 1, SG] jump and [PRS, 3, PL] jump as somehow the same, despite their irreconcilable difference in i-features, while treating [PSP] run and [PRS] run as distinct, despite their phonological identity. The difference is that between systematic (motivated, morph-rule-governed) and fortuitous (accidental) phonological identity. The full set of forms is partitioned into subsets of forms that are phonologically identical by virtue of morph rule application; each such subset I will call an “m-form.” It may well be that it is m-forms that linguists usually have in mind when they talk about “forms” or “inflected words.”

I have to stress here that I am not proposing representations for a lexeme’s set of entries. I am merely trying to make some essential conceptual distinctions in the underlying logic. I am not concerned here with how parochial generalizations or theoretical principles might be embedded in a system of representations, or whether systems of representation can be devised that are perspicuous, efficient with respect to storage space and/or access time, or psycholinguistically plausible, but rather with more fundamental issues, having to do with what sort of information should get represented in the first place.
Shapes

As if all this were not bad enough, there is another layer of lexeme-specific phonological alternation, involving not the i-features associated with Ws instantiating a lexeme, but instead the phonological and syntactic properties of the environments in which these Ws occur.

The English indefinite article lexeme has only one form (in the sense of "form" I am using here), but it still exhibits a phonological alternation between a and an (depending on the phonological environment of a W instantiating it). Welsh lexemes have various sets of forms, but for most lexemes – some are immune – each of these forms comes in a series of variants distinguished according to their initial consonants, and there seems to be no plausible way to treat this alternation (which depends on what syntactic construction the W instantiating the lexeme is in, on whether this W is initial in its constituent, and on what lexeme an immediately preceding W instantiates) as a matter of i-features. I will refer to English a and an, Welsh cathod, gathod, chatod, etc. (all versions of [PL] CATH "cat"), and similar examples as alternative "shapes" for the same form.

It appears that rather than associating a single phonrep with a v-set, an entry must (in general) provide an indexed set of phonreps, that is, an indexed set of shapes. The indices serve as pointers to particular shapes for the purposes of "shape rules" (SHRs), which express generalizations about the association between an index and the phonology of a shape with that index. Entries are then organized in at least two dimensions, once by v-sets and once by these indices. Within-lexeme morphrules of two types express generalizations about the two phonologically relevant dimensions within entries: IRs (the natural name would be "form rules"), expressing generalizations about the association between v-sets and phonology, and SHRs.

Some sort of articulation between the two dimensions, of forms and shapes, is obviously necessary. It appears that this is managed by having one special, distinguished, shape ("d-shape") in each set (note that I do not require that the d-shape have the same index in each entry for a single lexeme, or that d-shapes have the same index in parallel entries for different lexemes), which serves as the link between the two dimensions. IRs relate the d-shapes, and SHRs treat the d-shapes as defaults. This is (admittedly at some remove) an observation about factual matters, not a logical necessity.

Stems

In a very simple world, in the dream world of orthodox generative phonologists, for each lexeme there would be a single "stem," a single
coherent collection of rudimentary information about the lexeme's phonological characteristics, a single "basic" or "underlying" phon-rep, which would serve as the basis both for predicting d-shapes by IRs and for predicting the stems of other lexemes by cross-lexeme rules (so that it serves as the articulation between phonological information within lexemes and phonological relationships across lexemes). Once again, this is not the world we live in.

Indeed, there are problems with both parts of this proposal – with positing only one stem for the purposes of IRs and with positing only one stem for the purposes of cross-lexeme rules. The relevant phenomena are very familiar. In Russian, for instance, some verb lexemes have PST forms that use a stem that is one consonant (one dental stop, to be precise) shorter than the stem used by other forms (for the "place" verb, /kra/ in the PST, /krad/ otherwise). And for an Arabic verb lexeme, as discussed at length by McCarthy (1981), there is a whole series of stems, each used in one or more rules describing the phonology of related lexemes. Consequently, we must assume that for each lexeme there is an indexed set of stems, the indices serving as pointers to particular stems for the purposes of IRs and cross-lexeme rules.

Now the stem set for a lexeme is not itself an unorganized collection of phonreps. There are often systematic relationships between different stems, holding for whole classes of lexemes; these generalizations will be expressed by "stem rules" (STRs). Sometimes it is possible to hypothesize a single, perhaps strikingly underspecified, phonrep, an abstract stem (like the triconsonantal skeleton for verbs in the Semitic languages) which will serve as the basis for predicting several stems in this set, via other STRs. In general, there will one or more distinguished stems ("d-stems"), abstract or not, from which all the others can be predicted, either directly or through a series of predictions (with stem X predicted from a d-stem and stem Y predicted from stem X, for instance).

**Elementary idiosyncratic lexemes**

Much of the content of a morphicon is predictable, though much is not. STRs predict a lot of the stem list for a lexeme – in some instances, all of the stem list except one stem, though suppletive stems and other surprises (like the Latin verb stem *vert* - that is used in [PRF] forms and is anomalously identical to the stem used in the [PRS] and some other forms) pop up occasionally. IRs predict the association between v-sets and d-shapes – for some lexemes, in all of its details, though suppletive forms and other surprises (like the English [PSP] *run* anomalously identical to the [PRS]) pop up occasionally. SHR predict
the shape sets in forms—for some lexemes, in all of their details, though there are completely lexeme-specific alternations like the one between a and an in English.

There is at least one further type of within-lexeme rule, usually called "lexical redundancy rules" (LRRs). These express generalizations connecting different properties of lexemes, among them: syntactic subcategories to which the lexeme belongs; i-features associated with the lexeme (like gender for nouns); morphological properties, in particular "paradigm class" properties, which describe the set of IRs appropriate for the lexeme; phonological properties of some specified stem of the lexeme; and semantic properties of the lexeme. Though some of the properties of a lexeme are predictable via LRRs, some are surprises, and of course there must be some core of properties that follow from no others.

It is an important point, made in Jackendoff (1975) but insufficiently appreciated, that nothing guarantees that there is a unique minimal core of unpredictable information about lexemes, that there is only one way to reduce a redundant property structure to one that will serve for predicting the full structure by rules, cannot be further reduced without losing this first characteristic, and has no more properties stipulated than any property structure with the first two characteristics. Systems of the complexity of the typical morphicon will usually admit of multiple minimizations, thanks to the fact of mutual predictability (as when it is true both that nouns in a particular declension class are by default of the [MASC] gender and that nouns of the [MASC] gender are by default in that declension class). In fact, it seems to me that the whole project of minimization is doomed because it is ill-defined, there being no natural metric that will make comparable phonological properties (like monosyllabic nature), paradigm class properties (like using a special rule for the [PSP] rather than the default), syntactic subcategory properties (like serving as a head verb in the subject-verb inversion construction), i-features (like [FEM] gender), and aspects of meaning (like referring to an animal hunted for sport).

To return to rules predicting the content of a morphicon, there is also predictability via cross-lexeme rules. Some of these rules predict the entire content of certain lexemes, but again there are surprises (like the meaning of WOMAN-IZE) and again there must be some core of unpredictable lexemes, constituting the very large inventory of primary lexemes (HAPPY, KANGAROO, etc.). In fact, so much of the content of the morphicon is unpredictable that some linguists have come to look upon the morphicon as the location par excellence of idiosyncrasy. But it is clear that some lexemes are in no way (or only a
bit) idiosyncratic and that many expression types with idiosyncratic associations of meaning and sound are not lexemes. (I have in mind certain affixes, like the /z/ verb suffix or the -ness of abstract nominals in English, and syntactic idioms, like GET NP'S GOAT or GIVE VOICE TO NP). There is simply another domain of concepts here, which has only a distant relationship to the morphicon as I have characterized it.

Much of the content of the syntacticon is unpredictable, though much is not, and the same is true of the morphicon. We might think of the unpredictable stuff as constituting distinguished subparts of the syntacticon and the morphicon (an "idiosyntacticon" and "idiomorphicon," together making up an "idiosyncraticom," along the lines of the suggestions in Zwicky 1989a), but it isn't clear to me that there is any theoretical advantage to taking this view, given my comments above about minimization. What is important is that generalizations about the morphicon are captured (this is what cross-lexeme rules, LRRs, STRs, IRs, and SHRds do), that generalizations about the syntacticon are captured (this is what syntactic rules of various types do), and that idiosyncratic associations of meaning and formal characteristics can be grounded in some finite list of elementary idiosyncratic tactemes.

The point is that the idiosyncrasies of the lexemes WOMANIZER, WOMANIZERISH, WOMANIZERISHLY, and the like should be grounded in the (partial) idiosyncrasy of WOMANIZE, and that the idiosyncrasies of the syntactemes NO VOICE BE GIVEN TO NP, NO VOICE BE LIKELY TO BE GIVEN TO NP, BE HARD FOR ANY VOICE TO BE LIKELY TO BE GIVEN TO NP, and the like should be grounded in the (partial) idiosyncrasy of GIVE VOICE TO NP, in both cases grounded via the mediation of cross-tacteme rules. The tacticon of a language is infinite, but it arises from finite collections of elementary idiosyncratic tactemes (roughly, but only roughly, what Di Sciullo and Williams 1987 call "listemes") as combined according to a finite set of cross-tacteme rules.

