WORD-STRUCTURE

by

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SUBMITTED IN PARTIAL FULFILLMENT

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DEGREE OF DOCTOR OF
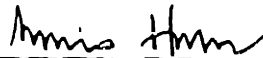
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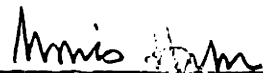August, 1974

Signature of Author_____

Department of Foreign Literatures
and Linguistics
August 12, 1974

Certified by_____

Thesis Supervisor

Accepted by _____

Chairman, Departmental Committee
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for M. and G.

And he said, Draw not nigh hither: put off thy shoes from off thy
feet, for the place whereon thou standest is holy ground.

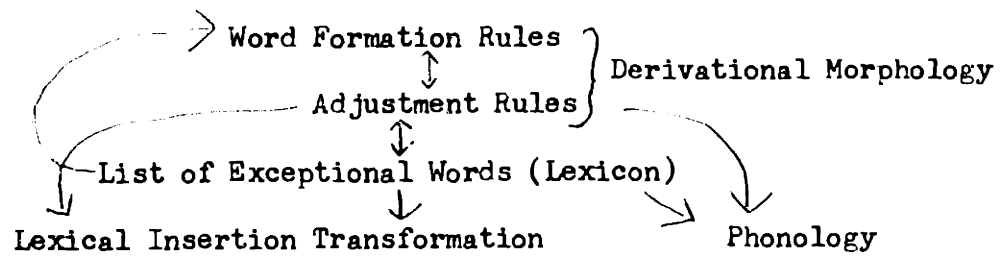Exodus 3:5

ABSTRACT

Word-Structure
Mark H. Aronoff
"Submitted to the Department of Foreign
Literatures and Linguistics on August 12, 1974
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy."

This work deals, in the main, with that aspect of word-structure
referred to commonly as derivational morphology, though other related
areas are touched upon in the course of discussion. Much, though by
no means all of the material discussed is drawn from English. The
framework presented pretends, however, to universal scope.

We divide the question of word-structure into two subparts, that of
word-formation, the coining of new words, and that of word-analysis,
the provision of structure to already existing words. We say that
there is a lexicon or dictionary, the main provision for entry into
which is that an entry must be a word, and be arbitrary (unpredictable)
in at least one aspect of its meaning or form. New words are coined
by the application of general rules called Word Formation Rules.
Such a rule forms a new word from an already existing one, one in the
lexicon, by performing an operation of a specific sort on that
existing word. Existing words are analysed by applying to them the
same Word Formation Rules, but as redundancy rules, i.e. as rules for
determining how a word might have been formed. Not all new words are
added to the lexicon. Whether a given word is added depends on
whether it is arbitrary, and this is correlated with the productivity
of a rule by which it is formed.

Because of the way in which Word Formation Rules are formulated
(one affix, one rule), it is necessary to posit a class of readjustment
rules, which operate on the output of the Word Formation Rules, and
whose output itself is the input to the phonology, and to the Lexical
Insertion Transformation.

The rules of derivational morphology are completely separated
from the other sets of rules of a grammar. They operate on words to
produce words or to provide structure to existing words. The figure
below gives a simplified picture of the interaction of the derivational
morphology with other parts of the grammar.

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                    ─ ⟩ Word Formation Rules ⎤
                              ↕           │ Derivational Morphology
                ─── Adjustment Rules ⎦
                              ↕.
        ─List of Exceptional Words (Lexicon)
                              ↓                           ↘
  Lexical Insertion Transformation              Phonology
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Thesis Supervisor: Morris Halle
Title: Professor of Linguistics

Foreword

I have been able to trace the roots of my interest in morphology
as far back as high school. There, for four years I studied Hebrew
grammar, which I consistently failed. Since my first introduction to
formal linguistics I have been trying to atone for this failure and
the present work marks yet another point along that frightful path.
It is quite clear to me, and will become so to the reader, that I have
not progressed far in that endeavor. Yet we know that one should not
even attempt to study the true mysteries unless he has reached middle
age, is happily married, and has a full stomach, and we know further
that of the three who saw the light only Akiba was capable of return-
ing to darkness intact. Therefore, I must not despair. Comfort
can perhaps also be gained from the words of that great sage, the
nameless sage who was the teacher of ben-Moshe, who said that of all
the morphological mysteries those of the Semites were the most dread-
ful. "Once we have understood the others, we may begin to look at
these." Though I cannot claim to have achieved understanding, even
of the others, I do have some conviction that I am proceeding in a
proper direction.

We find comfort in precedent. It is convenient, when introducing
a notion which is not uncontroversial, to defend the introduction with
an allusion to its commonness in older thought. This may reflect a
deep ecclesiastian conviction. It is more usually viewed as a sign
of modesty. Modesty, though, is a convenient cover in many instances
for a less virtuous attitude. When something is not ours, we can

easily disclaim ultimate responsibility for it. With this in mind, let me note that the basic view of the workings of morphology presented in this work, that words are formed from words, is not new. However, to my knowledge, there have been no previous attempts to integrate it into the general framework which I am presupposing, that of generative transformational grammar. I believe that this framework is essentially correct. The truth or falsity of my views must be proved within it, and not within some more general theory of epistemology, and all responsibility for the assertion of these views therefore rests with me. Nevertheless, I must acknowledge my predecessors and my debts to them.

I have benefitted greatly from the work of Hans Marchand, especially his The Categories and Types of Present-Day English Word-Formation (1969). His view of the workings of word-structure are a principal source of mine, though the framework in which he is working is radically different. The book has also been an invaluable source of data. There is no more complete work on the subject. I have also been influenced by two works on morphology which are closer in general perspective to mine: Morris Halle's Prolegomena to a Theory of Word-Formation (1973). and Dorothy Siegel's regrettably still unpublished Some Lexical Transderivational Constraints in English.(1971).

With regard to less central matters the sketch of English phonology presented in The Sound Pattern of English (1968) has been as indispensable as it must be to any work remotely connected with that domain. On the most general plane I must cite two works, Noam Chomsky's Aspects of the Theory of Syntax (1965), and Ludwig Wittgenstein's Philosophical Investigations (1953), which I can only hope not to have

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abused.

Most important and most ephemeral is the knowledge I have gained from personal contact. In my years at MIT I have talked about linguistics with many people. Among these I am especially indebted to Richard Oehrle, Alan Prince, Dorothy Siegel, and Edwin Williams, and to the members of my thesis committee, Professors Ken Hale, Morris Halle, and Paul Kiparsky. Ms. Siegel has been working on very similar questions to those that have interested me. Such a situation often leads to conflict, yet we have been able to cooperate most fruitfully. I will note hei only that many of my conclusions with regard to the interaction of morphological operations, boundaries, and the rules of the phonology are parallel and similar to hers, and will direct the reader to her discussion of these matters which is contained in her dissertation (Siegel, 1974). Professor Halle has taught me much of what I know about language and linguistics. I hope I will prove worthy of his attentions.

Throughout most of my stay at MIT, I received the financial support of the Canada Council and the Department of Education of the Province of Quebec. I must thank them for their generosity, and only hope that the future will allow me to pay the debt that I owe to my native land.

Finally, I must thank Ms. Dorothy H. Brandl for the time and care she took in typing this work.

ABBREVIATIONS

The following abbreviations will be used for works frequently cited in the text.

M   Marchand, Hans, *English Word-Formation*. (Munchen, 1969).

SPE   Chomsky, Noam, and Morris Halle, *The Sound Pattern of English*. (New York: Harper and Row, 1968).

W   Walker, J., *Walker's Rhyming Dictionary*, revised and enlarged by Lawrence J. Dawson. (New York: E. P. Dutton, 1936).

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CHAPTER 1

GROUND AND TELEOLOGY

We will be concerned in this work with the internal structure of words, a subject which, in the linguistic literature, is called _morphology_.

The notion _word_ has long concerned students of language. It's definition is a long-standing problem in linguistics, and entire volumes have been devoted to the subject (e.g., Worth, 1972). A reasonably detailed procedure for isolating phonological words (units which may be considered as words for phonological purposes) is provided in _SPE_ (pp. 366-70). Further refinements of this approach are discussed in Selkirk (1972). Syntactically, Postal (1969) puts forth a persuasive argument that the _word_, as a syntactic unit, corresponds to the _Anaphoric Island_, which is a syntactic string, the internal elements of which cannot participate in anaphora. Though semantic definition of the notion is a traditional goal, it has not, to my knowledge, been achieved.

To say that morphology is word structure is not to say that all of the structure of the word is encompassed in the domain of morphology. There is a branch of phonology, termed _phonotactics_, or _morpheme structure_, which concerns itself with the determination of possible sequences of sounds in a given language, "possible phonetic words." This is not morphology. Morphology treats words as _signs_, not just as forms, but as meaningful forms. It is therefore concerned with words which are not simple signs, but made up of more elementary ones. This concern encompasses two distinct

but related matters: firstly, the analysis of existing composite words, and secondly, the formation of new composite words. A unified theory of morphology should be capable of dealing with both of these areas in a unified and coherent manner, though it may not be possible or even desirable, as we will argue below, to treat them in exactly the same manner.

On the subject of unified theories, it should be stressed that morphology, as defined, is a small sub-system of the entire system of (a) language. A theory of morphology must be integrated or at least integrable into a fairly specific general theory of language. Morphology, as a sub-system, and a sub-theory, may have its own peculiarities; a system can be unified without being completely uniform. However, it does not exist in vacuo. The present work is conceived in the general framework of Transformational Grammar as outlined in such works as Chomsky (1965) and SPE. More particularly, it presupposes the Lexicalist Hypothesis of Chomsky (1970) and at least the spirit, if not the letter of Kiparsky's views with regard to phonological abstractness, discussed in Kiparsky (1973).

1.1. Derivation and Inflection

There are traditionally two types of morphological phenomena, derivational and inflectional. The distinction is delicate, and sometimes elusive, but nonetheless important. Inflection is generally viewed as encompassing the "purely grammatical" markers, those for tense, aspect, person, number, gender, case, etc. Within a Lexicalist theory of syntax, (cf. Chomsky, 1970), inflectional morphemes would be dominated by the node $\bar{X}$, and, perhaps, higher nodes (cf. Siegel,1974) while derivational morphemes would be dominated by the node X. Derivational morphology is thus restricted to the domain of Lexical Category.

It is generally true, and in accord with the Lexicalist formalism, that derivational markers will be encompassed within inflectional markers. In the English word compart+ment+al#ize#d, for example, the last morpheme, #d, is inflectional, and all those internal to it are derivational. The two sets may not be interspersed. Thus the word compart+ment+al#iz+at+ion#s is possible, though the word *compart+ment+al#ize#d+ation#s is not.

One peculiarity of inflection is that it is paradigmatic. Thus, every English non-modal verb exhibits a paradigm consisting of the following forms:

$$V \qquad V\#s \qquad V\#d_1 \qquad V\#d_2 \qquad V\#ing$$

For example:

sigh	sighs	sighed	(has) sighed	sighing
go	goes	went	(has) gone	going

The verb go exhibits suppletion, the filling of one of the slots of the paradigm by a phonologically unrelated form. Since derivational morphology is not paradigmatic, it does not show any suppletion, i. e. it does not concern itself with phonologically dissimilar, but semantically related forms.

Sometimes a paradigm is defective, lacking a form. The missing form is almost always the uninflected one. So, in English, we have scissors, pants, trousers, but not *scissor, *pant, *trouser, except, of course, in derived forms, where the constraint on the mixing of morphologies still holds, as the following examples demonstrate:

scissorlike	*scissorslike
trouserleg	*trousersleg[1]

A fuller description of some of the properties of inflectional morphology can be found in Bloomfield (1933). An independent character-ization of the properties of derivational morphology is more difficult. Nida (1949) suggests the following: if, in a syntactic class (defined by

substitution in his system, and in corresponding ways in other theories),

we find items which are monomorphemic, then the polymorphemic items in that

class are derived by the system of derivational morphology. The most

immediate problem for such a definition is the existence of suppletive

forms, such as went above, which, by Nida's criterion, would force us to

include the past tense suffix in derivational rather than inflectional

morphology. This is where the paradigm enters. We find that the past

tense is a paradigmatic category, and therefore must be inflectional. We

might also invoke more abstract syntactic evidence to show that though

went is monomorphemic, on the surface, there is evidence for an abstract

past tense morpheme. This is more difficult, though perhaps possible. In

any case, Nida's simple criterion, as he notes, must be amended to exclude

clearly suppletive forms which are members of paradigms.

More difficult are cases of syntactically or semantically arbitrary

forms. Consider the noun police in the following sentence:

(1) The police have arrested six people already.

The verb shows us that the noun is syntactically plural. Unlike words like

sheep, which are sometimes plural and sometimes singular, police, in this

sense, never appears in a singular context. We cannot, therefore, argue

in any straightforward way for the existence of a zero plural marker, as in

sheep. Nor can a paradigm help here, since there isn't any. Recourse might

be had to syntactic argument, but here there is the danger that a system

which permits abstract morphemes in this instance, where we believe we are

not dealing with derivation, might also permit us to posit abstract

morphemes in other cases, where we feel we are dealing with derivation.

Consider the set of agentive occupational nouns, exemplified below:

	(a)	(b)	(c)
(2)	baker	cook	chef
	packer	pilot	chauffeur
	painter	coach	smith
	hunter		mechanic
	tanner		surgeon

The items in column (a) exhibit a clear suffix: -er; those in (b) might
be derived by zero suffixation, from the corresponding verb. The items
in (c), however, correspond to police, in having no corresponding verb from
which they may be derived. This is clearly the sort of case Nida had in
mind. We wish to use the fact of the existence of (c) as evidence that the
other items are produced by the system of derivational morphology, and not
by inflection. However, if we can argue for an abstract morpheme in the
case of police, why not here. Clearly, we must constrain our system of
argumentation (syntactic theory) in such a way as to exclude the possibility
of positing an abstract morpheme here, but not in police. In any case, it
shows that Nida's criterion, even when extended to take suppletion into
account, is not sufficient to capture our intuitive notion of what
derivational category is, unless we have a better idea of what a possible
syntactic derivation is. I do think that once we have clarified that issue,
Nida's criterion, in its simple form, will prove to be correct.

1.2. Other Types of Morphology.

Derivation and inflection do not exhaust the domain of morphology.
There are "grammatical" morphological phenomena which cannot be subsumed
under inflection. The best known of these is that of incorporation or
cliticization. In Classical Hebrew, for example, definite pronominal
objects, under specific conditions (basically, when they are anaphoric,
rather than deictic) are incorporated into the verb, forming, phonologically,
a single unit with it. There is no question here of inflection, since

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this specific form of the verb only occurs when we would otherwise expect a definite pronoun object. A similar situation holds in English (cf. Selkirk, 1972).

A slightly more complicated example along the same lines comes from Syriac. Here, in addition to pronoun object cliticization, we have the copying of a pronoun for any definite object, other than anaphoric pronouns. The copied pronoun is cliticized to the verb, giving the same verb form as that containing the pronoun object. Clearly, the copying and the cliticization are both syntactic facts, and they are not paradigmatic.

Sometimes other material than pronouns can be incorporated into the verb. In Navaho, a specific adverb may sometimes occur inside the verb, and sometimes elsewhere in the sentence, but never in both places in the same sentence. This fact can be most easily captured by a syntactic movement rule.

There is no traditional term for this third type of morphology. It is clearly "syntactic", and can be grouped together with inflection, as opposed to derivation, on that ground. There is often a clear historical connection between pronoun copying and cliticization and verbal agreement, and it may very well be that all agreement arises by a falling away from and generalization of pronoun cliticization. This would of course strengthen the contention that this third type of morphological phenomenon and inflection are really of the same nature, and opposed to derivation.

We will accept this opposition in the greater part of the body of this work, and restrict the scope of further discussion to the domain of derivational morphology. This restriction will be relaxed only in regard to the interaction of phonology and morphology, where morphology encompasses both inflectional and derivational markers.

1.3. A Brief Survey of the Recent History of the Study of Morphology.

Morphology is not something new, or like syntax, something much talked about for many years, but little studied or understood. The early Indo-Europeanists, Bopp for instance, were interested almost solely in morphology, and morphology has remained one of the mainstays of the philological tradition (cf. the extensive bibliography in M). American descriptivists, though their tools were better adapted to phonological and morphophonemic purposes, did do much substantive work in the area of morphology as we have defined it.

In the specific area of English morphology, I have already cited M, and my debt to that work. Jespersen also devoted a volume of his Modern English Grammer to the subject. Of the more recent work, I will note Zimmer's work on affixal negation (1964) which is notable for its concern with semantics and the very general and difficult problem of productivity.

Within the Generative framework, morphology was, for a long time, quite successfully ignored. There was a good ideological reason for this: post-Syntactic Structures linguistics, in its zeal, saw phonology and syntax everywhere, with the result that morphology was lost somewhere in between. For proponents of early Generative Grammar, grammar consisted of syntax and phonology. Phonology, at last freed from its phonemic blinkers, encompassed all of morphophonemics and phonemics in a grand system of ordered rules. Syntax took care of everything else: "all of the grammatical sequences of morphemes of a language" (Chomsky, 1957, p. 32). Within such a framework, morphology is not a separate study. In fact, though some of the earliest studies in transformational syntax were specifically restricted to the domain of the word (e.g. Lees, 1960), this domain was not considered to differ in any real way from that of the sentence. Even very recently, the school of Generative Semantics has insisted that the

word is fundamentally no different from any other syntactic unit, thus espousing a position which, in its essence, as that of early Generative Grammar, denies the independence of morphology.

Recently, a substantial interest has arisen in the peculiarities of inflection as a separable syntactic phonomenon. The first study in this area was that of Bierwisch (1967). It has been followed by others, of which I will note Wurzel (1970) and Kiefer (1970, 1973). I will not discuss these works here, as their domain stands outside that established for the present work.

The Return of Morphology.

Morphology found its way back into generative linguistics through several rear doors, almost simultaneously. The first hints that there might be something between syntax and phonology are found in SPE. There the question is first raised of whether the output of the syntactic component is in fact the input to the phonological component. It is noted that there are "certain discrepancies", and that ". . .the grammar must contain certain rules converting the surface structures generated by the syntactic component into a form appropriate for use by the phonological component." The rules in question divide surface structure into phonological phrases. They are called readjustment rules, and are supposed generally to "involve elimination of structure." An illuminating discussion of such rules is contained in Selkirk (1972). But these are not the only rules called readjustment rules. There are in addition rules which "eliminate grammatical formatives in favor of phonological matrices", converting, for example,$[[sing]_V past]_V$ into sung, and $[[mend]_V past]_V$ into mended. The term readjustment rule is obviously being used broadly, for these last rules are clearly rules of inflectional mor-

morphology. Yet a third type of readjustment rule is in no way connected with elimination of structure. This sort applies "...to specific derivable formatives; for example the rule (110):

(110) $t \rightarrow d/$ _ mi___+ive   "(SPE p. 223)
          = ver___+ion

Rule (110) is a very different sort of morphological rule. It is a rule of allomorphy, which spells out the form of particular morphemes in specific morphological environments.

We see, then in SPE, the beginnings of a recognition of the independence of certain classes of phenomena from syntax and phonology. The term readjustment rule is not a particularly well-defined one, but among the rules so termed, we do find a significant number which are plainly morphological.

SPE inadvertently created in its wake a second entrance for morphology. The purely formal spirit of Chomsky's and Halle's approach to phonology in general, and of the sketch of English phonology presented in SPE, in particular, prompted a reaction. It was felt by many scholars, most prominently Kiparsky, that by disregarding concrete evaluation measures, Chomsky and Halle were often led to propose phonological systems which were too abstract, and abused the classificatory function of the phonetic features. These criticisms can be seen, historically, as a reaction to the excesses of revolutionary fervor. Remember that Chomsky and Halle were fighting against a theory which termed phonological only the most apparent of alterations, and put all others into one morphophonemic bag of lists, without regard for the differences in regularity among them. The revolutionary step of these pioneers was to pull down the phonemic barrier, and declare all alterations to be the province of phonology. But, said their critics, surely not all connections are

phonologically regular? Most of those which were earlier included under the morphophonemic label can indeed be treated as phonologically governed rules, but there is some limit. There are alterations which are just not determined by purely phonological features.

A further step, one which the critics have by and large not taken, is to ask whether some of these alternations, which are not phonologically determined, are in fact not part of the phonology at all. I will argue below that a class of rules which a more tightly constrained theory rejects as not optimal phonological rules, can be fruitfully included in a theory of morphology.

Thus, because of a desire to place restrictions on the power of phonological theory, we find that certain phenomena now lie outside the domain of the theory. Many of these phenomena can be seen as morphological. The same sort of pattern is found as we found in SPE.

A similar retreat took place at about the same time in syntax. In an attempt to restrict the power of grammatical theory, certain phenomena were removed from the domain of the syntax. In contrast with phonology, however, where the realization that the system as it stood could not be sufficiently constrained came gradually and inexorably, with very little objection on anyone's part to at least the spirit of the trend, and curiously few suggestions as to what should be done with the discarded material, or even what it was, morphology sprung out of syntax's thigh full-blown, and caused a great to-do when it did so. The birth of morphology, or at least the declaration of its domain, is simultaneous with, and contained in, Chomsky's "Remarks on nominalization" (1970). This paper presents a new theory of syntax, in which all of derivational morphology is isolated and removed from the syntax; it is instead dealt with in an expanded lexicon, by a separate component of the grammar.

This distinction legitimizes the field of morphology as an independent entity.

"Remarks on nominalization" was long and bitterly opposed, mainly, I believe, on esthetic grounds. Where previous and rival theories view language as one vast domain, encompassed by pervasive constraints (cf. Postal, 1972), Chomsky prefers to see language as divided into smaller well-distinguised units, each governed by its own, perhaps idiosyncratic, rules. As the reader will discover, I am more inclined toward the latter perspective, even within the narrow field of morphology.

Chomsky did not propose a theory of morphology, he merely suggested that there should be one, and that its properties, if he is correct in dividing morphology from syntax so sharply, should be very different from those of an adequate theory of syntax. I will attempt to elaborate such a theory. The theory which I will present bears, indeed, little resemblance to any prevalent theory of syntax. It will also encompass many phonological phenomena which cannot be easily incorporated into a reasonably narrow theory of phonology, and provide what I think is a unified account of morphological phenomena within a generative grammar. This unity is important. Critics of the new esthetic accuse its proponents of excessive rug-sweeping, clearing away so much data in the name of restricting the power of a grammar, that the describable residue becomes miniscule. However, if we can show that what has been swept aside can be gathered up again, then we are vindicated in our vision.

FOOTNOTE

[1]The behavior of pants is exceptional:

pantspocket   *pantpocket

pantsleg   *pantleg

CHAPTER 2

BASICS

2.1. Trouble with Morphemes.

The units into which words are analyzed, out of which they are com-
posed, are termed morphemes. We will be concerned in this section with
some problems concerning the defining characteristics of the morpheme,
concentrating on a central premise of the approach which has been most
pervasive in American linguistics. This premise is the definition of
the morpheme as "the smallest individually meaningful element in the
utterances of a language," (Hockett, 1958, p. 123). Accepting this
premise entails that every polymorphemic word be a compositional entity.
It is compositional in two senses, both semantically and structurally, the
semantics being a function of the morphemes and the structure, just as a
sentence is semantically compositional. Recent work has revived the
truism that every word has its own individual idiosyncratic traits, some
of which can be very erratic and elusive. (We are speaking here of
derivational word, inflection does not suffer from this erraticness). If
it is true of words that they are minimally meaningful, then what about
the morpheme? Does it have no status at all, or can we define it other
than semantically? The point of this chapter is to show that the latter
question can be answered in the affirmative. Specifically, we will
isolate a class of morphemes, show that there is no way in which the
members of this class can be said to have any meaning at all, and then
demonstrate that there are phonological criteria which allow us to

isolate occurrences of these meaningless morphemes. The importance of this demonstration is two-fold. First, it shows us that any theory of "minimally meaningful element" is misguided. Secondly, it shows that despite this fact, it is still possible to construct a theory in which the morpheme plays a central role.

2.1.1. Minimal Signs.

In order to understand what is at stake here, we must first have a better understanding of what is meant by "minimal meaningful element." This entails a short review of Saussure's concept of the minimal sign. The sign is one of the most basic concepts of linguistics, and the literature on the subject is vast, nor is the definition of the sign a closed matter. I will adopt in this discussion what I think is an orthodox view where orthodox does not, of course, mean totally uncontroversial.

The minimal meaningful unit of a language is the basic, minimal, Saussurean sign (cf. Saussure, 1949). The sign is an arbitrary union of the semantic and the phonetic. So the sign dog has a meaning and a sound; one cannot exist without the other; they are arbitrarily united. Arbitrarily, because there is nothing in the sound which dictates its meaning, and vice versa, except social convention. The sound may change, and the meaning remain, or the meaning change, and the sound remain.

Out of the minimal signs we can construct composite signs. These signs are not arbitrary. Their meanings may be predicted from their structure and the minimal signs out of which they are constructed. Sentences are composite signs.

It is sometimes argued that there are different degrees of arbitrariness. A sign like dog is completely arbitrary. However there are

other signs for which there is felt to be some intrinsic connections between their sounds and their meanings. Onomatopoetic words, and those which involve phonetic symbolism (cf. M 398 ff.), like _slurp_ and _quack_, are said to be partially _motivated_ (non-arbitrary) because of this intrinsic connection. The class of partially motivated signs also includes composite items whose meanings can be partially, but not completely, derived from the meanings of their parts. Thus a sign which formally consists of the signs _a+b_, but whose meaning must be represented as _A+B+C_, that is, the meanings of _a_ and _b_ plus something else specific in addition, is sometimes said to be partially motivated. I will hold with Saussure, and contra Bally (1940) and M, that only fully motivated signs are to count as non-minimal, that partial motivation is not significant. Thus, any sign which is the least arbitrary is considered to be part of the basic inventory of signs.[1] Most of what follows is devoted to deciding what sorts of elements form this basic inventory.

2.1.2. Words.

That there are minimal signs which are polymorphemic was first stressed as an important fact, at least within the framework of Generative Grammar, by Chomsky (1970). Chomsky notes that much of derivational morphology is semantically irregular, and should not be handled in the syntax. Out of this remark there developed two hypotheses. The Strong Lexicalist Hypothesis of Jackendoff (1973) excludes all morphological phenomena from the syntax. This means that the syntax cannot relate _some_ and _any_, or _ever_ and _never_, and that inflection, if it is referred to in the syntax, must be handled by some sort of filter. The Lexicalist Hypothesis which is more widely accepted than this one, but which to my knowledge has never been explicitly formulated in print, is that deri-

vational morphology is not ever dealt with in the syntax, but inflection is, along with other such "morphological" matters as <u>Do Support</u>, <u>Affix Hopping</u>, <u>Clitic Rules</u>, i.e. all of "grammatical morphology." This seems to be the position of, for example, Chomsky (1973).[2] This latter Lexicalist Hypothesis, which I will assume, does not say that derivational processes are always irregular and that their semantics is always non-compositional. Nor does it exclude from the domain of the syntax only irregular derivational pheonomena (as Chomsky (1970),says one might do). It says rather that derivational pheonomena are always separate from the syntax, regardless of their regularity. Postal (1969) presents very convincing evidence for this hypothesis.[3] Translated into a Saussurean framework, the hypothesis says that for the purposes of syntax, the word[4] (sans inflection) is the minimal sign.[5] This hypothesis says nothing about intra-word phenomena and relations; they may or may not be regular. Of course the main reason for the adoption of the hypothesis in the first place was semantic irregularity, and we must develop a theory of derivational morphology which allows for, and hopefully even predicts and accounts for, this observed irregularity.

I will now present evidence that the word is a minimal sign, not merely for the purposes of the syntax. To do this, I will show that below the level of the word we encounter morphemes which, while they must be assumed to be real linguistic elements, have no meaning which can be assigned independently of each of the individual words in which they occur. This demonstration is not a novelty. The most extended and formalized argument of the point I am making that I know of is in Hervey and Mulder (1973).

2.1.3. <u>Morphemes</u>.

2.1.3.1. Cranberry Morphs.

There is a class in English of hapax legomena, morphemes which only occur in one English word. They are often called cranberry morphs. Consider the following list:

(1)   cranberry   boysenberry
        huckelberry
        mulberry   bilberry

Since the words in our list are all names of berries, we may isolate this last unit as a meaningful morpheme. We are left with the items in (2):

(2)   #cran#   #boysen#
        #huckle#   #mul#
        #bil#

None of these items occurs either independently, or in any other words than those in (1). There is thus no non-circular way of assigning meanings to the morphemes in (2). Their meanings are intimately connected with those of the individual words in which they occur. As Hervey and Mulder note "both a and b ... each can be identifiable as a sign only if the other is also identifiable as such." One can, of course, ignore problems of circularity, and assign a meaning to the item in question. It is then merely an accident that this fully meaningful item occurs only in one word. However there are cases in which such a simple out is not possible.

2.1.3.2. Other Berries.

As noted above, it is possible to assign a meaning to items such as #cran#, simply because they do occur only in one word. With other names of berries, however, this simple device will not work. Consider the following list:

(3)   strawberry   blueberry
        blackberry   gooseberry

By removing berry again, we can isolate the morphemes in (4):

(4)   #straw#   #blue#
       #black#   #goose#

As opposed to the items in (2), these occur elsewhere than as parts of the names of berries, in fact they occur as independent words. However, when they do appear as independent words, they have meanings which can in no way relate to the meanings they might be assigned in (4). For example, one might think that a blackberry is black, however, not all black berries are blackberries, and furthermore, many blackberries are green or red (a fact also noted by Hervey and Mulder). There is therefore no way to assign a meaning to the item black which will be valid both when it occurs as an independent word and when it occurs in the word blackberry. The same holds for blueberry, and the connection between geese and gooseberries, or between straw and strawberries is not very apparent. The problem here is that we can not resort to the simple ruse of assigning the items in (4) constant meanings, for they do occur elsewhere than in the words in (3), but with meanings which a e totally incompatible with those we would like to assign to them on the basis of the meaning of the corresponding word in (3).

This problem of a morpheme having different meanings in different words can be gotten around without giving up the claim entirely that morphemes are meaningful. The basic tack is to give morphemes under-determined meanings, with contextually determined allomeanings. This is essentialy the tack which Chomsky (1970) takes. In order to handle idiosyncratic semantic differences in verb-noun pairs like refuse-refusal, he says that "the lexical entry may specify that semantic features are in part dependent on the choice of one or another of these categorial features" (noun or verb). To the extent that these dependencies are

regular, and syntactically motivated, there is virtue in such a device, or a similar redundancy convention, but to the extent that they are idiosyncratic, which many of them are, the device merely serves to obscure the truth, that it is the words that are idiosyncratic. Though this system may allow us to preserve the idea that morphemes are meaningful, it is only at the level of the individual word that these meanings can be fully specified.

In the particular case with which we are dealing, the device of underspecification and contextual filling leads to a particularly un-satisfying result. Since, as noted, some blackberries are red, and since something cannot be both black and red at the same time, the two allomeanings of #black# will be contradictory, and share almost no semantic features (+color?). Allowing a device which permits such a situation is very dangerous; it essentially gives homophony as the only criterion for deciding whether two things are instances of the same meaningful entity.[6]

One might also go entirely the opposite route. Thus one could claim that the various instances of #black# are completely unrelated, each a different morpheme. This rids us of the problem of morphemes with under-specified meanings, though we are still left with the circularity problem; is it the word or the morpheme which specifies the meaning? The next set of data bears on this theory.

2.1.3.3. Prefix=Stem (+latinate).

The last two sets of data consisted of what are traditionally called "partially motivated" forms. There was one element, berry, whose meaning was relatively constant, and another, which in a sense told us what sort of berry we were dealing with, but which never occurred, or never occurred

with the same sense, outside of the particular word with which we were
dealing. This next set of data differs from these in having no such
fixed element.

The set of data consists of the latinate verbs with bound stems and
prefixes which are always stressed on the stem. This class is marked
phonologically, in the system of SPE, by the presence of a special
boundary, =, between the prefix and the stem. Examples of such verbs
are refuse, convene, inject. I will not discuss verbs such as suffer,
proffer, differ, which diverge in their stress patterns from other pre-
fixed verbs with bound stems, and for which no = boundary is posited. Nor
will I discuss verbs such as re#fuse (fuse again), in which there is a #
boundary in the system of SPE. For this class (x = y) it is possible to
demonstrate that neither the prefix nor the stem has any fixed meaning.

First the stem. Consider the verbs in (5):

(5)	X=fer	X=mit	X=sume	X=ceive	X=duce
	refer	remit	resume	receive	reduce
	defer	demit		deceive	deduce
	prefer		presume		
	infer				induce
	confer	commit	consume	conceive	conduce
	transfer	transmit			transduce
		submit	subsume		
		admit	assume		adduce
		permit		perceive	

Let us presume for the moment that the prefixes in (5) have constant
meanings, much as the berry of (1) and (3). Is it possible to extract
any common meanings, however minimal, from the different occurrences of
each stem? At first glance, if we merely compare pairs of verbs, one

might be tempted to think so. Confer and transfer might appear to share something, similarly remit and submit, conceive and perceive, assume and presume, induce and deduce. However, if we attempt to extend matters beyond these select pairs, and extracting the common sense from each, assign it to the other verbs in the particular stem, the result is nonsense. What, even vague, sense does prefer share with confer and transfer? or commit with remit and submit? or receive with conceive and perceive? or consume with presume and assume? or reduce with induce and deduce? None. There is no meaning which can be assigned to any of these stems, and combined with the presumable constant meanings of the prefixes in a consistent way, to produce the meanings of all the verbs in that stem. Each stem occurs in different verbs, but never with the same sense. Rather the sense is determined by the individual verb.

As suggested above, one might attempt to reduce the whole problem to cranberries, (with, of course, the accompanying problems of that class) by calling each occurrence of a given stem a different morpheme. This system completely denies any linguistic reality to the stems, and re-places each of them by a list of homophones, each with its own meaning, and each occurring with only one, perhaps even two, prefixes. In such a system, one would not have, for instance, a stem mit which occurred in all the relevant words in (9), rather one would have many homophonous stems, mit, mit_2, ..., mit_n. This system would be fine if these stems had nothing at all in common. The problem is that all occurrences of the stem mit do share a common feature which is not predictable from any general phonological properties of the sequence [mit]. As carefully documented in Chapters 4 and 5, all instances of the latinate stem mit exhibit the same, phonologically arbitrary, variant (allomorph), before the suffixes +ion, +ory, +or, +ive, +able. The details of the rele-vant argument are given in Chapter 4. For the moment we can look at

the following paradigm:

(6) permit   permission   permissive

     remit   remission   remissory

     excrete   excretion   excretive

     assert   assertion   assertive

     digest   digestion   digestive

     prohibit   prohibition   prohibitive

The last column reveals the difference between verbs of the form X=mit and other verbs with final $\underline{t}$, before the suffixes in question. Mit always takes the form mis here, and the change of $\underline{t}$ to $\underline{s}$ in this environment is confined to this one stem. There are no exceptions to this rule either way.

This regularity, or the factors which condition it, cannot be phonological, but must be stated on another linguistic level, the level of the stem, or morpheme. Proof of this assertion is the fact that other instances of the phonological sequence [mit],   not instances of the latinate stem mit, do not show up as [mis] in the relevant environment. So we find vomit/vomitory. In the word vomit, there is no reason to presume that we are dealing with a prefix vo and a stem mit, in fact there is good reason to believe that we are not; vo never shows up as a prefix elsewhere, and the stress pattern gives us no evidence of a boundary, or at least of the sort of boundary for which there could be evidence. The alteration in question is therefore restricted to the latinate stem mit. This means that all the items which, in the theory in question, were more homophones, $\underline{mit}_1$,....,$\underline{mit}_n$, must be at some level instances of the same thing. Otherwise there is no way to express the fact that all occurrences of mit exhibit the same allomorphy. There is good evidence that the level at which the rule embodying the facts in question must be stated

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is that of the morpheme. First of all a feature such as <u>latinate</u>, which governs, among other things, what sorts of affixes can be attached to a word, is a property of morphemes. Secondly, the sort of rule that changes <u>t</u> to <u>s</u> in the relevant environment here is a rule which applies to a morpheme, and not to any other linguistic level, lower or higher. <u>Mit</u> is therefore a morpheme, though it has no meaning. Nor is <u>mit</u> the only case. As we shall see in Chapter 5, there are many stems which undergo rules of allomorphy.

It appears, then, that there is something fundamentally wrong with the theory of many homophonous <u>mits</u>, for there is good evidence that we are indeed dealing with one morpheme. This turns us back to the allomeaning theory, with its problems of under-specified meanings and circularity, or to the theory that morphemes are not minimal signs. The allomeaning theory had some plausibility with reference to preceding sets of data, (cranberry, blackberry, etc.) mainly because, as noted, we always had one constant element, with a relatively perspicuous meaning, and we could as a result attribute the residue of the meaning of each word to the problematic morpheme. Here, however, when we look at the prefixes, we find that just like the stems there is no constant meaning which can be attributed to any of the prefixes. How, then are we to segment the meaning of the individual words in a principled manner?

