4 Item and arrangement morphology: introductory remarks

A discussion of inflectional morphology must inevitably begin with the morphological theories of the 1940s and 1950s, and in particular with the dominant **ITEM AND ARRANGEMENT** (or IA) model. This is a model, in essence, which seeks to split each word (or, to be more precise, each phonological form of a word or **WORD-FORM**) into a number of independently functioning parts or segments. The form *ferrī*, for example, would be divided (as foreshadowed in Part 1) into *fer*, *r* and *ī*, and each of these **MORPHS** or **MORPHEMIC SEGMENTS** would be assigned to a different grammatical element or **MORPHEME**: *fer* to the verbal item itself (let us now symbolise this element by **FER**-), *r* to the Infinitive element, and *ī* to the Passive. Likewise, *fortū* '[he, etc.] is being carried' would be analysed into *fer*, *t* and *ur*, where **fer** functions as before, and *t* and *ur* function as the 'marker' of 3rd singular in the one case, and as a further 'marker' of Passive in the other. The more detailed character of this model will, of course, be familiar to many readers of this book, either from their instruction in linguistics or from the various standard textbooks; let us, nevertheless, begin by focussing attention on three of its most important and controversial features.

1. The first point concerns the generalisation of the **MORPHEME** as the
only minimal morphological element. According to a more traditional view, one might distinguish at least two different sorts of construct: radical elements (such as fer-) on the one hand, and inflectional elements (such as Infinitive or Passive) on the other. But in the Item and Arrangement view a distinction of this kind would for our present purposes be quite irrelevant. Fer-, Infinitive and Passive are formally undifferentiated morphemes, which are related to each other in precisely the same way (see the discussion of point III), which are related in precisely the same way to their corresponding morphemic segments (point II), and which are assigned to ‘morpheme classes’ (such as Verb, Mood and Voice) in precisely the same way. As members of different classes they could, admittedly, behave diversely: Fer- as a member of an ‘open’ class which supplies the nucleus (in some sense) of the word-construction, and Infinitive and Passive as members of closed classes with a loosely qualifying role. But this difference would have no bearing on the problems which we have set for the present investigation. The distinction between ‘radical’ and inflectional elements, as such, is eliminated entirely from the strictly morphological level.

II. The second point concerns the nature of what may be called the allomorph or alternancy relation. This is the relation which obtains between the separate segments, on the one hand, and the separate morphemes of which they may be said to be the exponents: thus the fer segment in ferris is the allomorph or alternant of fer-, and the r and is segments are allomorphs of Infinitive and Passive. It is possible, as we have seen, for a given morpheme to have two or more different alternants, each of which appears in a different set of word-forms: thus the ur segment in furter would be handled as a further allomorph of the Passive morpheme which alternates with the is of ferri: under statable conditions. What would not be possible, however, would be an overlapping of function between one segment and another. The segments of a word-form have to be assigned to quite distinct elements; one would contravene the model if, in furter for example, one were to relate ur to both Passive and the 3rd Person morpheme, while relating the latter to r as well. The allomorph restriction is, in other words, to a simple pairing of ‘bits’ or segments of each word-form with entirely separate ‘bits’ of its more abstract morphemic representation.

III. The third and last feature would be the extension, into word-structure, of the type of sequential or linear construction employed (on the basis of languages such as English) for the study of sentence-structure. According to the Item and Arrangement model, the construction of a word such as ferris may be expressed in terms of a straightforward sequence of morphemes:

FER- + Infinitive + Passive

in which Fer- precedes Infinitive, and Infinitive precedes Passive, in exactly the sense that ferris is as a whole, within the wider syntagm

brukedelicate et skelere ferris: (to be carried away by his crimes and

Harris, 1951a: 157ff., and compare ‘morpheme alternant’ (versus ‘morpheme unit’) in Harris, 1942, for ‘allomorph’ see Nida, 1948: n. 13 (= RIL, 258), 1949: 14. Finally, the reader should note that the term ‘alternation’ is commonly used both for the relation between ‘alternants’ (e.g. below and by Lamb, 1964a: 160) and for the relation between alternants and morphemes.

1 ‘Exponent’ and ‘exponent’ will be used, in this discussion, as general terms to refer to relationships defined between grammatical and phonological constructs; compare the Firthian usage for relations between descriptive constructs and the data (Robins, 1963: 21) and the wider or looser usage of Halliday (1961). Thus the allomorph-relation is a particular exponent-relation defined in terms of a given model (pp. 49ff., below). For an attempt at explication in terms of an alternative model see ch. 9, p. 185.

2 See below, pp. 49ff., for modes which might support such analyses.

3 The term ‘syntagm’ (Saussure, 1916: 170) may be used to refer to any piece or group of words within the sentence, whose members stand together in some syntactic construction. In the various ‘Saussurean’ schools its precise use varies, depending on the theory of constructions which is adopted; it is perhaps as well to stress that I am not using it in any more specialised sense, e.g. that of Bally (1944: 102).
crueity') would be said to follow the further word skelere or the phrase bru:delita:te et skelere. The model will accordingly destroy the usual distinction between syntactic and morphological structure: in the syntagm under discussion, the connections between the 'ultimate constituents' Fer- and Infinitive or, let us say, sceler- and Ablative are simply part and parcel of the same unified grammatical analysis as those between ferri: as a whole and the remaining 'immediate constituent' bru:delita:te et skelere or, within this constituent, between bru:delita:te or skelere and et.

3 Is there the slightest reason, then, for supposing that the word should play a crucial or pivotal role in grammatical theory?

These three features can hardly fail to be regarded as controversial - if only because the last, in particular, is so clearly at variance with the classical tradition. To what extent, therefore, can their predominance in the textbooks (and in general linguistic practice) be justified in the context of our present study? Certain points may, of course, be conceded at once: for example, the model is undoubtedly successful in the description of many agglutinative languages. But is it equally appropriate for the type of inflecting language which we ourselves are concerned with? Or will it be more illuminating, for our purposes, to say that there is, in fact, a distinction between radicals and inflections; that there is in fact an overlapping of function between inflectional segments; and that sequence within the word is in some sense grammatically irrelevant.

The answer must, of course, depend on a fairly detailed study of the descriptive problems. However, there are at least two a priori factors which suggest that the Item and Arrangement concept ought, for the moment, to be regarded as an important hypothesis. The first is the unusual simplicity and homogeneity of the model. It employs only one fundamental unit; it reduces all statements of sequence to one, very straightforward, relation; and it reduces the description of word-structure to a form in which it may be conflated with the description of sentence-structure. Let us suppose, moreover, that the grammatical representation of a word-form was not of the Item and Arrangement type: how then would the non-sequential (or only partly sequential) relations within the word 'mesh in' with the presumably linear relations which would obtain between words as wholes? For many constructions one would clearly have to refer both to elements with a sequential role (for example, the preposition per in the construction of per Italian) and also to certain non-sequential elements (e.g. the Accusative in the same construction); would this not lead to some rather complicated problems of formalism? One may be confident, no doubt, that a satisfactory formalism could eventually be achieved. Indeed there has already been some discussion (for primarily syntactic rather than morphological reasons) of the role which non-sequential relations might play in grammatical theory.

Nevertheless a purely linear model, e.g. of the Item and Arrangement type, will undoubtedly raise fewer difficulties.

Such considerations of simplicity can admittedly be pushed too far; one should be wary of any argument which does not go beyond the intrinsic or purely logical qualities of a theory. There is, however, a second factor which is at once more interesting and (one may add) of a more directly vulnerable nature. Let us consider, for the moment, not the task of a linguist analysing the structure of an utterance, but the position of the native speaker hearing and understanding it in a normal speech situation. How precisely does the human brain accomplish this feat of 'understanding'? How precisely, for example, might a Roman's 'mental processes' have succeeded in 'perceiving' an utterance of the word fertur in terms of the verb which we translate by 'bring' or 'carry', the semantic features of the Passive, and so on?

At this point the Item and Arrangement model appears to offer a

1 For the 'homogeneity' of IA compare Hockett, 1954 (= RIL, 397). The principal economies will be spelled out in greater detail in ch. 7, pp. 117ff. where we will also discuss the important 'isomorphisms' between 'morphemes' and contemporary theories of phonology.

2 'Through Italy': the construction involves both a fixed sequence (Preposition + Noun as opposed to Noun + Preposition) and a relation of governance involving the Accusative Case of the Noun.

3 See 7-4-3, in particular pp. 146ff., for references and for a thorough discussion of this issue.

4 Compare ch. 3, pp. 221.
particularly straightforward and seductive hypothesis. In the first place, the process of understanding or 'decoding' an utterance might plausibly be assumed to follow the hierarchy of levels established in a description.1 The first step (to continue with what is admittedly an example from a dead language) would be to decode the phonetic input to yield a perceived sequence of phonemes \( f + e + r + i + u + r \); the next step after that would be to decode this sequence, in turn, into the perceived sequence of morphemes 
\[ \text{fer} + \text{3rd singular} + \text{Passive} \]; the next after that would be to determine that \text{fer} was the head of a verbal construction, the sequence as a whole the head of a one-word sentence-construction, and so on. Given such an overall schema, the concept of morphs and allomorphs might be transferred intact to the second stage of the process. The decoding 'strategy' for the perception of morphemes would literally involve the isolation of tentative morphemic segments (for example, all of \text{fer}, \text{t}, \text{u}, \text{ur} and \text{r} might be possible morphs in Latin) and the association, with these segments, of the morphemes to which they could be assigned as alternants. The details of such a strategy are naturally unimportant; what is significant, however, is that the phonetic or phonological 'shape' of a morpheme could be said to act, in a very real sense, as the 'marker' or 'signal' of the various semantic features involved.2

The phonetic piece \text{ur} would be a literal signal, for example, of the Latin Passive: what other model - a model, that is to say, which does not incorporate the allomorph-relation - could yield such a simple and illuminating hypothesis concerning this aspect of speech-communication?

A sceptic, such as the present writer, will wonder whether this is really a revealing insight into the workings of the human brain. There is certainly no direct linguistic evidence which could support it; one could not claim that a grammar was inadequate (in the higher Chomskyan sense) if it failed to supply the basis for a decoding process of this kind.

1 Cf. Hockett, 1955: 4ff, particularly the 'morphemicizer' and 'speech receiver' boxes in fig. 1 (3) and the definition of levels on 14ff. Compare also our references in ch. 3, p. 30, n. 1; for computer simulation of human processes in terms of such models see Lamb, 1968: 174ff.

2 It has to be admitted that I cannot document this precise perceptual hypothesis in the literature - though the terms 'marker' and 'signal' are widespread (for 'signal' see already Bloomfield, 1935: 162, and cf., e.g., 'linguistic signalling is done...with morphemes' in a contemporary popularisation (Joos, 1950 (= RIL, 355)). More recently Joos (1964, 1967: 4) has gone further, talking as a postulate that 'text signals its own structure'; again this crystallises a point which is implicit, but only implicit, in earlier discussion. One has a strong suspicion that the truth of such hypotheses was simply taken for granted by the Item and Arrangement school.

But one is presumably allowed to make gestures of faith. If the facts of language would bear out an analysis into sequential signalling units - if the allomorph relation could genuinely display the properties which appear to be necessary - then one is surely entitled to claim that the model is 'natural' or 'appropriate' in some sense. The question is: do the facts of language bear this out? This is a crucial question, and one which will be the subject of particular attention later in our argument.1

For the moment, however, let us assume (in the light of the signalling concept and on considerations of simplicity, as above) that the Item and Arrangement model is an attractive initial hypothesis. The argument in Part II as a whole may then be organised as follows. First, we shall begin by formulating IA more closely (ch. 5): both the model itself and the general nature of possible rule-systems, insofar as it is necessary to consider these in the arguments which bear on its evaluation. We shall then examine various facts and problems in the analysis of the Latin verb, which are at once typical of the so-called inflecting languages, and which can be seen to conflict with the Item and Arrangement concept as it has been presented (ch. 6). Throughout this survey, which is unavoidably somewhat episodic in character, we shall increasingly hint at formulations of a different sort. At the beginning of the final chapter (ch. 7) we shall sketch an alternative model - 'Word and Paradigm' or WP - which stands, in effect, at the opposite extreme from IA (7.1). We shall then try to tie together the scattered arguments of the earlier section, in the form of a systematic comparative assessment of Item and Arrangement, Word and Paradigm, and of possible compromises between them (7.2–5). The conclusion will be that for Latin and other inflecting languages, some form of the Word and Paradigm approach should be preferred.

1 See pp. 81ff.
5 Item and arrangement morphology: an outline formulation

We shall begin, therefore, by attempting to formulate the Item and
Arrangement approach in greater detail. What, in particular, are
the precise restrictions which should be placed on the allomorph relation?
And how, in general, would this relation be handled by generative
rules? This second question did not, admittedly, arise for the original
‘morphemicists’; the methodology of the 1940s and early 1950s was, for
these writers in particular, of a markedly ‘non-generative’ sort.\(^2\) On the
other hand, it will be difficult to assess the model, within the terms of
this book, unless some generative reformulation can be assumed. Let us
accordingly take, as our starting-point, the most extreme and uncompli-
cated version of morphemic theory: a version which derives from the
earliest clarification of the allomorph concept,\(^3\) rather than the some-
what modified definitions of the 1950s. The task of formulation will then
be relatively straightforward – and we may return to the complications
in our later sections.\(^4\)

5.1 Basic terms and relations

First we must recapitulate the features which were illustrated discursi-
ively in the previous chapter. Briefly, the relevant part of the Item
and Arrangement model would postulate:

(a) A set of morphemes \(M\) (exemplified in Latin by \(\text{FER-}\) or Passive),
(b) A set of phonemes \(\Phi\) (e.g. \(f, e, r, t, \) or \(n\)), and
(c) A relation of sequence \(S\) over \(M\) at the grammatical and \(\Phi\) at the
phonological level.

Thus, in the analysis of our original example \(\text{ferris}\), \(\text{FER-}\) precedes
Infinite and Passive; likewise \(f\) precedes \(e\), and so on. The primitives

\[M, \Phi \text{ and } S\] together define the morphemic representation and the
phonemic representation of a word or word-form. Thus the morphemic
or grammatical representation of \(\text{ferris}\) may be shown by the formula:

**Morphemic Representation:** \(\text{FER-} + \text{Infinite} + \text{Passive}\)

(taking the plus-sign as a symbol for the sequence or S-relation), and its
phonemic or phonological representation by the similar formula:

**Phonemic Representation:** \(f + e + r + i + s:\)

- \(\text{FER-}\), etc. being (as we said) members of \(M\), and \(f, e\), etc. being members
  of \(\Phi\).