We might think of the full collection of elementary idiosyncratic lexemes as the "kernel" of the morphicon, the whole morphicon being predictable from this kernel via cross-lexeme rules. One way to interpret what has been called a "word-based" (as opposed to "morpheme-based") theory of morphology is that it takes this kernel as its primary object of description, consequently laying great stress on "the inheritance of irregularity, both semantic and phonological" (Aronoff 1988:768, who credits Booij 1987 with making inheritance a central matter; see also Zwicky 1987d).
CHOICES

Given this conceptual background, I turn now to various questions about how best to flesh out a framework for morphology. For some of these issues, a position has been foreshadowed in my initial discussion; for others, what I have said so far suggests no particular stand. I do not imagine that this list of issues is complete (see Zwicky 1989b, 1990a for discussion of some further choices, for the most part at a finer level of theoretical detail than the ones I consider here). No doubt the full range of morphological frameworks now under active consideration is on the order of McCawley’s (1982) “thirty million theories of grammar.”

In each subsection I oppose two ideas about morphology, maintaining in each case that some variant of the proposal listed first should be adopted over the other one. There are seventeen of these subsections, seven having to do with what I view as major fundamental issues in morphological theory today, ten treating subsidiary or background matters. The main points, which are so labelled in the text that follows, concern a full versus a minimal morphicon, static-condition versus derivational analyses in morphology, realizational versus combinatory morphology, autonomous morphology versus a unified morphosyntax, modular versus unitary morphology, monolayer versus layered morphology, and coanalysis of expressions versus lexemes that are coextensive with Ws.

Major issue: full versus minimal morphicon

Much of the literature on generative morphology maintains that there is a fundamental theoretical question as to whether something called “the lexicon” is full and highly redundant (so that morphological rules describe what is predictable within it), or minimal and redundancy-free (so that morphological rules supply what is predictable within it).

Now that we have cleared the conceptual underbrush a bit, it should be obvious that the two positions here are not necessarily incompatible. If “the lexicon” is the morphicon, then it is of course full; the morphicon is simply the domain of linguistic expression types that morphology concerns itself with. But there is also something that is both finite and notably less redundant than the morphicon in this sense, namely the kernel of the morphicon. The kernel is the list of lexemes that together with the set of cross-lexeme morrules provides an account of (what is predictable in) the full inventory of lexemes, and so is of some theoretical interest. But there can be no
issue as to whether theorists should concern themselves with the morphicon or its kernel.

On the other hand, I can find no good place in a morphological framework for a truly minimal list of any sort. And every defense I have seen of a minimal morphicon, right up to Bromberger and Halle (1989), rests not on theoretical observations but rather on claims about the mental morphicon. Somehow, whenever theorists start talking about the morphicon they end up worrying about what people can keep in their heads.

**Default-setting versus absolute rules**

The morrules of a language are generalizations about the contents of the morphicon for that language. All generalizations about a particular language are, of course, subject to exceptions, and this observation is as true of morrules as of any other type. Consequently, all morrules must be seen not as placing absolute conditions on the contents of the morphicon, but rather as providing default settings. For instance, the form predicted by an IR might be pre-empted by a form predicted by a different IR rule (as when [PST] hitted is pre-empted by hit); the form might be pre-empted by some brute lexeme-particular property (as when [PST] comed is pre-empted by came); or there might be a lexeme-particular gap (as when the [PSP] for STRIDE is, for many speakers, missing).

The default-setting character of morrules leads to a methodological problem. When speakers can, without reflection, produce a series of lexemes, forms, or shapes that they have presumably neither heard nor spoken before and that conform to some generalization, then we have clear evidence that these speakers have grammars that incorporate this generalization in one way or another. We have some information about how the morphicon for such a speaker extends beyond the lexemes this speaker happens to have experienced. But there are generalizations, such as the one predicting "zero" [PST]s for a set of lexemes (HIT, SET, TREAD, etc.) with monosyllabic stems ending in /t d/, for which evidence from systematic novel creations is wanting. The methodological problem arises when we try to decide whether the weight of the evidence (and abundant counterevidence) warrants positing a morrule in situations like this one. I take this to be a question about the grammars of individual speakers, so that I take it for granted that speakers might differ considerably with respect to the generalizations captured in their grammars, but there is still the problem of deciding what the system is for any particular speaker. I believe there is no simple answer to this question, no index of rule-hood
that can be calculated; the issue can be decided only by exploring the consequences that would follow from having, or not having, the rule in the grammar— which means that in many specific situations we will know of no way to remedy our ignorance.

**Major issue: static-condition versus derivational analyses in morphology**

Consider first an assemblage comprising syntactic and paradigm class properties, a stem, the i-features in a v-set, and a phonrep. How do IRs work to license such an assemblage, in particular to determine if the phonrep is the d-shape corresponding to the other properties? One way to view an IR is as a set of static conditions on the makeup of such assemblages. Another way is as part of a derivation; on this view, an IR is a set of conditions on the pairing of assemblages containing different phonreps, with the given assemblage licensed if it is derivable as the last in a licensed sequence of assemblages beginning with one that has the stem’s phonrep as its phonrep.

Then consider the corresponding question for two lexemes, where we have two assemblages, each comprising at least syntactic and paradigm class properties, semantic properties, and a phonrep. How do DRs work to license such a pair, in particular to determine if the phonrep of the “derived” lexeme characterizes the stem corresponding to its other properties? Again, a DR can be viewed as a set of static conditions on the joint makeup of the two assemblages; or as a set of conditions on the pairing of assemblages having different phonreps, with the derived lexeme’s assemblage licensed if it is derivable as the last in a licensed sequence of assemblages beginning with the other lexeme’s.

Metatheoretical considerations always favour the static-condition view (though of course they yield to arguments from factual considerations). The point is that a derivational analysis has available to it auxiliary representations, representations other than those whose wellformedness is at issue. Auxiliary representations can provide “scratch space” for checking whether the given ones are licensed, but this scratch space is not available in the static-condition view. The result is that a derivational analysis cannot in general be translated into a static-condition analysis without at least loss of generalization (which arises from having to embed the conditions on one rule in another rule when collapsing them into a single rule). Derivational frameworks for IRs and DRs are thus inherently more powerful than static-condition frameworks; in the worst case, the scratch work available in a derivational framework can mimic the effect of any arbitrary
Turing machine. This is power we do not want to use unless we have to.

It appears that the power of derivational frameworks is needed in phonology (that is, for systems of rules that function merely to determine pronunciation, without also pairing semantics with phonology, as IRs and DRs do), but in morphology as in syntax I believe that a program of static conditions is at least plausible.

Martin Kay observed, in his comments on the presentation version of this paper, that since reversibility is a crucial consideration in computation, static-condition, or declarative, systems of description can be hard to distinguish from derivational, or procedural, systems when these are modelled computationally. Auxiliary representations vanish when two types of representations are associated via a reversible mapping, Kay pointed out. For theoretical linguists, however, there is still an issue here, which turns on matters of descriptive power and of linguistic adequacy (in particular, whether generalizations are necessarily lost when derivational descriptions are framed as static conditions).

Chained versus direct association between “basic” and “surface” phonreps

Does the phonological side of morphology involve a chain of associations between “basic” and “surface” phonreps, or will a single association suffice? The latter claim lies at the center of Koskenniemi’s “two-level morphology,” which has been influential in computational approaches to morphology. As Koskenniemi (1983:15) describes this proposal:

The lexicon contains the morphophonological representations of word entries and endings. The phonemic surface level consists of phonemes, or letters of a phonemic alphabet. The essential difference between the two-level model and generative phonology is that in the former, there are no intermediate stages between the lexical and the phonemic representations. Instead, the two representations are directly related to each other. The relation is formulated with parallel rules which may refer to both of these two representations. Mathematically speaking, individual rules may be regarded as equations, and all rules together as a set of equations. The rules “do” nothing as such, they only test whether the correspondence is correct.

However, it seems that we cannot do without a certain number of auxiliary representations in morphology. Indeed, I have already argued that a (finite, and, one hopes, not very large) set of stems must
be available for each lexeme, and these stems can serve as auxiliary representations.

Consider how we determine if [PST, MASC] /kral/ is licensed as a form of the "place" verb lexeme in Russian. The relevant IR stipulates that the suffix /l/ appears in the [PST] and that stem X is used in the [PST]. When we ask whether /kra/ is in fact licensed as stem X for this lexeme, we appeal to a STR that relates a stem Y ending in a dental stop to a stem X that lacks this consonant, so that we ask whether this lexeme has a stem Y /krat/ or /krad/ (or /kra/, on which the STR would have no effect). Indeed, the lexeme in question has a stem X /krad/, and we conclude that [PST, MASC] /kral/ is licensed. Notice that this reasoning involves an appeal to the phonrep /krad/, which is not present in the representation we were checking in the first place.

