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EXPLAINING LEVEL ORDERING, AND HOW NOT TO PARSE A WORD¹

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Beginning with Allen 1978 (and cf. also the related work of Siegel 1974), a number of researchers have argued that English rules of derivational morphology belong to two disjoint blocks or "levels", and that all of the rules in the first block ("Level 1") must apply prior to those in the second ("Level 2") by virtue of the fact that Level 1 is a module of the grammar that is stipulated (only once) to be an earlier module than Level 2. If this is the case, then it is possible to dispense entirely with English-specific statements concerning the order of application of rules whenever it is the case that one of a given pair belongs to Level 1 and the other belongs to Level 2. In this way, some progress will be made toward **explaining**, rather than simply describing, the interactions of such rules. In this paper, I will argue that, while the level ordering framework seems promising at first glance, a closer look reveals significant flaws. I will then proceed to argue that it is desirable to eliminate level ordering—and in fact morphological rules. In their place would be "subcategorization frames" (Lieber 1980, Churma 1986; cf. also Fabb 1986), which are inherently unorderable. This latter approach will be seen to be preferable for several reasons, including its applicability in psychological and computational models.

1. Level Ordered Morphology.

In order for a level ordering-based theory to have any predictive power, of course, there must be some way of determining, apart from the relative ordering of a given rule, the level to which it belongs. In fact, it has been argued, Level 1 and Level 2 rules differ in a number of respects, in addition to their interaction properties. Some relevant properties of Level 1 rules are listed below:

- (1) Properties of Level 1 affixes:
 - a. Stress-determining (cf. grammatical/grammaticality, derive/derivation)
 - b. Trigger obligatory segmental rules (illegal, opacity, permissive)
 - c. Can attach to non-words (inept, ubiquity)
 - d. Can have lexical idiosyncrasies about which stems they can attach to (resistance/*consistance, *arrivation/derivation)
 - e. Frequently cause semantic opacity (personality, grievance)

Using these criteria, the following lists of representative derivational affixes can be derived:

- (2) a. Level 1: -ic(al), -ance, -ity, -ive, -ation, in-
b. Level 2: -able, -ness, -less, -ize, -ist, un-

If this division into levels is made, then the acceptability facts in (3) follow automatically:

- (3) iconicity, restrictiveness, palatalizable, *fearlessness

In the first example, Level 1 *-ity* attaches after another Level 1 suffix, and in the second and third words, we have a Level 2 suffix that attaches after a Level 1 and a Level 2 suffix, respectively. But the only way of getting the final example would be to have Level 1 *-ity* attach after Level 2 *-less*, and this is impossible in a level ordered theory of morphology such as the ones under consideration. Such a theory thus provides an explanation both for why the acceptable examples in (3) are acceptable and for why **fearlessness* is not. Furthermore, as Siegel notes, the Level 1 affixes are just those which are represented in Chomsky and Halle 1968 as attaching with a +-boundary, as opposed to those that have a #-boundary,

which are now the Level 2 affixes. Unlike in the *SPE* system, however, there is now a reason for the fact that the latter, which do not have the phonological effects that are characteristic of +boundary affixes, generally attach “outside of” the former.

In later work (cf. especially Kiparsky 1982, Mohanan 1982), it has been argued that by dividing up phonological rules in a somewhat similar fashion, it is possible to explain why the phonological properties in (1) correlate with the order of the affixes. Thus, if we assign phonological rules like velar softening and *in-* assimilation exclusively to Level 1, it will be impossible for these rules to affect the output of a Level 2 morphological rule (1b); similarly, if stress assignment is restricted to Level 1, we can see why affixation of a Level 2 affix has no effect on the stress pattern of the base. Kiparsky also argues in favor of making a further level distinction such that we have the following overall picture:²

- (4) Level 1: +-derivation and irregular inflection
 Level 2: #-derivation and compounding
 Level 3: regular inflection

By adopting such a system, Kiparsky argues, it is possible to explain also the following kinds of acceptability facts:

- (5) lice-infested, understood, *walkedable, *rats-infested

In the first two examples an irregularly inflected base serves as the input to compounding (where *understood* is analyzed as a compound), but the input to derivation and compounding in the last two would have to be regularly inflected forms—a violation of level ordering if this model is accepted.

In fact, however, the relationship between inflection and compounding is a good bit more complicated than these few cases might lead one to expect, and there appears to be no motivation for distinguishing between types of inflectional processes on the basis of their interaction with compounding. Consider the following:³

- (6) a. *children psychologist, *mice doctor, *was-in, Publications List, Jets fans
 b. Raider Rooters, Women Fencers, (*Girls Fencers; cf. also Lady Bucks)
 c. five-dollar bill, 55 mile-an-hour speed limit, trouser leg, Illinois[z]/Illinois[\emptyset] legislature.

In (6a), we see that the predictions made by Kiparsky’s model are not always correct, although there are cases (6b) which appear to be appropriately predicted. In (6c), we find that inflections can disappear from what are apparently plural-internal phrases, and that quasi-plural markers (cf. **a trouser*) and even innocent by-standers can be swallowed up by a compound if they look enough like the regular plural marker. Furthermore, as Kiparsky notes (p. 85), the fact that Level 1 inflection cannot provide inputs to Level 2 derivation (or Level 1 derivation, it should be added) “require[s] an ad hoc constraint to block it.” While it is unclear exactly what the appropriate explanation for this set of facts is (cf. Churma 1983b for discussion), I think it should be clear that they do not follow from the way in which the ordering of the levels has been revised. I will therefore confine the remainder of my discussion in this section to the interaction of Level 1 and Level 2 derivational affixation.