To relate these representations the model will then go on to specify:

\(d\) A set of morphemic segments \(\Sigma\) (e.g. the morphemic segments \(\text{fer}
\) or \(\text{r}\)), and

\(e\) An allomorph-relation \(A\) over domain \(\Sigma\) and range \(M\):

- \(\text{fer}\), for example, being an allomorph of \(\text{FER-}\). Looser and stricter
  versions of the Item and Arrangement model may now be said to differ,
in effect, as to the restrictions which are placed on \(\Sigma\) and \(A\) in any given
  word-form. Some allow morphemic segments to be discontinuous;\(^2\)
some allow a phonological residuum (or ‘empty morph’) which is related
to none of the relevant morphemes;\(^3\) some allow morphs to overlap;\(^4\)
some allow them to coincide completely;\(^5\) and so on. We ourselves
  propose – see the beginning of this chapter – to take the most rudimentary
  version as our starting-point; let us accordingly require that:

(i) Each member of \(\Sigma\) is a continuous sequence over \(\Phi\) (i.e. discon-
  tinuous ‘segments’ are excluded), and

(ii) For any corresponding phonemic representation \(P\) and morphemic
  representation \(G:\)

(1) Where \(G\) is a sequence of \(n\) morphemes, \(P\) is partitioned into
  \(n\) morphemic segments,\(^6\) and

(2) \(\sigma\) is the allomorph of \(m\) if and only if, for some \(i, \sigma\) is the \(i\)’th
  morphemic segment in \(P\) and \(m\) is the \(i\)’th morpheme in \(G\).

In other words, the morphs and morphemes are paired off in an exhaust-
ive one-to-one correspondence, according to the order in which they

---

\(^1\) ‘Generative’, note, in the second or wider sense of ch. 2, p. 9.
\(^2\) See 7.1, pp. 109ff. for general discussion of the post-Bloomfieldian background.
\(^3\) Namely, that of Harris (1943).
\(^4\) See successive reformulations of the allomorph-relation in p. 59, n. 1, p. 61, n. 3,
  and so on.
\(^5\) For the domain and range of a relation cf., e.g., Stoll, 1961: 28.
\(^6\) Cf. Harris, 1951b: 165.
\(^7\) Cf. Hockett, 1947 ("RIL", 236); discussion in pp. 78ff., below.
\(^8\) See pp. 69ff., below.
\(^9\) See pp. 54ff., below.
\(^10\) For the technical sense of ‘partitioning’ cf. Stoll, 1961: 14. The point of introducing
  it here is to exclude (partial) overlapping of morphemic segments.
appear at their respective levels. The complete analysis of a word-form may accordingly be shown by a diagram of the type shown in fig. 5.1, where the morphemes and morphs are arrayed in sequence from left to right, and the allomorph-relation is symbolised by an arrow pointing away from the morpheme. The analysis is, as we remarked in the previous chapter, no more than a straightforward pairing of ‘bits’ of the lower representation with ‘bits’ (minimal bits according to restriction (ii)) of the higher.

Morphemic Representation: \( \text{FER-} + \text{Infinitive} + \text{Passive} \)

Phonemic Representation: \( \text{fer} + r + t; \)

Fig. 5.1

5.2 Morpholexical and morphophonemic rule-systems

The preceding section is little more than a recapitulation, in semi-formal style, of material which has already been covered in the preamble. But how will this picture of morphology be rendered in generative terms? What, as we have asked, would be the nature of the rules which correspond to the allomorph concept?

Let us return to our original account of this relation. As we pointed out, a given morpheme may have two or more different allomorphs, of which one appears in one set of word-forms and another in another. Thus the Passive morpheme was said to be realised by \( \text{i} \); in forms such as \( \text{ferri} \), but by \( \text{ur} \) in forms such as \( \text{fertur} \). Similarly, a verbal root morpheme which we may symbolise by \( \text{REG-} \) (‘rule’) would have two different ‘shapes’ or alternants \( \text{reg} \) and \( \text{rek} \) – compare forms such as \( \text{reg-un} \) [‘they drag’] versus \( \text{trak-s-i} \): ‘I dragged’ or \( \text{trak-t-a} \) [‘she is, etc.] dragged’; the voiced/voiceless variation holds for the elements \( \text{nub-} \) in forms such as \( \text{mub-unt} \) [‘they marry’] versus \( \text{nup-s-i} \): ‘I married’ or \( \text{nup-t-a} \) [‘she is, etc.] married’; and both together recur in further forms such as \( \text{reg-unt} \) [‘they cover’] versus \( \text{tek-s-i} \): ‘I covered’ or \( \text{tek-t-a} \) [‘she is, etc.] covered’. On the other hand, there is no comparable alternation (say, between \( \text{ti} \) and \( \text{tur} \) or \( \text{e} \) and \( \text{or} \)) which can be set beside the alternation in the Passive. Here again it is, of course, the \( \text{REG-} \) example which raises a problem. The type of rule which is appropriate for the Passive should surely be supplemented by a more general form of statement, which will bring whole sets of morphemes under the same heading.

Finally, the conditioning factors for \( \text{reg} \) versus \( \text{rek} \) may be stated in a

---

1 Orthographically ‘\( \text{rex} \)’ and ‘\( \text{recta} \)’.
form which is substantially different from those applicable to *i: versus *ur.
In the latter case, the complete rule is one which might be set out quite
satisfactorily in the form:

(2) Passive has the allomorph *i: when preceded (in a morphemic repre-
sentation) by Infinitive, but the allomorph *ur when preceded by
3rd singular,

and which may be taken to show that the alternants are MORPHEMICALLY
(or GRAMMATICALLY) CONDITIONED. The conditions refer, in other words,
to the particular grammatical elements which form the "environment" of
the morpheme concerned. But in the case of the REG- morpheme the
alternation is systematic, in the sense that the alternant with the voiced
* g appears regularly before a following vowel (which is itself, of course,
phonetically voiced) and the alternant with the voiceless * k before a
following * s or * t (which are likewise themselves voiceless). Moreover
the alternants of TRAH- and TEG- appear also, as we remarked, under similar
circumstances. In the light of this, the particular morphemes which
happen to form the environment in particular cases – 3rd plural (with
the allomorph urit) in reg-urit, Perfective (with the allomorph s) in reg-
s-s-i: and so on – are strictly irrelevant to the rule which we want to
formulate. As compared with Passive *i: and *ur, the alternation here is not
grammatically, but PHONEMICALLY (or PHONEMICALLY) CONDITIONED.

To most readers all three points will, of course, be familiar from the
previous literature on morpheme-alternations. From the point of view
of rule-formation, however, it is perhaps the second which is of crucial
importance. In the case of the Passive morpheme, the description cannot
but be epistemic or quasi-lexical in character: a statement such as (2) must
be predicated of an individual element in precisely the way that (1), in
our first illustration, was predicated of the individual element 3rd singu-
lar. But in cases such as REG-, it is a matter of general agreement that the
description should involve two separate modes of statement: it has to be
episodic in part (since we must, of course, specify that REG- has initial *r
as against * tr in TRAH-,* n in NUB-, etc., medial * e: as against *a: in TRAH-
and so on); but it should also contain generalised rules of some kind

1 For the contrast between phonemic and morphemic conditioning cf. Harris, 1951: 208–10; Nida, 1945: Principles 2 (41f) turns 2 (41f). Textbook treatments in, for ex-
2 The most useful surveys are those of Bloomfield (1935: 210f), Wells (1949), Hockett
(1958: 277f.), Harris (1951: 208–12 and 220f). The three points which we have
mentioned here will be picked up again, in particular in ch. 10, pp. 209f.; see also ch.
14 for a discursive discussion of the distinction as it affects evaluation-procedures.
But these later discussions will not be restricted to an IA framework.

5.2 Morphological and morphophonemic rule-systems

(since we want to treat the variations of length and voicing as instances
of a wider regularity). What, then, would be the form of these generalised
statements? And how would the two modes of description fit together?

Different linguists will, unfortunately, give partly conflicting answers
to these questions. Let us, however, begin (for purposes of discussion)
by adopting a solution which would perhaps command the widest
measure of acceptance.1 According to this scheme, the difference be-
tween these two types of alternation will correspond, in effect, to two
successive stages in the specification of the allomorph-relation. Taking
the grammatical representation as the starting-point, the first stage will
specify a MORPHOphonemic or BASIC PHONological REPRESENTATION by
rules which are essentially of the format illustrated by (1) and (2) in our
preliminary discussion. For example, the Participial stem reg:- (as in
reg-k-t-a) would have the morphemic representation:

Morphemic Representation: REG++Past Participle,

and would be assigned a morphophonemic representation of the form:

Morphophonemic Representation: *reg++t3

by rules which introduce reg as the only "basic form" or BASIC ALTERNANT
of REG-4, and t as one of two basic forms of the morpheme Past Participle.
Thus:

(3) REG- has the basic form reg, and

(4) Past Participle has the basic form t when preceded by morphemes
such as REG-, but the basic form s when preceded by morphemes
such as LAB- "slip", PREM- "press", etc.

(Compare la:p-s-a [she has, etc.] slipped' or pres-s-a [she is, etc.
presessed].) Rules such as (3) and (4), and the comparable rules for 3rd

1 For a thorough discussion, in a context more general than IA in particular, see ch. 10,
below. In the present chapter there will be modifications in part, as a consequence
of criticisms of the IA model (see pp. 77f. and 79f.), but the most controversial
issues (cf. ch. 11, pp. 198f.) are independent of the IA/WP confrontation, and can
therefore be safely postponed.
2 See p. 54, n. 4, for references on this term.
3 The asterisk will be used to mark any word-form (or part of a word-form) which is
not in its actual or phonemic representation; thus *reg-t-a, and *reg in particular,
are not yet in their actual forms reikt and reik:. This asterisk is put in purely to
assist the reader, who might otherwise be puzzled as to the provenance of the forms
concerned; it is not actually PART of the basic forms which are postulated, and it plays
no role in any of the rules which are or will be proposed.
4 Terms such as "base" or "basic form" have a long history in pre-generative dis-
cussions; see Bloomfield, 1935: 164, 209f.; Wells, 1949: 101; Hockett, 1958:
28f.; Nida, 1949: 45, for uses which are relevant at this point, but which are often
wider and often defined in a different way. Note in particular that our concept
includes that of a "theoretical base form" in the sense of Hockett (loc. cit.) – i.e. a
basic alternant not identical with any "actual" alternant.
singular, Passive, and so on, will constitute the episodic or quasi-lexical part of the morphological description. They may accordingly be referred to as morpholexical rules, and this type of rule may be said to form the morpholexical rule-system.¹

Taking these morphophonemic representations as a fresh starting-point, the second stage of the description will then modify each basic alternant, where necessary, in a way which will take care of the remaining alternations. In the example before us, the basic form i is of course identical with the form in the actual phonemic representation; no rule need apply, and the segment will pass through unmodified. But in the case of REG- it is now possible to formulate two separate generalisations. So far as the vowel is concerned:

(5) Any short vowel is modified to the corresponding long vowel whenever it is followed by g (in the same basic segment) and an s or t (in the segment following)²

- hence e, in this context, must be replaced by e. Likewise, where the consonant is concerned:

(6) Any voiced consonant is modified to the corresponding voiceless consonant whenever it is followed by another consonant which is itself voiceless

- hence g, in this context, is replaced by k. Generalisations of this kind, which handle the relation between the morphophonemic and phonemic representations, are most commonly referred to as morphophonemic rules, and together may be said to constitute the morphophonemic rule-system.* Of the two cited, (5) could readily be extended to apply to the a in *trah-t-a (which we might establish as the basic representation of track-t-a) as well as to the e in *reg-t-a. Likewise (6) would apply as readily to the voiced b in a form *nub-t-a (which we might establish as the basic representation of nub-t-a). It is these rules, accordingly, which supply the non-episodic part of the description.

Morphemic Representation:  \[ \text{REG-} + \text{Past Participle} \]

Phonemic Representation:  \[ \text{re:k} + \text{t} \]

Fig. 5.2

In the light of this discussion, it is now evident that the simple pairing of representations which may be illustrated by fig. 5.2 (or by fig. 5.1 in the preceding section)¹ will, in effect, be broken down into the type of three-tier analysis shown in fig. 5.3, where the intervening representation is already largely identical, but yet not wholly identical, with the

Morphemic Representation:  \[ \text{REG-} + \text{Past Participle} \]

Phonemic Representation:  \[ \text{re:k} + \text{t} \]

Fig. 5.3

'actual' representation which follows. Further details, e.g. as to the precise formal status of this representation, need not concern us in our present argument.

in addition to the references in n. 1, above, compare the important article by Swadesh & Voegelin (1930). The term 'morphophonemic rule' will thus be used as in the text (for Hockett, for example, all our rules would be 'morphophonemic'), and likewise 'morphophonemic representation'; morphophonemics will be putative primitives at that level (for the relevant theory see 10.1.1) or alternatively 'basic phonemes' which are not distinguishable at the phonological level as such (cf. ch. 10, p. 207f.).

¹ P. 30, above.
6 Characteristic problems of inflecting languages

The model which we have described has, as we remarked earlier, certain *prima facie* attractions. For example, we have acknowledged that it is satisfactory (so far as this goes) in its application to agglutinating languages. How well, however, does it apply to an inflecting language such as Latin? It will, in fact, be clear at once that some features cannot be handled, at least within the extreme version of the model which we have outlined. For instance, the *ì* in *fertur*, which we have been obliged to treat in terms of a single, indivisible morpheme '3rd Singular', is normally assigned to two 'simultaneous' grammatical elements: 3rd Person on the one hand, and Singular on the other. Should one, in such a case, be so bold as to reject the description which is normally put forward? If so, there is no problem — or at least no morphological problem. But if one does not reject it, one is faced with an important theoretical dilemma. Is it possible, on the one hand, to modify this extreme version of the IA model in such a way that these analyses can be included? In the case of the *ì* in *fertur* some linguists might say that it is, and imply, in particular, that the essence of the Item and Arrangement approach is not destroyed by the modifications required. Alternatively must we switch, instead, to a model of a fundamentally different kind? The writer's view (as foreshadowed earlier in this part) is that we would have to modify and qualify to such an extent that, in the end, the Item and Arrangement approach must be abandoned.

Let us begin, therefore, by discussing individually the various facts and problems of analysis which are in question. Of these some are very well known: for example, languages such as Latin have often been characterised as 'synthetic' or 'fusional' solely on the basis of the feature illustrated above. Others, however, have not yet received nearly enough attention. From the viewpoint of the Item and Arrangement model, it may, however, be helpful to divide them very roughly into the following four headings:

I. The first heading (6.1, below) covers phenomena which are traditionally known as infixation and partial suppletion. These are taken together on the grounds that they raise similar problems for the rulesystem, which cannot be resolved without drastic alteration to that part of the theory at least.

II. The second includes three sorts of phenomena in which, in effect, the number of distinct segments in the word-form appears too few for the number of elements which may be established at the grammatical level. The *ì* in *fertur* illustrates one such case; in effect, we are faced with two 'morphemes' (3rd Person and Singular) but only one relevant segment to play around with. But this is not, as we say, the only type of case in which such a discrepancy can arise. The implications of all three are discussed in detail in 6.2.

III. The third (6.3) concerns, in effect, the opposite discrepancy: i.e., there are too few grammatical elements for the number of segments which can be distinguished in the word-forms. Again, it is possible to distinguish different sub-cases, which raise different problems for the model.

IV. Finally, there are various related arguments which link together two crucial questions: first, the question of the morph or segment as an individual signalling unit (already raised by the *prima facie* argument for Item and Arrangement morphology at the beginning of this part), and secondly, the status of the word in linguistic analysis. It is at this point (6.4) that we shall be seen to move decisively towards the 'Word and Paradigm' alternative.