Consider the following list:

(7)	<u>re=X</u>	<u>con=X</u>	<u>in=X</u>	<u>de=X</u>
	repel	compel	impel	
	remit	commit		demit
	refer	confer	infer	defer
	resume	consume		

receive   conceive   deceive

reduce   conduce   induce   deduce

Though it is more likely that one could attribute more commonality of

meaning to occurrences of some of these prefixes than one could to any

of the stems, there is no general meaning which can be assigned to any

of them. Thus one might try to assign to re a meaning "back", and def-

initely many of the verbs of the form re=X have something to do with

"back" (cf. Williams, 1973). What about receive, though? or reduce

in the following sentence?

    (8) The government reduced the size of the quart from 32
          to 31 ounces, in an effort to stop inflation.

Since the quart never was less than 32 ounces, there is no way in which

"back" can be involved in the meaning of reduce here.

Now, since we know from (7) that re= has no fixed meaning, and we

know from (5) that duce has no fixed meaning, how are we to segment the

meaning of reduce into two parts, one associated with re=, and the

other with duce, in a principled manner? We can't. The word principled

is important here. One can a priori split any word in two and give each

part a meaning. I can divide apple into a and pl, and give each of them

part of the meaning of the whole word. We don't want to do this, for,

as noted above, allowing such an analysis reduces the predictive power

of a theory to nil. It is unfalsifiable. Thus the fact that the allo-

meaning theory must be made so strong in these cases that its empirical

validity is reduced to nil, forces us to fall back on the only position

left to us. There are morphemes which have no meaning. The hypothesis

that morphemes are the "minimal meaningful elements of language" cannot

be maintained in any of its even most contorted variants. In many cases

this role of the minimal sign must be moved one level up, to the level

of the word. The sign gravitates to the word.

Note that we have not abandoned the concept of the morpheme.
It still remains, but not always as a sign.

2.1.3.4. A Similar Class.

The same argument as was made in 2.1.3.3 can be made for the
following set of data, which comprises a much smaller, though more
striking set of prefixed verbs:

(9)   understand/stood   undertake/took

        withstand/stood

                                  partake/took

There is no way to relate the putative meanings of stand in its two
occurrences, nor those of take. Nor can the meaning of stand, in under-
stand at least, be related to any of the multifarious meanings of the
free verb stand. Similarly for the prefixes under, with, and par. How-
ever, in the case of the stems, we must be able to encode the fact that
they always show the same variant in the past tense form. Nor is there
any way in which this variant can be viewed as phonologically con-
ditioned. It must be conditioned by some abstract property which is
common to all occurrences of the meaningless entity stand or take.

2.1.3.5. Defining the Morpheme.

A minimal sign is defined as an arbitrary union of significant
and signifié, sound and meaning. A morpheme does not always meet the
second criterion. In the light of the last two sets of data, and anti-
cipating slightly, we will define the morpheme as an arbitrary unit of
signifiant, or rather a set of such units which appear in mutually ex-
clusive, other than phonologically determined, environments. The
utility of such a definition will be demonstrated directly.

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2.1.4. Trouble with Words?

2.1.4.1. Cranberry Words.

There are words which, like cranberry morphs, concatenate only with specific words, and not with syntactic classes. So, the noun headway, for example occurs only as the direct object of the verb make, just as cran occurs only in cranberry. However, there is a difference in the manner of concatenation. On the phonological and syntactic surface, cran can only appear in one specific place, directly before berry. However, headway does not necessarily appear directly after make on the surface, rather it is the head of its underlying object NP; and as such may be modified and even moved about:

(10) We haven't made much headway lately.
(11) Are we making any sort of headway here?
(12) There isn't much headway being made.
(13) The only headway we were making was illusory.[7]

Because it occurs in these different environments, we can isolate other properties of headway than the fact that it is an arbitrary phonological string. It is a noun. It is not a count noun (*headways). It is not animate (...headway ... it). Thus we can say things about headway which are not dependent on make, and which have something to do with its meaning. This is not true of cran, and it is the complete interdependence of cran and berry which forces us to conclude that, strictu sensu, the former cannot be meaningful.[8] The point is that because cran is completely isolated from the syntax by its occurrence inside only the one word, there is no way in which it can have syntactic properties of its own, and hence semantic properties. Headway, because it is a noun, and the head of a syntactic phrase, is not so insulated. As the head of a phrase, it must, perforce, have syntactic properties, some of which may be related to meaning properties.

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2.1.4.2. The Numerous Verbs Stand.

I have argued that the various instances of stand in (9) were not semantically relatable, though they must be regarded as instances of the same entity because of their shared irregularity (stand/stood). It seems possible to argue exactly the same point from the various occurrences of stand as an independent verb. Many of the uses of the verb stand are semantically unconnectable, and yet the same irregular past form always appears. Consider the following sentences:

(14) We stood there for a while.

(15) We stood the chairs in a corner.

(16) I stood it as long as I could, and then left.

Though one might conceivably attempt to relate the verbs of (14) and (15) in some manner, perhaps even systematically, I cannot see how either of these two could be related to the verb in (16). However, though the meaning of this verb cannot be systematically related to the others, its form is. Therefore, extending the argument of 2.1.3.4. to this class, the word stand, one might wish to say, is a unit, but it has no meaning. There is no difference between morphemes and words.

With regard to meaning the same sorts of arguments hold here as hold in 2.1.4.1. The various verbs in the above sentences have different subcategorizations, and from subcategorication we can go to meaning. Therefore the individual verbs are not meaningless, or indeterminable as to their meaning. They each comprise a separate entry in the Lexicon.

The problem is accounting for a property that they share, which has nothing to do with their meanings. This is the common irregularity of their past tense forms. It is here that our definition of the morpheme comes into play. A morpheme is a set of arbitrary units of signifiant

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which appear in mutually exclusive, other than phonologically determined, environments. By this definition, all occurrences of <u>stand</u> are occurrences of the same morpheme, though they are not occurrences of the same sign (verb). The same morpheme also occurs in <u>understand</u> and <u>withstand</u>, though again there is no semantic relationship either between these two, or between them and the various free verbs <u>stand</u>. We see, then, that the concept   <u>morpheme</u>, as defined above, and the concept <u>word</u>, are not really concepts of the same sort. Two words can be instances of the same morpheme. In addition, we find that freeing the morpheme from the requirement that it be meaningful, which we have found to be necessary, allows us to use it to account for phenomena which in other theories could not be related (no prevalent theory which I am aware of is capable of encoding formal similarities among words of this arbitrary sort, which are not accompanied by semantic similarities). The numerous verbs <u>stand</u> thus present no problem in our theory, rather their oddities can be accounted for quite nicely.

2.1.5. <u>A Historical Note on Inflection</u>.

It should not be terribly surprising that morphemes are not the "minimal meaningful elements" they have been purported to be. This conception of a morpheme is very intimately tied in with certain structuralist assumptions. It is in part, at least, a consequence of a simple view of the relationship between sound and meaning, and the mappings which express this relation.

When dealing with inflection, this type of system is especially difficult to justify. Even very early, attempts by Hockett (1947) and Bloch (1947) to apply the definition of a morpheme as a one to one mapping between meaning and sound to real data led to very bizarre and counterintuitive results (cf. Nida, 1948 for criticism of the above two). Harris

(1948) discusses the problems that a paradigmatic set of data present for a theory in which the morpheme is the basic meaningful element. Chomsky (1965) made essentially the same point as Harris twenty years later, when he introduced the complex symbol and syntactic feature as a way of treating paradigmatic and cross-classifying phenomena. In a system like Chomsky's, the traditional concept of a morpheme as a one to one mapping between form and meaning is nullified. Chomsky makes this point explicitly, and argues for the virtue of his system over the old one with regard to the treatment of inflection (ibid. 170-174).

Thus, rejecting the morpheme, at least in its definition as a minimally meaningful unit, as a basis for a theory of derivational morphology, is not the radical step one might think it to be. As a basis for accounting for inflectional phenomena, it has long been under attack. We must now develop a theory of morphology, which does not crucially depend on the morpheme as a basic meaning bearing element.

2.2. Word Formation.

The goal of this section is to sketch out the underpinnings of a theory of morphology. In view of the preceding section, we will assume that such a theory must not include the premise that morphemes are necessarily meaningful.

2.2.1. Possible and Actual Words.

Just as the simplest goal of a syntax is the enumeration of the class of possible sentences of a language, so the simplest task of a morphology, the least we demand of it, is the enumeration of the class of possible words of a language. The difference between the syntax and morphology with respect to this enumeration is that in derivational morphology there is a distinction to be made between the classes of

possible words and actual words.

This difference has long been recognized. Early critics of Generative Grammar (Zimmer, 1964, Schacter, 1962) pointed out that there are many words which a grammar can generate in a language which, accidentally and unsystematically, never appear. This very pervasive phenomenon, they point out, cannot be handled in a morpheme based grammar which does not distinguish between words and higher syntactic entities, as pre-lexicalist grammars indeed did not. Of the few substantial works on morphology within a generative grammar, two have contained proposals, essentially the same in their content, designed to deal with exactly this distinction between possible and actual. Botha (1968) and Halle (1973) have suggested that, in addition to the list of morphemes of a language and the rules of morphology, which concatenate these morphemes into poss-ible words, there must exist a list of actual words, a dictionary, which they see as a sort of filter on the output of the morphology. Within a morpheme based theory of morphology, such as theirs, there are then two lexicons, one a list of morphemes and their meanings, which, together with the morphology defines the class of possible words of a language, and the other a word lexicon. The actual words are a subset of the possible.

But words are peculiar, not only in that not all of those that should exist actually do, but also in that those which do exist don't always mean what they are supposed to mean, or even look like what they are supposed to look like. Words, once formed, persist and change; they take on idiosyncracies, with the result that they are soon no longer generable by any simple algorithm of any generality. The word gravitates toward the sign. The actual words of a language, the members of the set of dictionary entries, are as a result not a subset of the items which are

generated by a regular morphology with meaningful morphemes, out of which are generated words and their meanings.

This is the basic trouble with morphemes. Because words, though they may be formed by regular rules, once they are in the lexicon, persist and change, the morphemes out of which words seem to have been formed, and into which they seem to be analyzable, do not have constant meanings, and in some cases have no meaning at all. It is this persistence which forces us to adopt a Lexicalist hypothesis.

Halle noticed this problem, and suggested that the dictionary contain not only the actual words, but also that the idiosyncrasies of each word, if there are any, be listed there as well. These idiosyncrasies would include the phonological and syntactic exception features which a word might have, as well as its semantic and syntactic peculiarities, i. e. those semantic and syntactic properties not provided by the general rules of the morphology. A problem which immediately arises, even in this less rigid framework, in which it is at least tacitly admitted that arbitrary meanings can be assigned at the word level, is that there are words which are so idiosyncratic that their meanings contradict that which is expected by the general rule. In Halle's system, a word can mean more than it is expected to mean, but it is difficult to see how it could mean the opposite of what its predicted meaning is, without severely damaging the rules of the system or weakening it to the point that its predictive powers are obliterated. For example, the word transmission, which according to the general rules of the morphology should be an abstract nominal, meaning something like "the action of transmitting", when it refers to a car's transmission, means nothing of the sort. It doesn't just mean more than it is supposed to. In a system such as Halle's, in

which a word is provided with a meaning by general rules, and this meaning can be expanded upon, words like this are very problematical.

Returning to our central task, enumerating the class of possible words of a language, we note that the list of the actual words of a language, which, because of their irregularities, must be listed in a dictionary, takes care of the greater part of this task. It enumerates the actual words of a language. The idiosyncracies which force us to list these words also prevent us from positing a simple system of rules for describing them.

However, the list of the actual words which a speaker has at his command at a given moment is not closed. The speaker always has the capacity to make up new words, which he can then add to his repertoire. It thus remains the task of a morphology to tell us what sort of new words a speaker can form. Note that we have suggested that the gross irregularities which words in the dictionary often have are due to their persistence, to the mere fact that they are listed. It seems reasonable to assume that such gross irregularities are not characteristic of the new words which a speaker makes up, which, simply because they have not existed long, have not had any opportunity to become fi d in some idiosyncrasy. We will assume, then, that there are rules for making up new words, rules which are regular and interesting, and turn now to the task of describing these rules.

Of course, we do not ask of a good theory of morphology merely that it perform this one task. The words in the dictionary, though they are idiosyncratic, do exhibit regularities, they do have structure. Morphemes, even though they may not be what they have been purported to be, are recognizable. Nor does a speaker make up all the new words he encounters. He hears words he has never heard before, recognizes them as

words of his language, if they are, and has intuitions about their meaning and structure. A good theory of morphology should tell us something about these things as well, and to the extent that they seem to be related phenomena, and related to the mechanism for making up new words, the theory we present should express this relationship.

2.2.2. Types of New Words.

We must determine what sorts of new words can be coined. The restriction here is very clear and pervasive. The only classes of words to which new words can be added by coining are the Major Lexical Categories: Noun(N), Adjective(Adj), Verb(V), Adverb(Adv). New coinings may not be added to the various "grammatical" categories: pronoun, determiner, quantifier, conjunction, preposition, particle, modal, auxiliary, etc. This fact can be related to the distinction between inflectional and derivational morphology, but I will not try to go into details of that relationship here.[9]

Nice confirmation of the restriction of new coinings to Major Lexical Categories is provided by the opening lines of Lewis Carroll's Jabberwocky, which are repeated below (the underlining is mine):

> 'Twas brillig, and the slithy toves
>
> Did gyre and gimble in the wabe:
>
> All mimsy were the borogoves,
>
> And the mome raths outgrabe.

All the words which are members of Major Lexical Categories have been underlined. All other words are "grammatical". If we accept Humpty Dumpty's analysis, then all of the underlined words, and none of the others, are new coinages. This accords perfectly with the claim being made here.

2.2.3. What Are New Words Coined From?

2.2.3.1. Oddities.

The underlined words are all rather unusual coinages. Those whose basis is not completely opaque are blendings (cf. M p. 451-454), or as Carroll calls them, portmanteau words, formed by merging parts of words into a word which meets the phonotactic restrictions of the language. More transparent examples are smog, from smoke and fog, and chunnel, from channel and tunnel.

A related type of coining is that of letter words and syllable words, collectively known as acronyms. Examples are Nato, radar, futhorc. This type is almost unknown in the languages of the world, and uncommon in our own, before this century. It is even possible that the modern use of it can be traced back to that of the Hebrew scholarly tradition, where the names of sages were abbreviated by means of such a device (rashi = rabbi shlomo ben yichag, rambam = rabbi moshe ben maimon). It does, in any case, presuppose an alphabet. At present it is most common in the official languages of the major imperialist powers. The device is, in short, very unusual, and certainly not a universal fact of language.

These two devices form words which have no recognizable internal structure or constituents. This makes them opaque, and hence uncommon. The logic of the hence is that when we hear a word, whose meaning we do not have any clues to, unless this word denotes an important thing, we will have difficulty retaining it.[10]

2.2.3.2. Words from Morphemes.

Another type of device which is also uncommon consists of the

stringing together of morphemes, with, of course, appropriate restrictions on what sorts of morphemes go where. Suffixes at the end, prefixes at the beginning, etc. This sort of coining accords with the sort of morphology we are accustomed to believing in.

An example of such a coining is the word transmote, (brought to my attention by Bob Fiengo), which has the structure [trans=mote]$_V$, and consists of the morphemes trans as in transmit, and mote, as in emote, in the latinate prefix stem pattern discussed in 2.1. The etymology of the word is curious. Officials of the Johnson administration needed a verb which would mean "transfer from one position to another", but would have neither negative nor laudatory connotations. Transfer is slightly negative,and demote and promote both imply a change in rank; hence transmote, with the trans of transfer, and the mote of the other two.

What is important to note about transmote is that despite its seeming structure, its meaning is not clear until explicated. Only when it is compared with transfer, demote, and promote is it possible to even begin to make an intelligent guess at its sense. The word thus resembles a blending like chunnel (channel tunnel), which is derived from other words, but not at all transparently. The lack of semantic transparency should not be surprising to anyone who has read the long section on meaning and morphemes in 2.1. There I took great pains to show that exactly these classes of prefixes and stems have no meaning. They are not signs. Since the parts have no independent meaning, the meaning of the whole is unclear. It follows from this, by the short argument given above, that the sort of word-formation of which transmote is a product will be as sporadic as blending. In fact, I think we can reasonably claim that the two devices are really one: take two words, stick them together, and chop out the middle.

2.2.3.3. Word-based Morphology.

I have dealt rather hastily, with several types of word-formation processes, which, I claim are really one. The main characteristic of this type of word-formation is the fact that any word formed by such a process is not regularly derivable from elements which are meaningful. I have also connected, by a simplistic argument, this characteristic and lack of productivity. I will not discuss these opaque processes any further.

It remains to establish what sorts of word-formation processes can be productive. This brings us to the main thesis of this work (and many previous ones):

Hypothesis: All regular word-formation processes are word-based. A new word is formed by applying a regular rule to a single already existing word. Both the new word and the existing one are members of Major Lexical Categories.

Any theory of which this hypothesis is a basic tenet we will call a theory of word-based morphology. In the rest of this work, I will try to develop a relatively detailed version of such a theory.

2.2.3.4. Word Formation Rules.

The regular rules referred to above will be termed Word Formation Rules (WFR). Such a rule specifies a set of words, on which it can operate. This set, or any member of this set, we will term the base of that rule. Every WFR specifies a unique phonological operation, which is performed on the base.[11] Every WFR also specifies a syntactic label and subcategorization for the resulting word, and a semantic reading for it, which is a function of the reading of the base.

Chapter 3 will be devoted to a more detailed discussion of the general form and characteristics of WFRs.

It is a fact that almost all new words are produced by WFRs. I will give only one example: from the adj. communal I form the verb communalize, by the WFR of #ize attachment. I know what this word means, since I know what its base means, and the rule is regular. X#ize can be paraphrased roughly as "make X". Quite different from transmote. From communalize, in turn, I form the abstract action nominal communalization, by the WFR of +At+iVn attachment. This word too is transparent in its meaning.

Note that WFRs do not operate on anything less than a word, i. e. on morphemes. As demonstrated, not all morphemes are meaningful. Since regular rules can only derive meaningful words from meaningful bases, it follows of course that meaningless morphemes cannot serve as bases for any such rules. But I have not specified meaningfulness as a criterion for serving as the base of a WFR. If there are meaningful morphemes, and I have not argued that such entities never exist, the theory as formulated does not permit them to serve as the base of any WFR. This is of course an empirical claim. In 2.2.5. a class of words which do not seem to be derived from existing words is discussed. Such a class would be counter-evidence to the claim being made here, if indeed one could show that these words were so derived. In this particular case there is good evidence that the base of the rules is a class of existing words.

One important peculiarity of the conception of the rules of word-formation which is being outlined here is that these rules are not viewed as applying every time the speaker of a language speaks. They are rules for making up new words which may be added to the speaker's lexicon. We

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can think of them as once-only-rules. They are thus very different from the rules of the syntax and the phonology which must apply every time we speak. This has been pointed out by other people in other contexts (e.g. Halle, 1973), however, normally as an observation, and not as a basic tenet of a theory of morphology.

2.2.3.5. Assumptions About the Lexicon.

The rules of word-formation are rules for generating words which may be stored in the dictionary of a language. The rules are a part of the grammar of that language. I assume that these rules are completely separate from the syntactic and phonological rules of the grammar. Thus when a WFR specifies a phonological operation, this operation is not merely indicated by the WFR in the form of some rule-feature, and then performed as a rule of the phonology. Rather the phonological operation is part of the WFR itself. The same position with regard to syntactic and semantic phenomena is a basic tenet of the Extended Standard Theory of syntax, one of the central claims of which is that lexical insertion, at the level of the Major Lexical Category, precedes all syntactic rules (cf. Postal, 1969).

A consequence of these assumptions is that each word may be entered in the dictionary as a fully specified separate item. It is possible, and not unusual, to conceive of a system in which all redundancies are removed from the entries and then somehow filled back in by general rule. Such an approach was long accepted in phonology, however, due to certain difficulties associated with the particular notation being used (allowing features to be specified +or -, or given no specification) brought to light by Stanley (1967), this manner of dealing with redundancies was replaced by a system in which all phonological features were completely

specified in the lexical entry for each word (or morpheme). Such a system is accepted and even presupposed by most leading contemporary phonologists (SPE ch. 9, Kiparsky, 1973). There is good reason for not factoring out syntactic and semantic redundancies either. This will be discussed later, in the context of a method for dealing with morphological regularities in the dictionary.

I will assume, then, that each word in the dictionary is an independent item, fully specified. Dictionary entries are not dependent on one another, or on rules. Each one is a complete sign in itself.

2.2.4. Evidence for the Proposal.

The theory proposed is essentially based on an observation: new words are by and large formed from old ones by recognizable rules. It also has the advantage of ridding us of the central problem of a morpheme-based theory of morphology (though, at present it does so at some expense, by removing from consideration all matters pertaining to words already in the dictionary).[12]

A good theory does more, however, than avoid problems. It also helps us to understand and account for things which hitherto were inexplicable. I would now like to discuss two matters which the theory so far outlined helps us to understand: the phonological cycle, and irregular back-formations.

2.2.4.1. The Phonological Cycle.

The phonological cycle is a much talked about subject. Some suspect the validity of the entire concept, and many have criticized what they have felt to be unmotivated uses of the device. Cyclical phonological rules are dependent for their operation on labeled bracketings. They

apply first to maximal strings which contain no labeled brackets, after which innermost brackets are erased (or equally, disregarded), and they apply to the next maximal string which contain no brackets, and so on (cf. SPE ch. 2). The most principled objections to the cycle have been directed against the arbitrary and high-handed manner in which these labeled bracketings are sometimes determined.

In an important article, Bramè (1974) has attempted to answer these objections by proposing a general constraint on such bracketing. The basis for Brame's constraint is the observation that the string constituting the domain of every application of the cycle of rules "always shows up elsewhere as an independent phonetic word sequence." The statement of this observation is ambiguous. An underlying phonological string of the form $x+y$ has a surface representation $X=Y$, which is not identical to the underlying representation. We will take the sense of Brame's observation to be that we may cycle on underlying $\underline{x}$, only if surface $\underline{X}$ is an independently occurring word. Another possible sense is that we may cycle on $\underline{x}$, only if there is an independently occurring surface word of the form $\underline{x}$, rather than of the form $\underline{X}$, i.e. identical to the underlying rather than the surface representation. It is difficult to tell which sense Brame intended. However, the second leads to certain puzzling consequences, and so we will presuppose the former.

Brame's constraint is a formulation of his observation. Before stating the constraint, we need a definition:

definition:

Two strings in phonological representations are said to be equipotent if they are identical and at least one of the two is not represented as a proper substring

in phonetic representations, (ibid. p. 56).

He then proposes a Natural Bracketing Hypothesis:

For a substring ψ to be bracketed, it must be

equipotent to a string δ , (ibid. p. 56).

Translated into simpler terms, and clearing up some ambiguities in the
definition of equipotency (a proper substring of what? Probably of
a string bounded by ## ... ##). Brame's hypothesis says that only a
string whose surface reflex shows up elsewhere as an independent word
can be bracketed. There is a slight problem with this hypothesis. One
wants to avoid bracketings like the following: $\left[\left[\text{fil}\right]\text{ter}\right]$. This bracket-
ing meets the conditions which the hypothesis imposes. Fil occurs
elsewhere as an independent word. Yet such a bracketing is intuitively
unsatisfactory. In order to avoid the possibility of this bracketing,
Brame suggests that we adopt, instead of the above hypothesis, the
following:

Strong Natural Bracketing Hypothesis

For a substring ψ of a string φ to be bracketed,

ψ must be equipotent to a string δ, and the meaning

of φ must be a compositional function of the meaning

of δ and $\varphi - \psi$ (φ minus ψ).

This rules out the bracketing $\left[\left[\text{fil}\right]\text{ter}\right]$.

The latter hypothesis, Brame notes, may be too strong,[13] but it
is interesting.

The question now naturally arises of whether a constraint like
that imposed by the Strong Hypothesis is a basic theoretical entity, or
whether it falls out from more general principle. There obviously is
some device which assigns these natural bracketings, and this device
should have some other motivation than the mere fact that it assigns

natural bracketings. Brame does not speculate as to the nature of this device.

Within the theory of SPE, the input to the phonology is supplied by the syntax. The bracketing which defines the phonological cycle is basically that of the syntactic surface structure (with a few readjustments). Within that theory, therefore, there is independent justification for the bracketings in question. Within the Lexicalist theory of syntax, however, the syntax doesn't extend below the level of the word, and as a result, cannot be called upon to generate any intra-word bracketings. Since in the earlier theory the bracketing could be syntactically motivated, we expect that in the new theory whatever replaces the syntax at the level in question should assign the bracketing in question. This is of course the morphology.

Within the theory outlined above, a new word is always formed by performing some phonological operation on an already existing one. In most cases, the effect of this phonological operation will be the addition of some affix to the already existing word. This means, in effect, that the new word will contain the old. The meaning of the new word will also be a compositional function of that of the one it con- tains. Since members of Major Lexical $C_a$tegories are always labeled (N, V, Adj, Adv), and all regular WFRs operate on such labeled words, and since there is no reason to assume that these labels are erased in the course of the application of a WFR, WFRs will, unless otherwise constrained, produce labeled bracketing in their output. It is clear that all the constraints imposed on intra-word bracketings by the Natural Bracketing Hypothesis are direct consequences of this theory. In fact, given this theory, no other bracketing is possible. This is

evidence of the highest order in favor of the central claims of the theory proposed.

Note that there is no reason to suppose that the sort of sporadic word-formation discussed in 2.2.3.1. and 2.2.3.2. results in any kind of labeled bracketings. Thus a new word like transmote will not have ∩ cyclical structure. Nor will such a word be bracketable according to Brame's hypothesis. As far as I know, words formed by such processes never need be treated in a cyclical fashion. This provides yet more evidence in favor of our theory, and in favor of the separation of word-formation devices into the two types.

Not all words are derived cyclically. There are sometimes even minimal pairs, the only difference between the members of which is the fact that one may be derived cyclically, the other not. Consider the following case, discussed by Brame. There are two words, Prohibition [proəbišən], and prohibition [prohibišən]: the first refers to a certain law or period in American history; the second is a deverbal action nominal. They are distinguished on the phonological surface by the fact that one has h followed by i where the other has ə. This difference can be accounted for if we give the two words the following underlying phonological forms:

spelling	surface	underlying
Prohibition	[proəbišən]	[pro=hibit+iVn]$_N$
prohibition	[prohibišən]	[[pro=hibit+]$_V$+iVn]$_N$

Thus the only difference between the two words is that one, and not the other, has cyclical structure. The differences then fall out by regular rule. In the first word, a rule operates which elides h before an un-stressed vowel, in this case i. This same i, since it never receives

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stress, is later reduced to ǝ by the general rule of vowel reduction which operates on all unstressed lax vowels. In the second word, this same i, the one which follows h, on the first cycle, will be stressed by the Primary Stress Rule as it would be in the verb. Though the next vowel is stressed on the next cycle, and consequently receives the main stress by the Detail Rule (cf. Halle, 1973b),[14] there is still sufficient stress on the i which the h preceꞁ ꞁ⸱s ᴄo prevent the application of the h elision rule (which only operates before an unstressed vowel), and to prevent also the i from being reduced. We see then that once the cyclic structure is imposed, all differences can be derived in a principled manner, without recourse to exception features or special rules. There is in fact no other principled way to derive these two forms, and they provide powerful evidence for a theory which includes the notion of the cycle.

According to ᴏur theory of morphology, every new word, if it is derived by a regular rule, must have cyclical structure, it must ᴏe bracketed internally. However, the first of the above two words has been shown not to have cyclical structure. This seems to be a problem for our theory. According to it, shouldn't all complex words be derived cyclically?

Remember that the rationale for discussing only literal word-formation, i.e. coining, and not discussing the structure of words which were already in the dictionary, was the fact that these latter tend to be irregular, lose some of their appointed meaning and gain individual nuances. Though I never suggested that their structure might change, or be lost, this is certainly a possibility, and would be in accord with the general observation that the words in the dictionary tend to go each

their individual ways, and lose their connections with each other. When we look at the two words in question, we find that the first is definitely much further in meaning from the verb prohibit than the second, in fact there is really no semantic connection between Prohibition and prohibit. This is of course not true of the second one; it is the derived action nominal of the verb prohibit. It is true that whenever we find two words which differ only in that one must be derived cyclically and the other not, the one which is not cyclically derived is always further in meaning from the base. This has been noted many times in the literature. We will say that a word which has been in the dictionary long enough to diverge from compositionality, i.e., a word whose meaning is no longer derivable from that of its parts, may lose its cyclic structure. This is of course a very rough formulation. We have not said how far a word must diverge before it loses its structure, and it may be that it must, rather than may, lose its structure. However it does account for the fact that only the divergent word has no cyclic structure. This is of course an addition to our theory. It now says that though we may not make up words which are not Naturally Bracketed, words may lose their bracketings as they go their own way. This does not seem to be a very serious addition, or to weaken our position much, and it allows us to encode very nicely the fact that only words whose semantics is not compositional will be susceptible to loss of structure, though it does not explain it. Other theories, though they allow for both cyclic and non-cyclic structures, have trouble accounting for the semantic differences between the two sorts of structures in a principled way. To the extent that our present theory can accommodate the two in a principled and interesting manner, it is superior to others.

Summarizing this section on the phonological cycle, we have seen that the exact sorts of structures which are argued by Brame to be the only ones which the phonological cycle can operate on are exactly the sorts of structures which our theory predicts to be the output of the morphology.

2.2.4.2. Irregular Back-formations.

Back-formation is, as M stresses, of diachronic relevance only. It consists of the extraction of a new word from an already existing word which appears bi-morphemic. Within the theory outlined it is thus just what its name says: a backwards application of a WFR. The most often quoted example of back-formation in English is the verb peddle, back-formed from the noun peddler. Historically, peddler is monomorphemic. However, since it is an occupational noun, and such nouns are often formed from verbs with the suffix -er, the er became analyzed as an affix, and the stem came into use as a verb subsequently. More common in English is the borrowing of a latinate derived form, whose stem is subsequently retrieved by back-formation. Such a case is the verb aggress, which must be presumed to be back-formed from the noun aggression, since most speakers have the latter in their vocabulary, but not the former.

The fact that back-formations of any sort are possible but not necessary is easily handled in a theory in which all words in the dictionary are completely specified separate items. In other theories there are some problems. So, for example, if aggression, as a derived nominal, is not listed in the dictionary as a completely specified form, then the form which presumably is referred to in completely specifying aggression at the point of lexical insertion, i. e., aggress, must for

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most speakers be marked -_Lexical Insertion_. In a theory in which all words are not independent, back-formation always results in a better system, in fact the system which does not have the back-form is very bizarre. But if this is true, then why don't all speakers adopt the back-form as soon as they are exposed to it, which they do not. There are, of course problems with the notion -_Lexical Insertion_ itself, which is so strong as to be almost vacuous. Thus the mere fact of back-formation seems more easily accommodated in a full entry theory of some sort. This same point is made somewhat more forcefully by Jackendoff (1974).

However, there is real evidence that some back-forms cannot be even accommodated in any theory but one in which every word is a complete entry unto itself. This evidence comes from phonologically "irregular" back-forms. Consider such words as _self-destruct_ and _cohese_, back-formed from _self-destruction_ and _cohesion._ Within most theories, one expects the forms _self-destroy_ and _cohere_, which presumably underly the nominals, and are merely marked -_Lexical Insertion_. The actual forms are thus impossible.

To see how they are predicted within the theory outlined so far, we must first digress a little. We have already mentioned rules of Allomorphy, in connection with the status of the latinate stem _mit_, and they will be discussed in detail in ch. 5. For the moment we only need to know that the morph _struct_ which occurs in _self-destruction_ can be an allomorph of two morphemes, one which appears word-finally as _stroy_ (_destroy_/_destruction_) and one which appears word finally as _struct_ (_construct_/_construction_). The "source" of _struct_/_ion_ is thus opaque. It could be either _struct_ or _stroy_. In a full entry theory _self-destruction_ is an entity unto itself, and when we back-form from it we

essentially ask ourselves "What word might this one have been formed from?" We on't know, and must pick the most likely one. By a principle of least effort (identical to that used in determining underlying phonological forms where the choice is indeterminate, in the theory of SPE ch. 9) when, in the course of our "reconstruction" we arrive at a choice which is arbitrary, we choose the form which is "closest" to the one we started out from. Thus we must, in this instance, choose struct, which is identical, rather than stroy, and we arrive at the word self-destruct as the most likely. Within any other system, since the "source" already "exists" though it doesn't occur, we need have no recourse to any "might have been" strategy, and as a result either make the wrong prediction, or can make none at all. The point is that in the full entry theory we must have a "recovery" strategy, and the most sensible recovery strategy that arises in the words-from-words hypothesis gives us exactly the right results. Our theory thus pushes us to make two decisions, both of which are vindicated by the data.

It must be stressed that within no other theory are we forced to make the right choices. A theory which does not have fully specified entries, as noted, tells us nothing about this situation. A theory which, like that of Jackendoff, has fully specified separate entries, but relates them by redundancy rules of an arbitrary form, and does not contain the notion allomorphy rule, tells us nothing about the proper strategy. It is only when we claim that words are formed from words by rules, each of which performs a unitary phonological operation[15] that the proper strategy is predicted.

The same account holds for cohese. Since Vs before +ion can be the reflex of Vs (confuse/ confusion, excise/ excision), or Vd (delude/

delusion, pervade/ pervasion, provide/ provision), or $\overline{V}r$ (adhere/adhesion),
it is only by the principle of least effort, and using the right strategy,
that we predict cohese.

2.2.5. Counter-Evidence.

Direct counter-evidence to the theory that words are formed from
words would be a case in which there are several words which are formed
from the same stem, but the stem never shows up as a word itself. Of
course, if there are only one or two such cases, we might reasonably
hypothesize that the non-occurring stem has unaccountably dropped out of
the language, after having done its duty, or that like the case of
aggression/*aggress, we are dealing with a borrowing from a language
which happens to have a similar morphology. However, when we find
many stems which exhibit this peculiar phenomenon, and with the same
affixes, we might reasonably hypothesize a regular rule deriving the
various forms from the stems, and this would, in our theory, be an
impossible rule. One such case is the common occurrence in English of
the following paradigm:

(17)	Xion	Xive/ory	Xor	*X
	incision	incisive	incisor	*incise
	gustation	gustatory		*gustate
	locomotion	locomotive	locomotor	*locomote
	malediction	maledictory		*maledict
	valediction	valedictory		*valedict
	illusion	illusory		*illude
	retribution	retributive/ory		*retribute
	emulsion	emulsive		*emulse
	revulsion	revulsive		*revulse/*revél

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Because of the number of cases, it is not terribly convincing to claim that they are all accidental, arising from the loss of the stem as an independently occurring item at some time after the formation of all the derivatives. This sort of thing can happen sporadically, but why should it happen so many times with this one paradigm? Furthermore, there is evidence that some of the derivatives, at least, entered the language at a time when the stem was not an independently occurring word. Such a case is not subject to the accidental gap explanation. It would appear, then, that at least some of the above constitute direct counterexamples to our theory.

Happily, this is not quite true. Martin (1972) has argued, for reasons which are completely extraneous to ours, that in the above paradigm the forms Xive, Xory, Xor, are based on the form Xion. Martin's main evidence is that one rarely finds any of the former in stems where one doesn't find the latter, though the reverse is not true, i.e. the number of words of the form Xion far outnumbers the total number of words in all the other forms combined.[16] This distribution only makes sense if the forms Xive, Xory, Xor are derived from the forms Xion. Secondly, when X does occur as an independent verb, and the semantics of X and Xion do not correspond exactly, the meaning of Xive, etc. always corresponds to that of Xion. Martin's example is the set communicate, communication, communicative. The verb has as one of its meanings "to receive the sacrament of Communion." The noun has no corresponding meaning, and neither does the adjective. A similar example is the set induce, induction, inductive. In one of its senses, the noun denotes a type of reasoning. The adjective has a corresponding sense, but the verb does not. There are many more such sets. They can be nicely

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accounted for if one of the adjective and noun is derived from the other. In light of the general distribution of the two, i.e. more nouns than adjectives, the noun seems the better choice. In any case, to choose the verb, or the "stem" as the base is to give up any hope of accounting for these facts. The third piece of evidence is historical. In all the cases Martin has been able to find documentation for in the OED, the -ion form entered the language before the -ive form. Exploit-ation is found earlier than exploitative, for example. This sort of fact can only be made sense of if the -ive form is derived from the -ion form. Furthermore, the derivation must be conceived of as the addition of a newly coined word to the dictionary, something which is possible only in the sort of theory outlined in this work.