Aside from the above considerations, the modifications incorporated in Kiparsky’s model appear to provide even greater explanatory power, since now both the order of affixation and properties (1a, b) follow from the theory—and without any need for recourse to abstract boundary distinctions. Unfortunately, there are numerous well-known apparent counterexamples to the predictions made by a theory that incorporates a Level 1 / Level 2 distinction of this type (cf. Aronoff and Sridhar 1983, Churma 1983b, and the references cited there):

- (7) analyzability, ungrammaticality, inalterability, standardization, capitalistic, transformational grammarian, set theoretic

Thus, we find Level 1 *-ity* attaching after Level 2 *-able* and *un-* in the first two examples, Level 1 *in-* and *-ity* both attaching after Level 2 *-able* in the third, and so on.⁴ It is possible, of course, to perform various gyrations in an attempt to save the theory. Perhaps the most obvious move is to claim that there are two different, but homophonous, affixes that belong to different levels (cf. note 13), but this kind of approach will not be able to account for, e.g., the behavior of *-ity* and forms in *-able*. Similarly, one can make strange category assignments; thus, Allen 1978—for whom compounding must follow both levels of derivation—proposes that *non-*, which attaches quite freely to the output of compounding (cf. *non-Jets fan*) is not a derivational prefix, but a (bound) word. (This will not, of course, account for the examples in (7).) Kiparsky 1983 makes a more radical move, suggesting that cases like *ungrammaticality* should be handled by means of a new kind of rule, “morphological reanalysis,” which would allow the generation of forms in which level ordering is not violated (e.g., [un + [grammatical + ity]]), but which are later reanalyzed so that their morphology and semantics are correct ([[un + grammatical] + ity]). It is also possible to simply relax some of the constraints of the theory; this is the tack taken in Strauss 1982, where it is argued that affix-ordering constraints should be required to hold only with respect to successive applications of the same kind of derivational process (i.e., prefixation or suffixation). (Note that this latter proposal actually entails giving up level ordering.) But even if these proposals are adopted, not all of the apparent violations of level ordering can be explained away. Indeed, given the rich variety of types of apparent counterexamples, it seems unlikely that any reasonably simple patch job can succeed in handling all of the different types of cases. There thus appears to be good reason to simply abandon (tentatively) the hypothesis that morphology is level ordered, and to pursue some alternative hypothesis.

Before doing so, however, I would like to discuss briefly a number of phenomena, in addition to the paradoxes, for which level ordering fails to provide an enlightening account; if an alternative hypothesis does provide a good account of them, then clearly this would constitute a good reason for preferring, other things being equal, the alternative theory. First of all, why does Level 1 come first? The answer is simple within a general theory of level ordering, but not terribly enlightening: it comes first because the analyst stipulates (once, to be sure) that it does. Thus, the theory does not rule out the possibility of there being a language just like English, except that Level 1 comes after Level 2. (The levels would presumably not be given these numbers, but that, of course, is irrelevant.) Secondly, no explanation is provided for why the clusters of properties that characterize the different levels should cluster together. Finally, the admittedly unexplained observation of Mohanan 1982 and Mohanan and Mohanan 1984 that the “lexical representation” (i.e., the output of the lexical phonology/morphology) is the only level that is involved in language games, speech errors, etc., is anomalous from the point of view of the theory.

2. An Alternative: Subcategorization Frames.

In the remainder of this paper, I will argue that, insofar as the above characterizations of the phenomena in question are in fact correct, they can make sense of only if level ordering is abandoned. The model that I will argue for depends heavily on the notion of “subcategorization frame” (henceforth SF), in the sense of Lieber 1980, the use of which will allow one to get rid of, not only the levels of level ordered morphology (and hence the paradoxes as well), but even the morphological rules themselves.⁵ Since it is not possible to impose an order of application on a pair of SFs, it will be impossible in principle to achieve the effect that extrinsically ordering a pair of morphological rules would give. At first glance, it might appear that this kind of approach is doomed from the start, since

the use of ordering statements plays a significant role in many approaches, especially with respect to the relative order of inflectional affixes (cf. Anderson 1982). An alternative to this kind of rule ordering will be outlined below.

Crude approximations to the kinds of SFs that would be required for English morphology are given below.⁶

- (8) Sample subcategorization frames:
- a. Plural /z/: [[X, +PL]_N ___]
 - b. *-ness*: [[X]_{ADJ} ___]_N
 - c. *-ance*: [[[resist, impede, malfeas-, ...]] ___]_N
 - d. *-ation*: [[[derive, attest, confront, ...], [X + {-ize, ...}]] ___]_N
 - e. *-ity*: [[[X, +LAT]_{ADJ}, [X + {-al, -able, ...}]] ___]_N
 - f. *in-*: [___ {{{complete, sufficient, -ept, ...}}, [X + -able, ...}]]