We must now plunge into a point-by-point examination of the individual problems concerned.

6.1 Morphological processes

Let us begin, then, with the problems posed by *infixes* and what may perhaps be most conveniently called *replacive formatives*.

---

1 See above, pp. 42ff.
2 For 'replacive' see the references to Nida (1948, 1949) cited below. From now onwards the general term 'formative' can usefully be introduced (compare the general term 'exponent', p. 43, above) when we wish to refer to an inflection or relevant complex of features within a word-form, independently of the particular IA model now under discussion. Within IA formatives must, of course, be 'morphs'; but here, for example, the premature use of that term might beg the question. For
phenomena can be illustrated with the same Latin verbs rumpo ‘break’, relinquuo ‘leave [behind, etc.]’, or fundo ‘pour’. On the one hand, in forms such as rump-o: ‘I break’, relinquixo-o: ‘I leave’, or fund-o: ‘I pour’, the Imperfective (or ‘Present’) stems rump-, relinquix-, or fund- are commonly said to consist of the root rump-, relinquix- or fund- with a nasal infix m, y or n; for the non-nasal forms of the root compare the Past Participle stems rump-t-, relinquix-t- or fund-t-. On the other hand, the Perfective stems for these same three lexical elements, which appear in forms such as rump-i: ‘I broke’, relinquix-i: ‘I left’, and fund-i: ‘I poured’, may be said to derive from the root by the REPLACEMENT of the radical short vowel with the corresponding term in the long vowel-system: thus the short u in the roots rump- and fund- is lengthened to u, and the short i of relinquix- is lengthened to i.\(^1\) It is clear that nothing in these descriptive statements will, in fact, conform to the particular model which we have outlined in the previous section; how serious, then, are the discrepancies?

It is instructive to consider this question from two rather different viewpoints: from the viewpoint of the model on the one hand, and from that of the rule-system on the other. If we ignore the formulation of rules, there is little doubt that the problem of infixation is far less crucial than that of ‘replacives’. An infix is, after all, a segment of some kind; the model can therefore be extended, with no great conceptual strain, to accommodate the type of analysis shown in fig. 6.1 where the morph m is merely enclosed or embedded in a discontinuous segment ru...p. The only change required would be the modification of restrictions (i) and (ii) in our earlier presentation.\(^2\) The notion of ‘replacement’, on the other hand, is one which is quite foreign to the Item and Arrangement view of language. What is involved is not a certain segment in a certain position (for example, the segment u: in the position shown in fig. 6.2), but the process by which the segment arrived in such a position; to speak of this process as a ‘morph’, or as the ‘allomorph’ of a particular morpheme, would be a blatant conceptual error.\(^3\)

---

1. Cf. p. 49. Restriction (i) would then read ‘...a segment (possibly discontinuous)...’, and the modified version of restriction (ii) would presumably require that for any phonemic representation P and corresponding morphemic representation G:

(i) P is partitioned into a morphemic segments,
(ii) A defines a one-to-one correspondence between the set of morphemic segments in P and the set of morphemes in G,

and (ii) A(m) precedes A(m) if and only if m, itself precedes m.

A less elaborate formulation would appear to allow too much else besides.

2. Cf. Bloch, 1947 (=Ril, 444), Bazell, 1953: 8, also the beginning of Bazell's comments (1952a (=Ril, 11, 273)), on Harris, 1951a, 292. The attempt to disguise 'replacement' as a segment is perhaps at its clearest in Harris, 1951a: 167; but see also Nida, 1948 (=Ril, 262-264), 1949; 54; Hockett, 1947 (=Ril, 240). This last passage was subsequently acknowledged an error by Hockett himself (1954: 11 (=Ril, 94)).

3. For example, the problem of infixes (and discontinuous morphs in general) is not discussed alongside of that of 'replacives' and 'subtractions' in Hockett, 1954 (=Ril, 397-411).

4. E.g. the Past formations of sang as against sing or tuk (‘took’) as against teik (‘take’); see, in particular, the references to Bloch (1947) and Nida (1948) given in n. 2, above, and more recently Robins, 1964: 205; Díez, 1957: 26ff.
from the generative viewpoint the issues appear in a somewhat different light. First of all, the term ‘replacement’ will clearly refer to a new type of rule and not, as in the preceding paragraph, to some aberrant type of analysis. We need a rule, in effect, which can assume the presence of some neighbouring form (rup- or relikw-, for example), which can analyse this form into its component phonemes (analysing rup-, for example, into a pattern of Consonant + Vowel + Consonant), and which can alter one or more of these phonemes by a process or operation of some kind. Thus the rule under discussion would operate on rup- or relikw- by substituting ut for u or i: i for t. A formulation of this kind undoubtedly has awkward implications for the Item and Arrangement theory, both for the specific form of rule and for the wider interpretation of the morpho-lexical system. But the impasse which was reached by the early 1950s owed something, at least, to the intrusion of an essentially generative problem into what was still a non-generative approach to language. The solution was hindered by an inappropriate methodology as well as by an inappropriate model.1

Let us assume, then, that replacements can be handled along these general lines. But would the treatment of infixation be significantly dissimilar? Our nasal segment, qua segment, may admittedly be specified by a statement which is comparable to those in the preceding section:

(7) Imperfective has the allomorph m, n, or u when preceded by rump-, relinqu-, etc.;

but how does one ensure that the forms will be rump-, relinqu- or fund-, instead of rup-m or the like? It seems clear that infixation, too, must involve the breaking down of some ‘existing’ form (thus rup– will again be broken down into Consonant + Vowel + Consonant), and an operation on this form which will, in this case, insert a wholly new segment. The m of rump-, for example, must be inserted at a specific place in the internal structure of rup-. When looked at from this angle, the exclusion of infixes would surely represent an equally important lacuna in the Item and Arrangement rule-system.

What now are the possible alternatives? The type of rule which we have outlined in the last two paragraphs will specify what may conveniently be referred to as a MORPHOLOGICAL PROCESS; should we reject these processes, and look for some more amenable description?

1 Two noteworthy suggestions, that is. Other apparent possibilities have been touched on in the literature: for example, that English teik and teik should be analysed into t...k, u (Past Tense altemant) and ei (Present Tense altemant). But even a more sophisticated treatment involving ‘vowel-alternation’ (Robins, 1964: 205; also Dik, 1967: 367f.) would appear to raise equal problems in the generative context.

2 The leading discussion is that of Bloch (1942). But compare one of the analyses of English men which were mooted, at the same time, by Hockett (1947 (=RiL, 240)).

3 For the latter alternative, read ‘a . . . segment (possibly null)’ in restriction (i), p. 49. For the former, amend restriction (ii) to the form shown in p. 59, n. 1; but read ‘...a subset of the morphemes in G’ for ‘the set of morphemes in G’ in (8).

Or should we accept them and try, eventually, to adapt the theory? There have, in effect, been two suggestions which would take the former course.1

1 The first, which is notorious, would involve the introduction of a so-called ZERO-ALTERNANT.2 The form rup-, for example, would be analysed in the form shown in Fig. 6.3, where rup- is a perfectly straightforward allomorph of rump-, and the symbol ‘o’ denotes a ‘zero morph’ which is deemed, for the sake of conformity, to serve as the allomorph of Perfective. Unfortunately, it is hard to discover any independent reasons which would support such an analysis. Its chief merit, in the context of the late 1940s, was precisely that it enables one to pre-

Morphemic Representation: rump- + Perfective

Phonemic Representation: rup- + o

Fig. 6.3

serve the letter of the Item and Arrangement model: all one need do is to allow a morpheme to lie outside the allomorph-relation or (if one must speak of zero) to allow a morphemic segment to be null.3 But why preserve the letter when the spirit is so clearly violated? The essence of the model is that each grammatical element should have a distinct and overt realisation: in furtur, as we have pointed out, the t and ur are discrete markers (and perhaps the perceptual signals) for the discrete elements ‘3rd singular’ and Passive. Why then should one abandon this insight in a case where it is, apparently, entirely valid? The vowel-length of rup- and the nasal consonant in rump- are perfectly good ‘markers’ in this sense; it is these features, in other words, which serve as the overt correlates of the Perfective/Imperfective opposition. Surely the
principle of morphemics will suffer less, in real terms, if one relates them
directly to the Aspectual morphemes.¹

II. The second suggestion would preserve this principle but would,
in effect, postpone the 'process' until the morphophonemics.² Let us
suppose, for example, that the symbol '˂' denotes an 'index' or
MORPHOPHONEMIC OPERATOR which may be associated with the rule:

(8) A plosive and following nasal consonant are transposed to the order
nasal plus plosive in the environment of the operator ˂.

Morphemic Representation: RUMP- + Imperfective

Morphophonemic Representation: \( \text{rup} \) + m˂

Fig. 6.4

Morphemic Representation: RUMP- + Perfective

Morphophonemic Representation: rup + L

Fig. 6.5

Would it not then be possible to establish the basic representation of
\( \text{rup} \)- in the form shown in fig. 6.4 and rely on this rule to transpose the
\( \text{p} \) and \( m \) to their 'actual' positions? Likewise, let us suppose that \( \text{L} \)
is a further operator, which may be associated with the morphophonemic
rule:

(9) Short vowels are lengthened before a consonant followed by \( \text{L} \);
we could not then establish the basic form of \( \text{rup} \)- in accordance with
fig. 6.5 and leave this further rule to lengthen \( u \) to \( \text{u} \)? The answer, of

¹ Compare the original criticisms by Nida (1948 (= RIL, 256)). The use and misuse of
zero is discussed in a very thorough article by Haas (1957) – criticised in part, how-
ever, by Saporta (1964); cf. also Allen, 1955, and in a different tradition Gedel, 1953.
² Compare the '< ˂' morphophoneme established for Monachi by Lamb (1966: 267). Also for 'L'
(see below) compare the 'A 2' about 'morpheme' established by Newell (1966: 92, 98) in the strati-
fication treatment of English; I am sure I have heard a similar analysis from Lamb himself, but I cannot find a published reference. Com-
pare also the treatment of \( e \) in English men, as a suffix... which has the property of
occurring in non-sequential order, since it always replaces the stressed vowel... of
the base' (Hill, 1958: 140). The term 'morphophonemic operator' (see also 14.3) is,
to the best of my knowledge, my own invention.

course, is that one could; moreover it is clear that \( \text{L} \) ('realised' or
'interpreted' as vowel-lengthening) and \( \text{m}˂ \) ('interpreted', in some
sense, as the infixation of \( m \)) might be referred to, rather loosely, as the
'allomorphs' of Perfective and Imperfective. But the difficulty, again, is
that there seems to be no independent motive for adopting such a
solution. The operators ˂ and \( L \) are quite unlike any genuine phono-
logical element; they are nothing more, in effect, than a notational device
which will 'trigger off' the rules which we have stated.¹ As such they are
descriptively quite superfluous. There seems no point in saying that
Perfective has an 'allomorph' \( \text{L} \) (where \( \text{L} \) is a 'trigger' for vowel-
lengthening), or that Imperfective has an 'allomorph' \( \text{m}˂ \) (where ˂ is a
'trigger' for infixation), if the operations could be 'triggered off'
directly by the morphological elements themselves.

In these circumstances, one may perhaps wonder why a process
solution was ever called into question. It can hardly be rejected on the
mere grounds that it offends against the Item and Arrangement model:
e.g. that a word will no longer have an analysis, in isolation, along the
lines of our earlier diagrams.² Nor is it difficult, if we do decide to adapt
the theory, for us to conceive of an alternative model (for which the term
'I' and Process' or IP is sometimes employed)³ in which the type of
descriptive statement with which we began this section would have a
perfectly natural place. Unfortunately, the argument has hinged for many
linguists on the purely \( a \text{ priori} \) objection to 'processes' as such. The
reader will doubtless recall the argument in Part 1 against the Priscianic
model, and our comments, later, on the degree of importance which
different scholars might attach to this point;⁴ is it appropriate, then, even
in this rather different context, for one to introduce the concept of
deriving one sequence of phonemes from another?

One must stress that the context is indeed rather different. In the first
place, we would no longer be concerned with the derivation of one word-
form from another word-form, or even a part of one word-form from a
part of another word-form, but with the derivation of a STEM or gram-

¹ The metaphor of a 'trigger' is borrowed from informal usage in transformational
syntax – where, for instance, an Interrogative 'dummy element' ("Q") in Katz &
Postal, 1964) is said to 'trigger off' the rule deriving Interrogative sentences.
² Compare, nevertheless, the opening argument of Bloch (1964 (= RIL, 243)).
³ The term 'Item and Process' is contrasted with 'Item and Arrangement' by Hockett
(1954, 1952). But what precisely is meant by it varies from one linguist to another,
and we shall in fact apply it later to something which is rather nearer to IA in
character (pp. 127 ff.)
⁴ Ch. 3, pp. 39, 36.
6.2 Fused and cumulative exponents

The problem discussed in the preceding section is not, of course, confined to inflecting languages. English, which provides the most notorious examples, is not markedly or predominantly of that type. But the second of our four groups of problems is often taken, as we remarked a propos of the *t in fortuit*, as the very basis for the typological distinction involved. In view of the importance of the typological background in this study, this is accordingly one of the phenomena which must be considered with particular attention.

We may begin with this same illustration. The essence of the problem, as we said, is that the *t in a form such as fortuit* 'he is being carried' would normally be said to correspond not to one, but to two distinct grammatical categories: '3rd [Person]' as opposed to 1st and 2nd, and 'Singular [Number]' as opposed to Plural. We are faced, in other words, with the synthesis of what are apparently two separate 'morphemes' into what is evidently a single 'morphemic segment'. Now on purely syntactic grounds, there is no doubt whatever that the two elements 3rd and Singular should be distinguished. 3rd Person may then be said to be selected, regardless of Number, by a whole range of possible Subjects, and Number agreement may, conversely, be stated independently of Person. Equally *t* is a single phoneme, and cannot even be 'split' into phonological features which recur in association with each of the elements. We may therefore assume, from the outset, that the validity of the phenomenon itself is not in question. How far, then, can a discrepancy of this sort be accommodated in the Item and Arrangement model?