Thus we find that the seeming counterevidence to our theory is rather evidence for it. The distribution, meaning, and history of forms Xive, Xory, Xor, Xion can only be made sense of if the first three are derived from the last in the manner described by our hypothesis of word-based word-formation. Though admittedly no Devil's Advocate with re-gard to the matter at hand, I have not found any set of data similar to (17), but not susceptible to conversion. In any case, it is clear that there is a certain sort of data which would constitute a counter-example to the claim put forth by our theory. Merely being able to determine what such data would look like demonstrates that our theory has merit as a theory. Insofar as no such data has been brought forward, and insofar as the theory sheds some light on the material which does not contradict it, it has some empirical merit.

The examples discussed in this section show another thing as well. When we speak of a word formed from another word, the simplest case will be that in which the former actually contains the latter. So[[farm]er]

contains [farm]. But this will not always be true. We have evidence
that _aggressive_ is formed from _aggression_, yet it does not contain it, on
the surface at least. One never finds words of the form *Xionive. The
notion "one word formed from another" must therefore be more abstract
than mere surface contatenation. This should be kept in mind.

2.2.6. Word-Structure.

A major fault of the theory so far delineated is that it only
deals with one of the areas which are considered to be the domain of
morphology. We have restricted the discussion to word-formation, and
disregarded the structure of already existing words. Almost all words
have morphological structure. This fact can be ascertained from the
fact that the phonology must have access to both bracketing and bound-
aries, both of which are morphological matters. Bracketing we have
discussed, and boundaries are morphologically determined to the extent
that they occur between morphemes, which they most always do. Now, from
what we have said so far, it is perfectly possible that only the words a
speaker actually makes up on his own will have morphological structure,
and that all the other words he knows, those he learns from hearing them,
which presumably comprise the great majority of the words the speaker
knows, have no structure. Another possibility is that the rules which
determine the morphological structure of the words which a speaker does
not actually make up are completely other than and different from the
words used to make up new words, and that the two sorts of rules just
happen to produce structures of the same sort. This is possible, but
highly unlikely. We have already argued that it is reasonable to
separate the rules for making up new words from those for analyzing
existing words, because of the general fact that already existing words

tend to be peculiar, and resistant to any system which derives their properties by general rule. This fact precludes our accounting for the disimilarities between word-formation and word-analysis in the most obvious fashion, that is by saying they are exactly the same thing. The two matters are the same, and yet different. It would be nice if the rules governing them also had this characteristic.

We have already seen that back-formation must be a sort of un-raveling of WFRs and other morphological rules (rules of allomorphy), that an individual back-formation can best be viewed as the answer to the question "What word could this one have been formed from by a regular rule?" A similar account of word-structure is perfectly plausible. It would also meet the same and yet different requirement. The differ-ence is that while the rules, as rules of word-formation are rules for generating forms, the same rules as rules of word-analysis, can be viewed as redundancy rules. They can be used to segment a word into morphological constituents, though the word may not be strictly generable from these constituents. The general strategy is to take a word, and by asking the question, "How could this word have been formd?" assign a structure to it. Because words are formed from words, the generaly strategy is to look for an analysis which results in a word plus a number of affixes. In such cases, e.g., if we attempt to analyze the word _information_ and arrive at the constituents _inform+at+ion_, an analysis which we deem satisfactory, since _inform_ is a word, another device will take over, which will compute the _distance_ (cf. Vergnaud, 1973) between the two words as a function of the number of features the two share (disregarding the features which are perfectly in accord with the WFR). So, for _information_, we will disregard the features which are in accord with those that the rule for _At+ion_ would assign to it,

i.e. , that it is a deverbal nominal of some abstract nature, and compare the rest of its meaning and syntactic behavior with that of _inform_. As happens, there are great dissimilarities between the two, and so they will be determined to be quite distant. All of this is of course very crude. However the general model has been used with success as the morphological component of natural language analyzing machines.

In cases where the general strategy does not result in a base which is a word, we do not worry about related items and distance. All that we can extract from it is the information a WFR gives us which is independent of the base. The word _possible_, for example, though it can be segmented into _poss+ible_, with the latter being an affix, has no lexical base. The rule of _+able_ tells us that it is an adjective, or likely to be one, and that it is (not terribly, because of the fact that the rule is not productive) liable to mean "capable of being Xed". Since we have no X, this is all we can know about this word from its structure. This strikes me as more or less in accord with what we do know about such a word. In fact, it seems that this system handles such words in a much more satisfactory manner than others, which, because they do not treat word-analysis in this derivative fashion, can not capture the fact that words such as this are peculiar in that one knows something about them from their structure, but not much.

Notice how our system will handle the two different types of berries discussed in 2.1. A word like _cranberry_ will be treated in an exactly parallel fashion to _possible_. We can, thus, in our system, account for what we know about a "partially motivated" form. Since we know what _berry_ is likely to mean, we have some idea as to a possible meaning for _cranberry_. However, since _cran_ occurs nowhere in our system of rules and words, we have no way to even guess at the complete meaning

of the entire word. Blueberry is segmentable into otherwise occurring parts. However, whatever parts of the meaning of blueberry are not attributable to the berry compound are shared with other meanings of blue are very few. Blueberry will thus be very distant from blue, which is I think the correct view of the manner and closeness of the relation between the two. Note that we do not run into any problems with regard to blue as a partially meaningful element in blueberry. We are concerned with the meaning of the entire word. The fact that blue occurs as an independent word is of interest, and demands that we compare that word with blueberry, not however with the morpheme blue which is part of that word. This is not a sophistic point.[17]

Viewing word-analysis as a backwards sort of word-formation thus does have virtues apart from its nice compatibility with our system. It allows us to account for what we know from general principles, and separate this from what is either not included in or counter to such general principles.

I will have a little more to say about the peculiarities of word-analysis in later sections. However, the general position taken here will not be changed.

One artificiality of the discussion of previous sections can now be removed. When we speak of a word-formation rule, we can now legitimately refer to all instances of the rule, whether it be used for word-formation, or used for word-analysis, as instances of one thing.

FOOTNOTES

[1]The idea of fully motivated must not be confused with that of fully meaningful. As Culicover has shown (1970), some signs are inherently unspecified in such a way as to cause any utterance containing them to be highly, perhaps infinitely, ambiguous. Thus the sentence "One more can of beer, and I'm leaving" (Culicover, ibid.), is ambiguous. "What about that can of beer?" we ask. "Anything" is the reply. The sign one more can of beer is not fully meaningful, but then neither is the entire sentence. It is probable that all linguistic entities are not fully meaningful, in this sense, all sentences are ambiguous, hence the poetic function. Instances such as these, however, differ fundamentally from those where two items, A and B, are concatenated, and the concatenation does not mean A+ B+ infinite ambiguity, but rather A+B+C, i.e. instances where some specific, isolable, constant part of the meaning of a concatenation cannot be derived from that of its parts. It is in these latter cases that motivation or arbitrariness, is relevant.

[2]In this work it is stated that the factors into which a string may be analyzed by a transformation may include "morphological material". As far as I can tell, this means inflectional and other "grammatical" morphological material.

[3]In the paper cited, Postal is arguing against the hypothesis. These

arguments are not very convincing to me, nor are those of Corum (1973).

[4]From this point on, word will be taken to mean word sans inflection, unless otherwise specified.

[5]One might think that the idiom, a unit which has long mystified linguists, is an arbitrary sign which occurs at a higher syntactic level than that of the word. Idioms, however, differ from words in the following curious manner. An idiom is generally ambiguous between its literal (sometimes nonsonsical) sense, and its arbitrary "idiomatic" sense. If John kicked the bucket, he either kicked some previously specified bucket (according to the literal, constructible sense of the sentence), or he died (according to the idiomatic interpretation). Complex words often do not enjoy such a consistent ambiguity between constructible and arbitrary senses. Take the word recital. If we were to attempt to construct its meaning out of that of its parts, as we did for the literal interpretation of John kicked the bucket, we might arrive at something like "the act(ion) of reciting", as an interpretation. But this word has no such interpretation. It has only an arbitrary sense, that of "public performance, generally of music, generally by one person." Thus the idiom differs from the complex word in being ambiguous between the literal and arbitrary.

[6]There is a basic dissimilarity between this device and the one I alluded to in the can of beer footnote (1). There, underspecification resulted in infinite ambiguity (infiniguity?), which is not the case here.

[7]Within an orthodox analysis of Relative Clauses, headway is never, strictly speaking the object of make in this sentence (cf. Vergraud, 1974).

[8]This argument does not really convince. The problem of idioms inter-
sects with this. A solution to that would, I think, elucidate this.

[9]Note that the latter, grammatical, categories, are not closed. They
may acquire new members, but they acquire them by a sort of drift. It
has often been noted that a word like near is an adjective on its way
to becoming a preposition. In other languages, prepositions are some-
times traceable back to nominal forms. Nor does drift affect only non-
lexical categories. The noun fun is on its way to being an adjective:

> That's no/not fun

The reverse course is being traversed by good:

> That's no/not good

A complete theory of language must account for this sort of thing, but,
because it involves existing words, and the changes they go through, I
think we can safely exclude it from the domain of morphology, as here
defined.

[10]Words which denote very important things tend to be monomorphemic.

[11]This operation usually consists of the addition of some affix. It can
however, be nul, and it may be more subtle.

[12]The theory is not specifically designed to avoid the problem of
meaningless morphemes. As stressed above, words are formed from words,
not from "meaningless elements."

[13]Brame lists some problematic forms from Maltese, which though they must
be derived cyclically in his system, intuitively at least, do not meet
the Strong Hypothesis. u is the plural subject marker in the following
forms:

nisorbu   "we drink"

titilfu   "you pl. drink"

In Brame's system they must be derived from the following:

$$[[ni+srob]+u]$$
$$[[ti+tlif]+u]$$

There is a cycle on u̲.

[14] Within the system of Halle (1973b) the only rule which actually has the effect of subordinating stress is one which stresses a [1 stress]. The Detail Rule is such a rule. It stressed the last [1 stress] of a word, unless that stress falls on the last vowel, in which case it stresses the penult. Last wins, unless it is last.

[15] I have not stressed the import of this central claim. It means essentially that a rule cannot perform different operations on different stems.

[16] Exceptions listed by Martin are the following:

Xive *Xion: conductive, divorcive, purposive, deducive, redressive, abusive, amusive, conflictive, combative, sportive, contrastive, appointive, effective, talkative, calmative, comparative, figurative.

The test was of course not administered under any rigorous conditions.

[17] For more on the treatment of partially motivated forms within a system like the one proposed, see Jackendoff (ms.

CHAPTER 3

PRODUCTIVITY

3.1. Preliminaries.

This chapter is devoted to a discussion of the words productive and productivity. The terms are widely used in studies of derivational morphology, and there is obviously some intuition behind this widespread usage. One is hard put, though, to find an even vague definition, or much discussion of what might be properties of a productive morphological process, as opposed to those of a non-productive one. The bulk of this chapter will be devoted to a rather detailed discussion of the two deadjectival nominal suffixes #ness and +ity in a particular environment: attached to adjectives of the form Xous (curious, curiousness, curiosity). From the behaviour of these two affixes in this same environment, we will try to extract generalizations about comparative productivity. First, however, some preliminaries.

It is sometimes claimed that productivity is a matter which never enters into the study of syntax. This is not quite true. Compare the two rules Dative Movement and Passive. It is often observed in the case of the former, that the predicates which permit it, while members of a more or less well-defined semantic class, are not all the members of that class, but rather some arbitrary selection of them. On the other hand, while there are some transitive verbs which do not allow Passive, the exceptions seem to be principled. One seems justified,

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therefore, in saying that Passive is more productive than Dative Movement. A more detailed discussion of this question is presented in Oehrle (forthcoming). Obviously, there are some syntactic operations which are immune to questions of productivity. Such rules as Subject Auxiliary Inversion, which are not optional in any sense of the term, cannot ever be thought of in terms of productivity.

A first attempt to articulate one's intuitions about the meaning and utility of the term productivity in morphology, generally identifies productivity with sheer number. If we want to compare the productivity of two WFRs we may simply take lists of the words formed by the respective processes, and count them. The longer the list, the more productive the WFR. One problem with this method, however, is that it does not take into account that there are restrictions on the sorts of words one may use as the base of a rule. Thus, +ity and #ness both attach to adjectives, but the latter is restricted to latinate adjectives. To be fair, then, we should count up the number of words that we feel could occur as the output of a given rule, which we can do by counting the number of possible bases for the rule, count up the number of actually occurring words formed by that rule, take the ratio of the two, and compare this with the same ratio for another WFR. In fact, by this method, we could arrive at a simple index of productivity for every WFR, equal to the ratio of possible to actually listed words.

Two problems face this simple method. The first is not crucial, but often overlooked in more cursory discussions of productivity (not, however, in some traditional accounts). It is simply that one cannot speak absolutely about the productivity of an affix. Rather, one must ask how productive an affix is when attached to words of a particular mor-

phological class. Thus, compare the two affixes #ness and +ity, attached to two distinct morphological classes of base adjectives, those ending in ive (perceptive), and those ending in ile (servile). The simple list tells us that #ness is more productive than +ity with the former class of bases (W lists approximately 5 times the number of words of the form Xiveness as those of the form Xivity). However, this result does not carry over to the second class of bases. The number of words of the form Xility overwhelmingly exceeds that of those of the form Xileness. We seem to have a contradiction here. In the one case, one is more productive, in the other case the other is. What we learn from this and many other examples is that there is no simple way to say one affix is more productive than another absolutely. Rather, one must always take into account the morphology of the base. There is some intuitive sense of general productivity of an affix. #ness, on the whole, seems to be more productive than +ity. However, I know of no way of express- ing this intuition, without losing, at the same time, the intuition that +ity is more productive in certain morphological environments. For the moment, we will not consider the question of general productivity, and only consider productivity in a particular morphological environment, with a particular morphologically determined class of stems.

The second problem with the simple mechanical method of computing productivity is that it depends very crucially on the idea that every time we make up a new word,it is entered in a list. If not all new words are listed, then we have no effective procedure for computing the ratio of existing to possible words, even when we restrict ourselves to a particular morphologically determined class of bases. Consider, for example, the adverb forming suffix -ly, which is far and away the

most productive suffix in the language. $\underline{W}$ lists 34 pages of entries of the form $\underline{Xly}$, more than are listed for any other affix, and yet when we glance at this list, we feel somehow that it is superfluous With such a productive affix as this, we don't need a list, we just take an adjective, almost any English adjective (there are a few principled exceptions, and a few arbitrary ones) tack on $\underline{-ly}$, and we have an adverb. We will return to this question shortly. The problem for the present is that we have no independent criterion for deciding whether a new word is entered in a list. We have merely declared that it must be.

One more point must be made before proceeding. Speakers of a language have intuitions about productivity. It is possible to take a single word of English, for example, and by applying two rival, equally applicable WFRs to it, produce two hitherto non-existent English words, both possible words by all formal criteria. We can submit these two words to speakers of English, and ask them which one is more likely to be an English word. We get consistent results: different speakers will consistently choose one of the words, and tell you that it is more likely to be an English word. Thus, by whatever method they arrive at these results, they do have intuitions, and, more importantly, intuitions about individual cases. It is this intuition that we are interested in tapping when we look at questions of comparative productivity, the intuition that encodes the notion "more likely to be an English word."

We will give an example. Consider again the two suffixes $\underline{\#ness}$ and $\underline{+ity}$, and their attachment to bases of the form $\underline{Xive}$. We have already noted that there are many more words of the form $\underline{Xiveness}$ than

there are words of the form Xivity. We will take one word out of the class Xive : perceptive. From it, we will form the two words perceptiveness and perceptivity, both of whicn are possible English words. When presented with the two words, speakers of English consistently choose perceptiveness as more likely to be a word of their language. This test gives the same result as the list test, yet, quite clearly, the speakers have had no recourse to lists. Direct access is being had to an intuition. Of course, we are not interested merely in this intuition. Rather we would like to explore some of its more objective correlates. What are the observable linguistic correlates of such an intuition? This chapter is largely devoted to exploring some of these correlates in one particular case.

3.2. #ness and +ity.

We will take as our example the two suffixes #ness and +ity which we have been discussing. As the base we will choose the class of adjectives of the form Xous. The results of our simple intuitive test are quite clear here. Taking the adj. dubious and forming the words dubiousness and dubiety we find that dubiousness is the preferred form. We will assume, therefore, that #ness is the more productive affix in this case. Why?

3.2.1. Gaps.

The simplest difference between the two cases is the difference in the number of gaps we find in lists of the two forms. This is our simple list test, and it is not very revealing. Simply, for every adjective of the form Xous, we can form Xousness, while this is not true of the corresponding forms in +ity:

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(1) mordacious   mordacity   mordaciousness

     fallacious   *fallacity   fallaciousness

     fabulous   ?fabulosity   fabulousness

     vicarious   ?vicariosity   vicariousness

     serious   ?seriosity   seriousness

3.2.2 Semantics.

An important difference is the one between the semantics of the
two classes in question. The semantics of Xousness is coherent. By this
term we mean roughly that if we assign to the WFR #ness a general mean-
ing, then the words of the form Xousness all adhere closely to this
meaning. This will become clearer as we proceed.

All nominals of the form Xousness have the following three para-
phrases.[1]

A. The fact of X's being Y.

B. The extent to which X is Y.

C. The quality or state of being Y.

Both A and B are possible paraphrases of the nominal in the following
sentence:

(2) His coarseness surprised me.

     A) The fact that he was coarse surprised me.

     B) The extent to which he was coarse surprised me.

Sentence (3) has the reading C:

(3) Coarseness is not a virtue.

     C) The state or quality of being coarse is not a virtue.

The same will be true of all nominals of the form Xousness. All the
readings A, B, and C will be possible in the proper setting.

The semantics of -ity derivatives is not no coherent. While many of them do have all three readings, they often have others as well; they are often used in a technical sense, sometimes they can be concrete or count nouns, and they may appear in particular idiomatic contexts more readily. There follow a number of examples:

(4) Various-Variety.

    A) The variety of fish in the pond surprised me.

    B) "   "   "

    C) Variety is not always pleasing.

Other. How many varieties of fish are there in the pond?

(5) Notorious-Notoriety.

    A. B.) His notoriety surprised me.

    C.)   Notoriety is not a virtue.

Other. The dog's mordacity won him great notoriety/? Notoriousness.

(6) Curious-Curiosity.

    A. B.) His curiosity surprised me.

    C.)   Curiosity is sometimes a virtue.

Other. They admired his dress, but only as a curiosity.

(7) Porous-Porosity.

    A. B.) The porosity of the material surprised me.

    C.)   Porosity is often a highly desired quality.

Other. The high porosity/*porousness of the clay made it unfit

       for use..

(8) Monstrous-Monstrosity.

    A. B.) The monstrosity of what he had done suddenly dawned

       upon me.

    C.)   ? Monstrosity is not a pleasant quality.

Other. What a monstrosity!

(9) <u>Continuous-Continuity</u>.

Other. This story lacks continuity.

(10) <u>Discontinous-Discontinuity</u>.

Other. There are many discontinuities in your story.

The contrast between <u>continuity</u> and <u>discontinuity</u> is especially

interesting. The adjectives only differ in the presence or absence

of the negative prefix.   Nonetheless, the difference between the

senses of the two nominals is considerable. Note that this is not the

case with the <u>#ness</u> nominals <u>continuousness</u> and <u>discontinuousness</u>.

They preserve the same distinction that the adjectives display.

We see quite clearly that the semantics and syntax of the <u>#ness</u>

forms is both coherent and transparent. Given the adjective, we can

always predict the meaning and possible syntactic frames of the <u>#ness</u>

nominal which corresponds to it. Not so with the <u>+ity</u> nominals.

Coherent semantics thus seems to be a characteristic attribute of a

productive suffix.

3.2.3. <u>Phonology</u>.

The two suffixes <u>#ness</u> and <u>+ity</u> differ in the manner of their attach-

ment.   <u>#ness</u> attaches with a word boundary, represented by #, while

<u>+ity</u> attaches with a morpheme boundary, represented by +. The differ-

ence between the two types of boundaries will be discussed from a

morphological point of view in the next chapter. For the moment we

will note that the fact that <u>#ness</u> has a # boundary results in the

segmental phonology of the stem and its stress being unchanged in the

derived form. (<u>porous-porousness</u>, <u>incongruous-incongruousness</u>). Not

so with <u>+ity</u>. Because it is attached with a + boundary, the stress

shifts to the syllable preceding the affix, which is then laxed by the

rule of TriSyllabic Laxing: mendacious-mendacity.[2]

The mere fact that the boundary is weaker is not so bad. There is
no direct correlation between boundary strength and productivity,
though boundaries do tend to weaken in individual fixed forms (cf.
Shelvador forthcoming). What is more important is the fact that there
is often (and this often is the real heart of the matter) a loss of
the ous which is the final morpheme of the stem. The mechanism of
this loss will be elaborated in Chapter 5. For the moment we will
assume that ous sometimes drops before +ity.

Again, the mere loss of a suffix before another is not so
damning. The morpheme +ate, for example, drops quite regularly before
certain suffixes (this is discussed in some detail in Chapter 5). How-
ever, in the case of +ate, the dropping of the morpheme is quite regular.
This is not so with ous before +ity. In most instances, there is no way
to predict from any general property of the word in question whether the
ous will drop or not. Thus, contrast the +ity derivatives of curious and
various. Phonologically, the two stems are almost identical, however,
their derivatives are curiosity and variety respectively. In the one case
ous has dropped, in the other it has not. Similarly credulous and
credulity contrast with fabulous and fabulosity. There is no possible
general phonological reason for this contrast. It is rather determined
by the individual words in question.

Formally, we can represent the situation by positing a rule, R1:

R1   ous → ∅   /__+ity

At the most abstract level, credulity and fabulosity will have similar
representations:

credulous+ity   fabulous+ity

The former, but not the latter, will be subject to R1. The next step in the derivation of both will be the following:

credul+ity   fabulous+ity

Both words will then go through the phonology in a regular manner, to give the correct surface representations.

Practically, the reflex of R1, for which, as we have seen, individual stems must be marked, is that the +ity derivaties of ous adjectives must be enumerated in a list. We must know, in every case, whether R1 applies, and the answer to this question is a lexical property, a property of individual words. Nor do stems exhibit free variation. We always say credulity, never *credulosity.

We see that we must have access to some list. The question which now arises is what list. The most obvious candidate is the lexicon. We have taken the lexicon to be the list of actually occurring words of the language. The lexicon is more conventionally viewed however, as the list of all the exceptional items of a grammar (cf. Chomsky, 1965), and it is assumed that these exceptional items will by and large be (derivational) words. We will say, then, that only exceptional words, words which are arbitrary in at least one of their various features, will be entered in the lexicon. From this definition it follows that the +ity derivatives of Xous adjectives must be listed in the lexicon.

Turning to the #ness derivatives of ous adjectives, we do not find a parallel situation. There is no counterpart to R1; stems do not have to be marked in any way, and the entire derivational process can be described without reference to a lexicon.

It is this contrast, I think, which lies at the root of the difference in the productivity of the two processes we are discussing. It

explains both of the other differences we have already noted, as well as another, more peculiar one, which we will turn to shortly.

3.2.4. Gaps Again.

First, let us look at the phenomenon of gaps in the paradigm. As I have just noted, +ity nominals of ous stems must necessarily be listed in the lexicon, for they must be marked for the rule R1. This does not necessarily mean that the stems must also be marked for whether they undergo the derivational rule itself, i.e. the WFR Xous]$_A$ity]$_N$. It is perfectly possible, a priori, that all possible Xous]ity patterns exist, but that for any given Xous there is also an arbitrary mark for whether or not it is to undergo the rule R1. Formally, there is no necessary connection between the fact that there is an arbitrary mark for R1, and the fact that not all possible ity derivatives of Xous adjectives exist. However, there is strong evidence that the two are indeed linked.

If we are correct that it is the arbitrariness of the marking which causes the WFR itself to be less productive, then if there is a subset of Xous adjectives where the application of R1 is not an arbitrary property of individual lexical items, but is governed by something higher, then this subclass should be more productive. This conjecture is borne out by an examination of the comparative productivity of the rule +ity with the two classes Xacious and Xulous (vivacious, fabulous). In the case of the first, the application of the rule R1 (ous→∅) is governed not by individual stems, but rather by the morphemes aci, oci, eci.

	(11)	Xacious	Xacity	*Xaciosity
		mordacious	mordacity	*mordaciosity

Xocious	Xocity	*Xociosity
precocious	precocity	*precociosity
Xecious	*Xecity	Xeciosity
specious	*specity	speciosity

The factor that governs the application of the rule R1 is thus not the individual stem, but rather the vowel that precedes cious.[3]

In the case of ulous, on the other hand, there is no such general pattern:

(12)	credulous	credulity	*credulosity
	nebulous	nebulosity	*nebulity

Now, we are claiming that the different nature of the conditioning factor of the rule R1 conditions directly the productivity of the WFR ity. When the rule is lexically governed, it will be less productive than when it is governed by some general factor. This claim can now be tested directly. For convenience, we will consider only the forms Xecious and Xulous. All the adjectives of these forms and their corresponding nominals in +ity listed in W are reproduced in the following tables.

TABLE 1

bibacious	*
efficacious	*
inefficarious	*
perspicacious	perspicacity
pervicacious	pervicacity
procacious	procacity
edacious	edacity
mendacious	mendacity
mordacious	mordacity
audacious	audacity
sagacious	sagacity
fugacious	fugacity
salacious	salacity
tenacious	tenacity
fumacious	*
contumacious	*
pugnacious	pugnacity
pertinacious	pertinacity

minacious	minacity
capacious	capacity
rapacious	rapacity
spacious	*
feracious	feracity
veracious	veracity
gracious	*
voracious	voracity
vivacious	vivacity
sequacious	*
loquacious	loquacity

TABLE 2

fabulous	fabulosity
sebulous	*
nebulous	nebulosity
noctambulous	*
bibulous	*
tubulous	*
miraculous	*
oraculous	*
flocculous	*
pediculous	*
ridiculous	*
folliculous	*
vermiculous	*
ventriculous	*
meticulous	*
calculous	*
loculous	*
monoculous	*
tuberculous	*
flosculous	*
credulous	credulity
incredulous	incredulity
sedulous	sedulity
acidulous	*
rigidulous	*
stridulous	*
glandulous	*
pondulous	*
undulous	*
nodulous	*
scrofulous	*
solidungulous	*
orgulous	*
cellulous	cellulosity
ramulous	*
emulous	*
tremulous	*
cemulous	*
granulous	*
crapulous	*
populous	*

scrupulous	scrupulosity
unscrupulous	*
scaberulous	*
querulous	*
torulous	*
garulous	garrulity
patulous	*
edentulous	*
tortulous	*
fistulous	*
pustulous	*

The data is very clearly in accord with our hypothesis. There are 29 adjectives of the form Xacious. All but 8 of these have corresponding nominals of the form Xacity. There are 52 adjectives of the form Xulous. Only 8 of these have corresponding nominals. We see that when there is a condition on the application of R1 which is not lexically determined, there are very few gaps in the +ity paradigm. Where we have no such general condition, we have, on the contrary, many gaps, and in fact, very few actually occurring nominals.

The connection between lexical marking and lack of productivity is perhaps better understood when we look at the matter from a broader, social, perspective. A speaker confronted with an adjective of the form Xacious, from which he wishes to form a nominal in +ity, will know that the nominal must be Xacity, and will, therefore, not hesitate to use it. When faced, however, with an adjective in Xulous, he is in a quandary. Which is correct, Xulity or Xulosity? He doesn't know, though he does know that one of the forms is correct, that there is no free variation. In order to avoid the stigma of using the wrong word, he simply uses neither, and falls back on the trusty Xness form, where he knows that though he is surely revealing the paucity of his vocabulary, he cannot make a mistake. Thus, on very general social grounds, we can see a direct connection between the condition on R1 and

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the mere use of the form in +ity. When the former is more general, the

latter is more likely to be used. In the case of #ness, there is no rule

corresponding to R1 at all. This makes R1 the most productive of all,

for there is no question to be posed.

3.2.5. Semantics Again.

The connection between lexical marking for R1 and semantic

coherence is a little less direct. We have assumed all along that the

mere fact that a word is in the lexicon, that it persists, is the main

root of its semantic wanderings. We admit that the +ity derivatives of

∅ most adjectives of the form Xous must be marked in the lexicon for

whether they undergo R1 or not. This is not a semantic matter. How-

ever it is clear that now the first condition for semantic drift is

fulfilled, entry into the lexicon. With the small subclass of Xous

adjectives where the marking is not arbitrary, those of the form

Xacious, there is no need to enter individual derivatives in the

lexicon, and hence no reason to expect them to drift. This expectation

is borne out by the data. A short perusal of the nominals in Table 1

shows that they are semantically coherent, and in accord with the gen-

eral pattern for deadjectival abstract nominals.

Semantic incoherence of a pattern, though it is a secondary

characteristic, attributable to more concrete arbitrariness, does, I

think, itself contribute to lack of productivity of a WFR. For, once a

class's semantics has become incoherent, we run into the same problem

concerning the meaning of coinages as we did at first concerning their

form. Assuming, of course, that the meaning of an affix is connected

somehow with its distribution, i.e. with its meaning in individual forms,

our ability to predict the meaning of a new form will be impaired by the

arbitrary meanings of the existing forms.

3.3. Blocking.

We now come to a series of most curious gaps in the +ity

paradigm, whose explanation both depends on +ity being a process which

involves the lexicon, as we have argued, and widens our understanding of

how interaction with the lexicon is a factor in the determination of

productivity.

For many adjectives of the form Xous there exist a semantically

related abstract nominal in the same stem. Sometimes the stem of the ous

form is itself an abstract nominal:

(13)	melodious	melody
	felonious	felony
	glorious	glory
	hazardous	hazard
	outrageous	outrage
	courageous	courage
	scandalous	scandal
	troublous	trouble
	libelous	libel
	famous	fame
	venomous	venom
	mysterious	mystery

Sometimes the relation is not so clear; it is not easy to decide which

form is derived from which, or even whether one form is necessarily

derived from the other. Consider first the pairs in:

(14)	synonymy	synonymous
	monotony	monotonous
	larceny	larcenous
	homophony	homophonous
	mutiny	mutinous
	felicity	felecitous

It is perhaps possible to argue from such correspondences as symphony/

symphonious that the final y-final nous in (14) are the base of the

adjectives, and that there is a lexically governed rule of y deletion at

work here. But this is not a very powerful argument. More difficult to

relate are the following:

(15) quotation   quotatious
      disputation   disputatious
      repetition   repetitious
      contradiction contradictious
      caution   cautious
      pretention   pretentious
      deception   deceptious
      superstitution superstition

Plainly, the _ion_ form cannot always be derived from the _ous_ form, because there is good evidence that the _ion_ form in cases such as deception and _flirtation_ are derived from the verb. However, it is not easy to construct a convincing case for the opposite derivation, since there are many _ious_ forms which do not have related _ion_ forms:

(16) facetious
      factitious
      captious
      bumptious
      scrumptious
      pervious

All this is merely to show that it is possible to have two items in the same stem without necessarily claiming that one is derived from the other. The notion same stem is independently valid.

With this in mind, note the following general fact:

Blocking: When there is, in a given stem, an adjective of the form Xous and a semantically related abstract nominal in the same stem, it is impossible to form the +ity nominal derived from Xous. This is not true of the corresponding Xness nominal.

Blocking does not occur when there is no corresponding nominal. The contrast is exemplified in (17).

(17)

Xous	Nominal	+ity	#ness
various	*	variety	variousness
curious	*	curiosity	curiosity
glorious	glory	*glosiosity	gloriousness
furious	fury	*furiosity	furiousness
specious	*	speciosity	speciousness
precious	*	preciosity	preciousness
gracious	grace	*graciosity	graciousness
spacious	space	*spaciosity	spaciousness
tenacious	*	tenacity	tenaciousness
fallacious	fallacy	*fallacity	fallaciousness
acrimonious	acrimony	*acrimoniosity	acrimoniousness
impecunious	*	impecuniosity	impecuniousness
laborious	labor	*laboriosity	laboriousness
bilious	bile	*biliosity	biliousness
pious	*	piety	piousness

The distribution of (17) is neither Xous and nominal or Xous and +ity,

never both nominal and +ity, but always #ness. I know of no except-

ions to this general distribution in the entire list of Xous adjectives

in W (circa 1500). That is, I know of no case where we find both the

abstract nominal and the +ity form. Sometimes we find either, but this

can be attributed to the already noted fact of gaps in the +ity paradigm.

Sometimes we find a nominal and an +ity form, but in these cases the

nominal is not of the correct semantic class:

(18)

gaseous	gas	gaseity	gaseousness
globous	globe	globosity	globousness
instantaneous	instant	instantaneity	instantaneousness

We can see from (17) that there can be no plausible phonological reason

for this distribution. We are dealing with near minimal pairs:

tenacious/fallacious, furious/curious. The only factor to which we can

attribute the distribution is the absence or presence of the nominal

in each case.

How do we explain such a distribution? Remember that we have

evidence that the +ity derivatives of Xous adjectives must be listed

in the lexicon of English, on the assumption that the lexicon contains

all and only the exceptional words of the language, and none of the completely unexceptional ones. We may assume that the lexicon is arranged according to stems, and for each stem there is a slot for each canonical meaning, where canonical means derived by regular rules (we will say more about the semantics of WFRs in Ch. 4). Let us furthermore assume that for each stem there cannot be more than one item in each meaning slot. If the +ity nominals are entered in the lexicon, then when we make up such a form, we put it into the slot for abstract nominal for its stem. However, when there is already a nominal in the stem in question, then there is no room for the +ity nominal, it is blocked by the already occurring nominal. When there is no nominal in that stem, then we are free to insert the +ity form, though, as we have already noted, this will not always happen. Thus the mere fact that the +ity nominals must be listed accounts neatly for the distribution of most of the forms of (17).

What about the #ness forms, however? Why are they not blocked? The answer to this is straightforward: we have found no reason to list them. On the assumption that only words which are exceptional in some way must be entered in the lexicon, there is no reason to enter the #ness derivatives of Xous adjectives in the lexicon. The most productive classes never have to be listed. If the #ness forms are never listed, then they can never be blocked, and this is what we find. Nor will there be any sporadic gaps, since the concept of gap presupposes a list, and we have no list. Nor will they drift semantically, since semantic drift, on our account, itself presupposes that the item which drifts be listed in the lexicon.

3.4. Conclusion.

We have found there to be specific differences in the properties of two rival WFRs, one productive and one unproductive. These differences concern the semantics of the two, the existence of gaps in one, and the blocking of one in a particular circumstance. We have independent evidence that the output of the less productive WFR must be listed in the lexicon and we can derive all the differences between the two WFRs from this fact and the added assumption that the output of the productive WFR is not listed in the lexicon.

If this added assumption is correct, and, on the most likely definition of the lexicon as a list of exceptional words, it must be, then it is of great importance for the actual determination of productivity. Remember that at the beginning of this chapter we suggested a simple mechanical means of assigning productivity to (a morphological subcase of) a WFR. Calculate the number of possible items produced by the WFR by adding up the number of possible bases. Divide this number by the number of the actually occurring items. This ratio determines the actual productivity. But this procedure depended crucially on the assumption that all actually occurring items were listed. Since our explanation of the differences between the two suffixes +ity and #ness depends crucially on the contradiction of this assumption, our mechanical procedure cannot work. We are therefore left without a simple method for determining productivity.

FOOTNOTES

[1]It is not clear here that we are dealing with three separate readings rather than one ambiguous one. I lean towards the latter, but due to the present state of the art of semantics and perhaps my own incompetence, I will leave this very interesting question open.

[2]+ity is an unwieldy affix. SPE has it as i+ty. I will not embark upon any analysis here, but rather direct the curious reader to the relevant passages in SPE and Halle and Keyser (1971).

[3]What sort of conditioning factor is this? It seems phonological, but the quality of the vowel in such a position does not strike me as a natural phonological condition for a rule such as R1.

PREFACE TO PART 2

The first half of this work has been devoted to questions of what we want a theory of word-structure to do, and in the light of very general evidence, I have presented a general framework in which these questions can perhaps be dealt with in a more fruitful manner than in previous work. There now remains the task of constructing an explicit theory within this general framework. In particular, we must develop a clear and constrained notion of what a Word Formation Rule (WFR) is, and how it interacts with the rest of the grammar.

This will be done in Ch. 4, and the content of Ch. 5 follows from the treatment of WFRs, for, as WFRs are formulated in Ch. 4, their output is not the input to the phonology, but must undergo some adjustment, the nature of which is explicated in Ch. 5. In a broader sense, however, the particular treatment of WFRs elaborated in Ch. 4 is difficult to comprehend fully without simultaneous understanding of what follows (from) it. This simulataneity forces us into an expository quandary. It would have been confusing to interweave the two matters, and yet, prior presentation of either must puzzle the reader.

I have chosen to present the material on WFRs first, in the main body of the text, because it is more central. In an attempt to alleviate the ensuing puzzlement, I lit upon the device of presenting here in this preface a rough outline of the content of Ch. 5.