The case of (regular) plural formation (8a) is quite straightforward: this suffix subcategorizes for attaching to a noun (stem) that has the feature [+PL]. The morphosyntactic/semantic feature [+PL] and the category feature N will percolate to the top of the morphological tree by virtue of a general set of percolation conventions, and hence be visible to further relevant morphological "processes" (in this case, none), as well as the morphosyntax/semantics.⁷ Similarly, in (8b), we require that the host of *-ness* be an adjective,⁸ but we must also require that the result of attaching this suffix be a noun.⁹ It is necessary, however, to stipulate precisely which stems may take *-ance* (8c), just as in the case of, say, nouns that form their plural in *-en*.¹⁰ It is this fact that accounts for why *-ance* may not attach after "Level 2" affixation—not because this suffix and "Level 2" affixes reside in different strata. We must also state which stems may take *-ation* (8d), but this suffix differs from *-ance* in an interesting way: it may also attach to a verb that has been formed by suffixing *-ize* (cf. *spirantization*, *relativization*, etc.). It is this latter fact about the lexical representation of *-ation* that allows violations of level ordering with respect to these two suffixes—but not with respect to *-ize* and *-ance* (**palatalization*, etc.).¹¹ That is, "Level 1" affixes are simply those that have fairly restricted SFs. Some are more restricted than others, in that no general subclass of items can be identified, unlike in the case of *-ation* and its fondness for things that contain *-ize*. The fact that *-ity*, too (8e), has a particular fondness for certain other affixes, as well as (monomorphemic) Latinate adjectives (cf. *Latinateness*/**Latinativity*, *restrictiveness*/**restrictivity*), explains why it can have a fairly restricted set of monomorphemic items that allow it to attach, and still attach freely to forms that contain "Level 2" *-able*.¹² This in turn entails that some kind of reference to specific affixes must be made in order to account for these facts regardless of the framework employed. The fact that *-ive* is not a member of *-ity*'s select group of affixal friends explains the *restrictiveness*/**restrictivity* contrast, as well as the experimental results of Anshen and Aronoff 1981, where it is found that subjects show a strong preference for *-iveness* words over *-ivity* words, but an equally strong preference for *-ability* over *-ableness*. The SF for *in-* indicates that (i) there is no category change involved, (ii) it is somewhat choosy about what it will attach to (cf. **ingrammatical*, **innatural*), and (iii) it is particularly fond of adjectives that contain (a relative of?) the *-able* that *-ity* is so fond of (cf. *incompatible*, *invisible*, *inviolable*, *imperceptible*, *inadvisable*; however, cf. also **inimaginable*, **inquestionable*).¹³

There are facts about English derivational morphology that might appear, at first glance, to be problematic for any theory that rejects level ordering. Kiparsky 1982:28 brings up the following set of acceptability facts, which he argues follow from level ordering:

- (9) a. unable *unability inability
 b. unequal *unequality inequality

These facts are said to follow from the ordering of the levels, since Level 1 *-ity* would be unable to attach after Level 2 *un-*, whereas Level 1 *in-* would allow further Level 1 affixation. But in fact the putative inputs to *-ity*-suffixation are themselves unacceptable (**inable*, **inequal*), which is problematic for any theory which, like the one advocated here and that of Kiparsky, prohibits the postulation of "bound derived lexical items" (Kiparsky 1982:23). Clear cases of this kind are not easy to find; a search of the on-line *Webster's New Collegiate Dictionary* on the Turing system at Stanford for words that begin with *in* and end with *ity* turned up in addition only *civil* as an example that agreed with both my judgments and the dictionary (I find *instable* unacceptable, but it is listed in *Webster's*). It is thus not clear that we are dealing with anything other than simple cases of lexical listing of the forms in *-ity*.

Note that nothing has been said so far about any distinctions involving derivation, inflection, and compounding. The model as it stands would thus predict rather free interaction of the various kinds of processes. In fact, all logical types of interaction are attested even in English, which has a fairly impoverished inflectional system and hence presents relatively few possibilities for inflection to interact with another type of process, and examples of each have already been given. There appears to be a universal tendency, however, for compounding to take as inputs uninflected stems—a tendency which is as unexplained in the present framework as in any other that I know of (but cf. Churma 1983b, Dressler to appear, for discussion). It is also quite rare for derivation to affect an inflected form, and this kind of interaction seems to occur in English only via an intermediate application of compounding, as in *non-Jets fan* (cf. Churma 1983b), or via zero-derivation of adjectives from past participles (Jensen and Stong-Jensen 1984, Zwicky to appear).¹⁴ But there are apparently genuine cases in other languages in which inflection and derivation abut directly and the derivation is of the garden-variety affixation type, as is the case in, among others, French (Moody 1977), Maliseet (Sherwood 1983), Slave (Rice 1985), Greenlandic (Sadock 1985), and Navaho (Newton 1986), and at least sporadically in German (cf. *Kind-er-chen* 'children-dim.' (Bloomfield 1933, Dressler to appear)). The SF for the regular French adverb-forming suffix *-ment* is given in (10):

(10) French *-ment*: $[[X, +FEM]_{ADJ} \text{ — }]_{ADV}$

That is, *-ment* attaches (in the unmarked case) to the feminine form of an adjective (cf. *lentement*, *franchement*), rather than the masculine. (This assumes, of course—contra Bloomfield—that the feminine is derived from the masculine, and not vice versa.)

Both compounding and inflection present kinds of difficulties that derivation does not. Note that it is probably not desirable to attempt to treat compounding in terms of subcategorization, since it seems counterintuitive to say that, e.g., a noun subcategorizes for attaching to a preceding adjective or another (preceding or following) noun, and it would be extremely difficult to describe the semantics associated with compounding. Much of what goes on in compounding is sensitive to pragmatic factors (cf. especially Downing 1977); surely it is not the job of the morphology to account for, for example, the quite different interpretations normally assigned to *paternity suits* and *maternity clothes*! Syntactico-semantic factors also appear to be heavily involved (Lieber 1983), and it would seem that essentially nothing needs to be said about compounding in the morphology aside from the fact that it happens. Since nothing in the SF of a noun, for example, requires that it attach to any particular kind of element, the facts about compounding will follow without further stipulation, as long as morphology-external factors such as these can be sufficiently well articulated.