It is, of course, very easy to devise a preliminary or essentially terminological solution. According to the most usual formulation, the *t would simply be included under the heading of a PORTMANTEAU MORPH or PORTMANTEAU REALISATION; qua 'morph' the segment remains (as we

1 This point was emphasised by J. Lyons, in particular, in comments on Part I above.
2 Compare again Nida's argument (1948 (= RIL, 256)).
3 The comparable Imperfective and Perfective stems for mon- 'advice'; compare monoe: 'I advise' and monoe: 'I advised'.
4 Cf. Dik, 1967: 368 — though Dik's own solution is itself not wholly of the IA type.
5 See above, pp. 56f.
6 Ch. 3, pp. 32ff.
7 The rather odd term 'portmanteau' is from Hockett, 1947 (= RIL, 236), 1954 (= RIL, 380); for a more general concept of 'portmanteau realisation' see Lamb, 1964b: 64–5, 1966a: 17. It is, however, fair to stress: I, that Hockett's original examples (1947) were all of the 'sporadic' (see 6.2.1) or else of the 'fusional' sub-type (6.2.1); it is not until the second article that the term was implicitly extended to cases with no 'non-portmanteau' manifestation; cf. Householder's remark (1959: 235) that 'some might extend the notion... to cover some such instances'. II, that Lamb's usage is tied throughout to his own particular scheme of levels (see ch. 10, pp. 200ff.); hence the cases of 'fusion' (6.2.1) might be handled more as we ourselves shall suggest.
have said) indivisible, but *qua* ‘allomorph’ it must (as we have implied) be related to an entire sequence of morphemes (3rd plus Singular) as opposed to the individual morphemes (3rd or Singular) as such. But a formulation which is as general as this will unfortunately do little to clarify the true nature of the phenomena. Consider, for example, the final *r* segment in a form such as *fer-ar* ‘I will be carried’. This is a portmanteau whose analysis might presumably be of the form shown in Fig. 6.6—a portmanteau, that is to say, which involves no less than three successive morphemes; but is that all that a description need say on the subject? In the first place, the Passive morpheme is one whose exponents

![Morphemic Representation: 1st + Singular + Passive](image)

![Phonemic Representation:](image)

Fig. 6.6

can usually be detached from the remainder of the word-form; compare the separate suffixes in *fer-r-i*, *fer-t-ur*, and the like. Neither 1st nor Singular, on the other hand, will ever appear in a ‘non-portmanteau’ realisation; compare 1st Person forms such as *fer-o*: ‘I carry’ and *fer-i-mus* ‘we carry’, or Singular forms such as *fer-o*, *fer-s* ‘you (sg) carry’, and *fer-t* [he, etc.] carries’. In other words, the portmanteau treatment of Passive is an occasional and at first sight an irregular phenomenon, whereas 1st and Singular are regularly or systematically inseparable. It seems unrevealing, in these circumstances, to impose precisely the same sequential relationship—1st plus singular plus Passive—on all three grammatical elements.

In the second place, the inclusion of Passive in the portmanteau can be shown to make sense on a purely phonological or ‘morphophonemic’ basis. In the partial paradigm which we have set out in Fig. 6.7 the Present Passive form *fer-o-r* can convincingly be presented as a modification of an ‘expected’ form *fer-o-r*;1 surely, since final *m* + *r* is impossible in Latin, it is equally plausible to regard the Future and ‘Imperfect’ Passives as modifications (only in a more drastic fashion) of the ‘expected’ *fer-a-m-r* and *fer-e-ma-r*. The *r* of actual *ferar* or *ferarb* could thus be said to represent the fusion of a basic *m* suffix, which appears unmodified in *feram* and *feram*, and a basic *r* which appears unmodified in *ferar*. But for the *m* suffix itself it would appear that no similar explanation is feasible; in *feramb* and *feram* alike we are simply faced with the cumulative exponentence (to use one traditional term) of two

<table>
<thead>
<tr>
<th>Present</th>
<th>Passive</th>
</tr>
</thead>
<tbody>
<tr>
<td><em>fer-o:</em></td>
<td><em>fer-o-r</em></td>
</tr>
<tr>
<td>‘I carry’</td>
<td>‘I am carried’</td>
</tr>
<tr>
<td><em>fer-a-m</em></td>
<td><em>fera-r</em></td>
</tr>
<tr>
<td>‘I will carry’</td>
<td>‘I will be carried’</td>
</tr>
<tr>
<td><em>fer-e-ma-r</em></td>
<td><em>fer-e-ba-r</em></td>
</tr>
<tr>
<td>‘I was carrying’</td>
<td>‘I was being carried’</td>
</tr>
</tbody>
</table>

Fig. 6.7

infectionally inseparable categories.2 It is obvious that this difference would be effectively obscured, both by the type of analysis shown in Fig. 6.6 and by the concept of a ‘portmanteau’ which underlies it. Surely it would be better to look for a more illuminating and less general formulation.3

6.2.1 Fusion. Let us begin by attempting to dispose of the fusional aspect. It is clear, first of all, that the type of analysis shown in Fig. 6.8 would still depart considerably from our original concept of morphophonemics. We were concerned, in 6.2, with alternations between the allomorphs of separate morphemes—e.g., the alternation between *reg* and *rek* or between *nub* and *nup*; here, on the other hand, we are

1 I.e., a Latin long vowel is regularly shortened before all final consonants except *r*: the effects of this change will be evident in many of the examples cited in this and later chapters. For arguments in terms of ‘plausibility’ ‘expected forms’, ‘explanations’, etc. (which the reader is, I hope, prepared to take on trust at present) see 10.2, passim, and ch. 14. Their nature is, of course, somewhat problematic.

2 Compare ‘cumulation’ in Roesen, 1964: 256. The contrary term is here used in a somewhat restricted sense, as compared, for example, with the chapter in Pike, 1967 (S2f). Earlier Pike, 1949: 126ff.) or even briefly, e.g., in Chao, 1968: 80. The parallel with these wider uses will, however, be brought in below and later in this part.

3 Similar queries might be raised against Martinet’s use of the term ‘amalgam’ (1966: 102, 113ff., 129, 1962: 46–8) to include cases of both varieties.
6 Characteristic problems of inflecting languages

Concerned with a 'link' between allomorphs which are syntagmatically related. But 'linking' of this kind is by no means out of tune with the general tendencies of Latin word-structure.

Consider once again, for example, the rule which we have established for rektis, nutpis, and the like. 1 This will admittedly effect a modification in only one segment, but the modification which is involved is clearly a matter of 'attraction' or PARTIAL ASSIMILATION to the segment which follows. The voicelessness of s is carried over, one might say, to the basically non-voiceless consonants of *reg or *sueh. Likewise, it is easy to find examples of COMPLETE ASSIMILATION; a form such as pres-s-s-i: 'I pressed' or jus-s-s-i: 'I ordered' would presumably be adapted, by precisely such a rule, from basic *prems-s (compare the Imperfective prem-o: 'I press') and *jubs-s (compare jubs-e-o: 'I order'). In such a context, it does not seem unnatural that assimilation or attraction should sometimes extend to the type of fusion or conflation which is in question. 2 From the descriptive viewpoint, there is no reason why such a phenomenon should be considered at all anomalous.

What we have, then, is an analysis (fig. 6.8) which is not anomalous in fact, but which we are nevertheless forced to regard as anomalous so long as we continue within the Item and Arrangement model. Whereas in forms such as rektis: and pressi: we are still able to draw a line between the exponents of the verbal morphemes (REK- and PREM-) and the exponent of Perfective, here our analysis obliges us to say that this principle of discreteness must be abandoned. Furthermore, it is not even clear that the heading under which we have sought to accommodate the 'anomaly' - that of the portmanteau morph - can be considered appropriate. Let us turn, for example, to the very similar problem which is posed by a form such as hais-s-i: 'I stuck' or hau-s-i: 'I drained'. The analysis of these Perfectives might, very reasonably, be of the type shown in fig. 6.9 in which the 'actual' allomorph of the first morpheme 'includes' the 'actual' allomorph of the second; 3 for the basic allomorphs involved compare the Participial haus-t-a 'It is, etc.] drained' on the one hand, and the s of rektis, nutpis, etc. on the other. In this example, the fusion of haus with s would leave a residue, hau, which would supply some independent 'signal' for the Haur- morpheme. But PARTIAL FUSION of this kind is no different, in principle, from the TOTAL FUSION which we began to speak of in terms of 'portmanteau'. If either phenomenon is recognised they ought both, accordingly, to be handled by the theory in essentially the same terms. The question is: what sort of formulation would then be appropriate?

**Morphemtic Representation:**

```
HAUR- + Perfective
```

**Phonemic Representation:**

```
h a u
```

At this point, there seem to be three directions in which the argument might reasonably be expected to develop. First we could, of course, go back on our analyses. In the case of ferar the basic *m could simply be deleted in the environment of following r; this would leave the r itself as a straightforward exponent of Passive, and a 'zero' which would be assigned as the actual allomorph of 1st and Singular. Likewise the stem of hais-i would consist of hau (a perfectly normal alternant of HAUR-) and s (a perfectly normal alternant of Perfective); it is not strictly necessary to talk of combining or fusing the first *s with the second. But is this the most illuminating way of accounting for these alternations? A crucial

---

1 Rule (6) on p. 54.

2 For the connection between assimilation and fusion - or between different types of 'fusion' in the wider sense - see Pike, 1967: 549.

3 For analyses of this kind compare Wells, 1949: 114, n. 20; Lounsbury, 1953 (=RIL, 380); Pike, 1967: loc. cit. As Hockett points out (1954: § 2.9 (4) (=RIL, 395)), there is only a trivial difference between 'inclusion' (as in fig. 6.9) and mere 'overlapping' (see below); but is there any greater difference between 'proper inclusion' (with a residue belonging to only one morpheme) and the cases which Hockett himself referred to 'portmanteaus'? We shall follow Lounsbury (loc. cit.) in taking all three cases together.
point for *haust* is that Latin had, it would appear, no phonological sequences of the type Diphthong + Double Consonant; we are concerned, as it were, with getting rid of an ‘unphonological’ geminate in the environment of *hau* and not with the mere removal of *s* in the environment of *hay* and another *-s*. Again, it is hard to detach the treatment of *ferar* from that of two rather different aspects of Latin phonology: the assimilation of nasals to *r* on the one hand (compare the compounds *cor-rogè*: ‘I call together’ or *ir-reverens* ‘irreverent’ as against *con-tehos*: ‘I weave together’ or *in-tornus* ‘unshaven’), and the ‘unphonological’ nature of final geminates (compare *es* ‘You (sg) are’ for ‘expected’ *es-s-* on the other. The reduction of *fera-m-r* can in some sense be explained by the intersection of these two separate phenomena; will we not weaken this explanation if we insist, instead, that the *m- r* is merely deleted? Such pleadings cannot, of their nature, be entirely convincing. However, one may wonder if there is any argument at all against the fusional solution - leaving aside, that is, the argument that it happens to run contrary to the allomorph concept.

The second reaction might be to extend our existing framework of description; this may be done quite easily, (a) by allowing morphemic segments to overlap (thus *hay* overlaps *s* in fig. 6.7), and (b) by allowing a morphophonemic rule, on occasions, to take two basic alternants together. Thus the rule which we might cast, provisionally, in the form:

(10) Geminate *s* is reduced to single *s* whenever it is preceded by a diphthong,

would apply simultaneously both to the alternant of *haur*- and to the alternant of *Perfactive*. However, it is not obvious that this is the solution towards which our arguments have been tending. Amendment (a) enables us, in effect, to preserve the gist of our technical formulation; the crucial allomorph relation (A in 5.1) can still be defined as a relation between morphemes and subsequences of phonemes. But the spirit of the Item and Arrangement model cannot be preserved so neatly. So far from ‘adapting’ the model have we not, in the end, destroyed the concept of discrete signals at the phonemic level? Likewise, amendment (b) is more than a mere ‘adaptation’ in the field of morphophonemics; it is a concession to a quite different way of looking at the phenomena. Will a concession of this kind have no implications for the body of our system?

Let us consider these last questions in greater detail. From the Item and Arrangement viewpoint, there is only one way in which a problem in morphophonemics may be tackled: one begins by relating morph *a* to morpheme *m*; one then compares morph *a* with a morph *b* which is related to *m* in some other word-form; and then one devises a rule, if one can, which relates *a* and *b* in a way which may be generalised for other morphemes. It is for this reason, of course, that the theory of morphophonemics has been placed within the typology of alternations; the crucial relationship, to put it in loosely Saussurean terms, is the ‘syntagmatic’ relationship between alternative exponents. But what of the ‘paradigmatic’ aspect? In the case of *reksi* (to take our original example), we could obviously begin by asking a quite different question: not ‘How can *recbh-* (in *reksi*) be related to *reg* (in *rego*)?’ but ‘How can *REG-,* on the one hand, and *Perfactive* on the other, be combined to form a stem of this shape?’ The answer will involve a *sandhi-rule* (or rule of ‘joining’) which will, as we have implied, combine the basic *reg* and *s-* by a process of assimilation. As a rule of sandhi, neither this rule for *reksi* nor the fusion rule for *haust* (rule (10)) would cause the slightest difficulty. As a case of alternation, on the other hand, the former example...
is normal (rule (6)) but the latter is most problematic. Surely, in that case, it is the more general concept of sandhi which we should now seek to extend to all the phenomena in question. 1

This leads, of course, to our last suggestion: namely, that the notion of an actual phonemic allomorph should simply be abandoned. Is this, in fact, all that unreasonable? The objection, doubtless, is that rekti, 3 and it, haus and it, fer, a and r, etc. etc. are all clearly segmentable; why should one want to throw out the baby with the bathwater? Why, too, should a model which will suit so many languages be abandoned for such a marginal advantage? This last question will have to be considered in a wider context; we are not committed, as we have said, to the necessity of a universal theory. 2 But the first objection would surely miss the point of our argument. The question is not whether a word-form can be segmented, but whether it ought to be segmented. In rekisi: or presisi: we have a rule of assimilation which will 'smooth the edges', as it were, between the juxtaposed formatives; in huisi: or ferar we have a further rule which will 'weld' the edges together: why, by segmenting in one case, and then failing to segment in the other, should one be forced to regard the fusion or 'welding' as some kind of essentially maverick phenomenon?

6.2.2 Cumulation. Let us now return to the 'cumulative' aspect. The portmanteau in fer-a-r 'I will carry' seems, as we have said, to have no phonological explanation; 3 likewise the t in our first example fer-t-ur 'he, etc. is carried'. Cumulation, by definition, is a problem as peculiar to 'morpholexicis' as the problem of fusion is peculiar to 'morphophonemics'. But how exactly should it be handled? Will the alternatives be similar, mutatis mutandis, to the alternatives which we have already sketched for the 'lower' level? Or will a different set of issues now arise?

At first sight, it may seem that the solutions would be very similar.

---

1 The two approaches to morphophonemics discussed in this paragraph correspond, I think, to the 'method of morpheme alternants' and the 'method of internal reconstruction' distinguished by Lounsbury (1953: = RLE, 379-81); but see also the later retrospective discussion of Item and Arrangement morphology by Hockett (1961: 32), which takes an apparently different view of Lounsbury's distinction. On the wider point of elements in contact compare, in particular, Pike's general concept of a 'dynamic' or wave-based (IP), as opposed to a 'static' or particle-based view of language (IA). See Pike, 1959, as we have pointed out, however, Pike, 1967: 545-7 is particularly relevant here. Koutsoudas's IA versus IP (1965 -- see above, p. 96, n. 1) is also relevant.