Ch. 5 contains material which is morphological in a slightly different sense of the term from the one we have been using so far.

Up to now, we have been concerned with morphology as a syntactic matter: how words are built up. But the word morphological is also part of the vocabulary of phonology.

Traditionally, there are two different kinds of phonological alternations. First, there are the alternations whose conditioning factors are totally phonetic (phonological). These alternations are the province of phonemics (with, in the American tradition, other addition strictures, such as bi-uniqueness, cf. Chomsky, 1964). Alternations which are at least partly conditioned by other factors are subsumed under the rubric of morphophonemics. This would include alternations which are restricted to certain syntactic classes, such as verb, those which have lexical exceptions, or are entirely lexical (governed by individual words), and those which take place either only in certain (classes of) morphemes, or only in the environment of certain(classes of) morphemes.

As we have noted already in Ch. 1, one of the major differences between Generative Phonology and earlier frameworks is that the former does not distinguish between phonemic and morphophonemic alternations (cf. Halle, 1962). Within Generative Phonology in its most general form, each morpheme (and phoneme) has a single underlying phonological form. The phonology is a, for the most part, arbitrarily extrinsically ordered set of rules which converts this underlying form into a surface phonetic form. This set includes rules of all the types mentioned, and they may be interspersed.

In the course of studying a small subpart of English phonology, I discovered that a very satisfying analysis could be obtained by isolating a certain small class of alternations, and ordering the rules which

specify these alternations before the rest of the phonology. This class could be defined as comprising those alternations which are restricted to specific morphemes and furthermore, take place only before specific morphemes. These are called rules of _allomorphy_. (One such rule was discussed in Ch. 2, that which takes _mit_ to _mis_, before certain suffixes). A further characteristic of these rules is that they map only onto the set of underlying phonemes of a language. For example, a rule of allomorphy of English could never aspirate a consonant, since aspirated consonants cannot be independently motivated at the underlying phonological level in English. It is obvious, that these rules are truly morphological, in the phonological sense of the word. They include, it should be stressed, only a subclass of the rules which are termed morphophonemic in earlier frameworks. They do not include syntactically or lexically governed rules, or even all morphologically governed rules, but rather only the most morphological of the latter.

Once we posit the separation of this class of rules from the phonology, and, more importantly, once we claim that they always precede the phonology, there immediately follow consequences for a theory of WFRs. The WFRs are basically the rules for concatenating morphemes, and the rules of allomorphy, we can claim, immediately follow them. Now, since the allomorphy rules adjust the shapes of concatenated morphemes, one can entirely remove operations of this sort from WFRs. In fact, every WFR can be formulated so as to be independent of the morphological composition of its base. The specification of the base of a WFR can thus be purely syntacticosemantic. Furthermore, the affix introduced by a WFR can have a single form, regardless of the morphology of the base, for, though the shape of the affix may differ,

depending on the morphological composition of the base of the WFR, this variation can be stated in the rules of allomorphy. Thus a WFR can perform a single operation on a uniform syntacticosemantic class of bases.

Allomorphy rules are a type of adjustment rules. They allow us to preserve the uniformity of word-structure. WFRs are morphological in the syntactic sense only. They do not depend on morphemes, but specify them. Allomorphy rules are morphological in the phonological sense of the word. They operate on, and are conditioned by, morphemes.

Once we admit allomorphy rules, we can admit another class of adjustment rules. These which we will call rules of truncation, change the shape of a morpheme in a more drastic manner, by deleting it. They serve the same purpose as allomorphy rules, with regard to WFRs, allowing us to preserve general statements. In the section on truncation in Ch. 5 there is considerable discussion of how this is accomplished.

One last point. It may strike the reader during the course of reading Ch. 4, that WFRs and adjustment rules can perform some of the same operations. Thus it might appear that in certain cases there is a choice of including some phonological process in a WFR, or relegating it to the adjustment rules. This is not so. The phonological operation of a WFR (generally an affix) is completely independent of the morphology of its base. Therefore, though a WFR may replace certain segments, it may not do so under morphological conditions, i.e. only in certain morphologically specified subclasses of the base. Conversely, adjustment rules must be morphologically restricted. There-

fore, though the two types of rules may in certain cases perform the same kind of operation, their domains are distinct. This point is perhaps a little prematurely made. It should be kept in mind, however, during the course of reading the second half of this work.

PART 2

A THEORY OF WORD-FORMATION

CHAPTER 4

WORD FORMATION RULES

Merely to say that words are formed from words is neither novel nor enlightening. To make the statement interesting, we must be able to make more precise claims about the nature of the rules which form words, their form, the conditions under which they operate, their relation to the rest of the grammar. The elaboration of such claims is the task of this chapter.

A basic assumption of this chapter is that WFRs are rules of the lexicon, and as such operate totally within the lexicon. They are totally separate from the other rules of the grammar, though not from the other components of the grammar. A WFR may make reference to syntactic and semantic and phonological properties of words, but not to syntactic, semantic, or phonological rules, nor to those properties of words which are directly associated with these rules, i.e. such properties as Rule Features, syntactic or phonological. This is not a strange assumption. Though it is not controversial to allow a phonological rule to refer to the fact that a certain item is a verb, for example, one does not allow such a rule to refer to the fact that it is a verb which doesn't passivize. We will assume that a WFR, as well as not referring to other types of rules, and related matters, cannot introduce rule conditioned properties. This assumption is stronger than the last, and it will be discussed below. It is tied in with

an earlier assumption that a WFR and its associated phonological operation are one and simultaneous and with the assumption which followed from that one, that words are entered in the lexicon in a fully concrete, specified form. A related assumption is that WFRs are different from other rules in the manner and occasion of their use. The syntactic and phonological rules are necessary and essential to the generation of every sentence. It is impossible to speak without using the syntax and the phonology. Not so the rules of the morphology. It is the dictionary entries themselves which are the input to the syntax and phonology, and the WFRs are merely rules for adding to, and, derivatively, analyzing, these entries. Thus it is very easy to speak a sentence without having any recourse to these rules. They are not "on line." Though this fact does not necessarily mean that these rules will differ from others in their formal properties, it does suggest that they are quite separate from them.

For every WFR we must know two basic sorts of things. Firstly, we must know what sort of information a WFR can have access to, and how it has access to this information. It is obvious that every WFR must have access to its base, i.e. the class of words on which it operates, and all the information contained in the base. It is possible that a WFR can have access to other information than that contained in the base. It might have access to its own output, or to forms related to the base. However, access to anything other than the base calls for rules of a much more powerful sort than we would prefer to have. We will therefore operate on the assumption that a WFR can have access only to information contained in its own base.

The second sort of thing one must know about is the sorts of operations a WFR performs, the sorts of changes it can make, and the

formal mechanism by which these changes can best be stated in a general way.

Perpendicular to this classification of phenomena there lies another. There are different types of information in a grammar; syntactic, semantic, phonological, morphological. Words contain information of all these types, and WFRs, as rules for making up new words, most likely all contain information of each of these types. This chapter will be organized along this latter axis. First we will discuss the syntax and semantics of WFRs, then the morphology, then the phonology. Under each of these headings we will discuss first phenomena relating to the base, then phenomena relating to the output and operation of the rules. Finally, we will attempt to synthesize everything in a grand description of the general properties of WFRs.

4.1. Syntax and Semantics.

4.1.1. The Base.

The base is always specified syntactically. So, for example, the rule which attaches the suffix #ness (redness, porousness) operates only on adjectives. Finer syntactic distinctions than the merely categorial are possible. Matters of subcategorization are commonly referred to. Thus, the suffix +ee (cf. Siegel, 1971) attaches only to transitive x verbs, (employee, payee, *travelee). WFRs may also be sensitive to the selectional restrictions of the base. So, this same suffix is further restricted to verbs which allow animate objects, or indirect objects (*tearee). More detailed, and a little more exotic, is the constraint on the base for the prefix re#, which forms words such as repaint, rewire[1] and which has been studied in some detail by Williams (1973). This prefix attaches only to verbs whose meanings

imply a change of state, generally in the object of the verb. Compare
the following sentences:

1. John punched Bill.

*2. John repunched Bill.

3. John punched the holes in the paper.

4. John repunched the holes in the paper.

The grammaticality of (4), and the corresponding ungrammaticality of
(2) can be accounted for by the above-mentioned constraint on the
meaning of the base. The verb punch of (1) does not imply any change
of state in its object. I may punch someone without my action having
any effect on the person. No change of state, therefore no re#,
therefore *(2). The verb punch of (3) on the other hand does imply
a change of state in its object. If I punch a hole in something, the
object punched has been tangibly changed, therefore re# is possible
with this verb, and therefore (4) is a good sentence.

It appears to be a general fact that the syntactic and semantic
conditions on the base of a WFR are those of category, subcategory,
selection, and lexically governed implication and presupposition.
These are the same sorts of restriction that are relevant to Lexical
Insertion. Note that there is, to my knowledge, no correspondence be-
tween the conditions on WFRs and those on other sorts of transormations
than Lexical Insertion. For example, the base of a WFR never need con-
tain a variable. This fact strengthens the assertion of Chomsky (1970),
that WFRs are very different rules from syntactic Transformations.

4.1.11. The Unitary Base Hypothesis.

From the evidence I have, it seems to be possible to assume that
the syntacticosemantic specification of the base, though it may be more

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or less complex, is always unique. A WFR will never operate on either this or that. The seeming counterexamples to this that I have found are analyzable as separate rules whose operations happen to be homophonous. Consider as an example the affix #able which attaches to both nouns (fashionable, sizable) and verbs (acceptable, doable). The most concrete evidence that we are dealing here with two different affixes is the fact that the nominals of N#able and V#able are formed by different rules. The denominal adjectives always nominalize in #ness and never in +ity (fashionableness, *fashionability, sizableness, sizability), while the deverbal adjectives, show no real preference (acceptability, acceptableness, moveableness, movability). This difference can most easily be accounted for if we regard the two sets as separate, formed by separate rules. Slightly less palpable evidence comes from the fact that the two #ables have very distinct semantics. The deverbal one means approximately "capable of being X$_e$d (where X is the base)". The nominal one means "characterized by X (where X is the base)". This difference shows up in cases where a form X#able can be derived from homophonous noun/verb pairs. If we are dealing with two affixes, then it is in only these instances that the word X#able should be ambiguous between the two senses noted above. Though there are few cases, the evidence is favorable. Fashionable, which may be either deverbal or denominal, has the two senses "in fashion" and "capable of being fashioned". Similarly sizable means "of great size" and "capable of being sized." Such a consistent correlation of homophony and ambiguity can only be accounted for on the hypothesis that we are dealing here with two different affixes, each with its own meaning, and each with its own base.

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The unitary base hypothesis is a strong assumption, and easily refutable. One must merely show that a certain WFR operates on two distinct classes of bases. The word distinct is important. It is not sufficient to demonstrate that a suffix attaches to both nouns and adjectives, for example. There are ways to formalize these two as constituting a single class, within the Extended Standard Theory (cf. Chomsky, 1972). However, if a WFR applied either to adjectives or to transitive verbs, two classes which could not be subsumed under one, without including others, then we would have a counterexample. Similarly, the rule investigated must be a reasonably productive one, for, as we have seen, less productive rules tend to be less coherent, and we should naturally expect more variation and exceptional behavior with such rules.

4.1.2. The Output.

The most studied aspect of morphology, at least the aspect most studied within the framework of Generative Grammar, is the relation between the syntax and semantics of the base and that of the output of a WFR, the common properties which the two share, and the ways in which these relations and commonalities can be accounted for. Most of those who have studied the subject have done so on the supposition that the relationships between words are no different than those between other syntactic entities. However, it is a presupposition of this work that words are not related in the same way as are higher syntactic entities, and that their inter-relations are independent of syntactic operations. We will encode the syntacticosemantic relationship between a base and an output merely by stating it. The WFR which attaches +ee forms nouns, and we will state this in the WFR. We know that the

syntactic category of the base must be syntactically specified, and in this case we know that it is a verb. We therefore know that the rule in question forms deverbal nominals, and we know this without recourse to any syntactic derivation.

We will not discuss the question of "possible meaning". It seems very likely that there are formal and other constraints on the meanings of words. Much of the work on lexical decomposition, though it may seem to be related to morphology, is really addressed to this question. I have nothing to say concerning this very interesting matter, and can only direct the reader to one very interesting recent work on the subject, Horn (1972), and the more traditional sources, especially Ullman (1962).

4.1.2.1. Semantics.

The meaning of the output of a WFR is a function of the meaning of the base. (This statement must be qualified. Our prediction of the meaning of the output is determined by the base, but the strength of the prediction is determined by productivity). Very crudely, consider the semantic operation of the WFR which we call agentive occupational nominal, and which we represent by the suffix #er, and which appears in baker, programmer, diver,..... The semantics of this rule can be roughly paraphrased as in (5):

(5) $\underline{V\#er}_N$ = one who Vs habitually, professionally,...

We know what $\underline{V\#er}$ means, since we know what V means, and we know what the #er rule means. Of course this is all very crude, but it does give us some idea of what it means for the meaning of the output to be a function of the meaning of the base of a WFR.

Paraphrases such as "one who" should not be taken to be theoretically significant. Hopefully, a well-developed theory of semantics will provide something better than mere paraphrase. Paraphrase also misleads one into thinking that the peculiarities of a rule such as #er are specific to it, that this is a rule of English, and completely unrelated to any rule of any other language. This is not true. Many other, completely unrelated languages have similar "one who Vs" nominals, and in these too modifiers such as "habitually, professionally" are also often valid. Modifiers such as these are also a puzzle for those who would wish to drive V#er from V by a simple syntactic-like operation. Words like habitually are not usually lost in the course of syntactic derivations, yet such must be the case if we wish to derive baker from bake. Traditional labels like "abstract nominal" are sometimes more helpful, and more able to account for things, than paraphrases. Consider the two English affixes +ation (as in derivation) and #ness (as in porousness). The first is a "deverbal abstract nominal" and has the meaning "act of Xing, or act of being Xed." The second is a "deadjectival abstract nominal" and has the meaning "fact or state of being X, extent to which something is X, or quality of being X." The set of meanings, as expressed in the paraphrases are mutually exclusive; there is no way for X#ness to mean "act of Xing", nor can Xation have any of the meanings of X#ness. Intuitively, this is because the latter is deadjectival, and hence cannot denote an action, and the second is deverbal, and hence cannot denote a fact, or quality, or degree. Intuitively, also, the paraphrases are not accidental. We know what a "deverbal abstract nominal" must mean, and what a "deadjectival abstract nominal" must mean. Yet

stating in terms of paraphrases makes it all accidental. We have no theory that tells us what the meaning of X+ation and X#ness must and must not be.

As expected, the same sorts of information that a WFR is sensitive to are the sorts of information it can introduce. Even such things as Lexical Presuppositions can be introduced by WFRs. Looking again at the suffix re# studied by Williams (1973), we find that this suffix, as well of demanding of its base that its meaning involve a change of state, has a separate presupposition of its own. Consider the following sentences:

(6) John washed the dishes.

(7) John rewashed the dishes.

The second sentence presupposes that the dishes were washed at some time previously to the time of the action of the verb of that sentence, by someone, not necessarily John. The presupposition is similar to that entailed by the adverb again. It is separate, however, from that on the base. This is indicated by the fact that though I may hit someone again (in accord with the presupposition of the output), I may not rehit him (since hit does not meet the condition on the base, as noted in 4.1.1. Though no other WFR has been studied so carefully as this one, with respect to its semantics, I suspect that others may be just as complex in their regularities.

4.1.2.2. Syntax.

Every new word must be a member of some Major Lexical Category, and exactly what category is determined by the WFR which produces the new word. #ness produces nouns (redness), and #able adjectives (definable).

The matter of the syntactic relation of the output of a WFR to its base is more complex and I have little to add to the extensive literature on the subject. I will mention only one fact, which is of a sort which has been overlooked in the previous work I am acquainted with. Though it is often noticed that there are many features which are taken from the base onto the output of the WFR, it is never mentioned that there are some which are lost.

The two verbs break and show allow specific Prepositional Phrases to follow them:

(8) They broke the glass into six pieces.

(9) We showed the film to the children.

To each of these verbs there corresponds an adjective of the form X#able, breakable and showable. These adjectives may not be followed by the corresponding PPs:

*(10) The glass is breakable into six pieces.

*(11) The film is showable to the children.

Without the PPs, the sentences are fine:

(12) The glass is breakable.

(13) The film is showable.

One cannot explain this distribution on the grounds that adjectives do not allow such PPs to follow them, since specifically those adj. of the form Xable which are not regularly derived from verbs do allow such PPs.

(14) This object is visible to the naked eye.

However, the Passives, which have been posited to be involved in the derivation of Xable forms, also allow these PPs:

(15)   This glass can be broken into six pieces.

(16) This film can be shown to children.

Since the above passives also roughly paraphrase the meaning of the un-grammatical Ss 10 and 11, we also know that semantic facts cannot be adduced to explain those sentences. It thus appears to be an externally unmotivated feature of the WFR X#able that PPs which closely follow the verb X may not follow X#able. What this means is that the simple view of a verb as consisting of the verb and everything it subcategorizes, combined with the simple view of the syntactic function of a WFR, which somehow conceives of the output as the base plus additional material, is not correct. The syntactic features of the output of the #able rule, at least, seem to consist of the syntactic features of the base, minus the closely bound PP, if there is one.

4.2. Morphology.
4.2.1. Morphological Restrictions on the Base.
4.2.1.1. Abstract Morphological Features.

It has long been recognized that the vocabulary of English is divided, for purposes of morphology, and to some extent phonology, into two distinct parts, native and latinate, and that there are many rules which are sensitive to this distinction. There are probably even further subdivisions, into greek, romance, etc.

A well known phonological rule which is restricted to latinate items is the rule of Velar Softening (SPE 219-223), which palatalizes k and s only in latinate words. There are many WFRs which are restricted to latinate bases.   A good example is the suffix +ity (oddity is the only exception I know of to the restriction) which contrasts very nicely in this regard with its rival #ness which does not discriminate at all between latinate and native words. WFRs restricted to native

words are less common. One is the suffix #hood, of motherhood and brotherhood.

The most important thing to be noted about a feature like latinate is that it is abstract, much like an abstract syntactic feature. A question then arises as to what this abstract feature is a property of, words or morphemes? There is good evidence that the feature latinate is a property of morphemes. If such a feature were a property of individual words, then we would expect that different words which contained the same morpheme would behave differently with respect to rules which are sensitive to the feature in question. This is not so. All words containing the morpheme +ity, for example, are latinate. This can be shown by the fact that all words of the form Xicity (lubricity, felicity) undergo Velar Softening, which as noted, only applies in latinate words.

Further evidence that it is the morpheme which is at least the basic carrier of the feature latinate, and also good indication of the abstract and arbitrary nature of the feature is the fact that mono-morphemic words tend to move into the native classification. For example, #hood, though restricted to native bases, attaches to words which are etymologically latinate, as in priesthood, statehood. But it is only monomorphemic words which are "exceptional." This makes sense if we are dealing with a feature which is both a property of morphemes and arbitrary. We expect monomorphemic words to lose this feature easily.

Stronger evidence comes from words which are made up of both native and latinate morphemes. Remember that the suffix +ity attached only to latinate words. Among the classes of words to which it attaches

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most productively is that of deverbal adjectives of the form X#able (advisable, digestible). This class includes words like doable, read-able, knowable, which have native bases. +ity attaches to these words. Readability and knowability are well-attested. With regard to the feature latinate these words have the structure -latinate +latinate + latinate. Since it is really the affix #able which is triggering the attachment of +ity and this affix is latinate, we can still preserve the statement that +ity attaches to latinate items only, in its simplest form, if we say that latinate is not a property of words, but rather of morphemes.

Finally, and most convincingly, the phonology of this last class of words can only be accounted for it we assume that the feature +latinate is a property of morphemes, and not of words. As we have seen, a word like forgiveable has the structure -latinate +latinate. Remember that the phonological rule of Velar Softening applies only in +latinate items. If we assume that an affix like #able, when it is attached, somehow causes the entire new word to be +latinate, by some sort of feature percolation, then the g of forgiveable would be in the proper environment for Velar Softening, since forgive is now +latinate, and should undergo that rule. This does not happen. Nor does it ever happen that a phonological rule "overapplies" in this manner. A mor-pheme like forgive never becomes +latinate for the purpose of phonology. This general fact can only be accounted for if we assume that abstract morphological features are properties of morphemes,and not properties of words.

Thus, one sort of morphological condition on the base of a WFR is a condition on abstract morphological features like latinate. From the

above examples, at least, it is possible to assume that a WFR will only be sensitive to the morphological features of that morpheme which is adjacent to the point of attachment of the morpheme of the WFR. A suffix would thus only be sensitive to the morphology of the last morpheme of the base, as in the last case, where +ity was only sensitive to properties of +able. Similarly a prefix would only be sensitive to properties of the first morpheme of the base. We could then rule out the possibility of a suffix being sensitive to the first morpheme of the base, and a prefix being sensitive to the last, and build up a theory which dictated the impossibility of these. A morpheme-based theory, for example, in which words are just strings of morphemes, could very easily incorporate such a restriction, and in a natural manner. Within the theory we are operating under, in which words are formed from words, and the base of every WFR is a word, it would not be so simple to incorporate such a general condition, for at the point of attachment of an affix, the whole word and its morphology are present, not just the adjacent morpheme. As we shall see, this simple assumption is false. There are suffixes which are sensitive to initial morphemes. This fact is in a very roundabout way evidence for our general theory, for though we might think on a priori grounds that a system in which conditions could only be stated on adjacent morphemes, and never on non-contiguous morphemes, is simpler, within our theory it is difficult to see how it could be simpler. The fact that the a priori simpler assumption in fact does not hold is thus evidence for a theory which does not deem this assumption necessarily simpler.

4.2.1.2. Restrictions Statable on Individual Morphemes.

More tangible restrictions that + or -latinate are common. Most of these are of a positive nature, and correlated with productivity. Thus +ity attaches most productively to bases of the form Xic, Xal, Xid, Xable (M. 314).

I will give two examples of positive conditions not statable on adjacent morphemes. The first is simple. The deverbal nominal suffix #ment attaches most productively to verbs of the form en+X and be+X (encroachment, bewilderment, embezzlement, bedazzlement)(cf. M. 332). As noted above, this single example refutes the simple theory just proposed as to the nature of morphological restrictions.

The second example is a more complex one. It involves the negative prefix un# (cf. Siegel, 1971), which, as Siegel has demonstrated, attaches only to adjectives. (She shows that nouns of the form un#X are derived from adjectives). This prefix attaches most productively to deverbal adjectives, a class which includes participles, present and past, (unflagging, unburied) and words in deverbal #able (unbearable). The first two types are difficult to analyze, since they involve inflectional categories, and perhaps drift as well. The class of adjectives in #able is clearly identified however, by its last morpheme, and un# is a prefix.

One question which arises in connection with these three classes is whether they are indeed three, or actually one, i.e. the class of directly deverbal adjectives. If they are one class, then it must be possible to refer to such notions, which are really quite complex, as deverbal adjective. For one, we would not merely be talking about non-adjacent morphemes, we would be dealing with internal constituent structure. This case is not clear. There are restrictions on each

of the three separate subclasses. Verbs with particles are treated differently for each. With past participles, the particle is generally tacked on: uncalled-for, uncared-for. With present participles it is always dropped: uncaring. With #able, the particle is sometimes dropped: unreliable: sometimes it is included: ungetatable, unrelyuponable, but these latter often have a "jocular tinge." Also, prefixed forms are treated differently in each case. If we are dealing with one class, then we must be able to account for the differences in an interesting manner, while still preserving the utility of the notion single class. If we are dealing with three classes, then these differences need not be explained at all.

There are also negative restrictions, cases where a certain WFR does not operate on bases of a certain morphological class. A simple example of such a restriction is one on #ness. This suffix, as noted above, is not restricted to + or -latinate bases. However, it does not attach to adjectives of the form X+ate, X+ant, X-ent: decent, *decentness, aberrant, *aberrantness, profligate, *profligate-ness. There are exceptions, but they are not common: accurateness.

A more complex case of a negative restriction also involves internal constituent structure, but that matter is clearer here than in the above case of internal structure. The rule in question is that of the denominal adjective suffix -al (global, organizational, regional). This does not attach to the class of nouns of the form $X_V$ment, i.e. the class of nouns of the form Xment, where X is an independently occurring verb. This restriction is exemplified in the list below:

(17)   ornament   *orna$_V$ ornamental

excrement   *excre$_V$ excremental

regiment   *regi$_V$ regimental

fragment   *frag$_V$ fragmental

employment   employ   *employmental

discernment   discern   *discernmental

containment   contain   *containmental

derangement   derange   *derangemental

There are two exceptions to our rule:

government   govern   governmental

development   develop   developmental

In terms of sheer number, these are trivial. W lists about 500 words
Xment of which the great majority are X$_V$ment. Also the semantics of
one of them, governmental, is curious. The noun government has at
least two distinct senses. One is directly deverbal, the other
extended:

(18)   His government of the country has been roundly criticized.

(19)   His government was defeated by a wide margin.

The sense of government in (18) is that of a deverbal abstract action
nominal, and is similar to that of most deverbal abstract nouns in such
diverse suffixes as #ment, +ation, #al (curtailment, finalization,
denial). The sense of the same word in (19) is an extended substantivized
one, similar to that of organization in (20):

(20)   The organization needs you.

Our exceptional item, governmental, has only one sense, that which
corresponds to the extended sense of government, that of (19):

(21)   These funds were used for purely governmental purposes.

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Let us assume that in a well articulated theory, we can define the notion "loss of structure", and that words which persist long enough to extend their meaning, will be susceptible to such losses of structure. In such a theory, the difference between the two senses of government could naturally be encoded into a structural difference, that between $X_V$ment and Xment. Since governmental is clearly derived from Xment and not $X_V$ment, if our constraint is on $X_V$ment, then governmental is no longer an exception. Whether the same can be said of developmental I do not know, as its exact meaning is not clear to me.

One of the things which our exceptions demonstrate quite clearly, is that the constraint is definitely one on internal constituent structure, and not merely one on X being a verb, or there existing a verb, or Xment being derived from a verb.

If we were dealing with a restriction to the effect that X cannot be a verb, then we would rule out departmental, because depart is an independently occurring verb. But clearly department is not derived from this verb, and does not have the internal structure $X_V$ment but rather Xment.

If we are constrained by sheer existence, then a problem arises with nouns of the form Xment from which verbs are derived (cf. Aronoff, 1974).

(22) He complimented me on my dress.

(23) Don't experiment with such things.

(24) Life is so regimented.

Since these verbs all correspond to nominals, and the derivation of Xmental is not blocked (experimental, regimental), the constraint can-

not be stated merely in terms of existence, but must refer to internal constituent structure.

Nor, finally, and most finely, can we extend the argument to all Xments derived from (derivable from) verbs. Such words as excrement, increment, medicament, are derivable from verbs excrete, increase, medicate, by a rule of obstruent deletion before #ment (obs. →∅ / _#ment). But the result is a structure in which X is not strictly a verb, having lost its final consonant. This distinction is of course very fine, and depends on the assumption that obstruent deletion is a real rule. If it does hold however, it shows that we are not dealing with some sort of global constraint, of the form "X is derived from Y", but rather one on the structure of the base at the point of the application of the WFR. The case, if real, is interesting, because it makes a distinction between a global and a structural statement, and obeys the more restricted structural statement.

We must conclude that there is a constraint against the application of the rule -al to bases of the structure of $X_V$ment. This restriction cannot be explained away on general syntactic grounds. Normally -al attaches quite freely to other deverbal abstract nominals: organizational, observational, reverential, preferential.

4.2.1.3. Encoding Morphological Restrictions.

Not all of the above restrictions must be stated directly. Most negative restrictions are the result of the very general rule whereby one cannot list more than one word in a given stem with the same meaning discussed in Ch. 3. We have called this the blocking rule. Consider, for example, the restriction on #ness noted above, that it does not attach to bases of the form Xate, Xant, Xent. This can be

traced to the fact that the rival deadjectival nominal suffix attaches very freely to adjectives of this form. So we have profligate/ profligacy, aberrant/aberrancy, decent/decency. The productivity of this latter rule, combined with the blocking rule, prevents #ness from attaching to these bases. One result of the blocking rule is that we will normally get more than one nominal in a given stem, only when they have two different meanings. Recital and recitation are both permitted, as are humanity and humanness, because they have different meanings. Our way of viewing the blocking rule is the following: in the dictionary of each speaker, for each stem, there is a slot for each possible meaning of a word in that stem, where possible is restricted to directly generable by rules. Only one item can fill each slot. The particular rule which is most productive with the morphological class of the stem will in most cases fill this slot, and effectively block the application of other rules in most cases, as above with -cy and #ness. Of course the productively formed item may drift, and in drifting will leave its slot, which then is open for the application of another rule, as in the case of humanity and humanness. Note that we are not excluding the possibility of there being two items with the same meaning, we are rather excluding two items with the same meaning and the same stem in the same person's dictionary at the same time. To exclude having two words with the same meaning without further qualification is to exclude synonymy, and this is ill-advised. It is also quite possible for two different speakers to have two different words in the same stem with the same meaning, and it is also quite possible for one person to forget the word he has in a particular slot at a particular moment, and to make up another one, for the moment. In fact, the blocking rule,

stated as a condition on the filling of slots, predicts that the fewer the number of stably filled slots one has, the more likely one is to accept new words. This seems intuitively correct. It is the many-worded pedant who usually objects with "That is the wrong word."

The blocking rule cannot account for all morphological restrict-ions. Firstly, it can only account for negative ones. Nor can it even account for all of these. The impossibility of the attachment of -al to X_Vment cannot be traced to blocking, simply because there is no other form to block it. Therefore some of the negative morphological restrictions on WFRs, and all of the positive ones, must be stated independently.[1a] I will review the one proposal which has been made in the published literature for a method of encoding these restrictions, that of Chapin (1967, 1970), and then go on to propose an alternative, and, hopefully, superior method of dealing with them.

4.2.1.3.1. Ordering of WFRs.

Ordering is a well known device in syntax and phonology. Chapin (1967) notes that if we have reason to believe that WFRs are syntactic rules, and if we have reason to believe that there can be extrinsic orderings placed on syntactic rules, then we have reason to suspect that there may be extrinsic ordering relationships among WFRs.

prima facie, there are good reasons to believe that WFRs are not syntactic rules. The fact that words persist, and all the concommitant properties of words which this fact gives rise to is one. Secondly there is the function of WFRs, compared with that of other rules.

What do syntactic transformations do? Given a Deep Structure, we apply to it the ordered set of Transformational rules, and arrive at a Surface Structure. If we decide, for one reason or another, to stop

somewhere in the middle, applying the first half of the rules, and forgetting about the rest, there is no guarantee, in fact it is highly unlikely, that we will have a recognizable Surface Structure. This is because the entire set of Transformations is really one huge rule or algorithm for converting a Deep Structure into an equivalent Surface Structure. The same is true of the ordered set of phonological rules.

Such, however, is not the case with WFRs. As their form implies, the application of any one WFR, which is always a rule for forming a word from a word, will give us a word. There are no intermediate abstract stages. Nor do these rules, as syntactic and phonological rules do, take us from one level of the grammar to the next. They do not turn an X of one level into an equivalent X of another level. Rather, they add someting to an X, something at once phonological and semantic, and produce a Y which is an element of the same linguistic level as X, and is not at all equivalent to or corresponding to X. It is thus quite clear that WFRs and Transformations do not do the same thing. Therefore it is highly unlikely that WFRs will be ordered among syntactic Transformations.

This does not mean that WFRs cannot be ordered. It merely means that there is no special reason for expecting them to be ordered among themselves. The type of ordering that has been proposed to exist among WFRs, it should be noted, is arbitrary extrinsic ordering. An extrinsic ordering is one that is imposed on two rules which, a priori, could appear in one order or another. The extrinsic ordering tells us that of two rules A and B, A applies first. There are nonarbitrary extrinsic orderings; such orderings are determined by external

principles like the cycle in syntax, or the finer principles of Williams (1974). An arbitrary ordering is one which is not governed by any general principle, but must be stated specially for a particular pair of rules. This type of ordering is, of course, the least desirable, since it is the least constrainted. As a matter of fact, all suggested arbitrary orderings hypotheses in the linguistic literature have been slightly stronger than this minimal one. Arbitrary extrinsic orderings are transitive, i.e. if A precedes B, and B precedes C, then A precedes C.

Let us look at an example of how arbitrary extrinsic ordering can be used to account for morphological restrictions on the base of a WFR. Consider the following contrast. The suffix #ism attaches productively to words ending in -al:

    (25)   constitutional#ism

           physical#ism

           animal#ism

However, -al does not attach to words ending in ism:

    (26) *Dogmat ism al

          *Fatal ism al

This restriction cannot be attributed to the syntactic class of words ending in ism, since -al attaches to other abstract noun classes: inspiration$_N$al$_A$.

Nor is the concatenation Xismal generally prohibited:

    (27) strabism al, dismal, catechism al, embolism al,

          rheumatism al, baptism al

These items crucially differ from those in (26), however, in that the Xism form is not derived by any rule; there exists no corresponding free form X:

    (28) *strab, *dism, *catech, *embol, *rheuma, *bapt

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Ordering of the relevant WFRs can handle all this material quite nicely. Rule A, the _ism_ rule, whose output is essentially $[[X]_A \#ism]_N$ is extrinsically ordered after rule B, the _al_ rule, whose output is $[[X]_N al]_A$. This ordering guarantees the impossibility of words of the form $[[[X]_A \#ism]_N Al]_A$, i.e. the words of the type we with to exclude. It permits us to generate words of the type of (25), since to generate these B must apply before A, which our ordering permits. It also allows us to generate the words in (27) (_strabismal_) since the _ism_ in these cases, in a theory of word based word formation, is not attached by a rule. We see, then, that the ordering handles a relatively complex set of data in a simple manner.

There is a mechanism which will produce the same result as ordering, at least when one is dealing with constraints on concatenation. One can simply state the negative concatenation conditions. So, one could put a negative condition on the base of rule B, which would say that B, _-al_ attachment, never applies to words of the form $X \#ism_N$. This will have exactly the same effect as ordering the rule which attaches _#ism_ after that which attaches _-al_. There are several differences between the two theories, all of which weigh in favor of ordering, if we consider the power of the two. Firstly, negative conditions on the base can refer to just about any property which a base could conceivably have, whereas ordering restrictions, in terms of what they can actually restrict, are much less powerful. Since conditions can encode all that ordering encodes, and then some, we must prefer ordering, until it is shown that we must have recourse to the extra power the other device provides us with. The second way in which the two differ is with regard to multiple conditions on one rule. The ordering theory, if it is perfectly transitive,

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makes certain very strong predictions, which the other theory is completely incapable of handling. If, in addition to the two rules above, which had the property that the output of A could not serve as the input to B, we have another rule C, whose output cannot be the input to A, within the ordering hypothesis, this situation forces us to order C after A. But this ordering, by transitivity, predicts that C cannot ever precede B, and predicts, completely independently of anything but ordering, that the output of C cannot be the input of B. Within the condition hypothesis, on the other hand, the fact that the output of C cannot serve as the input to A is an isolated fact, encoded as a negative constraint on A, with no predicted side effect on B. There is no reason, within this theory, why the output of C cannot be the input to B. The ordering theory thus makes a prediction, where the other is silent. We must therefore prefer the ordering theory, and look at its predictions, to see whether they are always correct.

With regard to the last issue, i.e. that of transitivity, Chapin adduces several examples which force the rejection of a complete transitive ordering. I will repeat one only. Ation attaches to verbs in #ize (standardization). #ize attaches to adjectives in -al (industrialize). -al attaches to nouns in Ation (organizational). Within the ordering theory we then have an ordering of the following sort:

(29) 1. ation precedes -al (organizational)

2. -al precedes #ize (industrialize)

3. #ize precedes +Ation (organization)

If the ordering of WFRs is completely transitive, and linear then this is impossible. +Ation both precedes and follows -al. Within an order-ing hypothesis, we must have recourse to some sort of cyclical ordering

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here, if our ordering claim is to have any force at all. As Chapin (1970) notes, because WFRs are all optional, the simple cycle of syntax, in the case of WFRs, is equivalent to no ordering at all. If we place all WFRs in a cycle, then any WFR may follow any other WFR immediately, given enough cycles. This leads Chapin to propose what he terms an _Epicycle_, whereby all WFRs are placed in a linear order by extrinsic conditions, and rules which apply cyclically, as in (29), must be ordered adjacent to each other in the complete linear order. Thus _Ation_, -al and ize will be immediately adjacent to each other, in the order given, and these three may be epicycled on. Possible epicycles will be marked off by some device, (cf. Chapin 1970, for a more detailed discussion of the epicycle).