Similar remarks hold for shapes. Determining that the Welsh "soft mutation" shape weld represents the [INF] form of the verb lexeme meaning "see" means determining that this form has gweld as its d-shape, there being a SHR relating d-shapes with initial /g/ to soft mutation shapes lacking this consonant.

Such references to auxiliary representations, even if they involve invocations of several phonreps in turn, do not of course bring upon us the full power of derivational frameworks, since the auxiliary representations in question are finite in number (and supplied in the morphicon).

**Static versus dynamic logic for rule interactions**

In a static-condition framework, interactions between rules follow (as in Zwicky 1989c) from a logic of overrides and invocations, involving both parochial stipulations and principled predictions as to these relations between rules.

The scheme of rule interactions in Zwicky (1989c) is not in general reversible, and so is not an attractive one for computational implementation. Reversibility can be achieved, but at the cost of linguistic generalizations. Consider what it would take to make the following conditions reversible: (a) the yes-no question construction (C1) in *Had Chris clapped?* invokes (or uses) the subject-auxiliary inversion (SAI) construction exemplified in the clause *had Chris clapped*; (b) the main-clause information question construction (C2) in *When had Chris clapped?* invokes SAI; (c) the fronted negation construction (C3) in *Never had Chris clapped* invokes SAI; (d) the conjunctionless counterfactual conditional construction (C4) in *Had Chris clapped, we would have blushed* invokes SAI; (e) the subject-VP (SVP) construction in
Chris had clapped is the default with respect to SAI. It can be done; we state one condition determining SAI (if C1 or C2 or C3 or C4, then SAI) and one determining SVP (if neither C1 nor C2 nor C3 nor C4, then SVP). But of course this description refers to each of C1 through C4 twice, and so misses generalizations.

In derivational frameworks (for any component[s] of grammar), interactions between rules are a matter of applicational sequence, or ordering, and the question arises as to whether these orderings can always be predicted by universal principles or whether parochial stipulations are required in some instances. Metatheoretical considerations lead us to hope for the former, since derivational frameworks with parochially stipulated ordering are more powerful still than frameworks without it; see the discussion by Pullum (1979), who reviews the situation in syntax (building on the work of Koutsoudas and his colleagues) and concludes that the program of universally determined ordering is well supported there. Matters are none too clear in phonology (but see the survey in Zwicky 1987c, which gives hope for universally determined ordering there) or in morphology (though Zwicky 1987b suggests optimism here as well).

However, not only do almost all generative morphologists adopt a derivational framework, most of them also allow parochial ordering restrictions to get the effect of feeding and bleeding. This is true even of relatively modular frameworks, such as those of Anderson (1986) (where inflectional and derivational morphology are separate modules) and Kiparsky (1982) (where there are layers of morphology – see Major issue: monolayer versus layered morphology, below – that do not necessarily correspond to the inflectional/derivational division).

**Rules as functions versus rules as (general) relations**

Static-condition versions of morrules (of all types) can be viewed as having some directionality, of course: the logical directionality that obtains if the relation described by the rule is a function from one set of morphological objects to another.

Despite occasional claims to the contrary (in particular, by Ford & Singh 1985a, 1985b), I will maintain that morrules of most types are in fact functions, rather than merely relations of any sort whatsoever. I have already adopted this position implicitly in my discussion of DRs, IRs, SHRs, and LRRs. But I will not assume that STRs are always functions; that is, I will not assume that stem sets generally exhibit the sort of internal organization characteristic of shape sets, given the arguments in Spencer (1988) and Perlmutter (1988), among other places, that stem X can be paired with stem Y without instances of
either being uniquely derivable from the instances of the other. Such
demonstrations arise from situations where distinct \( X_1 \) and \( X_2 \) are
systematically paired with a single \( Y \), while a single \( X \) is systemati-
cally paired with distinct \( Y_1 \) and \( Y_2 \).

In situations like these there must be multiple d-stems (in more
traditional terms, multiple underlying representations). This is a mat-
ter left open in my earlier discussion (Stems).\(^4\)

I should point out that for certain types of rules the question of
functionhood can be separately entertained for rules as wholes and
for the phonological relationships within them.

**Rules as redundancy predictors versus rules as generators of the morphicon**

The issue of a full/minimal morphicon intersects with the issue of
functionhood for rules, in particular cross-lexeme rules. If a full mor-
phicon is assumed, then the purpose of morrules is to describe regu-
larity, redundancy, predictability within it. If a minimal morphicon is
assumed, then the purpose of morrules is to generate the full morphy
con from this minimal object by supplying redundant or predictable
properties.

Gaps in the set of lexemes have different consequences depending
on which view we take. Gaps are of two types, missing sources and
missing derivatives, and (as is well known) both are attested: COR-
RODE - CORROSION - CORROSIVE, *AGGRESS - AGGRESSION -
AGGRESSIVE (missing source), INCITE - *INCITION - *INCITIVE
(missing derivatives).\(^5\) In a minimal morphicon framework, missing
derivatives can be described via rule features on the input; INCITE is
not subject to the DR building abstract nominals with -(t)ion or the
one building adjectives in -ive. But missing sources can apparently be
described only by some extraordinary move -- via ghost lexemes like
AGGRESS, via truncation or replacement rules (treating AGGRE-
SIVE as derived from AGGRESSION, as in Aronoff 1976), or what-
ever. Otherwise, the DRs in question are not functions.

The problem does not arise in a full lexeme framework, since the
effect of a DR is not only to relate sources to derivatives but also to
predict that the morphicon should contain lexemes with the properti-
ties of these derivatives. We would not posit a DR in the first place if
there weren't some source lexemes, but there is no problem if some
sources are missing.

**Major issue: realizational versus combinatory morphology**

How do phonreps get associated with meanings in morphology
(whether directly, via IRs or DRs, or indirectly, via STRs)? Is this association to be viewed as the realization of meanings via operations on stems (in a "process-based" framework), or as the combination of stems with morphemes expressing the meanings (in a "morpheme-based" framework)?

There is no dispute here that suffixal and prefixal morphology is the norm. There is also no dispute that other schemes—infixes, interdigitations, segment shifts, reduplications, metatheses, and subtractions (with various subtypes of each)—occur with respectable frequency in the languages of the world. A realizational approach to morphology, as advocated in Zwicky (1988) and Anderson (1988a), tries to assimilate concatenative morphology (that is, affixations) to the other schemes by treating them all as operations on phonreps. A combinatory approach, as advocated by McCarthy (1979, 1981), and many others since, tries to assimilate the nonconcatenative schemes to affixations by treating them all as assemblages of independently meaningful morphemes.

The frameworks are not as different as might at first appear. Realizational morphology must have its combinatory aspects; not only must prefixes and affixes be distinguished, but also, since inflectional morphology tends to have a flat structure (as Lounsbury (1957:384) observed, we often see a number of inflectional affixes in sequence but have no reason to assign internal structure to the sequence), IRs that affix material must stipulate which "slot" the material fills. And combinatory morphology must have its realizational aspects, since feature operations (of copying, spreading, docking, association, reassociation, dissociation, whatever) are required to get phonological features assigned to the right segments, and these are parochially stipulated at least to the extent that one must say which rules use which operations.

There is a certain amount of morphological literature (stretching at least from Lieber 1981, Marantz 1982, and McCarthy's early work already cited, through Martin 1988) in which realizational morphology is opprobriously characterized as "transformational," presumably because for generative linguists syntactic transformations are the formal operations par excellence. The suggestion is that with a realizational framework and its formal operations (but not with a combinatory framework) comes the juggernaut of transformational grammar. Now this is simply false if the realizational framework is of the static-condition, rather than derivational, variety. Indeed, a combinatory framework with derivations must itself find a way to step out of the juggernaut's path.

Transformational doom is avoided in combinatory frameworks by
three further choices, which I consider in the three following subsections. The first of these an intelligent realizational framework should make as well. The two others, which I am dubious about, together have the effect of claiming that there is no morphological component of grammar, independent of syntax and phonology. Such proposals admit that there are morphological objects of analysis, namely lexemes, and that there are rules describing them, but maintain that these rules are of exactly the same sort as the rules of (phrase structure) syntax, their phonological consequences following entirely via the operations of (automatic) phonology. Obviously some metatheoretical advantage attends proposals of this sort, since they simplify the ontology of rule types posited in a grammatical framework. The question is whether this metatheoretical simplification is justified by the facts.

A finite versus an infinite operation set

Transformational grammar lent itself to a formalization in which a "rule of grammar" was any operation describable by composing some number of elementary operations chosen from a finite kernel; crudely, a rule was anything you could represent in the formalism provided for this purpose. As a result, the set of formal operations available, that is, the set of possible rules, was infinite.