2 See ch. 3, pp. 31ff.

3 See above, p. 67.

---

6.2 Fused and cumulative exponents

The r in fer-a-r could, for example, be assigned the complete analysis shown in fig. 6.10, in which the 'morphophonemic fusion' of *m and *r is preceded by a corresponding 'morpholexical fusion' of 1st and Singular. Likewise, the t of furtar would involve a straightforward morpholexical fusion of Singular and 3rd; one need merely write a rule, say:

(11) The sequence 3rd + Singular is reduced to the unitary alternant t, 1 in which elements of the morphemic representation, on the one hand, and of the basic representation on the other, play a role which is similar to that of the *r and *s, respectively, in the case of huisi: The difference, in other words, would be a simple matter of fusion at one level versus fusion at the other. 2

Morphemic Representation: 1st + Singular + Passive

Morphophonemic Representation:

Phonemic Representation:

Fig. 6.10

Unfortunately, a parallel of this kind will not stand up to closer examination. In the first place, there can be nothing in morpholexicis to compare with the role of partial fusion. At the morphophonemic level, the representation has (as we have implied) a double structure: it is composed of basic alternants (e.g. huisi) which are in turn composed of phonemes or the like (e.g. s). It is therefore possible, as in the case of huisi, for one alternant to be merely linked to another. But the mor-
Morphemic representation is composed of members which (like the phonemes) are themselves unstructured. It follows that cumulation must, if handled in this way, fall entirely under the 'portmanteau' heading.

Secondly, there is the query which we raised at the outset of our discussion, concerning the relationship between the grammatical elements. In *ferar* it is natural to say that 1st and Singular precede the Passive morpheme: the position of Passive would then correspond to its position in *fer-o-r, fer-t-or, and so forth, and, in addition, the sandhi-rule may be accounted for more naturally in terms of \( *r + *m \) than

\[
\begin{align*}
\text{Morphemic Representation:} & \quad [1st] + \text{Passive} \\
\text{Morphophonemic Representation:} & \quad \downarrow *r \quad \downarrow *m \\
\end{align*}
\]

\( *r + *m \). Likewise it is obvious that the Perfective, in *hausti*, should follow the root-morpheme *haur*. But no argument of this kind can apply in the cases now under discussion, 1st and Singular never having separate exponents, as we have said; neither do 3rd and Singular (compare *fer-t* and *fer-t-or*), nor any other combination of Person and Number elements. In addition, all the portmanteaus seem (like \( m \) and \( t \)) to lack any morphophonemic explanation. Why, then, should we say that 1st and 3rd precede Singular? Or that Singular precedes 1st and 3rd, for that matter? Is there indeed any point in ordering these 'morphemes' at all?

It is, of course, this last question which is of the greatest interest. One objection to the Priscianic model (as we suggested in Part 1) was that the choice of leading form was inherently arbitrary: the theory creates a problem which it is then unable, or only partly able, to resolve. But the ordering of portmanteaus appears to be a further illustration of the same principle. There are no morphological criteria which would justify an analysis beyond the point shown in fig. 6.11, where the square brackets

\[ \text{enclose a set of unordered or 'simultaneous' elements. There therefore} \]
\[ \text{seems little justification for a model which will compel one to dissect the complex:} \]
\[ \begin{align*}
\text{[1st Singular]} \\
\end{align*} \]

\[ \text{into either 1st Singular or Singular + 1st.} \]

The only reply is that one might, perhaps, be able to find some kind of syntactic criterion. But is that at all likely? The best argument might be to take the 3rd Person as the syntactically basic (corresponding, perhaps, to the least 'marked') term in the Person class: one might start, in other words, by generating all verb-forms with initial representations of the type:

\[ \begin{align*}
\text{FER} \cdot + 3rd \\
\end{align*} \]

and derive their final representations by concord-rules of the type:

\[ (12) \text{ Alter 3rd to 1st wherever the Subject is itself a 1st Person Pronoun,} \]
\[ \text{or:} \]
\[ (13) \text{ Add Singular or Plural wherever there is a Subject which is itself Singular or Plural.} \]

If so, then it might conceivably be simpler for rule (13) to add the Number element to the end of such a representation instead of to insert it in the middle. But this would be an extremely weak line of reasoning, and would, in any case, be open to various prima facie objections. In addition, any argument of this kind would depend very largely on the characteristics of some particular syntactic theory; it is scarcely possible to draw any conclusions in the present, extremely confused, state of syntactic researches.

The point should, perhaps, be left in suspense. But if there are no decisive syntactic criteria, then it would surely be a defect of 'Item and

---

1 Above, p. 66.

2 The forms will, of course, be discussed in the eventual descriptive phase of our investigation. In the present context it is, however, worth observing that even if one were to segment these suffixes of *outrance*, on the basis of partial recurrences, one would still not get a constant order: thus \( -m-t \) would presumably have 'Plural' \( *t \) after 1st Person \( m \), whereas \( -n-t \) would have 'Plural' \( *n \) before 3rd Person \( t \).
Arrangement' that it forces one to 'arrange' items in cases where 'simultaneity' would be more appropriate.¹

6.2.3 Sporadic or irregular examples. We may end by drawing attention to one last aspect of portmanteau. At the beginning of this discussion we tried to separate the cases by two criteria.² First, were there or were there not distinct exponents in some other word-form? (In the fusional case, Yes; in the cumulative, No). Secondly, was there or was there not the possibility of a phonological explanation? (Again, Yes for fusion and No for cumulative). But it is clearly possible for the answer to be Yes to the first question and No to the second. Consider, in particular, the stem of tul-i: 'I carried'. There will again be two morphemes: the verbal element fer- (which has separate exponents, as we have seen, in fer-r-i, fer-t-ur, and so forth), and the Perfective element (which has separate exponents in rek-s-i, nu:p-s-i, and other forms discussed earlier). The ordering of the morphemic representation may, accordingly, be supplied from the 'regular' cases. But, at the same time, the sequence tul bears no resemblance either to fer, la- (as in la:t-a '[she is, etc.] carried') on the one hand, or to s, u (as in mon-u-i: 'I advised'), and so forth on the other. It is a pure irregularity and must, in our present terms, be handled entirely at the morpho-lexical level. Here, then, we have an example which goes with fusion by one criterion but with cumulation by the other: does it then require any special comment?

In one sense, it may seem that it does not. The analysis may be represented, quite satisfactorily, in the form shown in fig. 6.12 and a rule of the type exemplified by (11) in 6.2.2:³

(14) The sequence fer- + Perfective is reduced to the single basic alternant tul.

¹ May I remind the reader that the question of ordering elements in syntax will be discussed, in a more comprehensive way, in 7.4.3, below.
² Pp. 56–7, above.
³ P. 73.

will, in a sense, arouse little criticism. But it is the very simplicity and straightforwardness of this treatment which ought, in reality, to make us suspicious. Latin is, par excellence, a 'fusional' and a 'cumulative' language: 'linking' abounds and may (for all we know) have been more extensive than the writing will indicate; cumulation, as is well known, is involved in both the Nominal and the Verbal inflections.¹ But cumulation and fusion both give rise to searching doubts concerning the Item and Arrangement model. The case of tul-, by contrast, is of almost no typological significance; irregularities of this kind play at most a sporadic role within the structure of the language.² Yet the treatment of tul- fits morphemics much more neatly; it is the only unproblematic 'portmanteau morph' amongst the examples which we have considered. Does this not suggest that we are approaching the language with the wrong set of assumptions?

6.3 'Empty morphs' and intrusive elements

Let us leave this question in the air, and move on to the next of the four major groups of problems which we distinguished at the outset of this survey.¹ The essence of the portmanteau, as we said, lay in a discrepancy between too few morphs on the one hand, and too many morphemes on the other. But it is obviously possible to envisage the reverse discrepancy, in which there are too few morphemes and too many morphs. Again, how far would this sort of case imply a serious criticism of the Item and Arrangement model?

For a convenient initial example, consider a 2nd Plural Perfect such as rek:stis `You (pl) ruled'. On the one hand, this form has a final segment -tis, which is associated throughout the paradigm with what we may continue to call the 2nd Plural morpheme: thus fer-tis `you (pl) carry', and so on. It also displays the Perfective suffix -s, which we have already seen in the corresponding 1st Singular rek-s-i. But between these segments there is a residue, is, which appears to belong properly to neither. It is obviously independent of -tis, since the two are not juxta-

¹ Readers who are unfamiliar with Latin may refer, for the Nominal examples, to the descriptive treatments by Hall (1946), Householder (1947: 48–9) and Hill (1958: 449ff.). But the point is sufficiently notorious.
² The only relatively uncontroversial examples, elsewhere in the verbal system, would be the 2nd Person terminations in the Passive: -mini for the plural, -risi-re for the Singular. But even there, the latter could be analysed into 2nd Singular *s (rhota-
cised) plus a separate Passive suffix.
³ III on p. 57.
posed at any other point in the paradigm. Equally it is independent of s, since it also appears in forms such as mon-u-is-tis ‘you (pl) advised’, which have an u suffix for the Perfective (compare mon-u-i: ‘I advised’) instead. We have, in other words, three successive divisible segments, but only two successive morphemes, ‘Perfective’ and ‘2nd Plural’, to go with them.

Now in such a case it is again very easy (as at the beginning of the previous group of problems) for us to devise an essentially terminological solution. According to the usual formulation, we might simply establish the sort of analysis shown in fig. 6.13, in which the is is said to be an EMPTY MORPH which is present in the phonological word-form

\[
\text{Morphemic Representation: } \text{REG- + Perfective + [2nd Plural]} \\
\text{Phonemic Representation: } \text{re:ki + is + tis}
\]

Fig. 6.13

but which, unlike all normal morphs, fails to enter into the allomorph-relation. But the concept of the empty morph, like that of the portmanteau morph, can be used to camouflage an important difference of phenomena. Let us contrast the is in rekstitis with, for example, the i in fer-i-mus ‘we carry’ or in fer-i-mini: ‘you (pl) are being carried’. In these latter examples the i, again, does not belong to the -mus and -mini: suffixes; compare forms such as da-mus ‘we give’, mone-mini: ‘You (pl) are being advised’, and others with a range of different vowels before-

1 See above, p. 65.
2 The term ‘empty morph’ is from the same source as ‘portmanteau morph’, namely, Hockett, 1947 (= RIL, 216); Hockett’s examples (RIL, 195, 273–8) seem to be of both relevant kinds (see below). The term ‘empty realisation’ is also introduced, in parallel with ‘portmanteau realisation’, by Lamb (1964b, 1966a); but is it appropriate to talk of ‘realisation’ when the whole point is that there is nothing being ‘realised’?
3 The case is, in the formal sense, the reverse of the case of ‘zero morph’ (p. 61, n. 3). For the former we would have amended requirement (3), in the layout of p. 59, n. 1, to read ‘...a subset of the morphemes in G’; so now we might amend it to read ‘...a subset of the morphemic segments in P...’. An alternative, naturally, would be to say that it is not a ‘morph’ at all; in that case, the first requirement ((1) in p. 59, n. 1, and p. 70, n. 2) would be amended to read:

(1) P contains at least one morphemic segment.

The choice does not seem particularly important; Hocket himself (as Joos points out in RIL, 242) was not entirely consistent.

hand. Equally, it is not part of the fer- segment which precedes: in other forms belonging to the same Tense (fer-s ‘you (sg) carry’, fer-tis ‘you (pl) carry’, fer-o-r ‘I am carrying’, etc.), as elsewhere in the paradigm (e.g. fer-a-m ‘I will carry’), it is absent. We could therefore say that this too is an empty morph, and diagram the analyses (e.g. fig. 6.14) in a way completely parallel to fig. 6.13. Only fer and mus, that is to say, play any role in the actual allomorph relationships.

But such a parallel (like the parallel implied by fig. 6.6) will at the very least ignore the distinction between morphologies and morphophenomena. On the one hand, there seems to be no accounting, in phonological terms, for the appearance of is in the Perfective. It appears, as we

\[
\text{Morphemic Representation: } \text{FER- + [1st Plural]} \\
\text{Phonemic Representation: } \text{fer + i + mus}
\]

Fig. 6.14

have seen, before the -tis of rekstitis, which is a suffix of a CVC structure (Consonant + Vowel + Consonant) and which begins with a t. However, it does not appear before the CVC suffix of reksti-mus ‘we ruled’ or before the i of rekst-i ‘[he, etc.] ruled’. Its distribution must simply be stated by reference to the precise morphemes which happen to be involved.

By contrast, the i of ferimus is a quite different matter. First of all, the forms in which it appears (ferimus, ferimini, etc.) can be defined, phonologically, as those in which the r of fer- precedes, and a suffix beginning with an m immediately follows; we would, in other words, ‘expect’ fer-mus or fer-mini. This is indeed an important additional reason for separating the i from both elements in question. Secondly, we find a similar situation, involving a u-vowel instead, in forms such as sum ‘I am’, sumus ‘we are’, or sulmus ‘we want’. Again, one would ‘expect’ a form such as sul-mus or sul-mus (compare sul-t ‘[he, etc.] wants’ and sul-tis ‘you (pl) want’), but again the Consonants are separated by a segment which really goes with neither. Just as it was more illuminating, for the voiceless k in reks-, to refer to the voicelessness of the following s than to the presence of the morpheme Perfective, so here it is surely more illuminating to refer to this pattern of Consonant plus m, than to the individual morphemes FER-, 1st plural, and so on.

52, p. 52.
We must therefore consider the implications of each case separately. In a form such as ferimus the phenomenon is, of course, essentially the reverse of the sandhi-phenomena which were discussed in 6.2.1.1. Whereas one type of morphophonemic process (illustrated by rule (15), for example) has the effect of welding basic segments or formatives together, so the type of rule which we would write in a case such as this:

(15) If one morph ends in r, and the next begins with m, an empty i must intervene,

\[
\text{Morphemic Representation: } \quad \text{FER-} + \left[ \text{1st Plural} \right] + \text{Passive}
\]

\[
\downarrow
\]

\[
\text{Morphophonemic Representation: } \quad \text{fer} + \text{mus} + r
\]

\[
\downarrow
\]

\[
\text{Phonemic Representation: } \quad \text{fer} + i + m
\]

\[
\downarrow
\]

\[
\text{Fig. 6.15}
\]

would instead introduce a form of 'buffer' or EPENTHETIC segment which keeps them apart.\(^2\) But in this sense both types of rule can be seen to give rise to the same theoretical query. If we turn, for example, to the Passive ferimur 'we are carried', it would presumably be possible to adopt the treatment shown in fig. 6.15, in which there is both ephenesis (by rule (15)) and fusion (by extension of the rules for ferar);\(^3\) but is the allomorph-relation of any more value in the first case than it is in the second? The crucial point is not that i is an 'actual alternant' of nothing, but that the form ferimur as a whole may be derived from a hypothetical sequence (or what has been called a 'fictitious agglutinating analogue');\(^4\)

\[*\text{fer} + *\text{mus} + *r\]

which is a straightforward analogue of (15) – the rule for ferimus – at the morphophonemic instead of the morphophonemic level. In effect, the analysis in terms of the allomorph-relation proceeds, as nearly as possible, as though the segment did not exist. But the soundness of this solution can be questioned, particularly when we bear in mind the 'signalling' hypothesis which we associated with Item and Arrangement morphology earlier.\(^5\) The is segment is always associated with the Perfective, in the sense that it only appears in the Perfective part of the paradigm. In addition, it has the same phonological shape whatever the verb in question: thus reks-is-is-tis, non-u-is-tis, tul-is-tis 'you (pl) carried', kekin-is-tis 'you (pl) sang', and so on. Is it not plausible to regard it too, in some way, as a potential 'signal' for the presence of the Perfective morpheme?