The epicycle, as Chapin stresses, is a highly suspect construct. It does have the virtue of being refutable. A possible refutation, which Chapin rejects on grounds which I have discussed in another context is _governmental_ (cf. 4.2.1.2.), whose derivation violates an epicycle (cf. Chapin, 1970, p. 62). A more likely example is the class of words of the form _Xatorial_ (dedicatorial, investigatorial). Chapin establishes the ordering _al-ize-ation_. He shows that within an epicyclic theory the ordering _ation-al-ize_ is incorrect.

We mentioned in Ch. 2 that Martin,(1972)had established that words of the form _Xory_ are derived from words of the form _Xion_. This fact establishes an extention of the ordering to _al-ize-ation-ory_. However the class of words _Xatorial_ shows that _al_ must follow _ory_. This ordering is a violation of the epicycle. Words of the form _Xatorial_ thus refute the ordering theory even in its weakened epicyclic form, and we are led back to the less desirable alternative of simply stating conditions.

With regard to the first issue, what sorts of things can be conditions on WFRs, there is more substantial evidence that the ordering theory is incorrect. First of all WFRs must refer to abstract features like +latinate. The rule of +ity, as noted above, must refer to this feature, since it only attaches to latinate words. However, since many of the words to which +ity can attach are not derived, the specification for +latinate bases must be statable independently of ordering, i.e. as a condition on the base of the +ity rule. Thus there is need for such conditions even within a theory which utilizes ordering.

WFRs must also occasionally refer to the stress pattern of the base. Siegel (1971) discusses two such cases, both of which will be described in detail below. Stress is generated by phonological rules, and as Chapin stresses, it is impossible for a theory of conditions on WFRs which attempts to incorporate all these conditions into ordering statements to deal with such phonological conditions. Since there is no reason to doubt the reality of the phonological conditions which Siegel discusses, and there is no way to encode them into any other sort of restrictions, we must admit that they are strong evidence against the ordering theory.

Another major problem which the ordering theory faces is that of coexisting forms. To see how this is a problem, we must first review Chapin's account of the distribution of the nominal affixes #ment and +ation. Citing an unpublished work by Emonds (1966), Chapin states that the distribution of these affixes is by and large governed by phonological properties of the base "verbs with the prefixes eN- and be- take -ment; verbs ending in oral or nasal stops take +Ation; verbs ending in y or z, preceded by an optional liquid, nasal, or peripheral

stop, preceded by a lax vowel take +Ation (starve, sense, fix);
verbs ending in a liquid preceded by a vowel take +Ation (console,
explore). All others take #ment. All this can be accounted for by
ordering +Ation, before #ment, and marking verbs in eN and be- as
exceptions to +Ation. There are of course many exceptions, which are
noted, but "the generalizations are striking." In addition, all other
nominal affix rules are ordered before these two (-al, -ence, etc.).
This serves to eliminate the application of either of these rules
with stems which have nominals in the other affixes (occur/occurrence/
*occurrment/*occurration).

We will not consider the empirical validity of Emonds'
constraints. Rather we must ask exactly what ordering is being used
to encode here. It seems quite clear that ordering, when used with
rival affixes, is being used to encode the blocking constraint. The
reason we don't get *occurrment or *occurration is because we already
have occurrence. The same is true an least of the fact that +ution
does not attach to stems beginning in eN- and be-. Using ordering
to encode the blocking facts firstly and most importantly obscures the
actual function of the blocking. It predicts that there will never be
pairs of nominals in the same stem. This is false prediction. It is
perfectly possible to have more than one nominal in a given stem, as
long as the nominals do not have the same meaning. This fact is
exemplified in the list below:

(3)   ment/ation

         consolement   consolation

         assignment   assignation

<u>al/ment</u>

committal   commitment

<u>ence/ment</u>

condolence   condolement

<u>/ment</u>

advance   advancement

escape   escapement

abandon   abandonment

<u>al/ation</u>

approval   approbation

recital   recitation

proposal   proposition

<u>ure/ation</u>

striature   striation

juncture   junction

Another problem, of course, is the establishment of disjunctive
environments. Even Emonds runs into this problem. Because words
which begin in <u>eN</u>- and <u>be</u>- may otherwise meet the conditions for
<u>+Ation</u> attachment, all the words which begin in these prefixes must
be made exceptions to the <u>+Ation</u> rule by some sort of rule feature
marking device. Though it is possible to preserve the ordering theory
by having recourse to such a device, the fact that one must have such
recourse merely serves to show that ordering is being overextended in
these cases, and should be reexamined. We must conclude that order-
ing should not be used as a device to encode blocking, firstly because

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it predicts blocking where there is none, and secondly because the ordering can only be established by an abuse of the notion "disjunction."

I have not mentioned the fact that ordering is a most unsatisfactory device for encoding positive conditions on the base of a WFR. The fact that #ment attaches to verbs which begin in eN or be can be noted only derivatively. If a WFR is not ordered before the rule whose output is a certain form, then certainly this rule can apply to the output of that rule, but there is no way for ordering to express the fact that there is a certain affinity between WFRs and certain bases, that a WFR will be more productive with certain bases than with others. As long as #ment attachment is ordered after the eN and be prefix rules, it may operate on verbs in these prefixes, but there is no way to encode the fact that it _prefers_ these.

For all of the above reasons, and I have given a good number, ordering cannot be used as a device for encoding restrictions on the morphology of the base of a WFR. I have dwelt on the question of ordering for two reasons. Firstly, arbitrary transitive linear ordering is a relatively highly constrained device for dealing with certain morphological conditions on the base of WFRs, and as such deserves consideration. It is also the only device for dealing with such phenomena which has been proposed in the literature. Secondly, it has been proposed, though we may question the logic of the proposal, that since we know other sorts of rules, specifically syntactic rules, to be ordered, if we discover WFRs to be ordered in the same manner, we have some justification for believing that WFRs and syntactic rules are related. We have shown that ordering is not a good device for

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dealing with conditions on the base of WFRs. We therefore have no

reason to suspect that WFRs are ordered among themselves. This is very

strong indication that WFRs are not in the same class as syntactic

rules but form a separate self-contained set of rules. This gives

added weight to the proposal that word-formation is accounted by a

component of the grammar which is distinct from all others.

4.2.1.3.2. Unordered WFRs.

We must conclude from the above section that WFRs are not

extrinsically ordered among themselves. The only possible ordering

among WFRs will then be intrinsic. This means in effect that WFRs are

unordered. Within such a system, the morphological conditions on the

base of each WFR must simply be stated for each rule.

Most negative restrictions will be accounted for by the blocking

rule, and thus never have to be stated independently. Thus the fact

that -al does not attach to bases of the form $[X] \# ism]_n$, which we

dwelt on at length above, need never be stated directly, because words

of this form are usually subject to the rival rule of -ic (modernism,

modernistic (cf. Aronoff, 1974, for justification of this derivation)).

Positive restrictions, which can never be encoded into an ordering

framework, are different. They are closely correlated with product-

ivity, and are thus very different from positive syntactic and

semantic restrictions, which are of an absolute nature. It is never

the case that a WFR is more productive with a given syntacticosemantically

specified base than with another, for the syntacticosemantic specificat-

ion of the base is unique and unitary.

Productivity is a variable matter. A rule is not either

productive or not productive. Rather there are degrees of product-

ivity. It is therefore interesting that we can associate this variable property of productivity with the morphological composition of the base of a WFR, for it is just this one property which is not unique: a WFR operates on bases of different morphological classes.

One matter which the correlation of productivity with morphology does not address is the question of the "total productivity of a WFR." The strongest version of our theory would deny the validity of the notion, for, since productivity is associated only with morphological subclasses of a base, there is no direct way to encode the notion of "total productivity." One might perhaps try to calculate the total productivity of a WFR from the productivity of the morphologically determined sub-classes of the base, but this would be difficult.

It appears that in some cases at least, there is a valid sense of the notion productivity which is independent of morphology. These cases are those in which there do not appear to be any positive morphological restrictions on the base. Such a one is the rule of deverbal #able, which though it has no morphological conditions on its base, is intuitively felt to be very productive (cf. Chapin, 1967). Note that not all WFRs which do not have morphological conditions on them are necessarily productive. The rule of +ous (bilious, contagious), which has no morphological restrictions on it which I can find, is decidedly non-productive. It may very well be, then, that in cases where there are no morphological restrictions on a WFR, product-ivity is a property of the entire rule, but that this is not true in the more common case. This is an empirical question, and must await further work.

The important point to notice, though, is that since product-ivity is determined not for a WFR as a whole, but for the individual subcases of the rule associated with each morphological condition on the base, all of which must be stated independently, therefore semantic coherence, which is determined by productivity, will also be determined not for the entire WFR, but for each morphological sub-case of the WFR. What this means is that we are free to state the semantics of the output of the WFR as a compositional function of the meaning of the syntactic base. This allows us to preserve such statements as "#ness is an affix which forms deadjectival abstract nominals" and restrict any qualifications of this statement to individual and morphological subcases. This is, in a sense, the teleology of the empirically supported, determination of statements concerning probabil-istic matters, by morphological conditions on the base of a WFR. If such were not the case, no part of a WFR would ever be statable discretely.

Note again the importance for such a system of the unitary syntactic base hypothesis hypothesis (4.1.1.1.). If we did not have a unitary syntactic base for every WFR, there would be no way to isolate any discrete operation. The unitary base hypothesis is empirically testable, independently of any other of the claims being put forth. We see, then, that our theory depends very crucially on several different hypotheses, each of them independently falsifiable, yet the consequences of which are completely interrelated.

Summing up so far, we can say that a WFR has at least two parts. Firstly, there is a part which specifies the syntactic and semantic characteristics. There will be no disjunction in the specification of

these characteristics, and no negation. The semantics, at least, of the output of the WFR, is specified here, as a compositional function of the base. Secondly, there is a series of positive conditions on the morphology of the base. These conditions, are associated with productivity and semantic coherence (which are, in a sense, the same thing).

I will give a simple example, the rule of negative un#. I will assume for the moment that the morphological part of the change of the rule consists of the addition of the prefix and its boundary: un#.

(31) Rule of negative un#.

a) $[X]_{adj.} \longrightarrow [un\#[X]_{adj} \ adj$

semantics (roughly) un#X = not X

b) Forms of the base

1. $X_V ED$ (where ED is the marker for past participle)

2. $X_V\#ing$

3. $X_V\#able$

4. X+y (worthy)

5. X+ly (seemly)

6. X#ful (mindful)

7. X-al (conditional)

8. X#like (warlike)

(Each of these will, of course have some index of productivity and coherence associated. They are listed roughly in order of productivity. Remember that the mere fact that an item is not in one of the listed classes does not preclude it from undergoing the rule, unless it is subject to negative condition).

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4.2.2. The Morphological Operation.

We have said that a WFR specifies a base, and specifies some operation on the base, which results in a new word. This operation will usually have some phonological reflex, some morpheme which is added to the base. We will call this operation the _morphological operation_ of the WFR. It could just as easily be called the phonological operation.

This operation is generally quite simple, and consists of the addition of some affix to the base. The WFR specifies the phonological form of the affix, and its place in relation to the base. The rule of #ness, for example, adds ness to the end of the base. We will assume that the affix and its position are constant for a given WFR. This means that #ness, at least when introduced by the rule of #ness, is always a suffix, and always has the form #ness. Nor does this rule have any other form which it might add instead of #ness, in some particular environment. The affix is a phonological constant. We will also assume that the boundary associated with the affix is a constant. This means that if we find two affixes which are phonologically identical, except for the boundaries associated with them, they cannot be introduced by the same WFR.

We assume that the phonological form is constant and completely specified. No archiforms or abstract segments are allowed, in accord with the theory of Kiparsky,(1973).We will see in Ch. 5 that the phonological form of an affix, though it must be fully specified, may have different realizations in environments determined by the morphology of the base. These different forms, called _allomorphs_ are introduced by a later set of rules, called rules of _allomorphy_ (cf. 5.2.). It is significant that these allomorphs are determined not by

individual bases, but by the morphemes of the bases, and by morphemes in the most extreme sense of the term; semantically empty roots. This is parallel to the fact that productivity is determined by morphological features of the base. It is also susceptible to parallel treatment; it can be removed entirely from the main body of the rule. All morphologically determined variation thus lies outside the WFR itself. Furthermore, variation can be totally ascribed to morphological properties of the base.

4.2.2.1. Copying Rules.

Something which controverts the simple statement that affixes have a phonologically constant form is the general phenomenon of reduplicated or copied affixes. Reduplication rules copy one part of the base of a rule, and use this part as an affix or part of an affix. Reduplication rules are clearly morphological rules of word-formation. Firstly, there is no reduplication rule whose environment is totally phonological. (We are of course referring only to total copying rules, not to rules such as harmony rules which assimilate features of one segment onto another). Secondly, reduplication rules are never ordered among the rules of the phonology. Both of these statements are easily falsifiable, but if they are true, they are sufficient to demonstrate that reduplication is not a phonological process. Reduplication rules are often said to have a "function" which is the same as that of WFRs. The notion is a little obscure, but we will take it to be correct, and assert that all copying rules (NOT assimilation rules) are WFRs. However, it is clear that if they are WFRs, then the affixes introduced by them cannot be called phonologically constant.

Consider, for example the well-known rule of Klamath vowel copy (Kisseberth, 1972). There are prefixes in Klamath which have fixed consonants, but whose vowels are copied from the initial vowel of the stem:

(32) a) <u>Noncausative</u>   <u>Causative</u>

    pe:wa "bathes"   hespe: wa

    no?ga "is cooked"   hosno? ga

    ma:s?a "is sick"   hasma: s? a

  b) <u>Noncausative</u>   <u>Causative</u>

    qe:gi   "is absent"   sneqe:gi "loses something"

    qdo:ca   "rains"   snoqdo:ca

    tsa:ktgi "become light   snatsa:ktgi
          (in weight)

  c) <u>Nonreflexive</u>   <u>Reflexive</u>

    he?sla "has sexual   sene?sla
        intercourse from
        behind"

    lo:cwa "covets"   solo:cwa

    twa:qa "smears"   satwa:qa

The three prefixes all have different vowels in each case, the vowel being the same as the first vowel of the stem. Kisseberth gives evidence that the vowel of the prefix, though it must be present in underlying phonological representation, cannot be shown to be represented in this underlying form by any one of the surface vowels of Klamath. He therefore has recourse to an abstract segment V*. The prefixes are listed in their underlying forms as <u>hV̇s+</u>, <u>sV+</u>, and <u>snV+</u>. A phonological rule of vowel copy applies only to V*, and replaces it by the first vowel of the stem in all instances. Within

the theory being put forth here, and the theory of Kiparsky, (1973), V* is not a permissible underlying segment, and the rule of vowel copy is an impermissible rule of absolute neutralization. However, it is only by using an abstract segment like V* that we can preserve the hypothesis that the morphological operation of a WFR produces a phonological constant. It seems to be impossible to preserve both hypotheses; either we give up the prohibition of abstract segments, or we give up the phonological constant. This dilemma will be forced upon us in all instances of copying rules.

The dilemma can be easily resolved. We have hypothesized that all copying rules are WFRs and that they are never ordered among the rules of the phonology. Kisseberth specifically notes that both of these are true of the Klamath vowel copy rule, both that it may be considered "morphological," and that "It is significant that no phonological rule must precede Vowel Copy, to my knowledge" (ibid. p. 6, fn. 7). This is fine, if we are only worried about the abstractness of phonological representations, and are willing to allow abstract segments which are concretized by morphological rules prior to the phonology. But we are trying to make the even more restrictive claim that morphemes cannot be represented with abstract forms. The simplest way to retain this claim is to revise somewhat the view of a morpheme as a phonological constant. We will view a morpheme not as a constant, but as an operation.

Though morphemes are usually regarded as entities with independent status, just like stems, this isn't the only view one can hold regarding these items. It is equally possible, and perhaps preferable, to regard a morpheme as a product of a phonological operation associated with a WFR. In the case of a phonologically constant affix, like

#ness, there is no difference between the two treatments. However, when dealing with copying rules, if we wish to preserve the statement that no morphemes contain abstract segments, at any level of derivation, the two views of the morpheme are quickly decided between. We simply replace the notion of a morpheme as a phonologically constant entity by one of a morpheme as the product of a unique phonological operation. This simple claim allows us to replace the Klamath causative prefix $\underline{hV^*s}$ by the following rule:

(33) <u>Klamath Causative WFR.</u>

$$_V \begin{bmatrix} C & V & X \\ o & & \end{bmatrix}$$
$$1 \quad 2 \quad 3 \rightarrow \begin{bmatrix} h \\ V \end{bmatrix} \begin{matrix} 2 \\ -\text{lg} \end{matrix} \begin{matrix} s \\ \\ \text{Caus.} \end{matrix} {}_V \begin{bmatrix} 123 \end{bmatrix}\,]\,]$$

This rule will produce correctly all the forms of (32a). Similar rules will give us all the other forms of (32), and can be used for all other copying affixes. We no longer need worry about abstract morphemes, since by stating the copying rule and the rule which spells out the morpheme in the same rule, we have avoided the intermediate point in the derivation at which the abstract segment occurs. This will always be possible, if we are right in claiming that all copy rules can be ordered prior to phonological rules, for this claim implies that no rule will come between the rule which attaches the morpheme, and the rule which spells out the copied segment(s) of the morpheme.

By accounting for copying by rules like (33) we are making three claims.

a) Copying operations are parts of WFRs, and not phonological rules (the latter is already implicit in Kiparsky).

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b) WFRs are not labeled bracketings. (Rules like (33) cannot be represented by labeled bracketings).

c) Affixes, unlike stems, have no independent existence.

A possible objection to the encoding of copying operations into rules like (33) is that the use of such a device entails that if we have X affixes which contain copied material, we have X copying rules. If every affix contains a vopied vowel, the "same" vowel, as in the case of the three Klamath rules discussed so far, then we must repeat the same operation for each affix, in this case three times. It seems intuitively incorrect to have to do this.

This is a difficult objection to answer. It is not quite valid. There is nothing a priori which mitigates against having three vowel copy rules, except the general notion of capturing regularities. We don't want to state the same thing three times. However, each of the solutions, that which dictates that we have three rules, and that which allows us to collapse the three into one, is imbedded in a different theory, and in order to compare the two solutions, we must really compare the two theories.

Neither theory can account for all the observed generalizations in a satisfactory manner. One entails that we use abstract segments and says nothing about the place of copying rules in a grammar. The other forces us to use three copying operations in place of one. Either both of the theories are false, or one of the generalizations is invalid, not a true generalization.[2a] Of course this is very dangerous ground, the ground of true and false generalization.

Though I have no real evidence either way, I do have some observations bearing on the generality of the generalization that is

being missed in the theory being espoused, i.e. the generalization of the three copying rules into one. In Klamath, there is another reduplicated prefix, which copies the first C_0V of the stem (the V is short as above). Note that this prefix copies not just the first consonant, but the entire first consonant cluster. This is exemplified in the following paradigm:

(34) <u>Nondistributive</u>   <u>Distributive</u>

Nondistributive		Distributive
pe: wa	"bathes"	pepe:wa
no: ga	"is cooked"	nono:ga
ntopa	"spoils"	ntontpa
qniya	"has an erection"	qnqniya

Since the number of consonants in the affix is equal to the number of consonants in the stem, and this number varies with the stem, there is no way in which we can represent the consonants of this stem by abstract segments, because sometimes it will contain one abstract segment, and sometimes two, and how many it contains is predicted by the stem. The vowel of course can be represented as V*. What Kisseberth does is to invoke a "morpheme of reduplication" which he calls R. R is realized by a rule as a copy of the initial consonant (cluster) of the stem plus V*. This rule is called Reduplication, and is followed by vowel copy. This is a very awkward solution, for it uses a copy rule of the form of (33), as well as an abstract segment, in fact the copy rule <u>introduces</u> the abstract segment. Abstract segments are bad enough; when such segments are introduced by rules, and exactly the sort of rules which, as far as I can see, the abstract segments were designed to avoid, the system becomes very suspicious indeed. The above example demonstrates clearly the need for copying rules of the form and function of (33), in any system, and it

shows that such copying rules are the source of abstract segments. This
fact is bad for the theory which by using abstract segments, allows us
to state vowel copy as one rule, for it casts doubt on the validity
of the abstract segments.

The general phenomenon of syllable copy is immensely troublesome
for the abstract segment system. Consider the Hebrew Pilpel con-
jugation, which is formed by reduplicating monosyllabic roots:

(35)   root   Pilpel Stem

        kul   kilke:l   "sustain"

        gal   gilge:l   "roll"

We will not discuss the vowel pattern, which is characteristic of all
stems of the form CVCCVC. For the moment we can assume that the vowel
of the root is copied and that a later rule adjusts the vowels.
Within the system proposed, in which rules of the form (33) are
permitted freely, and there are no abstract segments, the derivation
of Pilpel forms is simple. If we use abstract segments, on the other
hand, the matter becomes immensely complicated. If we represent the
reduplicated part (we will assume the first syllable is the copy) by
C*V*C*, then we do not know which stem consonant gets copied onto which
C* unless we have two copy rules; one for the first consonant, and one
for the second:

(36) a) initial consonant copy

         $C \rightarrow C_j$ /#_VC+C$_j$Y#

      b) other consonant copy

         $C \rightarrow C_j$/X_CVC$_j$#

Assuming another rule for copying the vowel, we have a total of three
copying rules for this one affix, and rules which are not generalizable

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to any other segments, for they will only apply to these Pilpel forms. None of these rules can even be extended to the one other conjugation which is formed by copying. This is the much more common Pi??e:l conjugation, which is formed by doubling the middle consonant of the triliteral root as follows:

(37)   root   Qal   Pi??e:l

gdl   ga:dal   "grow"   gidde:l   "raise"

šbr   ša:bar   "break"   šibbe:r   "smash"

Disregarding the problem of the infix, which will be treated below, for the reduplicated middle consonant, in the abstract system, we need the following rule:

(38)   **Middle consonant copy**

$$C \rightarrow C_j \;/\; [_{root}CVC_j]\_\_$$

The root marker is there to ensure that the rule does not apply in the Hip?i:l form. One could alternatively restrict the rule to C*. In any case, whatever the exact formulation of this rule, it is not the same as either of the rules of (36). We need three consonant copy rules for two affixes, none of which has any other justification, all of which apply to abstract segments only, and all of which can be ordered before any other rule of the phonology (there is no ordering among them).

All of these idiosyncracies arise from the desire to state the Klamath copy rule once only, instead of three times. This desire leads us to the positing of four (including vowel copy in Pilpel) rules of no generality at all. This last example shows up perfectly the fallacy lying behind the rule counting argument. One theory gives us more rules in one case (Klamath) and the other theory gives us more rules in the other case (Hebrew).

Do we decide between theories idiosyncratically for each language, depending on the number of rules each theory needs? It seems wiser to disregard rule-counting, and ask what other sorts of things the two theories are saying. If we ask this question, there is no question that the theory which regards copying rules as a particularly complex sort of WFR is preferable. This theory predicts that copying rules will always apply at a certain point in the derivation of words, namely before the phonology, and honce will never follow any rule of the phonology; it rids us in a principled manner of a class of abstract segments which are problematical and undesirable on general grounds; it says that all copying rules are "functional", i.e. WFRs; and it gives us more clue as to the general form of WFRs. All of the claims of this theory are empirically easily falsifiable, and they are many. The other theory, which treats reduplicated segments as abstract phonological entities makes, as far as I can see, no interesting and restrictive claims at all.

Note that I have not disproved the abstract segment theory. It is probably not very easily disproved, if at all. What I have done is to indicate that the advantage which this theory seems to enjoy over the one being proposed is illusory at best, and not very interesting in the general case.

Note, in reference to the final remark of the preface to this section, that copying cannot be an allomorphy rule for it is completely independent of the morphology of the base. No matter what the base, the copying rule is always the same. If we got different copies in different morphological types of stems, then we might want to have recourse to a rule of allomorphy.

Summing up this section on copying rules, I have claimed that all copying rules are WFRs, and that the morphological operation of a WFR, rather than specifying a completely specified phonologically constant form, is in itself a unique phonological operation. If both of these claims are true, and we will assume them to be so, then we cannot state WFRs as simple labeled bracketings, but must state them rather as transformations. This in turn helps us to differentiate formally the affix and the stem, items which, intuitively are very different. Another point of this section is the fact that if we treat copying rules in a certain way, i.e. as WFRs, then we can ban such rules from the phonology. This result is the first phonological result of this paper, and I think that, if correct, it is a very important result. What I have done so far is to elaborate a theory of derivational morphology, on grounds which are completely independent of phonology, except of course, that I have accepted a particular, well-motivated on phonological grounds, theory of the phonological component, that of SPE, and on certain finer points, that of Kiparsky (1973). Despite the fact that my theory of morphology is not built on any phonological grounds, it has proven useful in solving a phonological puzzle of great particularity, that of abstract copied segments. It is the possibility of this sort of interaction which led me to investigate the entire area of morphology, in the hope that by discovering what was legitimately morphological, we might be able to determine what legitimately belong in other components of the grammar as well. We here see the first case of the (hopeful) success of this general method.

4.2.2.2. Infixing.

We have asserted that copying rules are WFRs, mainly on the grounds of "functional" similarity. We have been able to further assert that WFRs can not be written as simple labeled bracketings, because copying rules can't be so written. Because of the tenuous nature of the logic here, it would be better if we had independent grounds for the last assertion, that WFRs cannot be represented as labeled bracketings. Such an independent ground is the impossibility of describing infixes in terms of labeled bracketings. Consider as a simple the matter of the Hebrew Pi??e:l form, which is formed by reduplicating the second consonant of the root. The reduplication problem, which we have discussed, is independent of the question of where the copied consonant goes. As we see from the examples of (37), the copy goes next to the consonant it is copied from, and the rule has the effect of doubling the second consonant of the root. How can such a rule be stated? How do we specify the position of the copy? The copy goes inside the root, and therefore we must be able to factor the root, in order to know where inside it goes. Such a factorization is impossible if we restrict the statement of WFRs to labeled bracketings.

For the sake of clarity, and in order to avoid the problem of copying, which arises in this example, let us look at a very productive English infixing rule. The English infix fuckin, first studied, to my knowledge by Siegel,(1971),which more or less has the function of expressing a certain attitude on the part of the speaker, occurs in words like the following: (from Siegel)

(39) Mononga-fuckin-hela

Santa-fuckin-Cruz

$$\overset{3}{\text{fan}}\text{-}\overset{2}{\text{fuckin}}\text{-}\overset{1}{\text{tastic}}$$

The infix is restricted to stems which have a 3-1 stress pattern, and furthermore, can occur only immediately before the 1 stress, as the following unacceptable forms show:

(40) $*\overset{3}{\text{Mo}}$nong-fuckin-a$\overset{1}{\text{he}}$la

$*\overset{1}{\text{Tur}}$-fuckin-in

$*\overset{0}{\text{Chi}}$-fuckin-$\overset{1}{\text{ca}}$go

$*\overset{1}{\text{Chi}}$cko-fuckin-$\overset{3}{\text{pee}}$

Siegel states the rule for infixing <u>fuckin</u> as follows:

(41) <u>Fuckin rule</u> (Siegel p. 10)

$$\left[\text{x} \quad \text{V} \quad \text{C}_0 \; (\left[\underline{\text{V}} \; \text{str}\right] \text{C}_o')_o \quad \left[\text{infix}\right] \not{\text{V}} \quad \text{Y} \right.$$

The trouble with using such a labeled bracketing to express the rule in question is that there is no place in the rule where the base is specified as an independently existing entity. The rule as stated has no way of expressing the notion "formed from". It is incapable of encoding this notion and that of "infix" in a single string, because the infix is inserted inside the base (this is the meaning of the term <u>infix</u>). In order to be able to express these two notions, we must be able to factor the base string, and insert the infix between two of its factors, as below:

(42) <u>Fuckin rule (revised)</u>

$$\left[\text{X} \quad \overset{3}{\text{V}} \quad \text{Q} \quad \overset{1}{\text{V}} \quad \text{Y} \right]$$
$$\underset{1 \quad 2 \quad 3 \quad 4 \quad 5}{} \rightarrow 1 \; 2 \; 3 \; \text{fuckin} \; 4 \; 5$$
$$\text{where Q contains no } \not{\text{V}}$$

This statement of the rule allows us to express both the idea "formed from" and the infix. The form of the rule is the same as that of copy-rules like (33). All infixing rules must have this form. The general

phenomenon of infixation thus provides very strong evidence, independent of copying rules, for the impossibility of using labeled bracketings to express the morphological operations of all WFRs. This is assuming that the rule which places the infix in it's proper position is a WFR and not a rule of the phonology, i.e. that it is not ordered among the rules of the phonology. This assumption will be discussed in a later section, and is, as far as I can tell, essentially correct.

4.2.2.3. Consequences.

We have found two classes of rules which are best viewed as WFRs, and which force us to state WFRs in a particular manner, as transformations. This manner is different from that of the labeled bracket, mainly in that it forces us to divide the rule into two parts, a Structural Description, and a Structural Change. The first part specifies only the base. The second part contains the base and the result of the operation of the WFR, amalgamated into one unit. The formal nature of this bifurcation has an intuitive counterpart. Though the base is an independent entity, which we know already, for in order to qualify as a base, it must be an independently occurring word, and a member of a Major Lexical Category, the affix, (which in most cases is equivalent to the affixing operation) because it is nowhere given any representation of its own, cannot be separated from the rule. This intuitive counterpart is very different from the view which people normally have of affixes. An affix is an independently existing entity, according to the usual view. Just like a stem, it is a morpheme, and has all the properties a stem has.

This common view has led to many problems. I will mention only the two most commonly discussed. The first problem is that of dis-

continuous morphemes, like the Semitic vowel patterns. Though I cannot claim to have solved all the mysteries of Semitic morphology, it is clear that once we stop thinking of these vowel patterns as items of the same sort as the stems, we can stop worrying about the metaphysical import of these discontinuous patterns, and begin to develop a framework in which they can be studied. Another problem which this view of WFRs relegates to the status of an artifact is that of the zero morpheme. In English, there are WFRs with which no morphophonological operation at all is associated. Though the base undergoes semantic and syntactic changes, sometimes of a complex nature, nothing happens to its form. The most productive of these formsverbs from nouns. The semantics is very complex, and I don't know exactly how many rules are actually involved, but I have listed a few, of different types, below:

(43)

Noun	Verb
father	father
referee	referee
butter	butter
cement	cement
spear	spear
club	club
ship	ship
skate	skate
nail	nail
hammer	hammer
bale	bale

Within a theory in which WFRs are represented by labeled bracketings, or even simple concatenations, as the simplest theory supposes, how do

we represent rules like the above? The answer is the zero morpheme.
The rule taking <u>father</u> to <u>father</u> can then be represented as follows:

(44) $[[\text{father}]_N \phi]_V$

We can then refer to this ϕ as the suffix for forming verbs from nouns.
But the concept of a formless phonological substance like this is abhor-
rent, even ridiculous when we realize that for every WFR which has no
associated phonological operation, and there are several in English
(cf. M. 359-89), we must posit a separate such entity, with a resulting
proliferation of nul, one zero for every rule: $O_1, O_2,....,O_n$. Though
the zero morpheme is not a necessary entity in a theory which uses
labeled bracketings to represent WFRs (the theory of SPE uses labeled
brackets and no zero morpheme) is is quite clear that in the theory
being put forth here the zero morpheme has no place at all.

Lastly, we should note that the problem of a morpheme as a
meaningful entity, discussed at length in Ch. 2, though not resolved
within the framework being put forth, can now be reduced to the problem
of a WFR having meaning, since a morpheme is not independent from the
WFR which introduces it. The problem of the meaning of a WFR has been
approached by dividing a WFR into two parts, the central part, all of
whose operations, and elements are unique compositional, and discrete
(the base, the morphological operation, the semantic interpretation of
the output as a function of the semantics of the base, the syntax of
the output), and the morphological conditions on the base, which
determine productivity and the semantic coherence of the individual
output. Within such a framework, what should be constant about the
"meaning" of an affix is the syntactic category it is a marker of,
since the syntactic category of the output cannot vary with product-

ivity. This constancy is true to a very great extent. Words drift, and mono-morphemes, as noted (fn. Ch. 2), can drift just about anywhere. But morphologically complex words do not drift out of their syntactic categories. Also, rules which have no phonological reflexes, like the rules involved in the derivation of the items in (43), generally do not apply to morphologically complex bases. There is a miniscule number of exceptions to this observation, among which are proposition$_V$, referee$_V$ waitress$_V$, dirty$_V$, muddy$_V$. Marchand's explanation of this restriction is the fact that "suffixes are categorizers." The fact that suffixes are such strong markers of category is what we are predicting.

An interesting result of this last fact is that we can now have a somewhat finer view of the use of WFRs as rules of morphological analysis. When we encounter a word we have never heard before, one thing we can know pretty much for certain is the syntactic category of the word, (if it is polymorphemic), and this is about all we can know for certain, since this is the only constant part of the WFR, the only part which is unaffected by the morphology of the base. Once we isolate the affix and the syntactic category of the putative base, we can look at the morphology of the base, and if we know the meaning of the base, make guesses as to the "distance" of the newly encountered word from the base, on the basis of the coherence of the rule, which we know from the particular morphological category of the base. If the base is not a word, we know, as noted, nothing but the syntactic category of the new word. If the base is not a word, but is a member of a morphological category which is productive, we know more about the new word. If I have never heard the word tangible before, I know that it is an adjective, and that's all. If, on the other hand, I hear

the word solemnization, though I may not know the word solemnize, I know that the WFR of ation is very productive with bases of the form Xation and hence know that I am dealing here not merely with a noun, but with an abstract deverbal action. I think this is correct, though quite obviously it must be subjected to experimental verification.

4.2.3. **The Place of the Morphological Operation in the Grammar.**

WFRs have been viewed as rules for adding new words to a dictionary and rules for analyzing existing words. They are once-only rules; a word is made up by applying a WFR, and the newly made up word is added to the dictionary. The morphological operation has been claimed to be simultaneous with the other parts of the rule, and separate from the rules of the phonology. No part of a WFR can be a phonological rule, orderable among the rules of the phonology. Rather the word is formed entire, as a completely phonological entity, prior to all the rules of the phonology.

But this cannot be true. Consider the fuckin rule discussed above. The infix must be inserted in a word which has a 31 stress contour, and immediately before the 1 stress ($\overset{1}{K}$alama-fuckin-z$\overset{3}{o}$o). In order to know exactly where to insert the infix fuckin, we must know the stress contour of the base. But the stress is determined by relatively regular phonological rules. Therefore, the infixation process must be ordered after some phonological rules. The only way in which we can enter Kalamafuckinzoo in the dictionary entire, and not with some abstract marker, like [fuckin infixation], is to give up the entire theory of phonology, and enter the word in its surface form. We don't want to do this.

It appears that rules of infixation and copying are different from other WFRs, in that their morphological operations, which, as we have seen, depend crucially on the actual base, can be ordered among the rules of the phonology. The question that we must now ask is how they are entered among the phonological rules. If they are just phonological rules like any other, a possibility which we have repeatedly denied on external grounds, then they will have the same ordering properties in a phonological derivation, they will be subject to reordering, and what have you. If, however, we can show that these rules will intervene in the phonology, only at a specific number of places, then they are not phonological rules in the common sense.

To see how a rule could intervene in the phonology without being orderable as a rule of phonology, we must review the general conception of the sturcture of the phonological component, as outlined in SPE. In that work, phonological rules are sharply bifurcated into cyclic and word-level rules. Exactly what sorts of rules are cyclic, and what sort of word-level is a problem not discussed in SPE, and I have nothing to say on this matter. Cyclic rules apply first, cyclically, the limits of each application being determined by bracketings, which, we have argued, are determined by the morphology. Word-level rules are post cyclic, or last cyclic, and apply only once. From this outline, we see that there are several points at which a rule might intervene in the phonology, without being ordered strictly between two phonological rules. The rule might apply before the cyclic rules, as we have argued most morphological operations do, it might be ordered between two cycles, it might be ordered after all cycles, but before word-level rules, or it might be ordered after all

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word-level rules, that is, after the phonology. If we allow a phonetic component to follow the phonology, then these latter rules could conceivably follow morphological operations.

In the next section I will give evidence that morphological operations of reduplication may take place at two of the above breaks in the phonology. As we have assumed, they take place before the phonology. Secondly, they may take place after the phonology. This sharp restriction of the place of these rules shows that they are not phonological rules. We are thus allowed to retain the position that WFRs do not interact with phonological rules, though they do interact with the phonology. On the way, we also solve very puzzling problems in a very simple manner. Though these problems have been noticed before, previous solutions entailed very drastic revision and weakening of the theory of phonology. Again we see how a theory of morphology can lead to the resolution of phonological issues.

4.2.3.1. Reduplication Paradoxes.

I will now present a rather extensive discussion of selected re-duplication processes in a variety of languages, and show that the phonological peculiarities of these processes can be easily accounted for if these processes operate at the places designated above, and at no other places. The widespread peculiarities of reduplicated forms cannot be dealt with in any other principled manner.