This is obviously a dangerous move metatheoretically, and virtually all modern grammatical frameworks propose to limit the set of formal operations to an effectively finite collection, via both formal and substantive restrictions. In the scheme for realizational morphology of Zwicky (1988), for instance, there is a (universally given) finite kernel of specific operations (reduplicate the first syllable, affix /d/ in slot X, etc.) that any given rule can use. And there is a (universally given) finite limit on the number of operations any particular rule can use in concert; the limit follows in part from a formal incompatibility between many pairs of operations and in part from the fact that there is some universal, and not very large, upper limit on the number of slots available in inflection.

Distinct m-ops versus p-ops for both morphology and phonology

A striking difference between the realizational framework of Zwicky (1988) and most current combinatory frameworks is that I posit a set of operations (I'll call them "m-ops") used by morphological rules that is distinct from the set of operations (I'll call them "p-ops") that figure in the rules of automatic phonology, while the combinatory morphologists assume that there is only a single set here, and that it is
phonological in character. (It is probably not an accident that so many of these combinatory morphologists started out as phonologists.) The latter claim is explicit in Martin (1988:236) ("The operations available in morphology are exactly the operations available in phonology"), who cites McCarthy (1979) as the source of the idea.

My position is not of course that the two sets of operations have nothing to do with one another, but rather that the m-ops are (historically) morphologized p-ops – p-ops that have come to be viewed as markers of meaning in morphology and so can be expected to exhibit the telescopings, inversions, and other reinterpretations that accompany morphologization. The two sets of operations will thus bear a family resemblance to one another, but we cannot expect every m-op to have a plausible treatment as a p-op or sequence of p-ops. In Zwicky (1988) I used umlaut in modern German and "(trisyllabic) shortening/laxing" in modern English as paradigm examples of m-ops that are not p-ops, and it would be easy to extend the list for pages, with soft mutation in Welsh, velar softening in English, and so on.

The response of combinatory morphologists in such cases is to maintain that the phonological relationships described by the m-ops at issue can always be decomposed into p-ops; see, for instance, the heroic (and quite divergent) efforts along these lines by Yip (1987) and Myers (1985, 1987) for English laxing, and the general negative answer provided by Lieber (1987) to "the question of whether phonology and morphology require two different sets of principles for dealing with sound patterns of languages" (Archangeli 1988:792). Aside from the question of whether the proposed analyses cover the facts as intended, it seems to be a constant feature of these decompositions that they require parochial stipulations as to the order of application of the p-ops they appeal to. Usually this is a matter of assigning two "phonological rules" to different layers of a layered morphology (see *Major issue: monolayer versus layered morphology*, below), but that is still parochial stipulation. Presumably, with some ingenuity it will always be possible to mirror a diachronic sequence of p-ops as a synchronic one and so to translate any m-op analysis into a p-op counterpart, but I would scarcely view stipulated ordering of rules as a good trade for a bit of ontological simplification (especially if it turns out that there are other reasons not to adopt the layered framework).

*Distinct morphotactics versus phrase-structure morphology-syntax*

The other striking characteristic of current combinatory frameworks (Williams 1981, Kiparsky 1982, and Selkirk 1982 are representative
texts) is that they import the main features of phrase-structure syntax into morphology and treat the two domains as subject to the same general principles of organization – in particular, that constructs are made up of a head of category X and an argument of category Y, that the construct belongs to the same category (X) as its head, and that the head is subcategorized for occurrence with arguments of category Y. Morphology is then, in Selkirk’s words, “the syntax of words” (in my terms, the tactics of lexemes).

At least for category-changing derivational morphology in a combinatory framework, it follows that the affixes are the heads; on this account, -ness is a head N sub-categorized to combine with a preceding ADJ argument. In a realizational framework, the morphological rule itself not only specifies the relationship between the category of the source and the category of the derivative, but also selects the affix; on this account, a DR relates source ADJs to derivative Ns, the phonological content of this relation being an operation appending -ness to the source. There is not much to choose between the two views at this level. But there are two further questions to answer: Do other syntactic principles have morphological counterparts? Do other types of morphological tactics find a natural treatment in similar terms? I believe that the short answer to both questions is no.

No other syntactic characteristics in morphology

(I’ll confine myself largely to category-changing derivational morphology here, since this has been the primary domain for discussions of combinatory morphology.) The big point here is that essentially no characteristics of syntax beyond the ones already mentioned carry over to morphology, and these are simply the characteristics of what I called the “determinant” (morphological or syntactic) in Zwicky (1985a).

First, alternative orderings of head and argument are common in syntax, but there are no good parallels in morphology.

Second, syntactic constituents normally admit optional modifiers, but no (putative) morphological constituents do.

Third, syntactic heads frequently agree in various properties with their arguments, but there is no parallel property sharing between a derivational affix and its stem.

Fourth, syntactic constructions can require that some immediate constituent must contain one or more lexemes of some specified type, as the English focused negation construction does with respect to negative lexemes in its initial constituent (as in At no time did I claim that pigs could fly), but morphological constructions do not do so. Indeed, the very idea of “strict cyclicity” (Kiparsky 1982), according to
which the morphological composition of a lexeme is not available to
morphological rules building on that lexeme, presupposes that such
containment conditions do not occur in morphology.

Fifth, there are special elements (including zeroes) available in syn-
tax—sometimes filling the head position, sometimes the argument
position—for the purposes of deixis, anaphora, and indefinite refe-
rence. There are no special elements (zero or otherwise) devoted to
these purposes in derivational morphology. There is, for instance, no
way to suppress an argument of a putative head in morphology when
this head can be supplied from context, the way object NP arguments
can be suppressed in syntax (as in The deer moved to the stream and
drank or Terry didn’t know if pigs could fly, and Tracy wouldn’t say); there
is nothing like *They were sad, and their-ness bothered me (or for that
matter, *Their sadness bothered me, but not their happy, with a suppressed
head).

Sixth, certain properties of syntactic constructs are normally real-
ized as special forms of the head, via inflectional morphology (which
can be nonconcatenative as well as concatenative). Derived lexemes
do have inflectional forms, of course, but the properties in question
are not realized by material affixed specifically to the putative head
constituent, that is, the derivational affix (this would mean that the
internal structure of a form like sadnesses would have to be SAD +
NESS + ES]) or by an operation specifically on this head, which
would predict that the normal scheme for nonconcatenative inflection
would be an alteration specifically in the phonology of derivational
affixes.

**Morphotactic phenomena not in syntax**

For morphotactics of some interest, I turn to inflectional morphology,
where affixes flourish. The phenomena we see there call for a descrip-
tive scheme rather different from the combinatory rules of syntax,
something more like the morpheme order charts of the structuralist
morphologists (for instance, Harris 1951:Ch. 19) or the slot and filler
formulas of tagmemics (Cook 1969:Ch. 1, with references to the pri-
mary literature), something very close, in fact, to the templates pro-
posed by Perlmuttter (1971) for stating conditions on the combinabil-
ity and sequencing of bound word clitics. What we want is a
realizational framework incorporating a “slot calculus” of the sort
sketched in Zwicky (1990a).

First, while there is always default semantic content associated with
a slot in an ordinary syntactic construction, the slots of inflectional
morphology and of clitic groups are not necessarily characterizable in
semantic terms. For English verb forms, for instance, there is only
one slot, which is filled by material representing either a nonfinite
category ([PRP] or [GER] in jumping, [PSI] in jumped) or a finite cate-
gory ([PRS, 3, SG] in jumps, [PST] in jumped); there is no common
semantic content to these suffixes. For clitic groups it is commonplace
for the host slot to lack any semantic characterization, as when the
host is the first W of a clause, and it is reasonably common for some of
the clitic slots to be characterized phonologically rather than semanti-
cally, as when Tagalog (Schachter 1974) puts its monosyllabic pro-
nominal clitics in clitic slot 1 and its disyllabic pronominal clitics in
slot 3.

Second, there are plenty of "zeroes" in inflectional morphology, by
which I mean that the absence of phonological substance stands in a
paradigmatic relationship to the presence of meaningful phonological
substance. Consider what a combinatorial framework must say about
the simple fact that the English form child is specifically [SG] (in
contrast to the [PL] children) and [NOM/ACC] (in contrast to the [SG,
GEN] child's and the [PL, GEN] children's). I can see two sorts of
analyses, neither of which is entirely satisfactory.