Inflection presents a different kind of problem, especially in languages that are highly agglutinative or polysynthetic. This problem concerns how to represent the subcategorization restrictions of "outer" affixes, since there is a wide variety of morphological entities that such affixes may end up next to whenever the intervening material can be only optionally

present. Note that it is not possible to give the outer affixes SFs that contain a disjunction of all of the entities that they may follow (or precede), since there would be no way of preventing the affixes from occurring in alternative orders, unless perhaps there is some way of enforcing a kind of hierarchy with respect to the members of the disjunction, so that the otherwise outermost affix "wins". Even if this kind of hierarchical ordering within an SF were to be allowed, we would find, most strikingly in languages that allow long strings of affixes, that the SF for an affix that occurs immediately outside another affix would have exactly the same SF as the inner affix, except that there would be one more member of the disjunction. Clearly, this massive repetition indicates that a generalization is being missed. This generalization is expressed traditionally by setting up a number of "slots" and specifying which affixes may fill which slots. That is, inflectional affixes typically occur in a fixed order, and the disjunctive SF approach is essentially ignoring this fact, so the problems it faces should not be surprising. Lieber 1980 sets up an elaborate system whose purpose is to get diacritics that, in effect, encode affix-ordering information, and which will form part of the SFs of inflectional affixes, into just the right places. This system is extremely awkward even when dealing with the relatively simple case of Latin, and would become even more so if applied to a language that has more than the three inflectional slots that Latin has. I see no reason why the traditional approach should not be maintained, and adapted to the present system, where it would take the form of a "positive surface filter." (This is one of the alternatives suggested in Churma 1983b, and has also been advocated more recently in Muysken 1986 and Zwicky to appear.) That is, any sequence of inflectional affixes must fit what might be termed an "inflectional template" in order to be deemed well-formed. By making use of such a filter, it is possible to give inflectional suffixes, for example, a quite simple SF, such as the following, where Y is the relevant lexical category:

(11) $[[X]_Y \text{ ___ }]$

Given that the suffixes would necessarily be attached simultaneously under the SF approach, the result would be the flat structures that appear to be characteristic of inflection (cf. Newton 1986, Zwicky to appear), as opposed to derivation, where hierarchically arranged structures are the result of affixation. Notice that there is abundant evidence for hierarchical structure in the case of derivation, in the form of the category requirements in the SFs of affixes. For example, since *-ity* attaches only to adjectives, we know that *-able* must have attached to *alter*, and created an *ADJ* node at the top of the morphological tree, before *-ity* could attach (and itself create another higher *N* node of the tree). Due to the non-category changing nature of inflection, there can be no parallel kind of evidence for hierarchical structure in such cases. In the absence of evidence for hierarchicality, it would not seem unreasonable simply not to posit any, and this is exactly what the SF approach makes necessary.¹⁵ Thus, the fact that the order of inflectional affixes cannot be handled readily using only SFs need not lead to abandonment of the general SF approach.

The model being proposed, to sum up, has the following properties. Both derivation and inflection are handled in terms of SFs; compounding is the result of free concatenation of relevant words. The semantics of word-formation is strictly compositional (compounding aside), since it is part of the SF. Since no rules are involved (at least not in a crucial sense—cf. note 5), it will not be possible to achieve the effects that ordering full-fledged morphological rules could give. This is desirable, since, for one thing, there is absolutely no evidence that language-specific ordering of morphological rules is necessary (despite apparent evidence such as the order of inflectional affixes treated above). Furthermore, it would not be possible to maintain even that the rules must be linearly ordered, since it would be necessary to allow a single rule to both precede and follow another rule (cf. *Jets fans* and *non-ex-priest/ex-non-believer*) if the rule approach were to be adopted.¹⁶

It is also worth discussing some aspects of an overall theory of morphology that follow from this kind of model. First of all, morphological productivity will necessarily be a gradient phenomenon, since the degree of specificity of an SF in the lexical representations of affixes can vary widely. It has long been known that productivity is just such a phenomenon.¹⁷ Furthermore, given that the (productive) semantics is compositional, all semantically opaque derivatives must be listed in the lexicon as wholes; indeed, there is no reason even to consider them to be derivatives from the standpoint of the synchronic system, at least in many cases. In order for these kinds of items to develop historically, they would have to have been listed fully before the semantic change took place, as I believe Halle 1973 was the first to point out. There appears to be good reason to believe, however, that (productive) inflection differs from derivation in this respect (cf. Dressler to appear). In addition to the considerations that Dressler adduces, we may add the little-discussed observation noted in Hetzron 1975:870n¹⁸ that speakers of English are not aware of whether or not they have heard a (regularly) inflected form of a sufficiently uncommon stem. (Compare the situation in derivation: we are certain that we have never heard *unequality* (or if we have, that it caused a certain amount of pain in the asterisk-generating region of the brain), despite the otherwise gloriously productive nature of *un-*.) If regularly inflected forms are in the mental lexicon, then it should be possible for us to look them up; but we can't.¹⁹

If derived forms are listed, then as far as a processing model is concerned, there is no reason for speakers to do anything other than access the mental lexicon (and then inflect the stem appropriately) under ordinary circumstances. Thus, roots and derivational affixes would not normally play a role in the production of a word, or be given as the output of a parse. Only if it is necessary to generate or interpret a new word (or if an existing word cannot be accessed for some reason) will the accessing of anything other than whole stems be required. This picture is supported by the fact that the occurrence of "mistaken word formation" (Cutler 1980:48-9) is quite rare; Cutler found only 119 "clear examples" in her search of all of the error data known to her (e.g., *self-indulgement/self-indulgence*, *concedence/concession*, *inconsideration/inconsiderateness*, *expection/expectation*, and *derival/derivation*). What presumably happened in such cases was that the speakers in question were for reasons that I will not speculate about unable to retrieve the listed word, were thus forced to fall back on their rather feeble productive generation capabilities, and attached the "wrong" affix. Just how "feeble" our productive generation abilities are under ordinary (i.e., "on-line") conditions is unclear. I have one piece of anecdotal evidence that suggests that it is indeed rather feeble. The following discussion (roughly—I unfortunately did not commit it to paper) took place during a meeting of Stephen Stich's Philosophy of Linguistics class at the 1982 Linguistic Institute, in the course of which the need arose for a word meaning "the property of being a chair" (this was a philosophy course, after all!):