The data for the following section all comes from Wilbur (1973). Transcriptions vary with her sources. Wilbur begins from the observation that reduplicated forms are often exceptional, at least when viewed from the theoretical standpoint of standard generative

phonology. Their exceptionality lies in the fact that the reduplicated affix (R_r), and the part of the stem of which it is a copy (R_o), are often identical in their surface phonological representations. If we assume that reduplication is a morphological process which precedes all phonological processes, then this surface identify is sometimes only attainable at great cost, because the phonological rules, applying blindly, will produce different reflexes of R_o and R_r. The problem is to assure that this will not happen, that R_o and R_r will be identical.

It is important to notice that this problem does not always arise. It is not always the case that the two are identical. The following derivation, from Wilbur, of an Akan reduplicated form, demonstrates how such a situation can arise:

(44)   /dum?/   +Redup $(C_1V_1C_2)$

    Reduplication   dum dum?

    Regressive Homorganic Nasal Assimilation   dun dum?

    Progressive Nasal Assimilation   dun num?

    Closed Syllable Vowel Nasalization   dŭn nŭm?

    Output   dŭnnŭm?

Such a situation is normal within a theory which assumes that all copying takes place previous to the phonology. The rules, which are independently motivated, apply in their proper order with no regard for extrinsic facts, i.e. that this is a reduplicated form, and an incidental result of their application is that R_r and R_o are made dissimilar. This is the situation with which I am familiar. It holds in all the Semitic languages, in Greek, where the reduplicated initial consonant of the perfect prefix is subject to deaspiration,

and, I am told, in Sanskrit. In all these familiar cases, if re-
duplication is prephonological, then everything goes through
normally.

A simple example of an exceptional case is the following Madurese
form:

(45) /kun? "order   /kunkun/ "orders"

The form is exceptional because an otherwise general rule of nasal
assimilation which would give us the form */kungkun/ has not applied.
Nor is this an isolated form. Nasal assimilation does not apply to
reduplicated forms:

(46) /bangbang/ "wings"   */bambang/

/b-ar-ing-bing/ "stand on end" */barimbing/

/d-al-ang-dang/ "tall and thin" */ dalandang/

/t-ar-em-tem/   "peaceful"   */tarentem/

If the reduplication process precedes all the rules of the phonology,
then reduplicated forms all have to be marked as exceptions to the
phonological rule of nasal assimilation, for this rule fails to apply,
though its structural description is met. Wilbur presents several
other examples of this sort of exceptionality, where, within a
conventional theory, we must say that a rule has failed to apply in a
reduplicated form, with the result that R_o and R_r arc identical.

All of these cases can be handled simply be ordering reduplication
after the relevant phonological rule, in fact after all phonological
rules. This device accounts for the non-operation of the phonological
rule, and for the identity of the forms R_o and R_r. It is also
possible, by the proper manipulation of boundaries, to achieve the
same result. Boundaries, however, will not suffice for the next

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class of phenomena.

A more curious type of exceptionality, curious, that is, within the conventional framework, is one in which a rule seems to over-apply, i.e. applies to a segment whose environment does not meet the Structural Description of the rule. In the following Chumash data, a rule of aspiration is involved which combines a voiceless consonant with a following /h/ or identical consonant, to produce an aspirate.

(47) /k+kuti/

/k+kut+kuti/	Reduplication
k^hutkuti	Aspiration "to look"
/ma+k_hawa?/	
ma+k+hawhawa?	Reduplication
mak^{h}awhawa?	Aspiration "aunt"

We see that in these cases reduplication, and infixation, precede the phonological rule of Aspiration, which then makes R_o and R_r dissimilar. In the light of the examples in (47), consider those in (48):

(48)	Base	/s-soyin/	/ma-k-hatinet/
Redup (C V C) 1 1 2		s-soy soyin	ma-k-hat hatinet
Expected		* s^{h}oysoyin	*mak^{h}athatinet
Actual		s^{h}oys^{h}oyin	mak^{h}atk^{h}atinet
Gloss		"it is very black	"my joints"

The forms of (48) can be derived simply by ordering reduplication in these cases after the Aspiration, in fact after all phonological rules.[3] The difference between the forms of (47) and those of (48), is due to the relation between the reduplication rule and the phonology in the two cases. In (47) reduplication precedes the phonology, in (48) it follows. Note that there is no question of Reduplication being ordered

among the phonological rules themselves. This is impossible, because
the rule of Reduplication is not a phonological rule. I think the
difference between the two sets of data gives striking confirmation to
our theory, for it is just these two sets, and only these two sets,
which our theory permits, and it is only these two which actually
occur. Thus, by making one simple addition to the theory of mor-
phology, we can account for all and only the observed irregularities of
reduplicated forms.

There are other ways to account for the forms of (48). One
could reduplicate the prefix as well as the first consonant of the
root in these cases:

s-soy-s-soyin   ma-k-hat-k-hatinet

Aspiration will then apply to produce the correct forms. But in
this solution we have two very different reduplication processes, one
of which produces the forms of (47), and the other the forms of (48),
whereas in the ordering solution the rule is the same, only its
place is different. Since the utility of ordering has already been
shown, and that of changing the form of the rules does not have as
general an application, we must suspect the latter, in the same way
as we suspect a solution involving boundaries in cases like (46).
Boundaries can handle cases like (46), and a change in the rule can
handle cases like (48), but the ordering theory can handle both, and
in a principled and highly restricted manner, which cannot be claimed
for the other solutions.

Within the theory which orders the reduplication operation at
breaks in the phonology, only a small subclass of the data generable
by a theory in which reduplicated forms are "exceptional" is actually

possible. The above examples demonstrated this point in a simple

case. We will now turn to a much more complex case noted by Wilbur,

that of Tagalog reduplication.[4]

First, let us motivate a few relevant rules. There are prefixes

/pang/,/mang/, and /nang/. 'ng' is being used to transcribe /ng/. It is

one segment. The /ng/ interacts with the first consonant of the stem:

(49)   pang-pálo?   → pamálo?

       pang-bilmìt   → pamilìt[5]

       pang-táli?   → panáli?

       pang-dalángin   → panalángin

       nang-saríwa?   → nanaríwa?

       nang-kapā?   → nangapā?

There are two sorts of exceptions to this process. A large number of forms

go unchanged.

(50)   pangparikìt

       pangbambò

       pangtakìp

       pangdilìg

       pangsakày

This seems to be a principled class, and is marked by the pattern

XVC$_o$#. Secondly, when the stem has an initial liquid, the nasal

assimilates, and the liquid remains:

(51)   manlibák

Wilbur also cites the form <u>mandurukit</u>. The last two cases can

be accounted for if we say that there are two rules involved in the

derivation of the forms in (49). Firstly, a rule of regressive nasal

assimilation assimilates the nasal to the following consonant.

Secondly a rule deletes a consonant (non-liquid) after a nasal which is homorganic with it. By this theory the form mandurukit is an exception to the consonant deletion rule. The forms of (50) can be accounted for in two ways. Either they are principled exceptions to the nasal assimilation rule (principled in the sense that they have the same stress pattern), or the rule which adds the prefix is ordered after the phonological rules of assimilation and deletion. From the data given, we cannot decide between the two theories.

Note that the form mandurukit has been subjected to another independently motivated rule which takes d̲ to r intervocalically.

Reduplication can be ordered after the rules discussed above, as can be seen from the following:

(52)

Base	Gloss	Modal	Future (Redup. $C_1 V_1$)
/bigáy/	"give"	mamigáy	mamímigáy
/sumpâ/	"curse"	manumpâ	manunumpâ
/isdá/	"fish"	mangisdâ	mangingisdâ
/libák/	"scoff"	manlibák	manlílibák

The form manlilibák shows that the rule cannot reduplicate underlying C+C as was possible in the case of the Chumash rule. The form mangingisda, where the last consonant of the prefix is reduplicated, on the other hand argues that the Chumash solution is correct. The only principled way in which both manlilibák/*manlinlibák and mangingisdà/*mangiisda can be accommodated is by ordering reduplication after the relevant phonological rules, and adopting the boundary adjustment hypothesis of fn. 3. According to this solution all the forms of (52) are equally regular. No other solution can account for all the forms in the same manner.

So far we have five rules, which apply in the following order:
(4 is not ordered WRT 2,3).

1) Prefixing (Morph.) /pang/, mang/,nang/, are prefixed to the stem.

2) Nasal assimilation: ng → $\begin{array}{c}\text{λcoronal} \\ \text{λanterior}\end{array}$ / $\underline{\quad}$ C $\begin{array}{c}\text{λcoronal} \\ \text{λanterior}\end{array}$

3) Consonant deletion: $\begin{array}{c}\text{+cons} \\ \text{-voc} \\ \text{-syll}\end{array}$ → ∅ / +nasal + $\underline{\qquad}$

4) Flap rule: d → r/V_V

5) Reduplication: X $\begin{bmatrix} & \text{CV} & \text{Y\#} \\ \text{1 root} & \text{23} & \text{4}\end{bmatrix}$ → 123234

(1) and (5) are morphological rules, and can be ordered either before or after the other rules, though all the examples we have seen so far obey the proposed ordering. The other rules are phonological, and cannot be reordered. Therefore, beside the ordering given, which is the regular one, we can have the following: 2,3,4,1,5. 1,5,2,3,4. (5) cannot precede (1) for external reasons having to do with the meaning of the two rules.

As noted above, the forms of (50), which do not involve Reduplication (Rule 5), can be handled by the order 2341. There are also forms which show the same exceptionality as those of (50) with regard to rules 2,3,4, and are reduplicated:

(53) pang-bambo "a club for beating" 2,3,4,1.

nang-ba-bambo "is beating" 2,3,4,1,5.

pang-dilig "implement for sprinkling" 2,3,4,1.

nang-di-dilig "is sprinkling" 2,3,4,1.5.

We thus account for one class of "exceptions" by using one of the alternative orderings permitted by our theory. Note that no other solution than ours can account for the last two forms of (53) in an

interesting manner. These forms are "exceptions" to Rule 4. This is
predicted by the fact that 5 follows 1, and 1 in these examples must
follow 4. Since 5 feeds 4, in that it provides the intervocalic d̲,
the fact that it is ordered after it prevents d̲ from being realized
as r̲. We thus see that the majority of the "exceptional" forms can
be accounted for by the ordering 2, 3, 4, 1, (5), which our theory
permits, and that furthermore this ordering accounts for an interest-
ing class of "exceptions" within "exceptions", and shows that they
are not exceptions at all.

There is one form which argues for the ordering of Reduplication
before all the phonological rules. This is the form mandurúkit cited
above. It contrasts with the forms just discussed in that the
intervocalic d̲ which arises from Reduplication, has undergone rule 4,
and been changed to r̲. Note, however, that this fact is accompanied
by the assimilation of ng̲ to n̲ before d̲, which indicates that Rule 1
has also applied prior to the phonological rules. Note that this
form is listed as alternating with the form mangdudúkit which shows
the order 2, 3, 4, 1, 5.

The only other alternating form listed in Wilbur, and the last
"exceptional" form she lists is the following:

(54) pangungumı̀t/panguùmı̀t

The first of these forms can be assigned the order 2,3,4,1.5. The
second can be assigned the order 1,5,2,3,4, (on the assumption that
boundary adjustment is a syllabic process, and hence not available
in this instance, an assumption which is borne out by other forms in
which Redup. is applied to underlying forms: um-i-íbig "wish").
Since we have only a limited number of forms, we can only speculate

on the import of these last cases. The only forms which can be derived by the order 1, 5, 2, 3, 4, have alternate forms which are derived by 2, 3, 4, 1, 5. No other alternate forms are listed, and no other forms show alternation.

What the above examples, which exhaust the classes of examples in Wilbur, show very clearly, is that by assuming that the morphological operations of WFRs are orderable in certain very restricted ways with respect to the rules of the phonology, we can account in a highly principled manner for all and only the forms listed. Since no other account of the above data meets these three conditions (principled, all, only) we must accept these T_agalog data as striking confirmation of our theory. Again I must caution that the above is not an exhaustive analysis of Tagalog, but merely an analysis of the few forms listed in Wilbur. Further work is necessary before the analysis is accepted as confirmed.

I will now turn to a phenomenon, which though it can be assimilated into the theory presented, can only be so assimilated if we are content to leave something unstated, which, in some other theory, might be statable in a principled manner. I will only give a rough outline of the problem, and the reader is encouraged to look at the original presentation, which is in Munro and Benson (1973). The language is Luiseño.

Three rules are of import here. First, a rule of syncope deletes a vowel preceded by a short stressed vowel and a single consonant and followed by a single consonant and a single vowel, as in the following:

(55) caq^{w}i-   "to seize"   cáq^{w}la-   "to wrestle"

The second rule raises unstressed mid vowels to high vowels:

(56)   hédi-   "to open"   /   hidíki-   "to uncover"

The third rule, which we will call SH, changes <u>c</u> to <u>s</u> before a non-continuant, or #:

(57)   té:nalis̆   "medicine"   té:nalicum   "medicines"

         qe:nis̆   "squirrel"   qe:nicum   "squirrels"

SH applies to the output of syncope:

(58)   ʔe:ci   "above"   ʔeskawis   "upper lip"

         moci-   "to weave"   moš́lat   "belt

Stress is governed by a complex set of rules, and in turn governs certain vowel deletion and shortening rules, which are discussed in the sources.

What is important for our purposes is the interaction of the above three rules with Reduplication processes. Normally, Reduplication applies to underlying forms:

(59)   <u>Sample derivations</u>

        <u>Rule</u>   /capomkat_um/   /cik^{W}i:-/

        Redup C₁V₁   cacapomkat+um   ---------

        Redup C V C   cik^{W}cik^{W}i:-
              1 1 2
        Various rules   cacapomkat+un   cik^{W}icik^{W}i:-

        Syncope   cacpomkat+um   cik^{W}ick^{W}i:-

        SH   caspomkat+um   cik^{W}isk^{W}i-
                   "liar"   "to be sad"

There is a class of reduplicated forms, formed by what is termed Adjective Reduplication, which always have the surface form

C V C V   -C C V   -s
1 1 2 2   1 2 2

(60)   ʔáva-   "to be red"   ʔaváʔvas̆   "pink"

         máha-   "to stop"   mahámhas̆   "slow"

sáwa   "to wheeze"   sawáswaś   "hoarse"

This is the only reduplicated form which is de-intensificative, it

means "a little," and not "a lot." These forms can be generated

in the usual manner except for one exception. When the first

consonant of the C_1C_2 sequence is a ć, it does not undergo the SH

rule as expected. Instead of *carásras, which is the expected form,

we get caráćras. Similarly ćukáćkaś and not*ćukáskaś. Note that if

we do reduplication postphonologically we rid ourselves of the

exception. We will write the rule as an infixing reduplication rule:

(61) <u>Adjective Reduplication Rule</u>

C V C V X#
1 2 3 4 5 →1 2 3 4 1 3 4 5

The change in stress can be accounted for by including the infix

C C V in a class which is motivated independently, which attracts stres s
 1 2 2
to the syllable immediately preceding it.

However, there are two problems which face this simple rule (61).

First, by reduplicating $C_1C_2V_2$, instead of $C_1V_1C_2V_2$, a step which is

necessary if we are to account for the fact that SH does not apply to

these forms, we lose the possibility of accounting for the absence of

V_1 by the perfectly well motivated and otherwise general rule of

syncope. For, if we allow syncope to apply here, then how can we

disallow SH, which follows syncope (cf. 58), except by some ad hoc

exception marker, which we are trying to avoid? There is no way to

account for both facts in a principled manner, within the ordering

theory, the fact that SH has not applied, which is patent, and the

fact that syncope could have applied. The second problem is the

application of raising of o to u in ćukáčkaš, from the stem čoka-

"to be limp". Since stress is not determined until after reduplication, it must be presumed that raising has taken place after reduplication, which is a problem if reduplication is postphonological.

The first problem I have no solution for. It is true that the vowel in question could have been deleted by syncope, and that our theory denies this, and replaces what could have been analyzed as one process as two separate and unrelated ones. Note that there is no evidence that syncope must have applied. This brings us back to the problem of real generalization which we dwelt on without any conclusion when we first encountered reduplication rules. I have nothing further to say on that point.

The second problem is less trying. Raising is a late, phonetic rule. Reduplication will apply before these rules, and hence raising will apply to its output. We can conclude that the Luiseno data, though it can be accommodated in our theory, cannot be completely explained by it. Whether this is a problem must remain unknown until we have a better idea of what we are trying to explain.

There is a conceivable type of "exception" which cannot be handled by ordering of any sort, and hence is beyond the power of our somewhat restricted ordering theory. Wilbur describes what such an exception would be: if a rule of the phonology, whose Structural Description is not met until after a reduplication rule has applied, and which applies to R_r (the reduplicated part), also applies to the corresponding segment of R_o, even though this segment is not in the proper environment for the application of the rule.

She gives a hypothetical example. Let us presume that a language has an intervocalic voicing rule. In this language, a form <u>inuk</u>

could be reduplicated, and then undergo the voicing rule, to give inuginuk, or it could be reduplicated after the voicing rule had a chance to apply, and have the surface form inukinuk. There is no way, within an ordering theory of any sort, to derive the third possible form inuginug, in which the rule of voicing applies both to intervocalic k, and to the final one, which is its "mate" in a certain sense of the term. The sort of theory in which such a fact could be accommodated is discussed at some length in Wilbur. Needless to say, it is much more powerful than the one I am proposing. Since the ordering theory, any ordering theory, cannot accommodate such an example, and the other can, we are left with an empirical issue, and a question: Do such forms as that exemplified by our hypothetical case ever appear in natural languages? If they do, then any ordering theory is incapable of dealing with natural language, and must be abandoned. What we must do is go out and hunt for real cases.

Wilbur cites two "possible examples." Both examples are isolated words. One is noted by her source as the only case of its kind in the language, and Wilbur quite correctly hesitates to say that it is crucial. The other example may be the result of a typographical error, for the author's discussion of the word in question seems to imply that it has another form (cf. Hill, 1969, p. 362).

The fact that Wilbur has been able to find only these two words, one of which is an exception in the language, a unique exception, and the other of which may be spurious, seems to me to provide very strong evidence in favor of an ordering theory. Such a theory precludes words of this sort from being derived by regular morphological and

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phonological rules in a principled manner. Isolated forms, whose deriv-
ation is uncertain, cannot be considered as decisive evidence, except
insofar as they are isolated.

Implications.

All the above is strong evidence in favor of a theory in which
morphological operations can take place at certain very specific
places in a phonological derivation. There is no question of these
operations ever interacting with individual phonological rules. We
have shown that reduplication rules may have the peculiarity of
operating at the end of the phonology. The same can be easily
demonstrated for other infixation rules, like the English fuckin rule.
One question we may ask is whether this odd ordering is due to the
nature of these rules, which as we have noted, must have access to the
internal workings of the stem and base, unlike other rules, in order
to spell out the form of the affix they attach. We will see in the
next section that the same orderings apply in the case of other rules
as well, rules with constant affixes, and that the only difference
between reduplication and other morphological operations, is that
because of the ron-constant nature of a reduplicated affix, it is
easier to discern the interaction of reduplication with the phonology
than in other cases, where we must have recourse to evidence of a
much more roundabout nature.

Another implication, and here reduplication and infixation differ
from other operations, is that in cases where these rules apply at
points other than the the input to the phonology, there is no way to
list the output of the operation in a dictionary, without drastic
alteration of our views of the nature of phonological representation

and the role of phonological rules in a grammar. The WFR must carry an abstract marker like +redup which is attached to the base. At the appropriate place in the phonological derivation of the word, this marker triggers reduplication, and in some cases also infixation. Such cases obviously contradict the general statement that WFRs are once-only rules, for word-level reduplication cannot be so defined. I have not explored the further implications of this fact.

4.3 Phonology.

4.3.1. Phonological Conditions.

Phonological conditions are analagous to morphological conditions on the base. However, phonological representations differ from morphological representations in that a given morphological word has many phonological representations associated with it. There is the underlying representation, the surface representation, and all points in between. Whether WFRs can have access to any one of these levels of representation is an empirical question. In addition, when dealing with phonological conditions, one must differentiate the base of a WFR from its output, at least with respect to surface phonological form, for, due to the action of phonological rules, the surface form of the output will not always include the surface form of the base.

We have stressed the independence of WFRs from other rules, though not from other components of a grammar. Even in the light of the last section, we can maintain the separation of WFRs from the rules of the phonology, for in that section we showed that though certain of the morphological operations of WFRs can be ordered at

breaks in the phonological derivation, they never have to be ordered
between rules of the phonology at other points than these breaks.
In a parallel fashion we will exclude the possibility that phonological
conditions on WFRs are statable at points between phonological rules,
which are not breaks in the phonology. This means that phonological
conditions are statable at exactly the same points at which morphologi-
cal operations are applicable. In addition there is the possibility
of their being statable on the surface form of the output, a possibility
which does not arise in the case of morphological conditions, which
are statable, by their nature, only on the base. This exclusion
embodies an empirical claim which is easily falsifiable. We only need
to find a phonological condition on the application of a WFR which
is statable only at one of these many forbidden points.

4.3.1.1. <u>An Aside Concerning Negative and Positive Conditions</u>.

We found morphological conditions of two types, negative and
positive. The latter formed the overwhelming majority. We also found
a difference in the role of the two types of conditions. Positive
conditions were correlated with productivity statements, and thus
could vary in their strengths, while negative conditions were more
absolute, and like the positive syntactic specifications of the base,
allowed few, if any, exceptions (cf. the discussions of X#ment and
comparatives). With phonological conditions it is difficult to
bifurcate the classes. First of all, there are many fewer phonological
conditions. Most of them are very strong, and allow almost no
exceptions, but these are both positive and negative. They will say
that a given WFR never operates on bases of a certain type, or that
it only operates in certain environments. There is seldom any

question of sometimes, or often. One of the few cases where a phonological condition seems to be analagous in its function to a positive morphological condition is that of a supposed positive restriction on the affix en (deaden, toughen). By far the largest number of words in the affix have bases which end in dental consonants (cf. Siegel, 1971, app. II). When we look at the list, this is merely statable in terms of the largest number of actually occurring forms, and there are exceptions (toughen, freshen,weaken), however, according to M (272), in the last two hundred years, only verbs from adjectives in t and d seem to have been formed. As an active condition, then, it is absolute, and it is only the large number of residual words which might cause us to doubt this. It is possible that all phonological conditions, whether negative or positive, are absolute, and that the cases which seem looser, only seem so due to historical residue. We will assume that this possibility is fact, and that all phonological restrictions, be they positive or negative, are absolute. This assumption (which embodies an empirical claim) removes the similarity that one may have thought to exist between morphological and phonological conditions.

4.3.1.2. Conditions on the Underlying Form of the Base.

The condition on -en just discussed is a condition statable on the underlying form of the base. Note that though the information is available at later stages it is first available at this point.

Another condition which is statable on underlying forms, but which is not statable at all later stages, is discussed in Siegel (1971). She notes the fact that the adverbial suffix ly does not attach to adjectives ending in ly, as in the following:

(62) | Adj | Adv |

Adj	Adv
fresh	freshly
shameful	shamefully
ardent	ardently
likely	*likelily
ugly	*uglily
holy	*holily
wily	*wilily
silly	*sillily
slovenly	*slovenlily

This condition cannot be stated on the surface of the output, in
Siegel's southern speech at least, because of the following permissible
forms:

(63) | Adj | Adv |

Adj	Adv
shallow	shallowly
callow	callowly
mellow	mellowly
hollow	hollowly

In Siegel's speech, hollowly has the surface form [haleli],
where the last four segments are the same as the last four of holily
[holeli]. Underlying /li/ and /lo/ are neutralized on the surface of
the output.

We will assume in the light of this evidence, that there is a class
of restrictions statable on the underlying form of the base. Note the
correlation of this fact with the fact that there are WFRs whose
operations are definable on the underlying form of the base.

4.3.1.3. Stress Sensitive Affixes.

We noted in passing that the infix _fuckin_ was sensitive to the stress of its base. It is obvious that this phonological condition cannot be stated until the stress of the base is assigned. In this particular case, the infix is inserted after all the phonological rules have been applied. Both the base and _fuckin_ have the stress contours that they would normally have in isolation, and the infixing affects neither one, at least in terms of their segmental phonology and the relative stress levels in the base and in _fuckin_ considered separately. The 1 stress of _fuckin_, however, is subordinated to the 1 stress of the base as shown below:

(64) Kalamazoo   fuckin   Kalamafuckinzoo

This can be achieved if we insert _fuckin_ as a full word, ##fuckin##, into the surface form of Kalamazoo. The Nuclear Stress Rule will then apply, as in Madison Avenue, to give the correct stress contour.

What we see here is a striking correlation between three things: the point at which the phonological condition on the WFR is stated, which is the phonological surface, the point at which the morphological operation of the WFR (infixation) is performed which is the same phonological surface, and the boundary associated with the affix.

Another stress sensitive WFR is the rule which attaches the nominal affix #al, discussed in Siegel. This affix occurs only after a stressed vowel, followed by an optional glide, and no more than one consonant:

(65)   trial, denial, refusal, dismissal, *constructal,

       *organizal

The only exception to this rule is _burial_. Note here that the affix must be attached with a # boundary. Again there is a correlation between boundary and phonological condition. The difference between the last example and this one is solely that the morphological

operation here is not one of infixation, or of reduplication.

Let us assume that boundaries encode the place in the phonological derivation of the base of a WFR, at which the opera _ion of the WFR has been performed. Phonological conditions are correlated with the operation. If an operation takes place at a certain point, the phonological conditions must be stated at that point. This means that there will be as many different boundaries, and types of phonological conditions, as there are levels at which morphological operations may take place. In such a theory, the only difference between an infixation or reduplication rule, and other rules is the fact that the former cannot always be once-only rules. With an affix like #al, we can use the boundary to encode the place of the morphological operation. We never need to repeat the operation, once we have formed the word, because the boundary will make sure that the affix is not processed by the phonology until the correct point in the derivation. Though the boundary must be inserted by infixation rules as well, as we have seen, the character of the rule makes it impossible to use the boundary as we can with constant affixes. On the supposition that there are indeed only as many types of conditions and boundaries as there are levels at which morphological operations can take place, and that there is a perfect correlation of these (a very strong supposition), we can say that the WFRs which do not depend on the stem for their operation are exactly the same as those which do, except in the one respect in which we already know them to differ.

4.3.1.4. +Boundaries and Cycles.

In Ch. 2 we asserted that the phonological cycle is determined

by the morphology. There is a cycle on every affix, a cycle for every WFR. It might be possible that phonological conditions on WFRs are stated cyclically, that there is some phonological condition which cannot be stated until its affix is first seen by the cyclic phonology. This possibility, if realized, within a theory such as ours which correlates the point in a derivation at which a phonological condition is stated with the point at which the WFR on which the condition holds is said to operate, would entail that WFRs operate cyclically, as the first rule of each cycle. This in turn would destroy the complete correlation between boundary and WFR and condition which we have hypothesized. In the normal case, empirical verification of this question: are there phonological conditions which must be stated between cycles, depends on what the nature of the cyclic rules is. Can such rules change segments in a drastic enough way for there to be an actual difference between a condition stated at the underlying level, and one stated at a cycle boundary? The answer thus depends on thorough analysis of cyclic phenomena.

The first place to look would be in a language in which there were many reduplicated affixes, all of which, in the above terms, are +boundary affixes, and which have the further property of being able to stack up: if X is a stem, and a, b, c, d, are reduplication rules, is there a difference between the following two conceivable manners of deriving the form X+a+b+c+d:

(66) i1. WFRs a, b, c, d 2. cycles a, b, c, d

ii1. WFR a, cycle a

2. WFR b, cycle b

3. WFR c, cycle c

4. WFR d, cycle d

It is always the correct choice and always gives the right results,
then there cannot be cyclic conditions on WFRs. If $\underline{i}$ gives the
right results, then there could conceivably be such cyclic conditions.
Mary-Louise Kean, in her work on such case in Klamath, has found that
$\underline{i}$ is always a possible way of deriving the correct form. Tentatively,
then, we can say that cycles are not relevant for the operation of WFRs.
+ boundary WFRs operate on the underlying form of the base, no matter
how many of them there are, simultaneously: # boundary WFRs operate
at the surface, again simultaneously.

A possible explanation for their being no cyclical conditions
lies in a proposal of Kiparsky's (lectures, 1974). Kiparsky has
proposed that the operation of the phonological cycle is determined by
the number of affixes present in a word. In order for cyclic rules to
operate, there must be at least two morphemes in the domain of the
cycle. There is never a stem cycle unless the stem is bare. Now
remember that phonological conditions on WFRs are absolute, and hold
for every case in which a WFR operates. Consider the hypothetical
affix+a. If +a is attached to a stem X, which is monomorphemic, we
have the underlying form X+a. Since there can be no cycle on X, +a
cannot depend on cyclically introduced information. If in another case
+a is attached to a base X+b, then according to Kiparsky, there will be
a cycle on +a. But if conditions are absolute, then the fact that +a
cannot depend on necessarily cyclically introduced information in X+a
entails that it cannot depend on such information in X+a+b. Therefore
there can never be cycle dependent conditions on +a, for if there were
then +a could never be attached to a bare stem. If all +boundary

affixes have the property of being able to appear as the first affix
next to a bare stem, it follows that no affix which has a + boundary
affix can have derived conditions on its attachment, wherever it
appears in the order of affixes in a particular word. If the
phonological conditions cannot by cycle-dependent for any +boundary
affix, then the attachment cannot be cycle dependent. Therefore all
attachment is at the underlying level, for + boundary affixes.

4.3.1.5. # Boundaries and Cycles.

In Chp. 2 we asserted that the phonological cycle is determined
by the morphology. There is a cycle for every boundary. It has often
been noted that # boundary affixes block the application of cyclic
rules. In English # boundary affixes never affect stress. The fact
that there is no cycle on these affixes shows our simple theory of the
cycle false. It is not difficult to see, within the context of the
present discussion, why # boundaries block the cycles. They are the
reflex of the fact that the affix was added at a point in the phono-
logical derivation after the application of all cyclic rules. It
follows from this that no cyclic rule will every apply to them, that
they will block the cycle.

4.3.1.6. A Theory of Boundaries and Brackets.

A) There are as many types of boundaries as there are points
in a phonological derivation at which WFRs may operate.

B) The boundary is determined by the point of operation (There are
+ boundary WFRs, # boundary WFRs, ## boundary WFRs).

C) The phonological cycle is determined by + boundary WFRs:
there is a cycle for every such WFR, except the first.

D) There are no global phonological conditions on WFRs.

I don't think there is anything startling in the above theory. It tells us about the interaction of the morphology and the phonology. The morphology does not completely determine the phonology as one might naively think, for every WFR must carry a boundary, and the cycle is limited to one type of boundary. Now we see the point of the assertion that every WFR has a constant boundary associated with it.

4.3.2. Problems.

4.3.2.1. A condition on the Surface Form of the Output.

The deverbal affix #ment, aside from its other peculiarities, attaches productively to bases ending in the coronal fricatives š, tš, dž (abridgement, estrangement, impeachment). This fact is correlated with the lack of productivity of the rival suffix ation with the same phonologically determined class. There is a curious reason for the nonproductivity of ation in these exact cases. The reason is that a rule of English phonotactics rules out the occurrence of two coronal fricatives in adjacent syllables. It is not only words in ation which obey this rule. The rule of #ish gives evidence for it as well:

(67) sheepish, piggish, (fishish, (drudgish.

We are therefore dealing with a phonological surface condition which is completely independent of any one WFR. We can clearly see that the condition has reference to no other level from the fact that a form like admonition, derived, as we shall see in Ch. 4, by the ation rule and not by a separate ion rule, from admonish, which ends in the forbidden coronal fricative, is permitted because it does not violate

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the condition on <u>the surface</u>.

Note that there is no question of globality. There is no
constraint on <u>ation</u>, but rather a completely independent one which
just happens to affect some otherwise possible <u>ation</u> forms. This is
important. We have insisted that there are no global phonological
constraints on WFRs. All seemingly global phenomena must be traceable
to other sources. This one is.

Returning to ∤<u>ment</u>, we can trace its productivity with bases in
coronal fricatives to the completely external phonotactic rule. Because
the <u>ation</u> forms are forbidden, no blocking applies here. The nominal
slots of the verbs in question are open, and ∤<u>ment</u> fills them.

4.3.2.2. <u>A Global Phonological Condition</u>.

There appears to be a rule-particular condition on the surface
form of the output of the <u>en</u> affix discussed above. In addition to the
already noted condition on its base (ending in <u>t</u> or <u>d</u>), Siegel notes
that on the surface <u>en</u> must be preceded by one and only one obstruent,
preceded by an optional sonorant, preceded by a vowel (<u>glisten</u>, <u>harden</u>,
<u>dampen</u>, <u>whiten</u>, <u>frighten</u>). (I know of only one exception to this
remark: <u>brisken</u>). This restriction is not one statable on the under-
lying form of the base, for there are forms like <u>fasten</u>, <u>soften</u>, with
underlying obstruent clusters, which do not reach the surface, because
of the application to them of a rule which deletes <u>t</u> after an obstruent
and before <u>n</u>. Furthermore, the condition cannot be a surface one,
since there are words like <u>Boston</u> which violate it. <u>Boston</u> is not a prob-
lem for the <u>t</u> deletion rule, which can be written in such a way as to
generate it and others like it properly. However, it creates a

problem for our restriction, which must now become either a global condition on the en WFR, or an accident.

If we can find one other rule besides t deletion which serves the function of converting the output of the en rule from a form which is not in accord with the canonical pattern into one which is, then we have evidence that the canonical form is less likely to be an accident, and that we are indeed dealing with a global constraint on en, which allows us to form a word in this suffix just in case the word reaches the surface in the correct form.

The rule which meets this description is somewhat controversial, for it concerns a segment which undergoes absolute neutralization, the one written gh, and presumed to be a velar fricative. Arguments for the existence of this segment as an underlying phoneme of English are given in SPE (233-4) and Pope (1972). The rule in question deletes gh before t in words such as lighten, frighten, straighten. Believing in the rule presupposes our believing in the possibility of absolute neutralization, but if we do, then this is evidence that there is a global phonological constraint at work in the derivation of words in the suffix en.

4.3.2.3. A Transderivational Constraint.

A remark of Pope's, and of Siegel's, which the latter attributes to Morris Halle, suggests that a device even more powerful than the last is at work in the derivation of en words. en normally attaches only to monosyllabic adjectives, but there are several exceptions to this pervasive restriction. Pope notes that "-en attaches to the noun, rather than to the adjective, only when the adjective form would be unacceptable " (p. 126). As instances of this she cites the words

heighten, lengthen, and strengthen. -en, she claims, is attached to the nouns height, length, strength, because they meet the pattern which -en demands, while the corresponding adjectives high, long, and strong, if concatenated with -en would result in the forms *highen, *longen, *strongen, which are unacceptable because of the surface constraint.

Pope's remark, however, is not correct. There are instances of X-en where X is clearly a noun with no morphologically related adjective. They are threaten, hearten, and frighten. In addition there is hasten, which might be derived from hasty, but which is more likely to be derived from haste. Frighten may be semantically related to afraid, but the morphological relation is only tenuous. It thus appears that of the 7 denominal exceptions to the -en rule, $3\frac{1}{2}$ can be explained by Pope's remark, while $3\frac{1}{2}$ cannot. Additional evidence is hard to come by. The only adjective/noun pairs which meet the necessary requirements, that is, the noun, but not the adjective meets all the conditions, are those in which the noun is in th and the adjective ends either in a sonorant or vowel, or in an obstruent which is deleted in the course of a phonological derivation, i.e. gh or ng. The only pairs which meet this complex of criteria are true/truth, and slow/sloth. (The latter only if we are willing to stretch our tolerance). There are only two other monosyllabic adjectives in ng besides long and strong, namely wrong and young, of which one has no th nominal, and the other may be related to youth, though not by any rule of English. There are no other monosyllables in gh besides high.

In the absence of the possibility of any further evidence being procured, it would seem to be advisable, in this case at least, to reject the sort of device which an explanation on the order of Pope's

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entails, despite the initial appeal of the explanation itself.

I would like to note in passing, that in any case we are not dealing here with a phenomenon which is productive in any sense of the word. All the noun based -en words are relics, dating from a time when the suffix was not so strictly ajective based (except frighten, which is later). It is just because of the fact that in morphology, due to the persistence of words, we run into such relics, that I have decided to separate word formation from word analysis. The case in hand proves the utility of such a separation, for it appears to be only in the analysis of relic forms that we must have recourse to the most powerful sorts of devices.

4.3.2.4. Boundary Problems.

The first problem is the fact that, contrary to Kiparsky's assertion, there are cases in English where a cycle is motivated though there is only one affix in the word. Such a case is that of the two words prohibition discussed by Brame. One, [prohibišen] can be derived if there is an internal cycle on the verb, the other proæbisen , can be derived only if there is no cycle. Clearly, then, if prohibit has no WFR derived structure, the first is a problem. The same goes for the contrast between [kandensešen] and [kandensešen]. I know of no solution to this problem, and it does cast some doubt on the generality of Kiparsky's observation. The effect on our theory is not so strong. Though Kiparsky's observation somehow "explains" our theory, in accounting for why there should not be cyclical conditions on WFRs, the theory still stands, even if these examples are devasting, which I do not think they are.