One possibility would be to posit that all English noun forms have
two inflectional suffixes, either SG or PL immediately following the
stem, and either NOM/ACC or GEN immediately after that; the mor-
phemes SG and NOM/ACC are both stipulated to be always without
phonological substance. This line of analysis requires stipulated ze-
roes all over the place — as Zwicky (1985b) observes, forms that are
just unaltered stems are extremely common in the inflectional mor-
phology of the world's languages — and it misses a generalization that
a realizational approach captures nicely: that absence of inflectional
material gains its meaning by virtue of its opposition to actually
present material. A realizational approach can say that all forms use a
stem, which is altered only when some rule stipulates this. Thus,
given that English has a rule for the [3, SG, PRS] of verbs and no rules
for the other combinations of persons and numbers in the present
tense, we predict that the normal verb lexeme of the language has five
[PRS] forms with shapes identical to its stem.

This first sort of analysis might be acceptable in a computational
setting. The stipulation of affixes that are present in morphological
analysis but are without phonological content will serve to keep the
mapping between morphology and phonology reversible, and reversi-
bility is, as I noted above, an important goal in implementations of
linguistic descriptions. But, as before, the pursuit of reversibility
forces a loss of linguistic generalizations.

An alternative for the number and case forms of nouns in a combi-
natory approach would be to stipulate that the [SG, NOM/ACC]
form is identical to the stem and that the other forms are built on this form, rather than on the stem itself; *children's* would then be [CHILD, SG, NOM/ACC] + PL + GEN. This avoids the stipulation of zeroes, but at a considerable cost, namely that we have to stipulate the forms that are identical to the stem, rather than those that are not (a particularly distasteful necessity for English [PRS] verb forms, where we would have to stipulate that a form is identical to the stem if it either is not [3] or is [PL]), and we also have to give up a simple additive semantics for inflectional affixes, in favour of a scheme in which, whenever there is a conflict, the semantics of an affix overrides the semantics of the constituent with which it combines.

Third, in addition to the "zeroes" of the sort just discussed there are plenty of "zero" examples of another sort in inflectional morphology. These arise when an affix is suppressed under some phonological condition. Consider, to choose four well-known examples, the failure of the English [GEN] /z/ to appear in forms already ending in a /z/ affix (compare *kids* and *anyone who replies*' responsibilities with *children's* and *anyone who replied's* responsibilities); the failure of the German [PL] schwa to appear for noun stems that end in schwa plus a sonorant (*das Zimmer* "the room," *die Zimmer* "the rooms" versus *das Schaf* "the sheep (SG)," *die Schafe* "the sheep (PL)"); the failure of of the Russian [PST] /l/ to appear for verb stems ending in a consonant (/pek/ "he baked" versus /pek-l-a/ "she baked"); and the failure of *ge-* to appear in German [PSP] forms for verb stems that do not begin with an accented syllable (*trumpetet* "trumpeted" and *versagt* "denied" versus *ge-sagt* "said"). For the first three of these examples, a combinatory framework cannot simply block the appearance of the relevant affix under phonological conditions, for doing so would not supply a morpheme bearing the relevant semantics; apparently, these affixes must be inserted and then deleted, though so far as I know there is never any evidence that such forms in fact have any sort of affix in them.

Fourth, there are discontinuous selections of affixes in inflectional morphology. The German [PSP] forms, which combine a prefix *ge-* and a suffix *-t* or *-en* (*ge-sag-t* "said," *ge-nomm-en* "taken"), give a simple example of this discontinuity. Here it is possible to treat one of the affixes as secondary to, or parasitic on, the other – the occurrence of the prefix as dependent on the occurrence of a suffix, say – but this option does not seem to be available in the general case. Consider the expression of tense/aspect (T/A) and negation in Swahili verbs, for instance. The relevant slot template is, crudely, (NEG) - SUBJ - T/A - (OBJ) - STEM. Matters are straightforward in the future, which has *ta-* in the third slot and the negative marker *ha-* optionally in the first. For
the past and perfect, however, the occurrence of *ha-* in the first slot requires the occurrence of special T/A markers in the third slot, and for the two presents, the occurrence of *ha-* requires both the absence of a marker in the third slot and the selection of a verb stem in -i rather than -a (I have capitalized the T/A markers):

\[
\begin{align*}
\text{wa-TA-[som-a]} & \text{ "they will read"} & \text{ha-wa-TA-[som-a]} & \text{ "they won't read"} \\
\text{wa-LI-[som-a]} & \text{ "they did read"} & \text{ha-wa-KU-[som-a]} & \text{ "they didn't read"} \\
\text{wa-ME-[som-a]} & \text{ "they have read"} & \text{ha-wa-[A-[som-a]} & \text{ "they haven't read"} \\
\text{w-A-[som-a]} & \text{ "they do read"} & \text{ha-wa-} & \text{ [som-i]} \text{ "they don't read, aren't reading"} \\
\text{wa-NA-[som-a]} & \text{ "they are reading"}
\end{align*}
\]

The information that a form is negative thus appears in two out of three possible places within the form (which two depending on the tense/aspect in question), and no two of these three places are contiguous to one another. In a realizational framework, nothing prohibits multiple exponents of the same feature or a single exponent for a set of features; we can say in one rule that *ha-* in the first slot realizes [+NEG], and in others that *ta-* in the third slot realizes [T/A:FUT], that *li-* in the third slot realizes [T/A:PST, -NEG], that *ku-* in the third slot realizes [T/A:PST, +NEG], that the stem in -a is the default, that the stem in -i is used for [T/A:PRS, +NEG], and so on. A combinatory approach would apparently have to choose some "basic" set of fillers for the slots and map these into the actually occurring material by context-sensitive rules of replacement (replacing *li* by *ku*, say, / NEG + SUBJ + _) and deletion (deleting *a* and *na*, say, / NEG + SUBJ + _).

Fifth, in addition to contextually determined suppletive variants (as in the Swahili *li* - *ku* alternation), inflectional morphology occasionally exhibits portmanteau realizations of affix sequences, as when Swahili [NEG] verbs with [1, SG] subjects have *si-* where we would have expected *ha-ni-* (sitasona "I won't read"); and contextually determined variant affix orders (also known as "morph(eme) metathesis"), as when the [2]-subject affixes, [SG] *u-* and [PL] *ni-*, of Swazi (Ziervogel 1952) follow the [NEG] affix in [INDIC] forms (*ka-u-vali, ka-ni-vali* "you don't choose") but precede it in [IMPER] forms (*u-nga-vali, ni-nga-vali* "choose!"). There are well known parallels in the world of bound word clitics: contextually determined suppletive variants, as when "spurious se" occurs instead of the first of two third-person clitics in Spanish (Perlmutter 1971); portmanteau realization, as when *kita* occurs instead of any combination of the second-person clitics *ka* and *ko* in Tagalog (Schachter 1974); and contextually deter-
mined variant orders, as when first- and second-person clitics precede third-person [ACC] clitics with [INDIC] verb forms but follow them with [IMPER] verb forms in French (Tu me le donnes "you give it to me" but Donne-le-moi "give it to me!").

Now a combinatory morphology certainly can manage to describe facts like these, but apparently only at the cost of developing a fresh—and derivational—theory of rules that replace, delete, and reorder inflectional affixes within their forms and clitics within their groups. There is nothing truly comparable in garden-variety syntax. We do not have situations where some specific verb lexeme is missing, or is replaced by some other verb lexeme, just in case a specific noun lexeme serves as its object; or where some specific combination of verb and object-noun lexemes is always replaced by a special lexeme; or where some specific combination of verb and object-noun lexemes occurs in one order in the [INDIC] but another order in the [IMPER]. (General ordering principles of this sort do of course occur, but what is notable about the inflectional and clitic facts is their item-specificity.)

Major issue: autonomous morphology versus unified morphosyntax

Most current morphological frameworks draw a sharp line between the domain of lexemes and their properties, on the one hand, and the domain of constituents and syntactemes and their properties, on the other, and for good reason. I will do the same, with the intention of having a Principle of Morphology-Free Syntax (PMFS), a Principle of Syntax-Free Morphology (PSFM), and a No Phrase Constraint (NPC) all follow from this component division.

The PMFS—variants of which are known as the Lexicalist Hypothesis (see the summary in Scalise 1984:101-2), Atomicity (Di Sciullo & Williams 1987:Ch. 3), or the Lexical Integrity Hypothesis—says that a synrule has no access to the morphological composition, or the purely morphological properties, of the lexemes instantiated by the Ws whose distribution it describes. This is the analogue, in the morphology-syntax interface, of the strictness of strict cyclicity. The PSFM says that a morrule has no access to the syntactic properties of the expressions within which the lexemes and forms it describes are instantiated. This is the analogue, in the morphology-syntax interface, of the cyclicity of strict cyclicity, according to which you can’t refer to properties that are available only on a cycle external to the one you’re on. The NPC (see the discussions in Scalise 1984:154-6; Bates 1988:sec. 3.3) says that a morrule builds only on objects from the morphicon (stems, lexemes, forms, or shapes), not on constituents or syntactemes.
I must point out that there are facts that might be taken as suggesting that morphology and syntax together form a single component of linguistic description (a morphosyntax, or tactics), as in early transformational grammar (Lees 1960), in some of the tagmemic literature (Elson & Pickett 1983), and in recent work by Mark Baker (1985, 1988a, 1988b). These include (a) the very common phenomenon of alternation between or co-occurrence of “particle lexemes” and inflectional morphology, as when English has alternative comparative expressions (handsomer, more handsome) and Swedish marks definiteness doubly (det store huset “the big house,” with both the definite article det and the definite inflectional suffix -et); (b) the tendency for syntactic argument possibilities to be preserved in derivational morphology, as when the constituents of a clause like The serfs rebel against the landowners reappear within an NP, in the rebellion of the serfs against the landowners; (c) the reproduction of phrasal syntax to some degree within lexemes, as in the “synthetic compounds” apple-eater and purple-eyed; and (d) lexeme-internal constituents that bear syntactic relations outside the Ws that instantiate their lexemes, as in the noun incorporations discussed by Sadock (1985).