Now with a dead language plausibility is, perhaps, the most that one can speak of. But the point is that, according to rule (16), the is must be taken to play no role as a 'signal' whatsoever. If we go back to rule (2) for example – the rule for Passive /i/ur in 5.2\(^2\) – we find that it makes a clear distinction between the morpheme to which the allomorphs are assigned as exponents (viz. the morpheme 'Passive' of which it is predicated) and those which are merely referred to as environmental conditioning factors, Infinitive or 3rd Singular. If the hypothesis of

\(^1\) Pp. 45ff.
\(^2\) P. 52.
\(^3\) Apart from 'ephenesis', the terms 'anaptyxis' (of a sound-change e.g. in Bloomfield, 1935: 394) and the Sanskrit 'svarabhatki' (again of a vowel-insertion) are also in currency for synchronic or diachronic processes of this kind. For example, all three are defined by Marouzeau (1921).
\(^4\) See discussion on p. 70. The first rule can easily be made to assimilate r as well as m.
\(^5\) The phrase is from Lounsbury (1953 (=RL, 380)); Lounsbury's final formulation of the 'method of morpheme alternants' (15b in 3.11.1 (=RL, 380)) brings in empty morphs as well as portmanteaus, etc. For a more precise formulation of what I take to be this 'method' see now the subsection on 'Item and Process', 7.4-2, below.
discrete signals means anything at all, then according to this the \( i \) in \textit{ferrit}; for instance, is the signal for Passive but not a signal for the conditioning morpheme Infinitive, which is signalled separately by \( r \).

But now let us return to \( 16 \). On the one hand, in so far as it can be fitted into our rule-system, it must be said to be predicated of ‘nothing’. At the same time, the morphemes actually referred to (Perfective or 2nd Plural) are again no more than conditioning factors; their exponents are specified elsewhere, by rules which give \( s, u, \) etc. as allomorphs for one, and \( tis \) as the single allomorph of the other. If we are to be consistent, therefore, fig. 6.13 must be interpreted to mean that the ‘signal’ for Perfective is \( s \) alone, and that the \( tis \), in decoding the word-form, is ignored. But such a proposal, once made explicit, is implausible in the extreme.

The problem, unfortunately, is that there is no more satisfactory solution which will preserve the principle of one segment per morpheme. Obviously, one temptation is to go back on our segmentation, and say that \( sis \) and not \( s \) alone is the Perfective allomorph.1 But in that case the variation between \( s \) in \textit{rekt-s-i} and \( sis \) in \textit{rekt-s-tis} would completely parallel the variations between \( u \) in \textit{mon-u-i} and \( uis \) in \textit{mon-u-tis}, and so on; likewise \( s \) versus \( u \) completely parallels \( sis \) versus \( uis \). If these are separate and indivisible allomorphs then, failing some desperate solution, the conditioning factors would have to be duplicated at every turn.2

The only real alternative, accordingly, would be to weaken the allomorph-relation in a quite new direction, by allowing for what we might call extended or multiple EXPONENCE, in which a single grammatical element is associated with two or more distinct bits of the word-form. Thus in \textit{rektkis} both \( s \) and \( i \) would be exponents of Perfective; alternatively, \( s \) might be defined in some way as the ‘main exponent’ and \( i \) as a ‘subsidiary exponent’.3 Now this is indeed a major departure, particularly when added to the modifications which our original allomorph-relation has already undergone. But which is preferable, this solution or that of fig. 6.13?

1 Such an analysis has been proposed by Hill (1958: 467f.) in his analysis of the single verb \textit{amo}: thus, e.g., \textit{amai-u-s-e-i} (468).

2 A desperate solution, for example, would be to introduce at least one morphophonemic operator \( S \), let us say) such that basic allomorphs such as \( *s \) or \( *u \) come out not as \( si \) or \( uis \) before a 2nd Plural \( Stis \), but as \( s \) or \( u \) before 1st Singular \( St \); for morphophonemic operators in this sense see p. 62, n. 2. I leave the reader to explore the details of this, if he cares; I can think of no substantively different treatment which would not, in effect, divide \( is \), etc. into two separate segments.

3 ‘Main’ versus ‘subsidiary’ in the sense, for example, that by allowing overlapping exponents (p. 93, below) \( i \) might be a subsidiary exponent of 2nd Person as well.
It is in this sense, of course, that the problem is similar to those discussed earlier in this subsection: there are again reasons for distinguishing more 'morphemic segments' than the number of 'morphemes' which define the place of these stems in the paradigm. The difference, however, is that the t, s, etc. are so plainly related to the identical segments which appear in the Past Participles; indeed, if we are to avoid duplication, the forms in the Future Participle and in the Past Participle should somehow be handled together. For this reason the straightforward declaration of an 'empty morph' (as earlier in fig. 6.13) would not, in itself, be sufficient. Nevertheless one can imagine that a morphemicist might consider all the examples discussed in this subsection (fer-i-mus, rek-s-is-tis, and now rek-t-ur-a, etc.) to be significantly parallel. If we take ferimus first of all, what we had to do was to generate a new segment at the lowest of the three levels of representation which we have distinguished in our diagrams; this may be done by means of a rule for epenthesis in the morphophonemics (rule (15)). Likewise, if we turn to the case of rekxisis, what we must do (according to the 'empty morph' solution) is to generate a new segment at the next level: not, this time in the phonemic representation, but in the morphophonemic. This we did by introducing the rule which we have written as (16), which can be regarded as a rule for 'epenthesis' in the morphemics. But surely, he would argue, a similar epenthesis can be envisaged in the morphemic representation also. Why not simply write in the element 'Past Participle', and let its allomorphs follow?2

Now whether we accept this parallelism or not, one must obviously admit that this last proposal would take care of the generalisation. What it means, in effect, is that for a stem such as rekktus- we would establish a distinction between two sorts of 'morphemic representation': a representation for syntactic purposes:

FER-< Future Participle

whose elements are both syntactically and semantically incompatible, and a representation for morphological purposes:

FER-< Past Participle + Future Participle

in which an INTRUSIVE (and strictly incompatible) element Past Participle is inserted between the root and the 'true' Participial morpheme. The precise nature of the rule need not immediately concern us; but assuming that there was such a rule then, as we said, the allomorphs of the intrusive element (t in rek-t-ur-, s in larp-t-ur-, etc.) and of the Future Participle element (ur) would be handled in a perfectly normal way.1

But although this solution works, there are perhaps two observations which are worth making at this stage. The first point is that it saves the general model, in a sense, only at the cost of introducing yet another level of descriptive statement. On the one hand, the rule which we have postulated is not properly syntactic in character; every conceivable statement about word-combinations, within the sentence or the syntagm, must be made on the basis that is simply a Future Participle. On the other hand it is not properly morphological either, in that its purpose is to perform an operation on morpheme-sequences (adding one morpheme in front of another) in what is formally a characteristically syntactic way. Instead it belongs to some sort of pseudo-level, whose sole task is to iron out discrepancies between syntax and Item and Arrangement morphology. Surely this can at best be accepted as a pis aller. If one said that there must be a precise transition between syntax and morphology, then the phenomena which we are talking about would come down firmly on the morphological side. It is desirable therefore that they should be handled by formally morphological rather than syntactic means, if possible. So far the only justification for a rule which writes in 'Past Participle' is precisely that, by adopting such a rule, the concept of paired morphemes and allomorphs can remain apparently unscathed.

Secondly, one cannot but observe the neatness with which rekktura, lapsera, etc. may be handled in non-morphemic terms. A rule of the traditional type can, for example, be formulated very easily within the Priscianic system: rekktura would be derived from rekktu by the insertion of ur, similarly lapsera from lapsa, and so forth.2 Likewise, it is possible for a version of our original wording:3

1 For precisely such a solution see, for example, Chafe's treatment (1970: 41f.) of the Onondaga 'Dislocative'; thus rule (T9) simply adds the semantically irrelevant 'Purposive' wherever the Dialectic element appears. For Chafe this is a rule, apparently, of syntax - though the point has to be considered within his own theory of levels. Within a transformational grammar the best place (see discussion below) might be in the 'readjustment' between the 'surface structure' and the 'phonological component'; cf. Chomsky & Halle, 1968: 10, though they are concerned with a different sort of discrepancy.

2 See ch. 2, pp. 10ff; also Priscian's actual rule in K. 11, 557, 25ff. Priscian's rule would actually derive the Nominative Singular Masculine (e.g. rekktura) by the addition of ura to the ur form of the 'Supine' (e.g. rekktu), and would moreover be limited to verbs in which the 'Supine' is not deficient; but these differences do not, of course, affect the theoretical character of his approach.

3 I.e. the wording with which we originally expounded this example - see above.
6.4 Can the morph be a signalling unit?

Let us now turn to the last of the four parts of our survey. In the preceding subsections we have raised various queries concerning the naturalness or appropriateness of our original allomorph-relation. In particular, this has been true in the morphophonemics. Thus in the form *feriur, which we analysed in fig. 6.15, the three apparent segments *fer, *t and *ur would be at most an oblique and devious reflex of the three grammatical elements (FER-, 1st Plural and Passive) established in its morphemic representation. But arguments from morphophonemics are not, on their own, sufficient to discredit the Item and Arrangement model; as we have seen, one could abandon the concept of actual allomorphs only to reformulate the relationship at a more abstract level. Thus in the case of *feriur, once again, the basic or morphophonemic representation:

\[ *fer + *mu + *r \]

would still be such that (cumulation apart) it would cause no further problems: we would merely admit that the 'signals' of the successive morphemes may be masked or partly obscured in the actual word-form which results from the processes of sandhi. The crucial question, therefore, is whether the allomorph-relations must also be rejected in the morpholectics. Can we show that even our 'fictitious agglutinating analogue' is insufficient?

Now in some cases — that of cumulation, for example — it is not the 'signalling' aspect of the model which is impugned. In a form such as *feriur we can still speak of a succession of discrete 'markers', even though the second is the signal for two elements instead of one. But we have just raised one example (the so-called 'empty morph' in *rektisitis) which comes somewhat nearer to the quick. Is it realistic to propose that the segment *s should lie outside the exponence-relation? The answer, we have suggested, turns on the extent to which our argument could be supported by other aspects of the language. Are there other features which will lead to further criticism of the morpheme-allomorph concept of exponence? Or is the residual segment in *rektisitis no more than an isolated irregularity?

6.4.1 Ambiguity at the morphemic level. One feature is so obvious that it is, in a sense, surprising to find how little attention it has attracted in morphological theory. Let us begin, by way of contrast, with an English example such as *feiʃɪn 'chafing'. According to a morphemic description of English there would, first of all, be only one way in which this form could be divided into specified morphemic segments: namely

1. Pp. 5 if.
2. In this respect, there is some distinction between the American morphology of the 1940s and 50s and the more recent discussion of the 1960s. In the former period, the main point might have been discussed, if at all, under some such heading as 'morphemic overlapping'; if the parallel between 'phonemics' and 'morphemics' had been subjected to serious examination (see 7.2, below) it should have proved awkward. But it is for the most part ignored, even in the otherwise thorough discussion by Harris (1954: ch. 13). For the latter period one may, however, instance Pike's discussion of examples such as Spanish *digol/ojo (1967: 551f) and Lamb's typology of 'neutralisation' versus 'diversification'; for references to Lamb, and further references to Pike and his associates, see p. 92, n. 2. For an earlier European view see Martinet, 1949: 204 ('les désinences que ne sont identifiables qu'en contact avec un signe donné'); the point has, of course, been familiar to many practising structuralists (thus for Russian see already Karcevski, 1927: 34f). So far as the present section is concerned, my starting-point has been the illuminating discussion by Blooms (1939: 127-34).
3. Compare, in particular, the wording with which we began our discussion of *rektisitis, etc., p. 58.
5. The metaphor of 'parasitic formations' is from Matthews, 1968: 143, but a mixed reaction to that passage has persuaded me to lay less emphasis on this point in the present discussion.
the actual segments *tseif* on the one hand, and *iyn* on the other. There is no root *tsei*, no suffix *iyn*, etc. Furthermore, given such a segmentation, there is then only one morpheme for which each morph would be specified as a signal: *tseif*, in itself, cannot but be a marker of the verb *chafa* and likewise *iyn*, in itself, cannot but be a marker of the so-called *Present Participle*. The morphemic representation:

\[ \text{CHAFE + Present Participle} \]

...may thus be said to be determined, in a complete and unambiguous fashion, by the purely morphological aspect of an Item and Arrangement grammar. Given the mere list of morphemes, and the mere specification of their allomorphs, any other analysis is immediately excluded.

But now consider, instead, a Latin word-form such as *regeam* `I will rule`, *aperis* `You (sg) open`, or our earlier examples *ferimus, feram* and *ferri*. Taking the segmentation first of all, it seems clear that only *feram* (i.e., *fer-a-m*) and *regeam* (*re-ga-m*) could, in fact, be perceived as a morpho-sequence in a similarly unambiguous way. Of the rest, *ferimus* could equally be segmented into either *fer-i-mus* (as above) or *fer-im-u-s* (cf. *infim-us* `lowest`); all of *fer, i, mus, im, u* and *s* would, that is to say, be specified as morphs at some point in Latin morphology. Likewise *aperis* could be divided (on the basis of one possible description) into any of *aper-i-s, aper-is, aperi-is or aper-i-sis*, and *ferri* could be divided into either *fer-r-i* (as above) or *ferr-i*. But this ambiguity becomes even greater when we consider the possible assignments to morphemes. The forms *regeam* and *feram* are, as we have said, segmentally determinate: but of the segments concerned only *rege* is morphemically unambiguous. The root-segment *fer* could either be an allomorph of the verb *fer* or of the homonymous adjectival morpheme *ferre*-, let us say `wild` or `untamed`. Likewise the suffix *m* could either be a marker of 1st Singular or, alternatively, of Accusative Singular (compare *pella-m* [the] *girl*), and the *a* suffix could be related to any of Future Indicative, Present Subjunctive, Nominative/Accusative Plural (as in the Neuter *bell-a* `wars`) and Feminine. The upshot, of course, is that by the allomorph rules alone there are as many as eight possible representations for *regeam*, and no less than sixteen for *feram*; does this really suggest

---

1. I.e. that implied by Hill, 1958. For simplicity, I assume that the `thematic` vowel is treated as part of a root allomorph (hence *aperi-s*); I that vowel-length may (by the convention adopted by Hall, 1946 and Householder, 1947) be treated as segmentally independent of the vowel-quality itself. Hence *aperi-s*. The rejection of these assumptions would, however, have no effect on the main argument.

2. The morphemic representation:

\[
\text{FERR-} \cdot \text{Feminine} + \text{Accusative Singular},
\]

which are syntactically or `morphotactically` feasible?