- (12) Stich: ... chairness. Chairness? That's not quite right.
 Class: [General agreement that it's not.]
 Me: Chairity?
 Class: [General unhappiness about the suggestion.]
 Me: Chairosity?
 Class: [More unhappiness.]
 Another member of the class: Chairhood.
 Class: [General agreement that this is the right word.]
 Stich: Well, at least there's one native speaker around here!

All of the attempts at productive word formation, except possibly the last, were made by reasonably linguistically sophisticated native speakers of English who were not making conscious use of knowledge of English morphology—i.e., were not behaving as (amateur, in

the case of Stich, perhaps) linguists. We “knew” that we needed a noun, and searched our mental lexica for affixes that have as part of their SFs the fact that they create nouns; Stich hit on *-ness* (perhaps because it has the least restricted SF of the noun-forming affixes), and when it was noted that *chairness* is not only not a word, but not a possible word (*-ness* requires an adjective host—cf. (8b)), we had to look for another affix. I found *-ity*, but it was no better (except possibly for its (bad) pun value), for the same reason. One way to remedy the situation is make an adjective out of *chair*, and I did the best I could, but attached *-ous* to a stem that it does not subcategorize for in the process. Only later did one person (out of a class of 15 or so) hit upon the appropriate (and fairly uncommon, to be sure) affix. Unless this class consisted (mostly) of unrepresentatively poor word-formers, then the general human capacity for “on-line” derivational word formation must be sufficiently feeble that it presumably could not regularly take place during speech production.²⁰

It also appears that a number of aspects of the model outlined here are desirable with respect to an optimal mechanized speech understanding/generating system. For example, the listing of semantically opaque derivatives will obviate the need for a complex (and ad hoc) semantics in order to get the meaning right in such cases. It is not clear that the all of the psychologically-based aspects of the model should be carried over to a practical computational model. In particular, since it seems desirable that novel productive morphology be possible, affixes will have to be listed in the lexicon. Furthermore, since monomorphemic words, which for the most part form the ultimate base for word formation, will also have to be listed, it would therefore not be necessary to list any derived word that is semantically compositional and a morphologically regular combination of two or more items that are present in the lexicon. Whether or not one actually does so is presumably determined by purely pragmatic factors such as relative amount of processing time required, etc. Interestingly, however, derived forms are listed in most of the work on computational models of English morphology (e.g., Winograd 1983). The prohibition against listing regularly inflected forms **would** probably carry over, however. The sheer number of inflected forms—especially in languages with more than the minimal inflection that is characteristic of English—would make listing all of them prohibitively expensive (cf. Koskeniemi 1983). Note that lexicographic practice also supports this kind of approach.²¹ Another advantage of the present model is the inherent lack of directionality that is characteristic of SFs, since they can therefore be used straightforwardly to both generate and interpret morphologically complex words.

3. Comparing the Two Frameworks.

Let us now examine the two kinds of approaches considered here with respect to the phenomena summarized at the end of section 1. First of all, of course, the paradoxes will not exist, because the levels do not, if a SF approach is adopted. Similarly, we will not, strictly speaking, have to say anything about why “Level 1” comes first. But since it looks like, in many respects, there is a Level 1, and that it comes first, it is perhaps worthwhile to attempt to answer a slightly different question—namely, why it **looks like** there is a block of Level 1 rules, all of which (almost) always precede another block of rules, and furthermore are (almost) the only ones that share (usually) the characteristic properties listed in (1) above. Note that nothing in the model developed thus far has anything to say about the properties of “Level 1” affixes—or can in principle have anything to say, once it is denied that Level 1 exists. But, in fact, neither does a level ordering-based theory; these properties are simply inductive generalizations about phenomena that, as a matter of empirical fact, happen to cluster together in English. Level ordering in and of itself in no way predicts that these properties should be characteristic of Level 1, as opposed to Level 2, or for that matter, why some of them could not characterize Level 1, while others are applicable only to Level 2. Nor does it predict anything at all about what the properties of the different levels that might be found in other languages might be (note that the stress-changing criterion,

in particular, would simply be inapplicable, as it is in fact even in English in the case of prefixes). It is quite possible that no theory **should** make such predictions, since it appears that the existence of at least some of these properties (1a-c) is in part simply an accident of the history of English (cf., for example, Strauss 1982).