Counterbalanced against these are the familiar differences between the organization of morphemes within Ws and the organization of Ws within Ps (phrases): morpheme order is nearly always fixed, whereas “free variation” in the order of Ws is common; many W-internal morphemes are bound, whereas most Ws instantiate (free) lexemes; constituents of Ws cannot be separated by interposed syntactic units, whereas constituents of Ps often can be; the principles governing prosody and segmental phonology within Ws are different from those operative within Ps; and nonconcatenative morphology frequently serves as an alternative to affixal morphology, but serves as an alternative to independent Ws within Ps only for Ws instantiating particle lexemes.

The PMFS is supported by the fact that properties of individual affixes – their phonological shapes or even their (non)occurrence – seem to be irrelevant for syntactic purposes; only the corresponding (abstract) syntactic properties of whole Ws are germane. The fact that the forms dogs, cats, and horses have (automatic allomorphs of) the regular [PL] suffix, that oxen has a special [PL] suffix, and that sheep has no suffix at all plays no role in English syntax, which cares only about the i-feature [PL]. Similarly, the fact that HAPPINESS has one derivational suffix, that GAITY has another, and that JOY has none at all plays no role in English syntax, which cares only that all these lexemes belong to the syntactic category of abstract nouns. Stems in derivation and inflection are also syntactically inert; they cannot, for
instance, be extracted or replaced by pro-forms of any sort (*What did you want-ists in your band?, *I wanted people to play guitar, so I called it-ists).

The PSFM is supported by the fact that IRs and DRs seem not to be contingent on the syntactic surroundings of the Ws instantiating the lexemes they describe, except insofar as this environment requires forms with certain i-features or lexemes belonging to certain syntactic (sub)categorizations. For instance, whether a noun lexeme has a [PL] form at all, or which declension class its [PL] form is drawn from, depends only upon properties of the lexeme and its stem and not upon what sort of syntactic construction it finds itself instantiated in, and similarly for the ability of a noun lexeme to serve as source in a DR describing diminutive derivatives.

**Feature versus formative interface between syntax and morphology**

The PMFS, PSFM, and NPC all follow automatically if synrules and morrules constitute separate components of grammar, if a realization-al rather than combinatory view of morphology is taken, and if the interface between syntax and morphology is managed entirely via (abstract) features – in particular, syntactic (sub)categorization features, i-features, and "shape properties" (triggering SHRIs and prosodic domain formation) – rather than by having syntactic formatives that correspond to the affixes of morphology.

A formative interface between syntax and morphology is one hallmark of early generative grammar, where we routinely find affixes manipulated (inserted, moved, regrouped, conflated, and deleted) by syntactic rules. Consider, for example, the famous Affix Hopping rule of English (the "Auxiliary Transformation" of Chomsky 1957), which reorders and regroups the "underlying" material PRS (HAVE + EN) (BE + ING) DANCE into (HAVE + PRS) (BE + EN) (DANCE + ING), which is to say, had been dancing. In Lees (1960) this rule is extended systematically to derivational morphology, where it serves to yield dancer from ER + DANCE and happiness from NML + HAPPY. But not only is the familiar Affix Hopping analysis undesirable on metatheoretical grounds, it also suffers from a number of grave difficulties in its interactions with other parts of the analysis of English (subject-auxiliary inversion, the placement of the supportive modal lexeme DO, and the description of the negator n't), to the extent that it has been described as "more a liability than an advertisement for transformational grammar" (Gazdar, Pullum, & Sag 1982:614, where an alternative analysis is sketched that improves on Affix Hopping on every front, including adherence to the PMFS).
Major issue: modular versus unitary morphology

Should we posit a number of subcomponents of morphology, or should morphology be viewed as a single system? I opt for a modular approach, indeed for a particular version of this approach in which at least STRs, DRs, IRs, and SHRs are distinguished. (Further subcomponents will be suggested below.)

One attractive consequence of modularity is that it allows for the prediction of huge classes of rule interactions through the "natural cyclicity" imposed by the nature of the modules themselves. Consider the particular brand of modular morphology that I am espousing here. An STR describes a stem, so it applies before any rule (IR or DR, or for that matter, another STR) that uses that stem. A DR describing derivatives that can themselves serve as sources to another DR naturally applies before it. A DR describes a stem for a derivative lexeme, so it applies before any IR describing forms of that lexeme, which in turn applies before any SHR describing shapes for certain of these forms.

One objection to this sort of modular view is that it misses the generalization that exactly the same sort of operations (m-ops, I would say, though objectors usually suppose they are p-ops) figure in all the posited subcomponents, whatever these happen to be. Admittedly, in a modular view it must be stipulated that there is a set of operations available for all types of morrules, but then something similar must be said about the kinds of features and conditions on feature distribution that play a role in synrules of various sorts. I can't see that there is a problem here.

It may be that objectors are inclined to think that each (sub)component of a grammar must constitute a representational level, that is, a set of representations on a symbolic inventory unique to that component. Distinctness in representational level provides a strong argument for component separation, but I can see no support for the converse reasoning, no grounds for insisting that separate components must be distinct representational levels.

Another objection, which would follow from the expectation that distinct components should be distinct representational levels, is that the line between derivation and inflection (which plays some role in nearly all modular approaches) is not a formal difference and so is hard to draw in particular instances. The line between verbs and adjectives is hard to draw sometimes, too, and so is the line between subjects and direct objects. But in all of these cases there is a clear distinction between prototypical instances – the clear instances of derivation have the function of extending the morphicon by predicting
sets of lexemes from others, the clear instances of inflection provide forms of lexemes to serve as marks of syntactic constructions — and there is a set of classificational rules of thumb that either follow from the properties of the prototypical instances (as does the fact that IRs are external to DRs) or are associated with the classifications in a language-particular way (as is the fact that in English only derivation is prefixal). More than this one cannot ask for in theorizing about the facts of the world.

(Thanks to Anderson 1982 and the response by Jensen and Stong-Jensen 1984, the discussion of derivation and inflection has been framed as a subquestion under the general heading, “Where is morphology?” Is it all “in the syntax” (with or without derivation and inflection being distinguished from one another), or is it all “in the lexicon” (again, with or without derivation and inflection being distinguished from one another), or is derivation “in the lexicon” and inflection “in the syntax?” (Scalise 1988 provides a summary of this tradition.) The only way I can unpack the spatial metaphors here so as to get a coherent theoretical claim — it makes no sense, for instance, to ask whether DRs and IRs are “in” the morphicon — is to reduce these issues to the PMFS, which does not seem to be in dispute in this literature and which in any event concerns derivation and inflection equally. There may be a theoretical issue here, but I haven’t found it yet.)

Modular frameworks have also been scorned because they posit several “minicomponents” or “tiny modules,” as Sadock (1985:383) puts it. This might just be another variant of the objections already considered, or it might be a metatheoretical claim that unitary frameworks are in general better than modular ones. The metatheoretical issue is a complex one. On the one hand, we do not want to posit components without good reason — this was the basis for my objection above to combinatory treatments that required a set of replacement, deletion, and reordering rules for inflectional morphology — but on the other hand, we want to place as many substantive constraints as possible on rules and their interactions, and modular frameworks lend themselves admirably to this purpose. These particular metatheoretical considerations simply don’t decide the question at hand, and indeed I would not want to say that metatheoretical considerations can ever be said to decide a theoretical question.

Major issue: monolayer versus layered morphology

Having argued in favour of modular frameworks, I now turn to a
particular type of modular framework, namely the "level-ordered" morphology of Kiparsky (1982) and Mohanan (1986), which I will not adopt here.