The second problem is, in one sense, illusory, and in another quite mysterious. On the simplest version of our theory, since +boundary affixes attach to underlying forms, and # boundary affixes to post-cyclic forms, all the former should precede the latter. One should never find a + outside a #, for that would be paradoxical. Yet one does:

(68)   analyze#able   analyze#abil+ity

        standard#ize   standard#ize+ation

        govern#ment   govern#ment+al

The paradox disappears when we remember that most WFRs are still once-only rules, and that the boundaries are relics of this one applicat-ion of a WFR, which encode its effect in the phonology. The #ize of standardize, when it was added was added at the post-cyclic stage in the derivation of standard. However, standardize now has an underlying form: /standard#i:z/. The only pbstphonological WFRs whose outputs cannot be entered as underlying forms are the infixation and re-duplication rules. Now, since standardize has a legitimate under-lying form, we can perfectly well add +ation to this underlying form. The same goes for the other cases. As long as we regard word-formation as a historical process, which is not repeated in every derivation, there is no problem. The problem only arises if we try to reduce the boundary--WFR pair, in which the boundary is a marker left for posterity by the WFR, to a simple WFR, which then must apply in phono-logical derivations.

Note that we do predict that the above situation will never arise in the case of rules of reduplication and infixation, for they cannot be simply encoded into boundaries. A + boundary reduplication should never follow a # boundary rule. Again crucial data is very hard to come by.

Though the morphology of the forms of (68) is not problematic, the phonology is. The words of column a have the same stress as their bases, as is predicted by the fact that # blocks cyclic stress rules. However, this is not true of the words of column b, where the main stress is on the portion following the #, and the stress of the base is reduced to 3. A tempting solution is to reduce the # boundary on the cycle at which the + boundary is first scanned. This would account for the a items which would not be cycled on the affix, and the b items, which would be cycled on the stem, and on the second affix, the cycle being blocked on the first affix by the boundary, which, because it is erased by the last affix, does not block the cycle there. However, as Alan Prince pointed out to me, this entails that a word like standard#ize+ation, which would have on the last cycle the form standard+iz+ation, be treated on that cycle exactly like improvis+ation, which has no boundary adjustment. However, this is not so. In improvisation, on the ation cycle, after stress is placed on $\overset{1}{at}$, two further things happen. The Stressed Syllable Rule places a stress on $\overset{1}{ov}$ (two back from a 1 stress) and the Explanation rule (which reduces a 1 stress which immediately precedes a 1 stress and is separated from it by no more than one consonant, or rm) destresses iz, which is then reduced to ə. The surface form is $\begin{bmatrix} \overset{4}{impr}\overset{3}{av}\overset{1}{ez}eš\text{ən} \end{bmatrix}$. In standardization however, the Stressed Syllable Rule hasn't applied on the last cycle, ofr if it did it would produce the form $\begin{bmatrix} \overset{4}{sta}en\overset{3}{dard}\overset{1}{ez}ešən \end{bmatrix}$, parallel to $\begin{bmatrix} \overset{4}{impr}av\overset{3}{æz}\overset{1}{eš}ən \end{bmatrix}$, which is incorrect. The boundary adjustment solution thus fails, because it predicts that all cyclic rules will apply on the "last cycle", whereas "in actuality," only some do.

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Another solution is to simply treat standard#ization as two

words, i.e. as a compound, like we did with Kalamafuckinzoo.

This gives the correct output for all the forms of (68b), however,

it does not accord with the facts of (68a), where the affix has in

two cases no stress and in the other 3 stress (stàndardíze).

Why are the two columns not treated in a parallel fashion? Do we only

invoke the Nuclear Stress Rule (in its compound version) where there

are two affixes? Is there something about monosyllabic affixes? I

don't know. At present, then, the forms of (68) stand as an

important counterexample to any known theory of English phonology,

but not morphology. Formally, they are of a single type: X#a+ b.

They are not isolated exceptions, but represent large classes of words.

4.4. Summary.

In this chapter we have developed the notion of a Word

Formation Rule as an operation on a base, accompanied by various

conditions on the base.

The base is a word, a member of a Major Lexical Category. Each

WFR specifies the unitary syntacticosemantic class of which its base

must be a member. The specification of this class contains no dis-

junction or negation. The base is also a fully specified phonological

entity of unique form.

The operation is both syntacticosemantic and morphophonological.

It specifies the semantics of its output as a compositional function

of the meaning of the base, and assigns the output to a specific

Major Lexical Category in a specific subcategorization. The mor-

phophonological operation is phonologically unique, and takes place at

one of two levels in the phonological derivation of the base: the

input or the output of the phonology. The operation also assigns a boundary to the affix it produces. This boundary is dependent on the level of the phonology at which the morphophonological operation applies: a + boundary is assigned to underlying operations, and a # boundary to surface operations.

Conditions both morphological and phonological may be specified on the form of the base. Phonological conditions may be either negative or positive, and they are absolute: only items which meet the conditions may serve as bases for the WFR in question. Positive morphological conditions are different. They determine the productivity of the operation with different morphologically specified subclasses of the base. Productivity is also equated with coherence. The more productive a rule, the more coherent its semantics (in the sense of Ch. 3).

[1]We are discussing here the prefix re#, which is distinct from the prefix re=, and the prefix re+. The distinction is at least phonological. re#, unlike the others, bears stress, and affects in no way the stress as its base.

[1a]The only instance of a negative morphological restriction on the base of a WFR which cannot be accounted for by the blocking rule is that which prevents +al from attaching to bases of the form $X_V$ment, which we discussed at some length above. If we can somehow account for this one, then we can rid ourselves entirely of directly stated negative morphological conditions. This is a highly desirable goal. I have been able to find only one other seeming example of this phenomenon, and this can be explained in a rather elegant fashion. It is impossible to form verbs from comparative adjectives of the form X_{adj}er. The only exception to this observation are the verbs better (formed by a zero rule, which is very productive) worsen (formed from worse) and lower. Remember, however, that we have assumed that the only words which can form bases are those which are listed in the lexicon, and that the only words which are listed in the lexicon are those which are excep-tional. Now, since most #er comparatives are perfectly regular, they will not be listed, and hence will not be possible bases for a WFR. The reason for the occurence of better and worsen is thus

simply the exceptionality of the comparatives <u>better</u> and <u>worse</u>.
They are irregular, hence listed, and hence candidates for WFRs.
The sole remaining exception is thus <u>lower</u>, and it is peculiar in
several ways. Firstly, note that the adjective <u>low</u> has no associated
verb, though most of the other common adjectives in its semantic
class do: <u>deepen</u>, <u>heighten</u>, <u>widen</u>, <u>lengthen</u>. The reason for this
is that the affix <u>+en</u>, which we will discuss below, does not attach
to words ending in a vowel (*<u>grayen</u>). The existence of <u>lower</u> can
perhaps be attributed to this complex of reasons.

[2]We will disregard the possibility and associated complication of
<u>baptism</u> being derived from <u>baptize</u>, and <u>catechism</u> from <u>catechize</u>.

[2a]It is possible that there is some other way to capture a phonological
generalization that through a phonological rule. Perhaps there is
some independent way to note the fact that the three prefixes in
question all use the same copying operation, some evaluation metric.
If there were, then we would not have to worry about losing one
generalization by expressing another.

[3]A mechanism is required which permits reduplication of a surface
segment s^h, which is the reflex of two underlying segments <u>s+h</u>,
only one of which is part of the root proper. Syllabic structure
seems to be at work here, a matter which is not easily incorporated
into the theory of phonology we are working in. We will assume a
convention which matches syllable boundaries with root boundaries
on the surface. This means that on the surface, a root begins with
a consonant, if it can.

[4]The phenomenon is so very complex that we cannot be sure, merely

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from this small amount of data, that any solution is the correct
one. However, the data comes from Bloomfield, and Bloomfield
himself (1933, p. 222) gives the ordering solution. (Thanks
to Sarah Bell for pointing this out to me).

[5]There is probably a typo here.

CHAPTER 5

ADJUSTMENT RULES

The word which is derived as the output of a WFR should be pro-
cessable by the phonology. However, such is not always the case.
Rather, there are two sorts of rules which must apply in certain
instances to the output of a WFR before the rules of the phonology
may apply. The first sort of rule we will call <u>Truncation</u>. A
truncation rule deletes a designated stem-final morpheme before a
designated suffix. The second sort of rule we will call a rule of
<u>Allomorphy</u>. A rule of allomorphy adjusts the shape of a designated
morpheme or class of morphemes in the environment (immediately preceding
or following) of a designated morpheme or class of morphemes. Both of
these types of rules are morphological rules in the strict sense.
They apply to morphemes, with no regard for the meanings of the mor-
phemes, and they apply in the environment of morphemes.

It is true of both of these types of rules that they are forced on
us by the particular theory of word formation that we have developed so
far. Our theory would be strengthened, at least in plausibility, if
we had independent justification of these rules, for though such rules
are possible in other theories, it is only in ours that they are
necessary. I will therefore provide independent justification for both
of the types of rules. In the first section of this chapter I will
discuss Truncation rules, and in the second, rules of Allomorphy. The

methods of exposition will be slightly different in the two sections.

5.1. Truncation Rules.

A Truncation Rule deletes a morpheme which is internal to an affix, in the following general manner:

(1) $[[\text{root}+A]+B]$
XY

$\rightarrow$ Where X and Y are Major Lexical Categories

$$\begin{array}{ccc} 1 & 2 & 3 \\ 1 & 0 & 3 \end{array}$$

All the rules of Truncation which I have found in English apply exactly as the above, i.e. before suffixes, and only with +boundary affixes. I know of no general reason which would explain this, and the restrictions may well be accidental as far as I am concerned, and as far as our theory predicts.

5.1.1. +ee.

Truncation rules are necessary within our theory simply because without them we often find cases of regularly derived words, semantically transparent, formed with affixes which we know to be alive and regular in their operation, which on the surface, do not appear to have been drived from words. I will give an example. Consider the English suffix +ee, which was discussed briefly above. As Siegel (1971) notes, this suffix regularly attaches to verbs which are both transitive and take animate objects,[1] as with _presentee, employee, payee._ Thus Siegel states the following rule of +ee attachment:

(2) **+ee attachment**

$$[[\ ]^Y_{\substack{+\text{transitive} \\ +\text{animate object}}} \ ee]_N$$

Siegel notes, however, that there are a number of nouns in _ee_ which do not conform to the rule as stated. These are paired with and

presumably derived from in some way, verbs of the form $\underline{X\text{ate}}$:

(3)   nomin<u>ate</u>   nomin<u>ee</u>

      evacu<u>ate</u>   evacu<u>ee</u>

Here the suffix <u>ee</u> does not appear attached to any verb, but rather
to the root of that verb, which can be obtained by deleting its last
morpheme. Within a word-based theory of morphology, such an attachment
is impossible, and words of this sort would therefore seem to
constitute very strong evidence counter-exemplary to our
theory, for we know that <u>+ee</u> is a legitimate affix, and attached by a
WFR, however in this case the base is not a legitimate entity.

This problem is easily gotten around. All we need to do is to
invoke a Truncation of the form of (1), which operates after the WFR of
(2). <u>+ee</u> will then attach to $\underline{X\text{ate}}$, giving $\underline{X\text{ate}+\text{ee}}$, which is legitimate
in our theory, and subsequently <u>ate</u> will be removed from between $\underline{X}$ and
<u>+ee</u>, to give give us the form $\underline{X\text{ee}}$,[2] which is the input to the phonology.
But simply invoking a rule of the form of (1) is not enough. We must
show that it does something else than save our theory.

How do we handle a word like <u>evacuee</u> without a rule of truncation?
Siegel's solution is to modify (2) as follows:

(4)   <u>ee attachment revised</u>

      a)   as (2)

   and b) $[[\ ]_X \text{ee}]_N$ where there exists Y_V
                                              $+\text{transitive}$
                                              $+\text{animate object}$
                        such that $[Y]_V = [[\ ]_X\ ]_V$

There are several disadvantages to this solution which the previous one,
utilizing (2) and Truncation, does not have. We will call the solution
of (4) A and the other B.

I. B allows us to state the WFR as one rule. A forces us to bifurcate the WFR itself. Formally, (4a) and (4b) are two distinct and unrelated rules. If we want to relate to them, establish some connection between, which says that we are dealing with the same affix, then we must invent some new mechanism, the nature of which I cannot really speculate on, to express this relatedness between rules. Our theory is of course built on a very strict "one affix one rule" basis, permits only solutions of the form of B, and therefore avoids the extra mechanism. Apart from formal matters, there is the problem that (4a) fails to operate just in the places where (4b) operates. *Evacuatee *nominatee are evidence of this disjunction. Within A we need an independent restriction on (4a) to the effect that it does not operate in the places where (4b) does. Of course no such restriction is needed within B. This is very strong evidence. Solutions of the B type will always entail a disjunction of surface types, which solutions of type A will always be forced to state independently and ad-hocly.

II. A utilizes a labeled bracket [ ]$_X$ in (4b). The label on this bracket has no significance external to the rule (4b). It is arbitrarily resorted to, in order to express the fact that Xee is an analyzable entity. Within B, there is no recourse to be had to arbitrary brackets. To the extent that we wish to rid any theory of arbitrary brackets, B is the more highly principled solution, for solutions of this type never entail the use of other than syntactically motivated bracketing.

III. The condition on (4b) is strange. It says that we may have one word if there exists another from which it is not, strictly, derived. We have seen instances in which we may not have a given word if there exists another, and these were attributed to the blocking rule

of the lexicon, which is a convention on slot-filling. However, the case at hand, which is positive rather than negative, can have nothing to do with the blocking rule. In fact, the only examples of such constraints as that on (4b) arise in cases where we could alternatively use a solution of the form B, which uses Truncation, instead of the condition. Since Truncation has more uses than the mere encoding of this constraint, and accommodates it incidentally and necessarily, a solution which uses Truncation is to be preferred.

We see then that solution B enjoys several advantages over solution A. Solution A only enjoys the advantage of not necessitating a truncation rule. However, since it is the Truncation rule itself which is the source of the advantages of B, we must suspect this latter advantage. Note that Truncation enjoys all these advantages completely apart from our theory of WFRs.

5.1.2. +ant.

The advantages of I, II, III, which   a solution using Truncation enjoys over one which does not, are very general ones.   They will be evident in all cases. This next case, which is little more complex, shows us another sort of advantage of B over A, which will not always be evident.

A class of words closely related to those in +ee is that of nouns ending in the suffix +ant, such as lubricant and complainant. +ant can be said to be in some sense the active equivalent (not quite) of +ee. Words in this suffix fall initially into two classes: those which have some morphologically related verb (complaint/complain, lubricant/ lubricate), and those whose roots are not free words, (or relatable by truncation to free words) (merchant, penchant, pedant). We will

disregard the second group, which is not interesting for our purposes, and concentrate on the first, those with related verbs. This class is further subdivided into two classes:

a) Those items whose related verb is of the form X+ate, as officiant, negociant.

b) Those whose related verb is unsuffixed, as descendant, complainant.

There is only one exception to this bifurcation, which is deodorant, with the related verb deodorize. Class (a) is of course morphologically unique, marked by the final morpheme +ate. It is the only so defined class which is especially productive with the suffix +ant. (32 out of the 95 items in W which are in classes (a) and (b) are in class (a), a very high number for a single morphological class). It is also semantically coherent, as expected by our general association of productivity and coherence.

Now, there are two ways to state the +ant rule, which correspond exactly to the two ways we had to state the +ee rule. We will again refer to the solutions as A and B, where B refers to the solution which utilizes a rule of truncation, and A uses two WFRs instead, one for the (a) cases, and one for the (b) cases. All the arguments I, II, and III apply in this case, in favor of the B (WFR plus Truncation) solution. In addition, however, we have to encode the productivity fact, something which did not arise in the case of +ee. Within the B solution, the productivity of class (a) (X+ate base) is no problem, for the WFR precedes the Truncation rule, which happens to remove the relevant environment for the statement of the productivity. In solution A however, words of the form X+ate only show up in a condition on a WFR

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(... where there exists a word of the form X+ate...) as in (4b).
We have correlated productivity with the morphology of the base, and
found a simple way to express this fact within a theory which uses
word-based WFRs, and we see now, crucially, Truncation rules. Without
Truncation rules, this whole nice system falls apart, for what we take
to be the definining morpheme of the productive class of bases never
appears in a WFR itself, but only in an ancillary condition on one.

There is additional evidence here in favor of the truncation
solution, evidence in the form of exceptions. From inflate and dilate
we expect to have the words *inflant and *dilant, instead of which we
get inflatant and dilatant, seemingly contrary to the truncation rule.
These exceptions are easily explainable. There is a constraint in English
against having non-syllabic roots. If +ate were a suffix with the
two verbs in question, then they would have the following morphological
forms:

(5)   in=fl+ate   di=l+ate

This gives us the roots */fl/ and */l/, which we know on independent
grounds to be impossible. Therefore, (5) is the wrong representation
for the verbs, and it must rather be (6):

(6)   in=flate   di=late

But then -ate is not a morpheme, for it has no boundary, it is not
+ate.   Therefore, Truncation, which is defined as applying only to
morphemes, will not apply here.

It is important to note that these exceptions are not isolated.
The exact same thing happens with the truncation of +ate before +able.[3]
Normally, +ate truncates here as in (7):

(7)   relegate   relegable

penetrate   penetrable

                    consecrate   consecrable

However, with the verbs of (6) this is impossible:

     (8)   inflate   *inflable   inflatable

            dilate   dilable   dilatable

Because all Truncation is restricted to morphemes, there is no need to

note these exceptions in any way within a theory which incorporates

rules of Truncation. However, within a theory which does not have

Truncation rules, some other means must be found to incorporate this

generality into a theory. Within A, the only way is to put a restrict-

ion on the conditions on rules of the form (4b). Since this restrict-

ion is completely ad-hoc within theory A we are led to prefer the

theory which utilizes Truncation, for in that theory we need no un-

principled restriction at all.[4]

5.1.3 Comparative +er.

     For those who are beginning to suspect some intimate connection

between Truncation and the suffix +ate, I provide this last and most

striking case, which has to do with the adverbial suffix ly, and the

comparative suffix +er.

     With adjectives the comparative, except in a few suppletive cases,

may be formed in two distinct ways:

     a)   The suffix +er is attached to the adjective, as in

          big/bigger, small/smaller.

     b)   The independent word more is plac. l in front of the

          adjective, as in more interesting.

The choice of (a) or (b) is determined phonologically. Monosyllables,

and disyllables ending in y, take (a) (stupider and *apter are

exceptions), all other take (b). Some disyllables in _y_, namely those

which can be analyzed as X+ly, take either (a) or (b). The table

illustrates the various restrictions:

(9)

adj	more adj	adjer
big	*more big	bigger
fast	*more fast	faster
happy	?more happy	happier
silly	more silly	sillier
lovely	more lovely	lovelier
sprightly	more sprightly	sprightlier
comely	more comely	comelier
perverse	more perverse	??perverser
flagrant	more flagrant	*flagranter
pompous	more pompous	*pompouser

Turning to adverbs, we observe the monosyllables take +er:

(10) He ran fast/faster/*more fast today.

(11) He ran slow/slower/*more slow yesterday.

Most disyllables and longer take more:

(12) He did it skilfully/*skilfullier/ more skilfully.

Disyllables of the form $C_0VC_{OA}+ly_{Adv}$, that is, those formed from

adjectives by the regular adverb rule, are odd:

(13) a) I am strongly inclined to believe it.

     b) I am more strongly inclined to believe it.

     c) I am stronger inclined to believe it.

(14) a) He ran quickly (*quick).

     b) He ran more quickly.

     c) He ran quicker.

(15) a) He spoke softly (*soft).

     b) He spoke more softly.

     c) He spoke softer.

The forms in (a) and (b) in each of the paradigms, are expected. Those

in (c) are not. If +er was attached to disyllabic adverbs ending in _y_,

as it is to such adjectives (cf. (9)), then we would expect the forms

*stronglier, *quicklier, *softlier, which are not only nonoccurrent, but

impossible. The simplest and most elegant solution to the problem is to

have a rule of truncation which operates only on the class of adverbs in

question.

(16) Adverb +er truncation:

$$C_0 V C_0 + ly + er_{Adv}$$
$$1 \quad 2 \; 3 \rightarrow$$
$$1 \quad 0 \; 3$$

By using the truncation rule (16), which is, like all Truncations,

ordered after WFRs, we allow ourselves to state exactly the same

conditions on the distribution of +er and more for adverbs and adject-

ives. The only difference between the two classes is the operation of

rule (16) in the former, though not in the latter.

No other solution is even unproblematical. If we delete (truncate)

before we do the WFR, more or less as in an A solution, the conditions

for +er attachment are met (monosyllables) but in order to permit the

derivation of the (b) forms as well, we must somehow make the deletion

optional. We are then unable to capture the parallelism between the

present instance of being allowed to form two comparatives, and the

corresponding instances in (9). This is the +ant problem in another

guise. In addition, if we delete before we do the comparative rule,

what is the category of the item we form the comparative from? If

from softly we go through soft to softer, is this soft an adjective as

it should be if the ly rule is to have any validity? But, if it is an

adjective, then do we form the comparative of an adverb in these cases

from an adjective? And, if it isn't an adjective, then what is it, for

it is clearly not an adverb?. This is the problem of the label(II) in

another guise.

We see then that not only does the solution which incorporates a truncation rule avoid all the difficulties which are attendant on other solutions in the case at hand, it also allows us to express a generalization of some interest, and collapse the comparative forming rules for adjectives and adverbs.

Alan Prince has pointed out to me that substantially the same situation holds with superlatives as with comparatives, and that we might wish to extend the truncation to that case as well, in which case truncation of ly would take place before a class of morphemes rather than a single morpheme.

He has also noted that the truncation is restricted syntactically. Only the more form occurs before an adjective:

more deeply philosophical

*deeper philosophical

more frankly phony

*franker phony

Since I have not looked at conditions on Truncation Rules, I can not really comment on the import of this case. It is an open question at present whether comparative formation itself is a syntactic or derivational phenomenon. If it is syntactic, then it does not strike me as odd that the specification of the form of the comparative should depend on the syntactic environment of the compared adverb. But this must await further investigation.

5.1.4. TruncaWFRs.

One simple way to avoid Truncation altogether is to build Truncation processes into WFRs. This is at first not implausible. As we

have seen, it seems likely that for languages like Hebrew at least, WFRs must be powerful enough so they can not only add phonological material, but replace one piece of phonological material with another (replacing vowel patterns). If WFRs need to do this anyway, then why do we need Truncation rules. We can, for instance, have +ant simply replace +ate. Ruminate becomes ruminant in one step.

The answer to this suggestion lies in the "one suffix, one rule" ethic. If we allow a WFR to do the work of a Truncation rule in this or any other case, we will need a separate WFR for each morphological subclass of the base where truncation operates. In this case we need two +ant rules, one which truncates, and one which doesn't. We then run into the problem of how to relate the rules, a problem which, as noted above, Truncation avoids by its very essence.

The "one suffix one rule" ethic is the same as the unitary base hypothesis. Truncation rules serve the same function as does the separate statement of morphological conditions on the base. We are trying to separate out a central core for each rule, which will be uniform and will not vary with morphology. The various peripheral devices are then called upon to adjust this ideal situation to the vagaries of reality. This is the prime motivation behind the separation of the various types of rules. Of course mere esthetic motivation is not sufficient, we must have empirical confirmation of the merit of our system. This I have tried to provide.

5.1.5. Truncation and Phonology.

Though Truncation as a process does not resemble greatly any phonological rule type that I am aware of, one must still ask what the relationship is between the two, as we did with WFRs. As far as I can

tell, Truncation rules, like WFRs, never have to be ordered among phonological rules. All the cases I have found, which involve +boundary affixes, can be ordered before all phonological rules. Some Russian examples are discussed below, in one of which a rule of Truncation interacts with the phonology. There, we seem to be dealing with a #boundary affix, which triggers truncation of the last morpheme of the base, but not until the cyclic rules have been applied to the base. If this is indeed what is going on in this case, we can correlate the place of the Truncation rule in the phonology with the boundary of the affix before which Truncation takes place. The ordering of Truncation rules with respect to the phonology would then exactly correspond to that of WFRs, which, as we noted, is a a function of boundaries. The problem is that in order to establish the validity of the Russian example, a much greater knowledge of Russian phonology is needed than I have at present. It is clearly possible, even in the light of the Russian case, to claim that Truncation is not a phonological process, in the same way that we claimed that WFRs were not phonological, while at the same time maintaining that Truncation rules are not WFRs. Truncation rules will now be intrinsically ordered after WFRs, and enjoy (probably, though the evidence is scanty) the same ordering, w.r.t. the phonology that these latter do.

5.1.6. Russian Truncation.

The truncation mechanism proposed above is not novel. Though I know of no explicit mention of such a mechanism within the scant modern work on English morphology, there is at least one very thorough discussion of truncation in the literature. This is an article by A. V. Isačenko, in Russian, whose title translates as "The Role of Truncation in Russian

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Word Formation."

Many of the truncation phenomena which Isačenko discusses are strikingly similar to those which I have found in English (as indeed the case should be, if there is any realy significance to the device). Because of this coincidence, and because Isačenko's work may not be readily accessible to the reader of this one, I give below a brief summary, with comments, of the relevant examples.

Isačenko discusses various truncation rules which prevent surface suffix doubling. So, for example, the rule: 4a

$$\{ov\}_1 + \{ov\}_2 \rightarrow \{ov\}_2 : \begin{cases} \{(suvor + ov)\} + (ov +, \#c)\} \rightarrow suvorovec \\ \{roz + ov\} + (ov + at)\} \rightarrow rozovatyj \end{cases}$$

NB. Yj in the last example is an inflectional ending. Structurally, this rule is very similar to an English rule discussed in below:

truncation: $X + ate_V + At + ivn_N$

             1   2   3   4

             1   0   3   4

Firstly, $X + ov$, as $X + ate$, need not be semantically decomposable, i.e., X need not occur as a free stem. Secondly, it is the first occurrence of the suffix which deletes.

A second rule of double suffix truncation involves the suffix $\#sk$, by which leningrádskyj is derived from leningrád. When a stem is of the form $X + sk$, as in tomsk, we have truncation: tómskyj *tomskskyj. It is important to note in connection with this rule that not all Xsk roots allow truncation. So bask/ baskskyj/ *baskyj. From examples such as this Isačenko concludes that only morphemes truncate, and that bask is monomorphemic. It has already been seen, in the case of the exceptional

behaviour of forms such as <u>inflate</u>/ <u>inflatant</u>/ *<u>inflant</u>, that the
same holds of English truncation rules. Only morphemes truncate.

Isac̆enko stresses the importance of semantic evidence. Often
one form is based on another not only formally, but semantically as well.
Truncation rules, within a word based theory, allow us to express the
semantic regularities. He gives two particularly pretty examples of the
use of truncation to capture semantic facts. These examples also in-
volve powerful phonological evidence of a type which I have not found
for any English truncation rule, but which further search will hopefully
reveal.

The first case has to do with truncation of the adjective suffix
#n, in deadjectival verbs, whose semantics may be roughly described as
V = make (self) Adj. For example:

oburz̆uazit, (make bourgeois) vs. <u>burz̆uaznyj</u> (bourgeois)

Unless we derive the V from the Adj, and truncate the #n subsequently,
before the verbalizer (<u>it</u>, or <u>et</u>), we cannot express the semantic facts
in a simple manner. In many cases the adjective in question has a rel-
ated noun, from which one might wish to derive the V, since no trun-
cation rule would be necessary, the noun consisting of the bare stem,
without the adj. marker #n. Isac̆enko shows that in several instances
such a derivation is phonologically impossible.

For example, the Adj <u>sekretnyj</u> (secret) is formed from the noun
<u>sekret</u> (secret). One might be tempted to derive the verb directly
from the noun, obviating the truncation rule. However, in a case like
<u>cyngotnyj</u>$_{Adj}$ /<u>cynga</u>$_N$, the verb, <u>cyngotet</u>, cannot be derived phonologically
from the noun. This is evidence of a very strong sort for using the
truncation rule in all cases.

The second such case has to do with verbs of the following form:

o+bez+N + verbalizer (obezumeț)

These are traditionally derived from the phrase bez N (without N),
and semantically characterized as V=make Nless. So bez umá (without a
mind), obezúmeț (make mindless). Isačenko argues that such a derivation
is incorrect, and that the verb is derived rather from the adjective
bez+N+#n (here bezumnyj), the #n adj. ending being truncated in the
same way as in the case discussed above. Isačenko presents three pieces
of phonological evidence to support his contention. These can be extract-
ed from the following paradigm:

bez N (without N)	bez+N+#n$_A$ (Nless)	o+bez+N+vl.zer$_V$ (make Nless)
a) bez umá (mind)	bezúmnyj	obezúmeț
b) bez lóšadi (horse)	bezlošádnyj	obezlošádeț
c) bez vredá (harm)	bezvrédnyj	obezvrédiț
d) bez vodý (water)	bezvódnyj	obezvódiț
e) bez zemlí (land)	bezzemélnyj	obezzeméliț
f) bez nadéždy (hope)	beznad,óžnyj	obeznad,óžiț

1) The first bit of evidence is of the same sort as that of the
last case. In (f), the vowel e of the N corresponds to ₊o of the Adj.
The verb has the same vowel as the Adj., and thus must be derived from
it, for phonological reasons.

2) Secondly, the place of the stress, which is unpredictable on
the N, is constant on the Adj. (predesinential). The V has the same
stress as the Adj. Since the stress of verbs with the verbalizing
suffixes i and e is not usually predictable, one must derive the V from
the Adj.

3) Finally, in (e) there is a complex phonological connection
between the N and the Adj. This is because of the presence of yers

(represented by #) in the stem /zem#l/ and the suffix /#n/. # vocalizes before a syllable containing another #, otherwise it drops 4b. So /bez+zem#l,+#n+yj/→ /bezzemel,nyj/. (The first # vocalizes and the second deletes). The second yer provides the crucial environment for the vocalization, and vocalization must precede deletion. In the corresponding N, since there is only one #, the vocalization rule cannot apply, and this #, which was vocalized in the corresponding Adj. is deleted: zeml,a. However, in the verb, which like the N should have only one # underlyingly, that of the root, this # is unaccountably vocalized. Unaccountably, that is, unless we drive the V from the Adj. The # of the Adj. suffix will cause the # of the root to vocalize, and the suffix will be deleted via the familiar truncation rule. The derivation of the verb is given below:

input 1	bez+zem#l,+#n$_A$	
	e	# vocalization
	e′	stress
	0	# deletion
output 1	bezzemél,+n$_A$	
WFR:	o+output 1 +it,	

truncation: obezzemél,+it,

There is no other plausible way to produce the correct surface reflex of the # in the verb. 4c. An example such as this provides the strongest sort of evidence possible, for the existence of truncation rules. As I have already noted, I have not been able to find such strong evidence in English, but the similarity of the English truncation rules to those which are posited for Russian, and the existence of this evidence for the Russian rules, provide indirect support for the positing of the truncation

mechanism as a general one, and thus provide support for a word based
theory of morphology, because, as I have stressed, it is only in this
type of theory that the truncaction mechanism is necessary, and it is
only in this theory that truncation rules must follow Word Formation
Rules.

5.1.7. German ge- deletion.

Not all rules which delete specified morphemes are rules of trun-
cation. In order to be a rule of truncation, a rule must have an
entirely morphological environment. A rule which deletes a specific
morpheme, but in a phonological environment, is not a rule of
truncation. An expected consequence of this differentiation is that the
latter sort of rule can be ordered among the rules of the phonology, for
it is a phonological rule. We will give an example: the rule which
deletes the prefix ge- in German past participles. We will discuss
this rule as formulated by Kiparsky (1966).

In German, past participles normally have the prefix ge- , of the
participle is stressed. Otherwise ge- does not show up. There is a class
of exceptions to this simple generalization. Consider the following two
sets of participles, both of which are of verbs in the unmovable
prefix miss.

a) misfállen, missbráucht, missbílligt

b) míssverstánden, míssgestáltet, míssinterpretíert

According to our simple statement of the distribution of ge-, it should
show up in the participles in (b), since the stress in these cases falls
on the first syllable. Kiparsky solves this problem by a judicious
ordering of independently motivated rules. He notes that the prefix miss-
is itself stressed only before an unstressed stem syllable, as in (b),

in which case the stress on the stem itself is reduced by general convention. If we hypothesize that the absence of ge- in (b) is determined before the stressing of miss-, i.e. between the rule that gives the stem stress, and the rule which stresses miss-, then we can preserve the generalization that ge- does not appear before unstressed initial syllables, since the miss- in the items in (b) is not stressed until after the ge- distribution is established.

Note that if we posit a rule of ge- deletion, a rule which deletes a specified morpheme, then this rule is ordered between two phonological rules, the rule which stresses the stem, and the rule which stresses miss- before an unstressed stem syllable. If ge- deletion were a rule of truncation, then this ordering would constitute a counterexample to our general claim that rules of truncation cannot be ordered among phonological rules. However, consider the conditions under which ge- deletes: before an unstressed syllable. This is not a morphological condition, but rather a phonological one. Therefore, ge- deletion is not a rule of truncation and it is no counterexample to our general claim.

I have adduced this example because I wish to make it clear what the extent of the ordering claim is with regard to truncation rules. As we have formulated the notion, not all rules which contain morphological information are rules of truncation or allomorphy. Only those rules which delete specific morphemes in the context of other specific morphemes are truncation rules. I am claiming that these specific rules are ordered before all the rules of the phonology. I am making no claims with regard to other rules which may be similar to these in certain respects. The ordering of a rule such as ge-deletion is not predicted

in any way by the theory of rules of truncation being presented here.

5.2. Allomorphy Rules.

A rule which effects a phonological change, but which only applies to certain morphemes, in the immediate environment of certain other morphemes, we will call a rule of Allomorphy. We will claim that such rules are external to the phonology in the same way that Truncation rules and WFRs are.

An important restriction on the power of rules of Allomorphy is that they cannot introduce segments which are not otherwise motivated as underlying phonological segments of the language. This of course makes them very different from rules of the phonology. It also places a rather strong constraint on a powerful device. Unconstrained rules of allomorphy are the most powerful means of expressing phonological alternations available. They are capable of encoding all types of behavior, exceptional and regular, and do not differentiate between the various types. The ordering of allomorphy rules before the rules of the phonology, the strict limitation of the environments in which these rules may operate, and the restriction to underlying phonological segments place great restrictions on this otherwise omnipotent device.

Allomorphy rules are different from Truncation rules in that they look like phonological rules, which the latter do not. Our first task is thus to isolate Allomorphy rules from phonological rules, and the major claim in that regard is that rules which have the formal property of being restricted to certain designated morphemes, in the immediate environment of certain other designated morphemes, are always outside (previous to) the phonology. We then see that, in their

restriction, at least, Allomorphy rules are the same as truncation rules, and that they have the same ordering properties w.r.t. the rest of the grammar. We therefore group them together as rules of morphological adjustment.

In accord with the order of tasks, we will first provide a relatively detailed account of certain problems which arise if we attempt to give a detailed analysis of English nouns of the form <u>Xion</u>. We will show how these problems can be solved by positing a class of rules of allomorphy. Then we will see how Allomorphy rules fit into our general theory of word-formation and morphology.

5.2.1. <u>-ion</u>.

This section is a detailed study of the English suffix +At+ion and its variants, and the variation it conditions. We will refer to the suffix as <u>ion</u>, but this is merely for typographical convenience. The basic form of the suffix we will suppose to be +At⨍ion; this is the form inserted by the WFR of <u>ion</u>.

The suffix is very widespread and productive. <u>W</u>. lists about 2,000 words ending in it, comprising a total of approximately 4% of the words listed in that dictionary. In its active use as a WFR, <u>ion</u> is a deverbal abstract action nominal suffix, with both active and passive senses (<u>fascinate/fascination</u>, <u>relegate/relegation</u>). The semantics and syntax of the suffix are very interesting, however we will not concern ourselves with these here. We will include in our study nominal whose stems are not free words <u>compunction</u>/*<u>compunct</u>, <u>salvation</u>/*<u>salve</u> on this reading). We will also include the very few <u>ion</u> nominals whose bases are adjectives or nouns instead of verbs (<u>contrition/contrite</u>, <u>ideation/idea</u>).

Note that not all instances of orthographic -ion are to be taken as instances of the suffix ion. All forms in which the i is syllable are not(dandelion, accordion, ganglion), as well as words like onion, companion, million, which can probably be excluded on semantic grounds. The exclusion of these latter forms is not crucial to our argument however. According to our theory of WFRs they can be analyzed as words with the same status as a word like possible, and they probably are so analyzed by the majority of people, though their etymology shows them to be otherwise derived.