If the clustering is really accidental from a synchronic perspective, then one might expect it to show at least some signs of deteriorating. In fact, this is exactly what is happening. Thus, alongside forms such as *adorable*, we have *preferable* and *admirable*, and with the stress shift in the opposite direction, *microwávable*, as well things like *pianist*. Similarly, Level 2 *-ize* and *-ist* trigger velar softening (*cliticize*, *Chadicist*, *Slavicist*). And words such as *ostracize*, *pliable*, and *malleable* show that the hosts of Level 2 affixes need not be words. Deverbal *-al* is quite choosy about what it will attach to (cf. **despisal*, **surprisal* vs. *revisal*), and there are a number of cases in which the semantics of Level 2 derivation is not compositional (*recital*, *communist*, *weatherize*, *wilderness*, and various forms in *-able*-cf. note 13).²²

We still do not have an explanation for why it looks like "Level 1" precedes "Level 2," however. Within the SF approach, all that needs to be said is that "Level 2" affixes subcategorize for attaching to a wider range of things than "Level 1" affixes, typically anything that belongs to a given lexical class (cf. (8b)). This entails that they will be able to attach to items that contain either "Level 1" affixes or "Level 2" affixes (or both), as long as the lexical category restrictions are satisfied. Some affixes subcategorize for attaching only to a set of specific items (8c). And some affixes subcategorize for attaching to, among other things, elements that contain "Level 2" affixes (cf. (8d,e)), which is why they appear to violate level ordering. That is, there are, in effect, three different types of derivational affixes in English—not just two, as level ordered morphology would have it. And there is no need to assign these different types of affixes to different levels, since the order in which these affixes occur follows directly from the nature of their lexical representations. The ultimate explanation for the nature of the affixes themselves will have to be a historical one: the "Level 1" affixes came into the language as the result of borrowing. Given that uninflected words are listed in the lexicon (see above), and that it is words that are borrowed, not morphemes, this means that speakers of subsequent generations would have been presented with a set of words that all ended in the same way and had a common element of meaning. But if there were other words that also shared this element of meaning but ended differently, as would have been the case for English deverbal nominalizing suffixes (cf. *arrival*, *derivation*, *contrivance*), there would be no way for a learner to formulate a rule that predicts which one is the "default case." If it turns out that it is possible to formulate a partial rule (*-ation* attaches to many forms in *-ize*, but the others do not), this subregularity will turn up in the SF of the affix in question. Apart from this kind of situation, however, the most reasonable thing to do when faced with such facts is simply to memorize which words get which affixes, and this is apparently what speakers do. In the case of infrequent bases, however, it will be difficult for a speaker to find out exactly which form is "correct," and doublets will, on occasion, result (cf. *in-/un-alterable*).

We are still without an account of the phonological effects that "Level 1" affixes have. I would like to suggest that these effects are due to the presence in the SFs of the relevant affixes of a diacritic, to which the rules in question are sensitive. Thus, instead of saying that these rules apply only in Level 1 (and hence never in Level 2), we say simply that the rules apply if the diacritic is present, and they don't if it isn't. Note that this is not really adding anything new to the theory, since in a level ordering-based theory, there would have to be something in the lexical representation of affixes that encodes the level to which they belong—i.e., a diacritic (cf. Lieber 1980:35-6). Unlike in a level ordering-based theory, however, the diacritic and the order of affixation need not always correspond, which the level ordering paradoxes show us is exactly how it should be. If we increase the number of

possible diacritics, so that there can be one that refers to stress-shifting, while another refers to, say, velar softening, we can provide an account of the behavior of *-ize* and *-ist*, where there is no stress-shift,²³ but velar softening applies (as in the examples given above). Of course, allowing for an apparently arbitrarily large number of diacritics lessens substantially the restrictiveness of the theory,²⁴ but some way must be provided for handling cases of this nature, and a theory that requires level ordering simply cannot provide an account other than lexical listing (and note that the affixes in question attach quite productively, as the examples cited attest).

Note now that Cutler's speech errors show clearly that it is not the case that such errors apply only at (Mohan and) Mohanan's "lexical" level, given that faulty affixation is possible. Similarly, J.-M. Hombert found that Chinese tone sandhi can provide the input for a game that he taught speakers of various dialects, and a French game rule applies after elision (*l'école* → *qu'école*) (cf. Churma 1979, ch. 5 for discussion and references), which indicates that rules that have applied after the lexical level has been reached can determine the input to game rules, and a Fula game rule applies well before the lexical level is reached (cf. Churma 1986). But a weaker form of this claim does in fact appear to be true: the abstract "underlying representations" posited in, say, *SPE* and Halle and Mohanan 1985, never play a crucial role in phenomena such as games and slips. But if such representations exist in a psychologically real grammar of English, then we would expect that they would. Since no one has presented an explanation for why they do not (cf. Mohanan and Mohanan 1984:596n), the most reasonable conclusion to draw, it seems to me, is that these representations simply do not exist. The system outlined here will allow for representations that do play a role in the phenomena in question, and while it does not appear to prohibit the use of overly abstract (in view of the above considerations) representations, it does provide a way of accounting for the productive use of the morphological system in creating and interpreting new words without encouraging abstractness. In this system, there is evidence for the psychological reality of all representations posited; moreover, these representations show considerable promise with respect to playing a role in the construction of computational models of productive morphology.

FOOTNOTES

¹ This paper was written while I was a visiting scholar in the Department of Linguistics at Stanford; I would like to thank the members of the department for their hospitality, and the Center for the Study of Language and Information for providing me with invaluable word processing facilities and support staff, the latter primarily in the person of Emma Pease, whom I would like to thank especially voluminously. I have benefited greatly from discussions with W. Dressler and A. M. Zwicky concerning the issues pursued here; questions after the oral version of this paper by Zwicky and C. Fillmore have led to the inclusion of hopefully clarificatory footnotes. Many of the examples cited here are taken from the works cited in the list of references; I have made no attempt to cite exactly where an example or example type was first discussed. In addition to the "theoretical" literature, I have found Jespersen 1909-49 to be a valuable source both for examples and analysis.

² Halle and Mohanan 1985 have proposed an even more elaborate model. Level 1 for them is as in Kiparsky's system, and the last level (Level 4) also contains only regular inflection, but Level 2 contains only #-derivation and Level 3 only compounding. The distinction between Level 2 and Level 3 is made for purely phonological reasons, and they need to adopt a "loop" from the Level 3 morphology back to Level 2 in order to account for the fact that compounds can be the input to #-derivation. This model is thus identical to that of Kiparsky 1982 as far as the morphology goes.