The key claim is that each morrule, or at least each DR and IR, can be stipulated as belonging to exactly one of a small finite set of linearly ordered components of morphology, which I will call "layers" (they are known in the trade as "levels" or "strata"), and that from this assignment follow a variety of properties of the morphological configurations that the rules describe, all having to do with what is intuitively the "closeness" of affixes to their stems. The properties in question are the following: (a) how close, in linear distance, an affix is from the ultimate stem within a form (the layers of affixes nesting within one another like Russian dolls or the layers of an onion); (b) how closely bound an affix is phonologically to the stem it attaches to (the affixes of the interior layers being tightly bound to, phonologically active with, their stems, and the affixes of the exterior layers being relatively independent of, phonologically inert with respect to, their stems); and (c) to some extent, how closely bound an affix is semantically to the stem it attaches to (the affixes of interior layers tending to be tightly bound to their stems in semantically idiosyncratic combinations, the affixes of exterior layers tending to be independent of their stems in semantically compositional combinations).

(For the purposes of this exposition, I am pretending that all the world is affixes, rather than translating everything into terms that are neutral as between concatenative and nonconcatenative morphology.)

There is no doubt that there is a considerable tendency for these three sorts of properties – one having to do with morphotactics, one with morphophonology, and one with morphosemantics – to co-vary. Such co-variation is the natural result of historical change; we would expect interior layers to reflect older morphological formations, exterior layers more recent ones. A certain amount of co-variation also follows from the distinction between derivation and inflection, given that derivational morphology is both internal to inflectional morphology and more likely than inflectional morphology to be semantically idiosyncratic. The question is whether the layers, insofar as they do not simply reproduce the derivation-inflection distinction (as indeed they do not in any of the detailed layered analyses in the literature), should be treated as theoretical constructs.

Note that the scheme of layers does not follow (as does the distinction between derivational and inflectional morphology) from foundational considerations in grammatical theory. Layering is posited on the basis of observations about linguistic facts. This lack of grounding in an existing theory makes layering metatheoretically suspicious, on
the one hand, but exciting to theoreticians, on the other, since we have the sense of discovering an unexpected fact about language, of being surprised by a hidden orderliness in our world. Neither the suspicion nor the excitement makes an argument, of course.

There is now a considerable destructive literature on layered morphology, which argues, by reference to various paradoxes in layer assignment in particular languages, that the hypothesis of layering brings in its train more problems than it solves; see the gloomy summary in Gussmann (1988:237-9). Much of this literature – I have in mind especially Aronoff and Sridhar (1983), Zwicky (1987b), Churma (1987), and Fabb (1988) – is also constructive, in that it suggests ways in which the very real facts that layering attempts to cope with (facts like the contrast between *Mendelianism* and *Mendelismian* in English) can be described without extraordinary theoretical steps.

For example, rather than stipulating layer assignments for a mor-rule, in some instances we stipulate eligibility conditions on the units the rule builds on (possibly including conditions on their purely morphological properties, such as belonging to the “Latinate” or “Anglo-Saxon” class of lexemes in English). In other instances we appeal to a distinction in stem types, stipulating that the morrule uses a particular stem (and hence appealing in turn to STRs); distinctions between “inner” or “primary” morphology and “outer” or “secondary” morphology (as in Sanskrit, or in the English distinction between comparative -er as in longer and agentive -er as in singer) are then distinctions between rules using one (root-like) stem and those using another (word-like) stem. In still other instances we appeal to assignments of the “phonological” rules to distinct components of the grammar, whose interaction is governed by universal principles; for example, nonautomatic phonological alternations are described in a component or components that are presupposed by the component(s) describing automatic alternations – this is the “rules before processes” arrangement of Donegan and Stampe (1979) – with the result that the (nonautomatic) regressive voicing assimilation in left “precedes” and consequently bleeds the (automatic) progressive voicing assimilation in beefed versus heaved.

*Overlapping versus independent accounts of phrasal and isolation phonology*

Can phonological variants that appear in phrasal contexts be described together with isolation variants, or do the two sorts of phonological variation always have separate accounts?

I assume (with virtually everyone who has considered the phenom-
ena of external sandhi) that each form of a lexeme has a d-shape that serves as the default phonrep for describing the phonology of phrases, and I assume, along with Kaisse (1985) as well as with Kiparsky and Mohanan, that nonautomatic phrasal phonology (a matter of "postlexical rules," in their terms) belongs to a separate component or components from lexeme-internal and form-internal phonological alternations (a matter of their "lexical rules"). The question is whether the formal apparatus for SHRs is significantly different from the apparatus for DRs and IRs (as, I would argue, the formal apparatus for nonautomatic phonology is significantly different from the apparatus for automatic phonology), or whether the formal resources a framework makes available to SHRs–these constitute the inventory of m-ops for SHRs–are shared with those for other phonological alternations (as the formal resources for DRs are shared with those for IRs).

The simplest assumption from a metatheoretical point of view is that there is a single inventory of m-ops, available for STRs, DRs, IRs, and SHRs. The ones that serve in SHRs as well as in morrules of other types would be said to be involved in "rules used both postlexically and lexically" in Kiparskyan terms. Such m-ops are not uncommon. The Welsh consonant mutations, for example, serve not only in a variety of celebrated syntactic contexts, but also in DRs involving prefixes; Williams (1980: sec. 187) lists prefixes requiring the "soft" mutation on the stems they combine with, others requiring the "spirant" mutation, and still others requiring the "nasal" mutation.

Note that the assumption is merely that the framework provides a single inventory of m-ops, not that the grammar of any particular language uses the same m-ops in SHRs as in other morrules. The proposal is perfectly consistent with a language that exhibits some m-ops used only in its SHRs and others never used in its SHRs.

I have no great stake in this assumption, and it would be possible to argue that substantive differences should be expected between the m-ops of SHRs, rules primarily serving a demarcative function, and the m-ops of DRs and IRs, rules primarily serving a signifying function. Certainly the differences in their predominant functions can be expected to lead to strong preferences for certain sorts of m-ops in SHRs and for other sorts in DRs and IRs–edge mutations are "good" m-ops for SHRs, while affixations are "good" m-ops for DRs and IRs, to speak of these things in the fashion of the natural morphologists (Dressler et al. 1987)–but that does not mean that these preferences should be elevated to theoretical restrictions. Indeed, if the semiotic considerations provide a sufficient account of the way in which m-ops
are deployed in the grammars of the world’s languages, there is no need for the theory of grammar to say anything on the matter.

*Major issue: coanalysis of expressions versus lexemes that are co-extensive with Ws*

With some awkwardness, I have maintained the conceptual distinction between Ws as syntactic (and syntagmatic) tokens and lexemes as morphological (and paradigmatic) types, which has meant talking about Ws that instantiate lexemes rather than simply about “words.” This is not simply the stubbornness of someone with just enough training in logic to care about such things. There are morphosyntactically problematic phenomena – including, though not necessarily limited to, some of the things that have been labelled as compounds, incorporations, serializations, phrasal affixes, bound word clitics, and (syntactic) portmanteaus – that resist any simple analysis in which each W of syntax instantiates a single lexeme and each lexeme is instantiated by a single W. Sadock (1985) and Di Sciullo and Williams (1987) make this point very clearly, though with a rather different set of conceptual distinctions and theoretical hypotheses from mine, and from each other’s.  

First, there are lexemes whose parts act as Ws syntactically. Compounds, for instance, are lexemes built (by “compounding rules,” or CORs, formally parallel to DRs) on two or more lexemes, each represented by one of its stems or one of its forms. Syntactically, compounds are Ws composed of Ws; that their parts are Ws can be seen in co-ordination examples like *plumbing and concrete repair* and *plumbing installation and repair* and in anaphora examples like *I’m the Congo representative, and you’re the Togo (one)*. And (bound-word) clitic groups are lexemes built (by “cliticization rules,” or CLRs, formally parallel to IRs) on two or more lexemes, one having its full set of forms and the others represented by special shapes. Syntactically, clitic groups are just sequences of Ws, not necessarily forming a syntactic constituent of any sort; this is quite clear in English Auxiliary Reduction examples like replied’s in *The person who replied’s going to be in trouble.*

Second, there are lexemes that are indivisible from the point of view of morphology but which correspond to sequences of two or more syntactic Ws. I argued in Zwicky (1987a) that this is the situation for the French portmanteaus *au “to the [MASC, SG],” du “of the [MASC, SG],” aux “to the [PL],” and des “of the [PL]”* (though rather different things are going on in superficially similar German and Italian examples). The portmanteau *du* corresponds to the sequence of syntactic
Ws *de* *le*, and I can see no evidence in favour of saying that syntax concerns itself with anything other than this sequence.

Third, there are Ws which as wholes instantiate no lexeme, though the Ws that make them up do instantiate lexemes. This is the sort of analysis I would give to French causative clause union constructions, like *fait partir* in *j'ai fait partir Jean* "I made Jean leave." Syntactically these behave like units for the purposes of object ordering (*j'ai fait Jean*le *partir*, but *Je l'ai fait partir* "I made him leave"), but there is no reason to treat such a unit as instantiating a single lexeme.