A similar query arises, as one might expect, in the case of *ferimus, aperis* and *ferri*. For *ferimus* there would, apparently, be four ways of taking our original segmentation: *mus* is unambiguous, but *i* could be either empty or a further alternant of Present Subjunctive (compare *si-i* `[he, etc.] may be` or *rek-s-i-r-i-i-t* `may have ruled`) and *fer*, again, could be either *FER-** or *FER-**. In addition, there would be at least six ways of taking the second segmentation: either *FER-** or *FER-** for the root would be paired with any of 2nd Singular (as in *fer-s*), Nominative Singular (as in *fer-us* `wild`), or Perfective (as in *rek-s-i-i*) for the final *s* suffix. For *ferri* there would be six in one case, but as many as twenty-four in the other: *fer* is the marker of a Nominal morpheme *ferr-`iron`, fer* again of *FER-** or *FER-**, *r* of either Infinitive or Passive (as in *fer-o-r*), and *i* of any of the elements Passive, 1st Singular (as in *rek-s-i*), Genitive Singular, Dative Singular (as in *regi-i* `to [the] king`), Nominative Plural, or Present Subjunctive (as in *rek-s-i-i-s*). Finally for *aperi-s* the total would (depending on the analysis) amount to at least twenty-three: *aper, aperi* and *aperi* can all mark the verb *APER-** open`, *s* only Accusative Plural (as in *pella-s* `[the] girls`) and *is* only Dative Ablative Plural (as in *puell-is*), but both *s* in *aperi-s* or *aperi-i-s* and *is* (in *aperi-s* or *aperi-i-s*) would have the same three-fold and six-fold ambiguity as before. For these forms it is thus possible, if we confine ourselves to a mere inspection of the allomorphs, to arrive at fifty-three or more distinct morphemic representations. But as any reader who is familiar with Latin
will readily appreciate, the form *ferrī* is in fact only doubly ambiguous — either:

\[
\text{FER-} + \text{Infinitive} + \text{Passive}
\]

or:

\[
\text{FER-} + \text{Genitive Singular},
\]

and neither *aperīs* nor *ferīmus* is ambiguous at all.

What implications may we draw from these and similar examples? It must be acknowledged, first of all, that they do not pose a technical problem of the type considered earlier in this section; there is no difficulty whatever in stating, in Item and Arrangement terms, that both fer- and fer- have the basic form *fer*, that Passive has the basic alternant *i* when it is preceded by Infinitive, Genitive Singular the same alternant.

\[
\begin{array}{c|c}
\text{Morphs} & \text{Morphemes} \\
\hline
\text{mon} & \text{MONT-} \\
\text{mone} & \text{MON-} \\
\text{mone:} & \text{MON-} \\
\text{t} & \text{3rd singular}
\end{array}
\]

\text{Fig. 6.16}

*i*: when it is preceded by a 2nd Declension Noun,\(^1\) and so on. However they do cast considerable doubt on the motives which underly a statement of this form. Just how far is the allomorph-relation an approximation — even a loose approximation — to the type of "signalling" function which the perceptual hypothesis would imply? For certain morphemes, admittedly, the function will hold: for example the subset of the relation which is diagrammed in fig. 6.16 involves three segments (*mon, mone* and *mone*:\(^2\)) which, qua segments, can function only as the allomorphs of *MON-* ‘advise’, and another (*t*) which can function only as the alternant, and moreover the only alternant, of 3rd Singular. But this situation, though common enough for roots, is significantly unusual for the crucial suffixal morphemes.

Fig. 6.17, for example, is a fragment of a similar diagram involving four of our ambiguous examples. As we pointed out, each of these morphs

1 The traditional declensions would, of course, be formalised in IA morphology as classes of Nominal morphemes, conditioning different allomorphs of some Case/Number elements.

2 The reader will recall our provisional treatment of the thematic vowel (p. 88, n. 1).

is related to at least two morphemes: *r* to Passive and also, by the broken arrow, to Infinitive; *a* to Present Subjunctive and also (for clarity we supply only one broken arrow) to Feminine and so forth; *i* to as many as six; *m* to both Dative Singular and Accusative Singular. In addition,

\[
\text{Morphs} \\
\hline
r & \text{Passive} \\
\hline
a & \text{Present Subjunctive} \\
\hline
n & \text{Nominative Plural} \\
\hline
i & \text{Genitive Singular} \\
\hline
\text{t} & \text{Dative Singular} \\
\hline
\text{m} & \text{Accusative Singular}
\]

\text{Fig. 6.17}

each morpheme can (with the exception of Accusative Singular) be related to at least two segments: Passive to *r* and *i*; (and also, leaving aside portmanteaus, to the *ur* of *fer-tur*); Present Subjunctive to *a* (*reg-*-*a-*), *i* (*i*-*) and also, leaving aside purely morphophonemic variants, to the *e* of *am-*-*e-* ‘I may love’; Nominative Plural to *n* and also to the *s* (let us say) of *reges* ‘[the] kings’;\(^3\) Genitive Singular to *i* and also to *is* (e.g. in *reg-*-*is-* of [the/a] king’); Dative Singular to *i*; but

\(^3\) Again, we shall simply follow Householder (1947: 49ff.); for *es* in the Nominative Plural (thus *reg-*-*es*) but *is* in the Accusative (thus *reg-*-*is*) compare *aur-*-*es*, *aur-*-*is* ‘ears’ (Householder, 50).
also, presumably, to some different form in *ferro* (‘to [the] iron’); *ferro-a* and 1st Singular to *o*; (as in *reg-a*: ‘I rule’) as well as to *m* (as in *reg-a-m*) and *i*; (as in *rek-s-i*). If the role of morphs is to serve as the ‘signals’ for the relevant semantic properties, then one can only say that these morphs, at least, are unnecessarily ill-suited to such a purpose. Furthermore it is the inflectional morphemes—at once the fewest in the inventory but the commonest in actual samples of the language—which are characteristically prone to this type of patterning.

### 6.4.2 ‘Morphemic conditioning’ of inflectional allomorphs

Is there a positive as well as a negative side to this argument? The preceding remarks are concerned, in effect, with only one aspect of the overlapping shown in fig. 6.17: namely, with the phonological syncretism or ‘neutralisation’ of various morphemic distinctions. But there is, as we have pointed out, a reverse aspect: namely the phonological ‘diversification’ or alternative realisation of individual morphemes. Just as *si*; for example, may realise any of 1st Singular, Dative Singular, etc., so, conversely, a morpheme such as 1st Singular may be realised by any of *si*; *o* or *m*. Does this discrepancy, in turn, have any more serious implications for the model?

It must be stressed, once again, that such examples pose no kind of technical problem: in the case of 1st Singular it is easy to write a rule of morphemic conditioning:

\[(18)\] 1st Singular has the alternant *i* when directly preceded by Perfective, the alternant *o* when directly preceded by the verbal root, etc.

---

1. I.e. vowel-lengthening (*ferro-o*) or, following Housholder (1947: loc. cit.), basic *ferro-o*.
2. For ‘neutralisation’ in this sense see Pike, 1965: 190ff.; Callow, 1968: 33; Lamb, 1964a: 64, 1964b: 18; the concept plays a major role, in particular, in Pike’s application of ‘matrix theory’ to morphology (see further Pike, 1963; Pike & Erickson, 1964; Pike & Jacobs, 1968). It must, however, be stressed: I that this is only one of the different uses of the term which have been proposed in morphology (see: La neutralisation dans la morphologie et le lexique for a compactus and references to 1957); II that there is no real parallel between morphological ‘neutralisation’ (in any sense) and the established concept in phonology (see *passim in loc. 2. below*); III that above all there is no parallel if the term is used in this sense. The term ‘syncretism’ (cf. Hjelmslev, 1934, 1942 (1953: 39)) is accordingly preferable here. For the question of parallelism in general see Basbøll’s contribution, in particular, to the volume mentioned (Basbøll, 1957; also his review, 1961); the only significant contribution that I know of is that of Martinet (1968: 13ff.).

3. ‘Diversification’ is opposed to ‘neutralisation’ by Lamb (1964b: loc. cit.). The term ‘realisation’, which I have also borrowed in this context from Lamb (see ch. 16, p. 202), can be taken as another term for ‘exponential’ so far as IA is concerned. We will, however, distinguish the two in WP; see ch. 9, pp. 164, 185.

—and so forth, which is, of course, entirely comparable to rule (2) in our initial illustration. But why, in considering a formative of this kind, should one be obliged to detach the ‘realised’ morphemes, on the one hand, from the ‘conditioning’ morphemes on the other? It is true that if a form such as *rek-s-i*; (1st Singular) is compared with *rek-si*; (3rd Singular) or *rek-simus* (1st Plural) then it is indeed the long *i*; contrasting with the *t* or *mus*, which serves to mark the Person and Number categories. But what if we contrast *rek-si*; qua Perfective, with *rego*; (Imperfective)? The overt difference between these forms is that *rek-si*; has *a* (an) *s*-suffix (with consequent morphophonemic adjustments); and (b) an *i*; in final position; whereas *rego*; has *a* (an) absence of *s*; and (b) an *o*; in final position. Is there any reason, in treating these differences, for one to say that Perfective is the morpheme ‘realised’ in case (a), but merely a ‘conditioning’ morpheme in case (b)? Why not treat final *i*; as a marker, in an equal sense, both of Perfective and of 1st Singular?

The choice which is presented at this point may perhaps be compared to the dilemma which we considered earlier in the context of morphological processes. In each case we have a solution which will preserve the technical integrity of the Item and Arrangement model: a solution involving the concept of a ‘zero morph’ in one case, and the solution presented by rule (18) in the other. In both cases, furthermore, the argument against these solutions is that an overt difference between the total word-forms (the differences, for an example such as *rumpe:/ru#:p:*, between the nasal infixing and vowel-lengthening in the stem, or the *o*; and *i*; of the termination) is relegate to a sub-morphemic contrast between conditioned alternants. Is the argument, then, much weaker in this new context than in the former? It is clearly possible to conceive of a model which will allow the analysis in fig. 6.18—a model, in other words, which will permit a concept of extended and overlapping exponential comparable (at least in very general respects) to the fissional overlapping which we have already considered at the morphophonemic level. Moreover such analyses do not seem open to any

---

1. Above, p. 58.
2. See the argument on pp. 61ff.
3. See the argument on pp. 70ff.
a priori objection which is as powerful as that provoked by the introduction of ‘processes’. Why, then, should we insist on suppressing one of the marks of Perfective while concentrating on the other? Or insist on treating one of the ‘signalling functions’ of *i* as the only ‘function’ of real significance?

The answer to these questions is crucial to the acceptance or rejection of the morphemic model, insofar as its application to Latin inflections is concerned. If we allow the type of exponent shown in fig. 6.18, and with it (*a fortiori*, one assumes) the type of extended but non-overlapping exponent which was mooted earlier, then the original allomorph-relation is transmogrified into a quite different relationship altogether: one is no longer pairing bits of the higher representation with bits of the lower in any credible sense. But it is not at all easy, here as elsewhere in

of approaching the material from a ‘wave’ as well as a ‘particle’ viewpoint. But there are surely differences which are connected, once again, with the ‘double structuring’ of the morphemic representations (p. 73, above).

1 *Vid.* the objection discussed in pp. 63ff.

2 Above, p. 82, apropos of *requis.

3 I.e. it can no longer be formulated as a function or mapping (cf., e.g., Stoll, 1961: 37; Hockett, 1967: 26-7) of any kind. To summarise: I: We began (p. 49) with a bi-unique mapping of a string of morphemes onto a set of distinct segments. II: The notion of ‘segment’ was at once extended to include segments which were discontinuous (p. 59 and n. 1), and later, III: we queried, in our discussion of sandhi, the principle that segments need be distinguished at all at the lowest, phonemic, level (pp. 72, 80f.). Meanwhile, in the discussion of cumulation and morphological portmanteaus (pp. 65f., 72ff.), IV: we were obliged either to sacrifice the bi-uniqueness (allowing two or more morphemes to be mapped onto the same segment), or to take the domain as a set of substrings of morphemes, instead of the individual morphemes as such (p. 86, n. 1); furthermore, in our discussion of empty elements, V: we dropped the requirement that the relation should be a mapping onto (Stoll, 1961: 38) as opposed to a mapping into. If we consider these five amendments together, the original insight is already seriously attenuated; in Palmer’s words (1968: 291; cf. also 1964: 10ff.), each successive amendment in the history of IA has changed if not destroyed the original character of the model. But fig. 6.18, if accepted, would set the lid on it. Of course, it does not follow that the resulting generalised relationship need be subject to no restrictions whatever; but it will no longer be a relation of this special type.

**Morphemic Representation:**

\[ \text{REG} + \text{Perfective} + [\text{1st Singular}] \]

**Morphophonemic Representation:**

\[ *\text{reg} + s + i: \]

**Fig. 6.18**

this survey, to find some independent motive for preserving the morpheme/allomorph concept. One cannot simply argue that if formative *a* is clearly an exponent of *x*, and formative *b* is at least an exponent of *y*, then this in itself is sufficient reason why *b* cannot also be an exponent of *x*. To do so is to assume precisely the point which is under dispute: namely, that grammatical elements may be nailed down or pinpointed to distinct positions in word-structure. Nor could one argue that the *i*: *a* contrast is always, ‘in fact’, of a merely subsidiary value. In pairs such as *metuo*: ‘I fear, I am afraid of [so-and-so]’ and *metu*: ‘I feared’ the terminations would, indeed, provide the only phonemic cue to the distinction. If this is ‘subsidiary’ then it is only so because the assumptions which we are making do not allow it to be anything else. Surely the most one can claim (failing some argument which we have overlooked) is that these assumptions have some wider or overriding justification.

In the light of these remarks, it is interesting to see how little attention this phenomenon has attracted in morphemic theory. It is perhaps important that the notion of ‘morphemic conditioning’ would be reinforced by the parallelism with ‘phonemic conditioning’: isomorphisms of this kind, however facile they may appear, can rarely be neglected in the history of linguistic theory. In addition, one must admit that the solution of rule (18) is not absurd in the way that ‘zero morphs’ were absurd to many writers of the period: if an analysis is at least feasible there is, of course, less desire to disturb the model. But the argument against the zero morph in *book, sang*, and the like was not merely that ‘nothing’ could not contrast with the ‘absence of nothing’, but also that a quite unrealistic status was assigned to the opposition in vowel-quality. Why, then, should one not have extended this argument to the remaining cases where an inflectional or ‘minor’ morpheme was said to condition an alternation elsewhere? The more recent literature does contain at least some treatments which explore the implications of overlapping exponentence, and it is to be hoped that these will lead to a fresh assessment of the standard approach.

1 Note ‘phonemic’, morphophonemically one may argue for *metu-ci* versus *metu-u-ci*: or the like (see ch. 13, pp. 394f.).

2 Cf. Bazell, 1953: Preface, for a general comment on the ‘isomorphisms’ of this period. For the particular parallelsisms between morphology and phonology see below, 7.2, pp. 109f.