³The acceptable compounds have been attested; unacceptability judgments are my own. The *Publications List* was put out by Academic Press, and the *Women Fencers* construction appeared twice (I do not recall which sports were actually involved) in headlines in a recent issue of the *Stanford Daily*. The *X(s) fan* construction is of course quite productive, but there is much idiolectal variation (which is apparently cause for concern among at least some prescriptively-oriented journalists, since the Los Angeles *Times* consistently uses the plural form in such cases); cf. Churma 1983b for discussion. The *Raider Rooters* were advertised some years ago on a San Francisco Bay area bus (although the San Jose *Mercury News* has reported (2/18/87) the recent formation of the "Oakland (not Los Angeles) Raiders Rooters National Fan Club"). The *Lady Bucks* (which is not the contradiction in terms that it appears to be, since *Buck* is an abbreviation for *Buckeye*, a type of nut, so at least no contradictory properties are being attributed here) are the Ohio State University women's basketball team. (If I recall correctly, it is only basketball players that can be *Lady Bucks*; female volleyball players are *Volleybucks*, and participants in other sports are, it would appear, only *Women Xs*.) The *Illinois legislature* example is from Jerry Sadock (p.c.).

⁴When dealing with a pair of suffixes, it is easy to tell which one attaches first (as long as it is maintained that there is no infixation in English). In determining what is going on in the other cases, I employ two criteria, which provide the same characterization in all cases: (i) morphological category restrictions on affixation (e.g., *un-* may attach only to adjectives, not to nouns); and (ii) the semantics (*ungrammaticality* is "the property of being ungrammatical").

⁵There will in fact have to be at least one rule—one which can be stated quite simply in informal terms: "Combine!". In Lieber's framework, the corresponding rule is an unformulated one that is said to create unlabeled "binary branching tree structures" (p. 47), into which lexical items are inserted.

⁶The most significant respect in which these SFs would have to be elaborated is the inclusion of the semantics associated with them. Productive word formation in such a framework would thus have a strictly compositional semantics (unlike, allegedly, in the frameworks of, e.g., Williams 1981 and Selkirk 1982— although these systems do not contain an explicit account of how a non-compositional semantics might work). Semantically opaque derivatives would be listed in the lexicon as wholes.

⁷Alternatively, one could treat [+PL] as being a feature of the suffix, which would then percolate to the top of the tree from the suffix, rather than the stem (this is, in effect, Lieber's approach). I adopt (8a) on the basis that it will provide a way of handling subregularities involving stem-internal changes (which may well not exist in the case of English pluralization), such as those found in the inflection of English verbs that end in *ing* (note that these patterns are extended to new items in language acquisition and historical change—cf. *brought* > *brung*, *dived* > *dove*), without having to posit some kind of special phonologically null suffix (or prefix) that triggers the change. Instead, we would simply have a morphologically conditioned phonological rule that performs the required change. It will also allow agreement morphology to be done in the morphology (contra, e.g., Anderson 1982): English verb stems that have the features [−PL, 3 PERS, +PRES] can be appropriately inflected in the lexicon, and then inserted into frames for which the morphosyntax requires such feature specifications. Again, the most convincing case for having the features as part of the stem would be when inflection is not done via affixation, and English does not have any productive cases of this type; Fula subject-verb agreement, which is signaled quite generally by a change in the stem-initial consonant, would be a relevant case in point (cf. Churma 1986). Finally, this kind of approach will allow for an explanation of why stems without obligatory inflectional affixes (e.g., Latin person/number suffixes for verbs) are unacceptable; as Lieber 1980:95 notes, in her system, in which the affixes bear such features, affixation must be

optional in order to allow different person/number combinations to be generated, whereas under an approach that makes these features part of the representation of verb stems, inflectional affixation can be considered to be obligatory, applying whenever the stem has the features for which the affix in question subcategorizes.

⁸The strangeness of forms such as ?*grammaticalness* would be attributed to some form of the blocking constraint (Aronoff 1976), whereby the formation of a word is inhibited whenever there is an existing word with the same meaning.

⁹In this respect, I depart from Lieber's approach, in which affixes may be members of the same kinds of lexical classes as stems (in this case, *-ness* would "be a" noun), with the category of the combination being derived via percolation of the classes of the affixes. Nothing crucial hinges on this difference between my approach and that of Lieber, as far as I can see. I adopt the revised approach because of a strong feeling that, e.g., *-ness* is **not** a noun, and for more technical reasons that I cannot go into here (cf. Churma 1987 for discussion).

¹⁰I have made a simplification here (and an analogous one elsewhere) that is quite likely spurious: given the complete list of items to which *-ance* may attach, it is not necessary to state in the SF for this suffix that the items on this list are all verbs, although there would certainly appear to be a generalization missing from this account. Given the brute-force nature of SF for *-ance*, there is some question in my mind whether there should be an SF in this case at all, since the words in question could simply be listed as units in the lexicon. I include it here because its semantics is, by and large, compositional, and because it can apparently be attached to an inappropriate host in speech errors (see below).

¹¹The concern for specific affixes in the formulation of SFs/rules is shared by the quite different theories advocated in Aronoff 1976 and van Marle 1985, although of course there are many respects in which the present approach differs from these. A somewhat uneven critique of Aronoff's approach can be found in van Marle, especially chapter 5.