The regular phonology of the suffix is dealt with very convincingly in SPE. There an underlying phonological form (+At)+iVn is given to it, with V standing for an indeterminable lax vowel, which we will represent by o. +ion must be bi-syllabic because of stress facts, namely the placement of primary stress on the syllable preceding it (prohibition, SPE p. 87), and the operation of tri-syllabic laxing on on the immediately preceding vowel (decide/decision, SPE p. 182). A later rule takes i to y (SPE p. 225-227). Further rules of spiranti- zation and palatalization yield the correct output.

5.2.1.1. Allomorphs of Ation.

As many people have noticed, the suffix +Ation has several diff- erent forms, as shown in (1):

(1)

realize	realization	*realizion	*realizition
educate	*educatation	education	*educatition
repeat	*repetation	*repetion	repetition
commune	*communation	communion	*communition
resume	*resumation	resumption	*resumition
resolve	*resolvation	*resolvtion	resolution
		*resolvion	

From table (1) it is easy to conclude that ion has at least four, and
maybe five, forms:

(2)   +Ation, +ition, +ution, +ion, +tion

The distribution of the forms of (ℓ) is complex, but I will describe
it thoroughly, and show that it is morphologically governed, and
determined previous to phonology.

5.2.1.2.   +Ation.

This is the unrestricted variant. There are no conditions of
attachment on it, except that it does not attach in cases where the
conditions of attachment of the other variants are met. Again I must
stress the importance of this disjunction, for it shows that we are
dealing with variants of the same thing, and not with five different
affixes and a blocking rule.

The following chart demonstrates unrestricted attachment of
+Ation to stems ending in various segments and clusters. It appears
to be unrestricted, except for the matter of coronial fricatives,
discussed in 4.3.2.1.

(3) | Labial | Coronal | Velar |
|---|---|---|
| perturbation | cessation | evocation |
| formation | degradation | purgation |
| exhumation | elicitation | prolongation |
| usurpation | accusation | |
| | revelation | |
| | declaration | |
| | examination | |
| | representation | |
| | deportation | |
| | manifestation | |
| | consultation | |
| | affectation | |
| | commendation | |
| | sensation | |
| | indorsation | |

There are only a few instances of +Ation after a vowel-final stem.
This fact can be traced to the fact that +Ation attaches only to
Latinate stems and the paucity of Vowel-final Latinate stems. A few
examples are vary/variation, continue/continuation, renounce/renunci-
ation. There is one very interesting class of vowel-final stems, which
we will discuss in some detail. As noted in SPE, verbs in +fy and
+ply generally have nominals in -ication, as amplify/amplification,
imply/implication. This rather singular alternation is covered by the
following "ad-hoc" rule of phonology:

(4) $k \rightarrow \emptyset \ /+C_1\bar{i}\_\#\#$ (SPE p. 201, rule 62)

Amplify is thus derived from amplif$\bar{i}$k, and the short $\underline{i}$ in amplification
arises from the application of the Explanation Rule, which destresses
pre-stressed vowels. Rule (4) is ad-hoc in the best sense of the
word. It is so formulated as to have its structural description met
only by verbs with the roots +fy and +ply. Even the +boundary does
its job, preventing dis#l$\bar{i}$k from being converted into dis#l$\bar{i}$. One
pecularity of the rule is that there is no known rule of English
phonology which must precede it (vowel shift must follow it). We can
wonder whether the rule's ordering can be attributed to any of its
other peculiarity.

We will return to this case below.

Note that apart from any other of its peculiarities, a form like
amplification is also different from those in (3) in that the ion
nominal does not include the free form of the verb. We will look at a
similar case, one in which, though we can motivate the form +Ation, the
unrestricted variant, there is no simple agglutination on the surface,
or at the underlying phonological level, because of the intervention of

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a Truncation rule.

5.2.1.3. Stems of the Form X+ate.

Verbs of the form X+ate (equivocate, prevaricate), form one of
the most productive base classes for the ion rule, rivaled only by
the base form X#ize (communalization). As Siegel (1976) notes, in the
nominal derived by ion, one finds only one At, instead of the expected
two: equivocate/equivocation/*equivocatation. This is quite general
(the only real exception is dilatation from dilate).[5] This fact can be
accounted for by a rule of Truncation, like those of 5.1.

As in some of the Russian cases, we have here a Truncation rule
which reduces double suffixes to one. A question arises of which +at
gets truncated, the first or the second, or is there a difference? I
have no simple answer here. Note that in all other cases of +At
truncation, the +at which occurs as the first one here is deleted
(nominee, dominant, penetrable). For reasons of symmetry, and if we
wish to combine all +At truncation in one, we might like to delete the
first+At here, which, as noted, corresponds to the one deleted else-
where. On the other hand and, there is some very complex evidence,
from Brame's (1972) analysis of words of the form X+At+ory, that it is
the second At which must truncate. I will not go into Brame's evidence
here, but if he is correct,[6] then we must opt for a different truncation
rule here than with other +At truncations.

5.2.1.4. The Marked Roots.

We will now turn to the other variants of the suffix, those that
are restricted to certain morphological environments. The distribution
of these restricted variants is governed by latinate roots, of the sort

discussed in Ch. 2.1. These are true morphemes, with, as demonstrated at length in 2.1 no meaning. The form of the variant is never determined by a specific word. It is never the case that one verb in a given stem will allow one variant, and other verb in the same stem a different variant. The form of the variant is root governed, morphologically governed. There are no exceptions to this. It is the first law of the root, first discovered by the great Semitic grammarian ben-Moshe, (ms) and called ben-Moshe's first law.

We will illustrate ben-Moshe's first law with the root <u>sume</u>, in (4). The variant of ion which appears after <u>sume</u> is <u>+tion</u>:

(4)	subsume	subsumption	*subsumation
	consume	consumption	*consumation
	resume	resumption	*resumation
	presume	presumption	*presumation
	consume	consumption	*consumation
	assume	assumption	*assumation

Note that the form <u>consummation</u>, as in Shakespeare, is not an exception. Rather it is derived from the base <u>consummate</u>, by Truncation. Note also that there is nothing phonological at work in the conditioning at least. The stem <u>hume</u>, as in <u>exhume</u>, is not restricted, and its nominal is therefore <u>exhumation</u> and not <u>exhumption</u>. Similarly for <u>deplume</u>/<u>deplumation</u>/<u>deplumption</u>.

We have noted that the restricted variants are root governed, but we have not noted what they are. Basically there are two. For roots ending in non-coronals, that is labials and velars, the restricted form is <u>+tion</u>; <u>sume</u> is one example of a non-coronal-final root. Others are listed below:

(5) duce   deduce   deduction

     scribe   prescribe   prescription

     ceive   conceive   conception

     deem   redeem   redemption

     sorb   absorb   absorption

     stroy   destroy   destruction

     struct   destruct   destruction

These exhaust, I think the restrictive non-coronal roots. As we might expect, some of these are very productive morphological bases for the ion rule. For both ceive and duce, there exists a nominal for every verb, as documented in (6) below. One supposition which (6) dispels is that only the nonrestricted form of the affix, that is +Ation, can be productively attached. Such a supposition is actually counter to the entire theory of WFRs that we have proposed. If the variants of ion are indeed merely morphologically determined variants of one suffix, which they are, and if productivity is determined solely by the base of a WFR, and not by the variants of the suffix, which are really not available for reference at point of application of a WFR, then this supposition, which crucially depends on the variants of the suffix to determine productivity, must be false. It is:

          receive   reception

          deceive   deception

          conceive   conception

          perceive   perception

          apperceive   apperception

deduce	deduction
reduce	reduction
seduce	seduction
induce	induction
conduce	conduction
produce	production
introduce	introduction
reproduce	reproduction

The restrictive coronal roots are the most interesting and irregular class. The form of the affix after this class is not transparent. Many investigators have assumed it to be +tion (cf. Householder, 1972, Schnitzer, 1971), the same suffix as that which appears with the non-coronal roots. This, however, cannot be the same. Rather the affix with this class must be +ion, as in SPE, for the following reasons.

Firstly, pairs such as rebel/rebellion, commune/communion, demand the positing of +ion at least after some liquids and nasals.

Secondly, as alternations like decide/decision, revise/revision, argue, the vowel preceding ion must be laxed by the tri-syllabic laxing rule. In such cases as abrade/abrasion, rotate/rotation, this vowel has further undergone a tensing rule, which tenses non-high vowels in the following environment (SPE p. 181):

$$/ \underline{\hspace{1em}} C_1^1 \begin{bmatrix} -lo \\ -cons \\ -back \\ -stress \end{bmatrix} V$$

This rule also operates in alternations such as Canada/Canadian, Abel/Abelian. Crucially, there must be one and only one consonant after the affected vowel. If the suffix in abrasion is +tion, then the environment of the tensing rule is not met. There must, therefore,

be a rule which deletes the t, before the above rule applies. Since this t-rule has no other function, and is not orderable after any phonological rule, the form of the suffix may as well be +ion after all coronals, exactly as we know it must be in communion and rebellion. Note also that the environment for the putative rule of t-deletion cannot be stated phonologically, but rather must be stated in terms of certain coronal roots.

The root vene (convene/convention) shows an interesting conjunction of the matters just discussed in the two arguments above. One might be tempted to regard the alternation of this root as evidence for the suffix's being +tion after at least some occurrences of n. However, if the suffix is +tion, then in most cases it must be deleted, before the application of the tensing rule, as just shown. One would, therefore have to mark vene as an exception to the deletion rule. The alternative way to get vention, instead of the venion, which we expect if +ion is attached to vene, is via a rule of allomorphy. I will discuss this solution below.

The other variants of the suffix are +ition and +ution. Evidence for the first is the following:

(7)   add   addition

        vend   vendition

        define   definition

        X+pose   X+position

        compete   competition

        repeat   repetition

        imbibe   imbibition

The only trouble with positing another suffix in this case is esthetic.

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Note that previously, though we had two restricted suffixes, their

environments were phonologically complementary. Because of this

complementarity we might say that we have really only one restricted

suffix, which attaches to verbs ending in restricted roots, and that

the exact form of this suffix is subsequently determined by the

phonology of the root. However, if we allow +ition to be a restricted

suffix, we can no longer use this simple system. Roots must now not

only be marked as restricted, but also for the particular restricted

affix they take. We could avoid this by changing rather the form of

the roots by adding it to them, and then have them take the restricted

suffix that is appropriate (+ion). Though this latter is less

complicated in terms of its repercussions, I see no empirical

grounds for deciding between the two.

The possibility of there being a suffix +ution is provided by the

following:

(8)   revolve   revolution

           resolve   resolution

           dissolve   dissolution

           solve   solution

The two roots are peculiar. Both end in lv. One could simply mark

them for restricted suffix, which in this case will be +tion because

v is not a coronal. Then, a rule could take v to u/_t, giving the

correct output. Alternatively, we could have a suffix +ution, and

drop the v instead of vocalizing it. The second solution gives us the

same problems as the positing of +ition, but again I know of no

empirically relevant argument for one or the other solution.

We have established that the affix ion has at least three variants,

an unrestricted variant +Ation, and two restricted variants +tion and

+ion, restricted to bases which end in certain latinate roots (not all latinate roots). The choice between these two variants is governed by the last consonant of the root. +tion goes with noncoronal roots, and +ion with coronal roots. There may also exist the affixes +ition and +ution. We will assume that they do not. How are the variants assigned? By a rule of allomorphy. The rule is a little complex:

(9) Allomorphy of ion:

$$\text{+Ation} \rightarrow \left\{ \begin{array}{l} \text{+ion} \\ \text{+tion} \end{array} \right\} / X \left\{ \begin{array}{l} \text{+cor} \\ \text{-cor} \end{array} \right\} \quad \begin{array}{l} \text{where X cor is one of} \\ \text{a set of specified} \\ \text{latinate roots.} \end{array}$$

Rule (9) is a rule of allomorphy because it applies to a designated morpheme +Ation, in the environment of a designated set of morphemes. Rule (9) follows no phonological rule of English to my knowledge. This point is crucial. It is claimed that all rules of the form of (9) precede all phonological rules, and are not phonological rules. This claim is easily falsifiable.

5.2.2. Root Allomorphy.

More striking than the allomorphy of ion is the fact that many of the marked roots are susceptible to allomorphy before it. This was first noticed by ben-Moshe, and is usually known as ben-Moshe's second law, though it is not really a law.

It is perfectly plausible that after the application of rule (9) determining the proper allomorph of ion, a word will go into the phonology without any further adjustment. This is not always so. Let us look at two pairs:

(1)   invert   inversion [inveřsən]

        insert   insertion [insəršən]

In one case we get a $\acute{z}$ in the same place where we get a $\acute{s}$ in the other case. Both correspond to a word final t. The only difference between the two pairs of (10) is that one has v where the other has s, or stated in another way, the only difference is in their roots. No phonological rule of an orthodox type can be at work here. Note further that all ion nominals with roots vert will show $\acute{z}$, and that all ion nominals with roots sert will show $\acute{s}$. The only plausible solution to (10) is a rule of allomorphy in at least one of the cases, which changes the root's last consonant. The simplest rule is one which voices the t of vert to d before ion. After that, well motivated phonological rules will grind out the correct forms of (10). Note that the allomorphy rule takes place before all the phonological rules, as claimed in general.

There are other ways to get the forms in (10) right. We could use an abstract segment t_1 which shows up as t everywhere except before ion. This sort of solution is undesirable on general grounds. We could use a rule feature, which triggers the relevant rule only when a word has the root vert, and not when it has sert. But this latter solution necessitates two things; first, we are using a positive rule feature in the company of a minor rule, a rule which only applies to segments which are marked to undergo it. Secondly, we must specify the order of this minor rule in the phonology. As it happens, it is the first rule, or at least follows no other rule. These two things are a coincidence. By using a rule of allomorphy, we are claiming that there is no coincidence, all these things must fall together. We are simultaneously ridding our grammar of a minor rule/positive rule feature complex, a very suspect and powerful entity.

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Again, let me stress that though a rule of allomorphy is formally a very powerful device, its power is highly restricted by the restrictions on its use. The difference between the forms of (10) can be captured by an allomorphy rule only because of the coincidence of three features. One, the difference is morphologically governed in the strictest sense. Two, the difference can be marked prephonologically, and three, related to two, the difference can be represented by using otherwise motivated underlying segments of English. Only if these three conditions are met can we have recourse to a rule of allomorphy. The rival method of using a minor rule and positive rule feature is not so constrained, and by its very nature cannot be. Therefore the allomorphy solution, because it can be used only in this narrowly restricted type of case, is empirically more adequate.

The utility of Allomorphy in cases like (10) is demonstrated. It allows us to make sense of what was previously an exception of the highest order. What we will now do is survey all the marked roots, and show that there are many similar Allomorphy rules at work, though none so blatant perhaps as this one.

One of the problems with investigating allomorphy before _ion_ is that several rules of English segmental phonology are at work in this environment, and prevent us from finding the underlying allomorphs in a simple fashion. This was true in (10), where spirantization inter-vened, and forced us to speculate a d̲ in vert̲ /__+ion̲. An observation of Martin's (1972) allows us to circumvent this problem. Martin notes that words in all the suffixes _ion_, _-ive_, _-ory_, _-or_ are built on the same form of a given root. If this is true, and we can assume that it is, then we can look at the relevant _-ive_, or _ory_ form, where the

phonology has not wreaked much havoc, to find out the underlying form of the <u>ion</u> form, and by comparing this form with the word-final form, we can discover what, if any, allomorphy is at work.

I will first look at coronal-final roots, since these form the majority of roots, and exhibit the most allomorphy. The following table is exhaustive, and shows all possible alternations in the relevant environments:

Table of Marked Coronal Alternations

Sample Verbs	Verb-Final C	/ +iVn	/ +ive
excrete, X+sert	t	š	t
X+mit (permit)	t	š	s
X+vert (convert)	t	ž	s
digest	st	sč	st
connect	kt	kš	kt
decide, explode	d	ž	s
X+cede (concede)	d	š	s
apprehend	nd	nš	ns
commune	n	n	
scan	n	nš	
convene, retain	n	nš	nt
prevent	nt	nš	nt
recense	ns	nš	
coerce	rs	rš	rs
disperse	rs	rž/rš	rs
submerge, asperge	rdž	rž	rs
adhere	r	ž	s
recur	r	rž	rs

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rebel	l	l	
X+pel (expel)	l	lš	ls
convulse	ls	lš	ls
revise	z	ž	
percuss	s	š	s
admonition	s	š	t

First, we will extract what generalities we can from the whole list. Note firstly that of the full consonants, only s̲ and t̲ occur before +ive. The absence of any voiced full consonants before this suffix can be easily captured by the following ad-hoc rule:

(11)   C → -voice/_+ive

Note that there are no voiceless counterparts to l̲ and n̲. (Rule (11), if it applied to these segments, would result in an impossible form. It is perhaps for this reason that there are no cases of Xl+ive or Xn+ive. Rebellion has rebellious, and communion has no corresponding adjective.

The second general fact to be noticed is that, except after l̲ and n̲, +tion is preceded only by palatals: š, ž, č. This is the result of palatalization, an apparently simple process (but see below and SPE 229-231).

Another general fact to be noted is that the same form that shows up before +ive shows up before +able in many instances. This will prove useful in one or two cases.

Looking at the alternations, we find only eight cases where the final consonant (cluster) of the bare verb is in a one-to-one correspondence (one way) with the consonant preceding +ion and +ive (disregarding (11) and palatalization). These are V̲s̲, V̲z̲, s̲t̲, k̲t̲, n̲t̲, n̲d̲, n̲s̲, l̲s̲.

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Except for <u>nd</u>, all these have exactly the same consonant before +<u>ive</u> as

they do word-finally. This is the prime evidence for a rule of

palatalization /_+ion, a phonological rule. The general correspond-

ence is as in (12):

(12)   z/ž, t/š, s/š, st/šč

Though we would like to state this as one rule, because of its

seeming generality, there are many phonological problems which face

such an attempt, which I will not discuss here. Most of the relevant

facts can be found in SPE (229-235). We will state two rules of

palatalization.

(13)   Palat. I. $t \rightarrow \check{c}/s\_yV$

(14)   Palat. II. $\begin{bmatrix} +cns \\ -voc \end{bmatrix} \rightarrow \begin{bmatrix} -ant \\ +strid \end{bmatrix} /\_yV$

Turning to the one case where there is a one-to-one correspond-

ence, but a different consonant before +<u>ive</u> and (and +<u>able</u>) than word

finally, which is <u>nd</u>, we find the following:

(15)   nd#   nsen#   nsive#   nsebl#

This is true of all the roots in -<u>nd</u>:

(16)   fend   defend
        hend   apprehend, comprehend
        tend   pretend, contend, extend
        pand   expand
        scend   ascend, descend, condescend

nsen# tells us that the nominal/adjective stem must be either <u>Xnt</u> or

<u>Xns</u>. <u>nsiv</u> and <u>nsebl</u># tell us that it must be <u>Xns</u>. We may therefore

posit the following rule of allomorphy:

(17) $d \rightarrow s/n$ _ $\begin{bmatrix} +ive \\ +ion \\ +ebl \end{bmatrix}$

A difference between this rule and the rule (11), both of which we

called rules of allomorphy is that this one applies to all roots of the

form <u>Xnd</u>, whereas the latter applied to only one root. One might wish

to claim that (17) is a phonological rule, and not a rule of allo-
morphy, and this despite its odd environment. But (17) only applies
in marked roots. Consider the root _mend_, as in _commend_, _emend_, _amend_,
recommend. This is not a marked root. It has nominals in +Ation;
recommendation, _commendation_, _emendation_. If (17) were a true phono-
logical rule, it would apply to the +able derivatives of _mend_ stems.
It doesn't: _commendable_/*_commensible_, _amendable_/*_amensible_. Therefore
(17) must be restricted to marked roots, therefore it is not a rule of
the phonology but a rule of allomorphy, for it does not apply to all
stems of the form _Xnd_ but rather to marked roots of the form _Xnd_.
This is an important distinction. A rule of allomorphy applies to a
designated class of morphemes, and this designation should not be
phonological, but rather morphological. This is true of (17).

š-final stems are curious:

(18)　　　abolish　　　abolition

　　　　　admonish　　　admonition　　　admonitive

　　　　　punish　　　?punition　　　punitive

The fact that we have t/__+ive shows that these stems have a non-final
variant _Xt_. We therefore have a case in which roots show the same
surface segment (š) in two environments, but where there is good
evidence that these two segments must be derived from two distinct
underlying segments, in different allomorphs.

t-final stems show the most varied alternations. As noted, we need
an Allomorphy rule for _vert_, and a glance at our table shows that the
non-final allomorph must be _verz_, rather than the _verd_ originally
proposed. _mit_ too is odd; it shows the form _mis_ (_submissive_, _admiss-
ible_). Other t-final roots require no allomorphy. _t_ remains before

+ive and +able: _assertive_, _transitive_, _excretive_.

From _vert_ we can turn to other cases of rś/rž́:

(19)   coerce   coérsən   coersiv

          disperse   dispérzən (?śən)

          immerse   immérsən (?žən)

          emerge   eméržən   (*śən)

          asperge/se   aspérzən   (*śən)

          submerge/se   subméržən (*śən)   submersible

          deterge   detérzən   (*śən) detersiv

It is clear that with stems in <u>Xurge</u> we always have rž́. The s in

<u>detersive</u> suggests a rule similar to that involving <u>nd</u>:

(20)   g → ž̌/____ +ion
                      +ive
                +abl

Though the change is suspiciously natural, a rule of palatalization,

the fact that it takes place before +able should be sufficient evi-

dence against its naturalness. Note also the back forms <u>asperse</u>,

<u>submerse</u>, <u>disperse</u> and <u>immerse</u> are also back forms. <u>dispersion</u> is ME,

<u>disperse</u> 1450, <u>immersion</u> is 1450, <u>immerse</u> is 1650. That these were

backformed in this way shows the opacity of (20).

The two roots in <u>r</u> are good examples of roots with their own

allomorphs. One is <u>kur/kurz</u>, the other <u>hēr/hēz</u>.

Two <u>n</u>-final roots are of interest, <u>vene/vention</u>, and <u>tain/tention</u>.

We promised earlier to discuss the first. It should be clear by now

that we derive <u>vention</u> by declaring the combinatory allomorph of <u>vene</u>

to be <u>vent</u>. How does this **compare** with the one which attaches +<u>tion</u>?

As noted, the latter needs an exception feature, to make sure the <u>t</u>

doesn't drop, as it presumably does elsewhere. By positing the

allomorphy rule we rid ourselves not only of an exception feature, but of the entire already suspect rule of t-deletion to which vene is a supposed exception.

This rule of t-deletion is put to use in one place in the phonological literature on the subject. Schnitzer (1971) attempts to derive succession from the underlying form sub=kēd+t+iVn. He uses the t to devoice the d. However, there is no way for him to shorten the ē, and his final output is *suk=sēs+ion. One could of course lax the ē before deleting the t, but this is not the general case (excrētion). We conclude that Schnitzer's use of t-deletion is not valid, for it cannot lead to the proper output. In order to derive succession, SPE lists cede (cēd) as exempt from the tensing rule. e is thus shortened by tri-syllabic-shortening, and exceptionally not lengthened again. By using the device of root allomorphy, we can list cede as cess in the relevant environments. The double consonant will prevent the tensing rule from applying. Again we see that allomorphy can be a useful device for encoding an exception feature. Note that successive supports very strongly our rule, for the SPE theory would derive *succetive, as would Schnitzer's, if it worked. One allomorphy rule can be used to cover many irregularities, and sometimes, irregularities which are not encodable into rule features at all. Nor is allomorphy a more powerful device than that of rule features. Rule features interact with phonological rules in ways in which allomorphy rules, because they are prephonological, cannot. This makes rules of allomorphy quite restricted in some respects, as compared with rule features, which can refer to any stage in a phonological derivation.

Returning to n-final roots, we will look at the root tain/tent

(retain/retention).   The t is clearly allomorphic. However, more interestingly, we expect not tain/tention, but rather tain/*tantion. SPE 9p. 202) accounts for this curiosity by having tain undergo the short vowel shift rule. This is a very suspect rule. We will discuss its use in another case below, and show that it is unmotivated. In fact, an inspection of all the items that this putatively general rule applies to reveals that it is a minor rule which only items positively specified to undergo it are subject to. Again we can use allomorphy to rid ourselves of such a rule.

d-final roots, except for cede all show the same forms: d/ž/ž (decide/decisive/decision). We can therefore posit a rule taking d to ž. Again we must ask whether this is really a rule of allomorphy, or a phonological rule. Normally d does not appear before y (the reflex of the i of +ion) except here. In fact, the rule that takes i to y after coronals is blocked idiosyncratically by d in all other cases (pavilion/enchiridion). There is only one case where d+y arises, other than /_+ion, and that is in the word cordial, where it shows up as dž, presumably palatalized from dz. It appears, then, that this supposedly general palatalization of d is confined to the morphological environment in question. It is therefore a rule of allo-morphy as we expect it to be.

Marked non-coronals were listed in (5). They are not too interesting. The only real allomorphy here is stroy/struk (destroy/ destruction) which we noticed in Ch. 2, and ceive/cept (deceive/decept-ion).

This ends our discussion of root allomorphy. I would just like to stress the strength of ben-Moshe's first law here, the law of

allomorphy. If a root takes a given shape in a given environment by a rule of allomorphy, then it takes that shape always. There are no lexical exceptions to rules of allomorphy, and they are a living part of a language.

5.2.2.1. fy and ply.

Rule (4), as stated, is not a rule of allomorphy. This is because its environment is not totally morphological, for it is bounded on one side by #. There is a way to make the alternation expressed by (4) a morphologically conditioned, which is to state not (4) but its reverse, a rule of k insertion. I know of no deciding factor between the two.

It is of some note that a restricted form of the suffix sometimes shows up with verbs in fy. The only common word of this sort is satisfaction. Others are putrefaction, liquefaction, and calefaction. There are about ten all told. The form fac is derived in SPE by applying the rule of Short Vowel Shift to fik, after the i is shortened /_CC (kt). The same case of Short Vowel Shift, incidentally, accounts for the sing/sang alternation. The rule is, as we have noted, dubious. In any case, all these words must be idiosyncratically marked to undergo it. The noncommitant irregularity of the fak forms, that they take +tion, instead of +Ation, was not noted in SPE. Since in their solution the only difference between the fak forms and the regular ones is the rule feature governing the application of Vowel Shortening, presumably the choice of the affix is governed by this rule feature as well. Either that, or it is not decided until the rule in question has applied, i.e. until we can tell fik from fak. Neither system is satisfying. In the one, a rule feature governs something other than

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its rule, a strange situation; in the other, the form of the affix is
not chosen until a late stage in the phonological derivation, a
singular case, for in all others the variant of the affix is deter-
mined at the underlying level.

A simpler solution is to have fak be derived by an allomorphy rule,
conditioned by the preceding morpheme: satis, putre, lique, cale, tume,
tabe, lubri, labe, as well as the following: +At+ion. fak, in its
turn, determines the variant of ion, namely the restricted one. This
solution entails that Allomorphy rules be ordered. Note, however, that
the ordering is from the inside out. Though we have no other evidence,
we might claim that allomorphy rules are always so ordered, in which
case, the ordering, though extrinsic, would not be arbitrary. This,
however, is not exactly a central case, and to base a broad theory
on it is not advisable.

5.2.3. Other Allomorphy.

Though the foregoing account of rules of Allomorphy is detailed,
it is based on one English paradigm, that of the suffixed forms
X+At+ion. The reader is entitled to be skeptical about a vast system
which is based on one example, or even, as in this case, one
phenomenon, though the phenomenon is widespread. In order for my
theory to be plausible, I must find other examples of its utility.
This is not so simple. One must have a good idea of what the phono-
logy of a language looks like before proposing rules of Allomorphy.

One place in which rules of Allomorphy surface is in the
selection of theme vowels. Such vowels are uncommon in English,
however they do appear before certain affixes. The following data are
in part from SPE (p. 129-30):

professor	professorial
manager	managerial
president	presidential
periphery	peripheral
orient	oriental
habit	habitual
tempest	tempestuous
industry	industrious, industrial
arab	arabian
excrement	excremental
exponent	exponential
calamity	calamitous

It is clear that there is often a difference between the unsuffixed and suffixed forms of the base. Sometimes i is inserted before -al, -an, -ous, sometimes u, sometimes nothing. Sometimes we even get deletion of the final segment of the stem (peripheral). The conditions for these variations are not phonological: periphery contrast with industry. Words ending in +ment never have a vowel before +al (*departmential), but other words ending in the same phonological sequence -ent sometimes do, and sometimes don't(parental/torrential, continental/exponential). SPE stresses that whether an item takes i or u or nothing or itself loses a segment, is a property of the item itself. This determination is morphological.

In SPE, this variation is handled by assigning to each stem a stem vowel, which is dropped word finally, but shows up before suffixes. Professor will be entered in the lexicon as /pro=fes+Or+i/, habit as /haebit+u/. They do not discuss forms like peripheral, presumably

derived by a minor rule.

A problem for this analysis is the nominal suffix +y, which

occurs in such words as presidency. According to the above analysis,

the underlying form of presidency must be /president+i+y/. However, the

y is normally vocalized to i by the following rule:

$$y \rightarrow i/C\_\_boundary \quad (SPE\ p.\ 130)$$

This rule operates in such words as industry. Note that the

presence of the stem vowel i in president+i+y will block this rule.

Either there is no stem vowel before y in the first place, or it is

deleted by a new rule:

$$i \rightarrow \emptyset \ /\_\_+y$$

The stem vowel thus shows up before some suffixes and not before

others. It does not show up word-finally. The SPE analysis of the

forms in our paradigm, in order to generate the data correctly, needs,

in addition to the stem vowels, three phonological rules. One deletes

the stem vowel finally, another deletes it before +y, and a third

deletes y in peripheral. These are all phonological rules. They are

all, as far as I know, preceded by no other rule of the phonology.

The allomorphy solution to the paradigm is transparent. Before the

suffixes in question, certain allomorphic changes take place. This step

is equivalent to the marking of stem vowels in the lexicon, which is

needed in any solution. However, now, no more is necessary, no rules.

We have simply incorporated the three questionable rules into the allo-

morphy rules, a step which simultaneously rids us of them and accounts

for their ordering properties.

I don't think that there is any question as to which is the

better solution. They are both descriptively adequate, however the

SPE system is completely ad-hoc, while the allomorphy solution, within a theory which includes rules of Allomorphy, is the only possible one. It is also the correct one.

5.2.4 Allomorphy and Other Parts of a Grammar.

The central import of allomorphy rules is for the phonology. By using these rules, which, it must be emphasized, are highly restricted, we are making predictions about the range of material that can be covered by rules of the phonology, and about the ordering of certain "irregular" processes. It is also important to note that because rules of allomorphy are not phonological rules per se, they are not subject to any of the naturalness constraints that the latter are ruled by. In theory, a rule of allomorphy could change m to t, something we do not expect of a rule of the phonology.

The intuition behind the positing of rules of Allomorphy is quite widespread. People have felt that rules which refer to morphological categories, morphologically governed rules, are ordered earlier in the system of the phonology than phonologically governed rules. Lightner (1972) argues that there is a class of minor rules characterized by the facts that

1) They always apply before all major rules.

2) Their environment always contains a reference to some morphological category.

It is clear, however, that (2) is not a sufficient condition for (1). The English k → s spirantization, which is governed by the morphological feature +latinate, is a rules of the phonology, and cannot be ordered before all major rules. Allomorphy rules are finer than Lightner's rules. k → s is not a rule of allomorphy as defined, for it is governed not by

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a morpheme, but by a morphological feature. For a rule to be a rule of allomorphy, it is a necessary and sufficient condition that it be totally morphological, in the sense defined: it applies to a morpheme, or other than phonologically designated set of morphemes, in the immediate environment of a designated morpheme, or set of morphemes. This is a much narrower definition than Lightner's. Rules of allomorphy, unlike Lightner's minor rules, are defined as morphemes, and not segments. Also, we have narrowed somewhat our version of Lightner's (2), which would include umlaut rules under the category of minor rules, falsely.

By narrowing the scope of our definition, we are of course narrowing the scope of our claim. We are not claiming that all morphologically and lexically governed rules are early rules of the phonology. We are not denying the validity of the English main stress rule, because of its baroque complexity. Phonological rules may be as baroque as they wish to be, but rules of allomorphy, as defined, will precede the rules of the phonology, always.

A particularly fine example of a rule, which, though morphologically governed, is not a rule of allomorphy, and hence orderable among the rules of the phonology comes from Masoretic, and was pointed out to me by Alan Prince. Consider the following pairs:

(al) a) ka:tabti "I wrote" ktabtihu: "I wrote it"

      b) ka:tabt "You (fem. sg.) wrote" ktabtihu: "you wrote it"

The problem is that though the suffixed forms are identical the un-suffixed forms differ. A relatively detailed study of Masoretic phonology reveals that the underlying forms of a and b must be identical (katab+ti), and that i is deleted word finally, in 2.sg.fem.

perf. forms <u>only</u>, at a relatively late point in the phonology, the i

serving to block several otherwise well-motivated phonological

rules which would apply to a form *<u>katab+t</u>. We will formulate the

rule:

(22)   i → ∅/t_#/   2.f.sg. perf.

This rule, as we noted, must be a relatively late rule of the phono-

logy. It is a minor rule in Lightner's sense, and hence should not be

ordered so late in his theory. However, (22) is not a rule of Allo-

morphy. This is because of the presence of the #boundary as the

immediate environment. The morphological category, though it is

crucial to the rule, is not sufficient to make (22) a rule of allo-

morphy. Therefore (22) may be a phonological rule in our theory, in

fact it must be, and hence is orderable at any point in the phonology.

Note that the reverse of (22) would insert i in suffixed forms of

the 2.f.sg.perf., and would be a rule of allomorphy. It is signifi-

cant that (22) and not its reverse, is the correct rule, for the

latter, as a rule of Allomorphy, could not, in our theory, ever be

ordered at such a late point in the phonology, or anywhere in the

phonology, for that matter. For those who doubt fine points, 1 should

point out that Masoretic phonology is one of the best studied of all

linguistic systems, and that the formulation of the rule in question

as (22) and not its reverse, has been established and accepted for

centuries (cf. Gesenius).

Note the similarity between this and the German Truncation Rule

discussed above, which, though it applied to a designated morpheme

<u>ge+</u>, had a phonological environment, and hence was not a rule of

truncation as defined, and hence orderable among the rules of the phono-

logy. The general similarity between rules of Allomorphy, and rules of Truncation should be apparent by now. Both types are defined on morphemes, in the environment of morphemes. The only difference is that one deletes morphemes, while the other adjusts their shapes.

Rules of Allomorphy stand in exactly the same relation as rules of Truncation to WFRs. Their necessity within our system of word formation is brought about by the same separation of all matters concerning the morphology of the base of a WFR from the WFR itself. In the case of rules of Allomorphy, the morphology of the base both itself varies with certain affixes, and causes variation in affixes which have been introduced by phonologically constant operations. Whether both are true of Truncation rules as well is not clear from the examples we have. If Truncation in the forms $\underline{X+At+At=ion}$ applies to the second $\underline{At}$, which is part of the affix, and not to the first, then we have an instance of a Truncation rule which applies to an affix. As noted above, however, the exact formulation of this rule is not clear.

FOOTNOTES

[1]+ee used to attach to verbs which took animate indirect objects as well. This is now obsolete, though the forms still exist. Exceptions to the general case are escapee, refugee, devotee, absentee, standee.

[2]The word dedicatee is an exception to the rule. Its exceptionality might be traceable to the fact that because of English spelling the c of dedicee would undergo the k $\rightarrow$ s rule, giving the surface form [dedisi:].

[3]Truncation only takes place before +able, and not before #able. They are differentiated in a separate work on able.

[4]I am arguing ahead of myself here. Truncation as defined, intrinsically follows all WFRs.

[4a]The transcription and notation are Isačenko's in all the Russian examples cited below. In particular ɨ stands for the vowel (s) commonly termed yer, it is not a boundary.

[4b]I have glossed over the problem of the ɨvowel. Isačenko notes that this vowel must be deleted before the truncation rule applies. Such an ordering is not possible within a theory which sharply separates Truncation rules from the phonology. The whole depends on the reality of the ɨ vowel. It is supported by Halle (73b) but runs counter to the general theory of Kiparsky,1973, for ɨ is a forbidden abstract seg-

ment. If we accept Kiparsky's position, then the rule deleting # is no longer a problem, for it cannot exist.

[4c]This analysis depends crucially again on the existence of a rule of yer deletion, i.e. on the underlying phonological reality of yer. If there are no yers, then another explanation must be sought. In addition, we rid ourselves of the ordering problem (truncation following a phonological rule).

[5] Truncation does occur in relation, and inflation, and similar cases, where it does not occur otherwise. This may be evidence in favor of truncating the second, rather that the first +At, as Brame's analysis suggests.

[6] Brame's analysis here also contradicts Martin's analysis of the affix +ory, which she claims, with much evidence, is derived from the nominal X+ation.

[7]Allomorphy shows up before +able, not #able.

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