There are two rather different sorts of morphological formations here, illustrated by the COR for *Congo representative* and the CLR for *replied's*. Ordinary CORs are just like DRs except for the fact that they involve more than one source lexeme. A DR or COR stipulates the syntactic and morphological properties of the derivative lexeme, describes the derivative's semantics as a function of the semantics of the source(s), and describes the phonological content of the derivative's stem(s) as a function of the phonological properties of the source(s). The phonological side of such a rule can ask for a stem, either unaltered (as in "zero derivation" and in the compound examples of the type *Congo representative*) or as modified by particular m-ops (as in ordinary affixal derivation, or in German compounds with first elements extended by *-s* as in *Arbeitszeit* "worktime" or *-en* as in *Sternenschein* "starshine" (see Lieber 1981:13, who observes that these are not inflectional affixes)). Or it can ask for a stipulated form, again either unaltered (as in the zero conversion of English [PSP] forms like *frozen* or *broken* to ADJs, or in English and German compounds with [PL] first elements, like *abstracts committee* and *Bücherfolge* "series of books" (see Lieber 1981:14f., who observes that in this set of German examples the idiosyncrasies of [PL] inflection are reproduced in full) or as modified by particular m-ops (as when French builds manner ADVs by affixation to the [FEM, SG] form of an ADJ, for instance in *faussement* "falsely").

Clear examples of CLRs are really very different. To begin with, it scarcely makes any sense to ask what syntactic or morphological properties a clitic group like *replied's* has. Such an object is not a syntactic constituent of any sort, so we can obtain no information about its syntactic properties, and it does not participate in further morphological formations, so that we have no evidence as to its morphological properties either. As for its semantics, this is supplied entirely from the syntactic constructions that it···Ws *replied* and *is* participate in; the CLR contributes nothing. All the CLR provides is information about the phonology, and it does this not by stipulating stems or forms for the participating lexemes, but rather by stipulating
shapes for particular participating forms. The rule responsible for replied's calls for two sources, the first of which is the d-shape for any form of any lexeme in the language, the second of which is (let us say) shape 2 for a finite form of an auxiliary V lexeme, which for the [PRS, 3, SG] form is of the lexeme BE is /z/. (The SHR in question stipulates that shape 2 is the final consonant of the d-shape, and it provides a shape 2 for only certain finite forms of certain auxiliary V lexemes— for am, is, are, will, and would, but not for was or were.)

CLR in general are like CORs, in that they combine two or more lexemes to yield a morphological object, but (as I observed in Morphotactic phenomena not in syntax, above) they are also like IRs, in that they call for a single head element and specify ways in which dependent elements may, must, or must not fill various slots. They are unlike either CORs or IRs in that they build on shapes, not (stems or forms of) lexemes, and in that the objects they describe are inert so far as the rest of the morphology is concerned. I will refer to such large morphological objects (whether these are clitic groups or something else) as “superlexemes.”

The description of superlexemes involves a syntactic side, a morphological side, and an account of the articulation between the two. With respect to the interface between morphology and syntax, we must at the very least require that all $\text{W}_{\text{MIN}}$s in an expression instantiate some lexeme, and that no $\text{W}_{\text{MIN}}$ simultaneously instantiate two distinct lexemes, and we must provide some scheme for adjudicating conflicts between the morphicon and the inventory of syntactic constituents with respect to the properties they assign to corresponding units. I will assume (following the lead of Sadock 1985) that the universal default for negotiating such conflicts is that morphology overrides syntax, in particular, that when the morphicon and inventory of syntactic constituents disagree as to chunking for the purposes of prosodic domain formation, it is the morphicon that wins (thus, clitic groups will generally constitute prosodic domains, regardless of syntactic constituency).

But parochial stipulations are also needed. We need to be able to stipulate, for instance, whether a superlexeme in the morphicon of a language is merely an alternative (as is the clitic group realized's with respect to the sequence realized is) or whether it precludes the matching of one W to one lexeme (as du does in French, *de le being unavailable as an alternative). And we need to be able to stipulate a matching between a particular morrule and a particular synrule, so as to get the effect that the expressions in question must satisfy the requirements of both rules. This is the sort of analysis implicit in Perlmutter's (1971:96) formulation of the morrule for the English “serial verb”
construction in *Ronnie will go wash the dishes* versus *Ronnie goes wash(es) the dishes*: "Output condition on the go Verb construction: go Verb." The reference to the "go Verb construction" invokes a particular syntactic rule combining a head V with a complement VP, and so calls up conditions that allow only a few V lexemes in the head slot (GO and COME, plus for some speakers RUN, HURRY, and a few others), that require an activity interpretation for the complement VP, that mark the construction as informal in style, and so on. The main part of the morrule itself is intended to impose at least two further conditions, that the head V lexeme be in a form whose shape is identical to the shape of the lexeme's [BSE] form, and that nothing intervene between the two Vs in this superlexeme. The point here is that other V + VP constructions of English don't have to satisfy the conditions provided by the morrule; *Ronnie helps wash the dishes* is fine, for instance, even though *helps* is not identical to the [BSE] *help*, and *Ronnie wants desperately to wash the dishes* is fine, even though it satisfies neither this condition nor the constraint against intervening material.

NOTES

* My thanks to the participants in the conference on Formal Grammar: Theory and Implementation -- especially to my official discussant, Martin Kay, but also to Janet Dean Fodor, Mark Gawron, Tom Hukari, Polly Jacobson, Mary-Louise Kean, Bob Levine, and Ivan Sag -- for their comments and criticisms on the presentation version of this paper. And at Ohio State to Ted Fernald and Mark Libucha, for their useful critiques of the June 1989 version, and to Gina Lee, for her aid in matters of style. This is the version of 9 September 1989.

† The following abbreviations for grammatical categories are used in the text: 1 (first person), 2 (second person), 3 (third person), ACC (accusative case), BSE (base verb form), FEM (female gender), FIN (finite verb form), FUT (future tense), GEN (genitive case), GER (gerund verb form), IMPER (imperative mood), INDIC (indicative mood), INF (infinitive verb form), MASC (masculine gender), NEG (negative verb form), NEUT (neuter gender), NOM (nominative case), OBJ (object marker), PL (plural number), PRF (perfect verb form), PRS (present tense), PRP (present participle verb form), PST (past tense), SG (singular number), SUBJ (subject marker).

1 I adopt the term "rank" from Halliday (1961), though not of course the full conceptual apparatus of systemic grammar that goes along with it.

2 I would have preferred to use the term "lexicon" here, as in Zwicky
(1989a), and I did use this term in the oral version of this paper, but it seems to be hopelessly confusing to many linguists, for whom the word "lexicon" unavoidably calls up either a repository of idiosyncrasy - Aronoff (1989), in fact, decides to use the term unambiguously in this sense - or else what I refer to below as the "mental lexicon." Apparently the only workable solution is to avoid the word entirely.

3 SHR's as characterized here are rules describing the phonological properties of different shapes, where a particular shape is named by its index. Other types of rules distribute "shape properties," that is, the indices themselves. Some such rules are matters of syntax - shape properties often serve as marks of particular syntactic constructions, as Zwicky (1986, 1990b) points out - and others are matters of morphology in an extended sense, a topic I consider in a later section.

4 It is possible, though not necessary, to extend this treatment to situations in which one stem is suppletive (as for the Latin "be" lexeme, for which the stem /fu/, used in [PRF] forms, has nothing to do with the rest of the paradigm), and to situations where one stem is missing (as for the Latin COEP "have begun" lexeme, which lacks the stem used in [PRS] forms).

5 The significance of such facts to generative grammars was first emphasized in Lakoff (1970).

6 I assume in all of this that both synrules and morrules have a semantic side and a phonological side, that in fact what characterizes a rule of morphosyntax is the welding of the two sides into a single coin. Thus I do not explore here the proposals of Beard (1988) to treat the semantic side and the phonological side as two distinct components of grammar.

7 The idea that inflectional morphology is normally flat rather than hierarchical was rediscovered in Thomas-Flinders (1983).

8 Each operation instantiates one of a small number of universally given operation types, but that isn't relevant to the point at hand here.

9 In the appendix to Zwicky (1988) I argue that Myers's proposal does not.

10 Notice that I say merely "not necessarily" here. Particular slots are quite often characterizable in semantic terms, just as particular stems are. The point is that such a characterization is not always available, for slots or for stems, so that a general framework must allow slots and stems to be identified in an arbitrary fashion.

11 Most of the discussion of "postlexical phonology" (as in Mohanan 1986) counterposes automatic phonological alternations to the nonautomatic alternations that are the province of STRs, DRs, and IRs, but there is a growing literature on nonautomatic "postlexical" alternations, much of it the work of Ellen Kaisse (for instance, Kaisse 1986, 1987).

12 Zwicky (1987a) is the immediate predecessor of my discussion here.
REFERENCES

Donegan, Patricia J. and Stampe, David L. (1979). The study of natural pho-
Elson, Benjamin and Pickett, Velma (1983). Beginning Morphology and Syntax. Dallas, TX: Summer Institute of Linguistics