¹²Note that these facts suggest that [+LAT] does not percolate like "real" morphological features (contra, e.g., Lieber 1980, Selkirk 1982). In fact, there would appear to be no need for this purely diacritic feature at all, since we could refer directly in the SF for *-ity* to the monomorphemic roots that may take it.

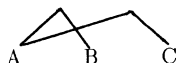
¹³Things are pretty messy with respect to the relationship between *in-* and *-able*, partly because it is not clear how many items that are pronounced [əb] there are (cf. Aronoff 1976, Selkirk 1982). The *-able* of *fashionable*, *palatable*, etc., seems clearly to be different from the others, in terms of both the semantics and the category membership of the base, and allows only *un-* as a prefix (**infashionable*). What seems to characterize the *-able(s)* that accept *in-* is that they (i) attach to a non-word, (ii) can have a slightly different semantics than "standard" *-able*, and/or (iii) have a radical phonological effect on the base, via "truncation" (if it exists)—cf. Aronoff 1976—or, say, whatever it is that relates *perceive* and *perceptible*. Thus, "standard" *-able* tends to allow only *un-*, as the starred examples in the text indicate. However, both *inalterable* and *unalterable* appear to be acceptable, and most speakers whom I have consulted have only a slight preference one way or the other (*Webster's New Collegiate Dictionary* gives the latter as a synonym of the former, but has a separate listing only for the former). I will not elaborate further on this phenomenon, due to lack of anything to say about it.

¹⁴There is no explanation within the present framework for the existence of this tendency, but at least it is capable of providing an account of all of the attested kinds of cases, unlike in the case of theories that prohibit universally this kind of interaction of derivation and inflection (cf., for example, Allen 1978, Anderson 1982). For a functionally based approach to an explanation of these facts that is not incompatible with the present framework, see Dressler to appear.

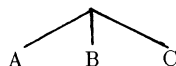
¹⁵There remains a somewhat technical problem with this kind of approach to inflection:

it requires only that each affix and the stem be sisters; it does not require that all of the affixes be sisters of each other. It thus allows the generation of overlapping tree structures such as (ia), in addition to “normal” structures like (ib):

(i) a.



b.



The sisterhood requirements that the relevant SFs would impose on the suffixes B and C are met in both structures, as are the linear order requirements of the filter. Since (ia) does not represent an acceptable word, I take it, there must be some way of ruling out such structures. Of course, we could attempt to add this kind of structure to the set of things that the filter disallows, but it is not easy to see how to do this, especially if we want to maintain the “positive” characteristic of the filter. The best approach, then, appears to be positing that there is a separate principle, analogous (or identical) to the line-crossing prohibition of autosegmental phonology/morphology, that disallows structures like (ia).

¹⁶Note that, unlike in phonology, it will not in general be possible to squirm out of a (linear) ordering paradox of this type by appealing to cyclic application, since each affixation “process” (or, perhaps, process schema, in the case of the approach taken to inflection taken by Selkirk 1982) creates its own cyclic domain.

¹⁷It follows from the nature of SFs like that for *-ance* (8c) that such affixes will be totally nonproductive. Whether this is in fact the correct SF for this suffix is an empirical matter, and if it should turn out that there are subregularities concerning its preferences for stem types, then it, like *-ity* and *-ation*, would be “semi-productive.” Even totally nonproductive affixes can on occasion be found in the formation of new words, however. Such cases are argued to be due to surface analogy in Churma 1983a, partly on the basis that a single form can provide the basis for the formation of new words, as in the case of the recent adoption of the *-burger* morpheme. For discussion of the role of analogy in word formation from a somewhat different perspective, see van Marle 1985.

¹⁸Hetzron cites a personal communication from Arnold Zwicky as the source of this observation; Zwicky (personal communication) informs me that a personal communication from David Stampe is his source. It is not hard to see why this observation has not been widely discussed.

¹⁹Exactly what is meant by the term “regular” is not entirely clear to me. Notice that the verb *stride* lacks a past participle (despite the fact that everyone knows what it would be if it existed), which suggests that the entire paradigm is listed in cases of this nature (cf. also Halle 1973 for a similar Russian example). Some way of distinguishing regular and “irregular” inflection will eventually have to be given, since the two types also appear to behave quite differently with respect to historical change: in the case of irregular inflection, we find “bidirectional leveling” of inflectional paradigms (cf. Tiersma 1978, 1980; Churma 1979, 1983a; Newton 1986), which strongly suggests that the members of such paradigms have been lexically listed. In order to avoid circularity in the explanation for the existence of this kind of leveling, there must be some independent way of determining when an inflected form must be lexically listed.

²⁰Notice that our recognition capabilities, at least in this kind of situation, in which the word is presented, in effect, pre-parsed, are considerably better, as indicated by the near-instantaneous recognition, apparently by everyone in the class, of the appropriateness of *chairhood*.

²¹The fact that only inflection is done productively could be a language-specific property; in polysynthetic languages, in particular, much of derivation could conceivably be done on-

line, both by native speakers and computers. Similarly, in languages with extremely fusional inflectional systems, it could well be the case that no inflectional morphology is done on-line.

²²It could be argued that the suffix in these cases is a Level 1 homophone (cf. Aronoff 1976, Selkirk 1982, although they do not frame their discussions in terms of level ordering). Note, however, that the semantics is most opaque in the case of *palatable* and the like, and that if this latter *-able* is in fact attached at Level 1, there will be no explanation for the fact that the result does not allow prefixation of *in-* (cf. note 13).

²³Forms such as *pianist* will simply be listed in the lexicon with stress present. This case appears to be truly exceptional in view of forms like *guitarist* and *trombonist*.

²⁴Making such use of diacritics does not necessarily result in an "anything goes"-type situation; cf. Churma 1986 for discussion.

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