3 References, to Haas and Nida in particular, in 6.1, p. 62, n. 1.

4 E.g. in the work of Pike and his collaborators (p. 92, n. 2); see also my treatment of Modern Greek (Matthews, 1957b) which provides a specific comparison between an analysis on the lines of fig. 6.18, and an earlier treatment (Koutsoudas, 1962) on classical ‘Item and Arrangement’ lines.
6.4.3 The word as a morphological unit. There is, finally, a third aspect. In the preceding subsections we have discussed the signalling hypothesis, first, from the standpoint of the single formative (6.4.1) and, secondly, from the standpoint of a pair of formatives working together (6.4.2). But what of the word-form as a whole? In forms such as *lapis* 'a stone' and *regis* 'you (sg) rule' the final *s* might, in itself, be a marker either of Nominative Singular or of 2nd Singular; it is only by the combination of root and termination (there being no Nominal root *reg-*) that the ambiguity is resolved. Again the form *lapis* alone might, by purely inflectional cues, be perceived either as *lapis* (Nominative Singular) or as *lap-is* (Genitive Singular). It is only the precise distribution of root allomorphs (*lapis* in *lapis* but *lapid-* in *lapidis* 'of a stone') which excludes the second possibility. In such cases an indeterminacy at the formative level is clearly rescued by wider restrictions within the word-form: is there any reason, therefore, why one should deny that the word has any special status in morphological description?

The notion of the word has already been the subject of a rich literature in structural linguistics; let us therefore confine ourselves, initially at least, to the role which it plays within the description of Latin in particular. In the first place, there is, of course, no doubt that the traditional word-boundaries represent a genuine demarcation between linguistic units. The unit concerned may be established both by grammatical and by phonological criteria; furthermore, these criteria would coincide (so far as one can tell), with at most a handful of exceptions.1

1 A useful introductory essay is that of Martinet (1966). For detailed surveys and substantive contributions of various sorts, see Reichling, 1953; Rosetti, 1947 (though this is unfortunately weak on the literature in English); Tochle, 1949a (particularly clear and useful); Ullmann, 1957: 43ff.; Krämker, 1969; some more references can be found in Pike, 1967: 481–3. Relevant passages in general textbooks are indicated below, passim; see also references for morphology/syntax in ch. 2, p. 11, n. 2, or ch. 4, p. 44, n. 2, and for different formal sorts of 'word' in 9.1. Of the earlier substantive contributions, that of Sapir (1921: ch. 2) is still of the highest value and relevance.

i.e. the following remarks do not hold for all languages, even some which do have 'words' in a partly similar sense. The charge of non-universality (if it is a 'charge') will be picked up in 7.5.

2 For grammatical criteria see below. For phonological criteria in general cf., e.g., Ullmann, 1977: 47ff.; Robbins, 1964: 197f.; one must remember that although such criteria are often of practical utility in particular languages (in Latin the predictable stress – see Appendix 2 – is a striking feature) nevertheless they are in the last resort of an auxiliary or secondary character (see Bazell, 1972: 26ff.; briefly, Lyons, 1968: 226). So far as Latin is concerned, the few known discrepancies concern the enclitic particles *ne* (Interrogative), *k’re* 'and', and see *or*; thus in a form such as *armes*.

Whatever one’s precise theoretical viewpoint, there is therefore no doubt that in a sentence such as *veneer tiberius* 'Tiberius will have come', the split between *veneer* and *tiberius* is not merely a split between a Verb and ancillary morphemes:

VEN + Perfective + Future Indicative + 3rd Singular,

and a Noun and ancillary morphemes:

TIBERIUS + Nominative Singular,

but a split between two parts of the sentence which, despite differences in construction, are nevertheless comparable in several important ways.

In the second place, the properties of the word are of a type which point to some special theoretical status. In the syntax, above all, there are at least four independently significant peculiarities:

I. First, the word is the minimal sentence-construction: no group of morphemes smaller than the word could appear or be instantiated as an utterance in its own right. Thus in the example cited either *veneer* or *tiberius* could readily stand alone; but not the stem *vene-*, the termination -*s* Nominative Singular*, the suffix-complex *-erit*, and so forth.

II. Next, the word in Latin would never exhibit a recursive or self-repeating construction. If we take a larger syntagm of the type *posthavior suminos advenire* after the senators arrived, and another of the type *senatores hooes* wohasvarat the senators he had invited, it is obvious that the relative clause *hooes wohasvarat* could modify the Subject in *senatores advenire*; that *wohasvarat* could, in turn, be modified by a further temporal clause; that this could, itself, have a Subject or Object with a further dependent relative, and so on. There is nothing in the grammar, that is to say, which would exclude a clause

*marumbe* 'arms and the man' an immediate-constituency boundary of the inter-word type cuts across a unitary word (*wirumbe* *ke*) established in the phonological sense.


3 See the discussion of 'minimal free forms' in Bloomsfield, 1926 (= Rbl, 27, 1935: 158. A similar definition was also suggested briefly by Sweet (1876); see also Poiyam, 1936: 79–86. As a definition of the set of all words in a given language, this has never been considered adequate (cf., for example, Lyons, 1968: 20ff., and in part Horns, 1938: 14ff.), the defect being that there are some 'words' (even in Latin, though more so in English for example) which do not share the potential sentence function in this sense. But granted that the class concerned is established by some descriptive criterion, then the minimal sentence-bound of the majority is surely sufficient for the term 'word' (as a term in general linguistic theory) to be applied to the class as a whole; cf. Lyons, 1968: 318, 1966: 210f. for a similar point concerning 'parts of speech' terminology, and compare Bazell’s discussion (1953: 6ff.) of Bloomfield’s definition.
or phrase of indefinite extent and complexity. By contrast a word can never exhibit more than a certain fixed number of elements. It would be quite impossible, for example, to take the word adowe-nernunt [they] arrived' and add a second Aspecific morpheme, a further, contrasting, termination, or the like.

III. Thirdly, the cohesion of morphemes within the word is considerably greater than the cohesion of words within the sentence. In the sentence it is possible, and even normal, for a syntactym to be interrupted by certain extraneous elements; thus an example such as the following: attwea hāt ait omnia fakere se: diginitatis kausa: 'and he says he is doing all these things to save face', shows that the construction both of a dependent clause (hāt ... omnia fakere se: diginitatis kausa: 'that he is doing all these things to save face') and of a phrase within the clause (hāt ... omnia 'all these things') may be broken into by a single word (the main verb ait 'he says') belonging to the wider construction of the sentence. A word, on the other hand, can never be interrupted in a similar way: there are no instances whatever of the type s-tiberius -eri, tiberiu- weier- -s, and the like.

IV. Finally, the pattern of elements within the word is both fixed and non-contrastive in character. At sentence-level it is well known that one could have both s-nernit tiberius and tiberius weier- - with, presumably, a difference in emphasis or conditioning from the discourse context. But again there is no parallel whatever at word-level: one could not have a variation - let alone a semantic or stylistic contrast - between tiberius and s-tiberius, or between weier- and it-tiberius, or even weier- and it-tiberius, et-tiberius, and so forth.

1 For recursiveness in grammar see, for example, the textbook discussion by Lyons (1968: 221). Non-recursiveness as a criterion for the word is often ignored, presumably because it does not apply to all languages which have words in general; nevertheless for those where it does apply, the coincidence of this and other criteria seems particularly striking.

2 Non-separability is 'probably' the most interesting criterion for the word according to Martinet (1949: 293). However as Martinet points out (see also Martinet, 1968: 44f.) and as Kránsky has recently reminded us (1969: 27f.), this criterion must emphasize the extraneous character of 'interrupting' material. Thus, to parallel Kránsky's examples (Czech: světla 'I sing'; světlačam 'I am accustomed to sing'); it is irrelevant that, e.g., amat (*ama-t) could be 'interrupted' by a fresh piece such as bar, to yield amata - (*ama-bar-t). The point is that it cannot be interrupted by rearranged material from elsewhere within a given sentence.

3 For this aspect of the word see Wells, 1946 (= RL, 1971; a similar point has also been stressed by Bazell (e.g. 1953: 14f.) For textbook discussion see Lyons, 1968: 202-4, and Robins, 1964: 104f.; for both these writers our points III and IV are expressed in terms of the cohesion or 'internal stability' of the word, and for Robins point I (and II) also come under this heading. The present separation of 'limitations of insertion' and 'limitations of sequence' is taken from Bazell (1957a: 26).

6.4 Can the morph be a signalling unit? The most that can happen is that a certain morpheme (via., the Perfective) may show a variation in position from one paradigm to another.

For such reasons the concept of the word is generally accepted to be one of central importance in the description of Latin and similar languages. But how precisely would this bear on the problems of inflectional morphology? In what way might it help the description of lapis, lapidis or any of our previous examples, to refer to the word as a whole as well as to its individual parts? In the preceding paragraphs we have, in effect, considered the background for such a query; but now, granted that the unit 'exists', in what way does it exhibit a morphological as well as a syntactic and phonological function?

The first major point is that the word does, in fact, define the domain for 'morphic conditioning'. It is quite normal, as we have seen, for elements to influence each other within the word-form: thus in reihst: the allomorph of 1st Singular would be conditioned by the neighboring Perfective morpheme, and the allomorph of this morpheme, s, would in turn be conditioned by the adjacent root. On the other hand, there is no case where one morpheme would be said to determine the phonological form of another across a word-boundary: no suffix, for example, is conditioned by the root of the word which follows, by a following derivational prefix, etc. Even if no other considerations were involved, surely this in itself is a generalisation which the morphological section of a grammar may be expected to express.

But other considerations are involved, of course. We have already seen that there is extensive ambiguity amongst formatives: for example, the formatives in fig. 6.17. We have also seen that the words, as wholes, may resolve this ambiguity to a very significant extent: thus of fifty-three distinct combinations for ferris, ferimus, and aperiis: only four would effectively survive the morpheme-to-word stage of a hypothetical decoding procedure. Does not this fact, too, have an important bearing on our present problem? If we consider the point in isolation, then there is a case at least for suggesting that the resolution of ambiguities at word-level should not simply be confounded with the wider 'disambiguation' within the sentence or utterance-context: ferris, for example, might be resolved into fer- + Infinitive + Passive and ferris + Genitive Singular.

1 I.e. in reduplicated Perfects of the type bu-lurrari: I ran! (see below, p. 135) as against suffixal Perfects of the type teeh-iti, etc. The importance of contrastive ordering is discussed below, pp. 105f., 122. 2 See p. 91, above.

2 Pp. 89f. For the stage-by-stage concept of speech-recognition see our introductory remarks and references on p. 45.
Before, as it were, this residual ambiguity is itself disposed of. But if we consider, in addition, the points which we have referred to in the preceding paragraphs, are there not also grounds to suspect that the nature of word-perception is different in kind from the recognition of wider grammatical structures? Specifically, that the former is, in some sense, a matter of morphology rather than of pure syntax or semantics?

There seem to be two lines of argument which might be advanced to support such a plea. The first is simply a development of our earlier observations concerning word-syntax: particularly the features of fixed order and internal cohesion (points IV and III in our summary). To begin with an example, it is obvious that in the form reksit 'He, etc.' ruled' the termination t could not but be a marker of 3rd Singular; although a t in itself could also mark Past Participle (as in rekt-a-a 'She is, etc. ruled'), it would never, in that case, appear in final position. Likewise the s, in medial position, could not but be assigned either to Past Participle or to Perfective; it would be impossible for it to mark 2nd Singular (as in regis) or Nominative Singular (as in lapis) since these, again, have to be terminations. In this way, an interpretation of the type:

(a) REG- + Nominative Singular + Past Participle

or:

(b) REG- + 2nd Singular + 3rd Singular

is excluded on purely positional evidence. It may be excluded, that is to say, without any reference at all to the syntactic compatibility or incompatibility of its members.

Now it may be objected, of course, that this is precisely what syntactic 'disambiguation' is about: s could realise the 'item' Nominative Singular or 2nd Singular, and t could realise the 'item' Past Participle, but the 'arrangements' (a) or (b), which result from such assignments, cannot be fitted to any relevant constructional pattern. But is this a realistic objection in the Latin context? The essence of Latin syntax, in its formal aspect, resides in the relative independence of syntactic and collocational interconnections, on the one hand, from the relation of word-order on the other. Thus in the fourth line of the Fourth Ecologue:

ultima kumai: wevite jam karmis aitas 'The last age of the Cumaean prophecy has now arrived', the fact that ultima 'last' could modify

aitas 'age' but not karmis 'song, prophesy', or that kumai: 'Cumaean' could, conversely, modify karmis but not aitas, is made clear by the patterns of Case agreement: ultima cannot be Genitive (whereas karmis can be nothing else), and kumai: cannot be Nominative Singular (whereas aitas, again, cannot be anything else). Similarly, it is the Cases which signal the dependency of the inner Noun-phrase kumai:... karmis 'of the Cumaean song' on the outer Noun-phrase ultima... aitas; a Genitive may modify a Nominative, but not vice-versa. In all this the mere ordering of the elements - with both Adjectives separated from their Nominal collocates, with kumai: (which could, in itself, be Nominative Plural 'the Cumaean') immediately adjacent to a non-collocate (the Singular Verb wevite 'has arrived'), and so on, is notoriously neither here nor there. In a drastic sequential variant, say: kumai: karmis ultima aitas jam wevite, the interconnections would remain precisely the same.

By contrast, the morphotactics or word-syntax is founded almost entirely on a sequential framework. The fact that Nominative Singular and Part Participle can, in fact, go together - as in the representation:

REG- + Past Participle + Feminine + Nominative Singular

for reksit - is quite irrelevant in considering a word such as reksit; it does not matter whether the morphemes are compatible (as in interpretation (a)), or incompatible (as in (b)), if the ordering of their allomorphs is wrong. At first sight this may, perhaps, appear to be an argument for rather than against the morphemic treatment; the morphotactics would thus lend itself to an 'arrangement' concept in a manner which is more, not less, marked than for the syntax. But this is to neglect the parallel point concerning non-contrastiveness. The classic Item and Arrangement system is one in which each feature of arrangement supplies an 'episememe', or feature of constructional meaning, to add to the 'sememes' or individual meanings supplied by the items: thus in English John has come as against Has John come? But since this is not true of Latin morphotactics, there seems little point in treating the identity and the position of an element by separate statements. Instead of

---

1 An example that I have heard discussed in a lecture by R. H. Robins; but I cannot trace it in print. For the point in general see, for instance, the remarks of Meillet, 1933: 166f.

2 See our point IV. For warnings against projecting morph-sequence onto a higher level see, in particular, our reference to Bazell, 1953: 12f.

3 Cf. Bloomfield, 1935: 166f.; also, e.g., Wells's account (1947a: §19 (=RIL, §f.) of Saussure.
saying that 3rd Singular, for example, has the allomorph \( t \) (Rule (1) in our original illustration), and then saying that the morpheme can only appear as a word-final, we may neatly combine these statements into the single rule:

(19) 3rd Singular is marked by a formative \( t \) in final position,

- a rule, in other words, which would specify the termination in a traditional sense.

The second line of argument derives, of course, from the point about morphemic conditioning. Consider \( ferri \), once again, in the syntagm \( eam brude:litate et skelere ferri \): ‘that she is being carried away by her crimes and cruelty’. If we examine the external relations of \( ferri \): within the sentence, its verbal nature is forced, so to speak, by the failure of the alternative representation:

\[FERR- + \text{Genitive Singular}\]

(‘of iron’) to enter into a satisfactory collocational relationship with \( skelere \) ‘by crime’, by the clearly non-verbal character of the remaining words, and so on. It is, in brief, a matter of syntactic and semantic constructability in the usual sense. But if we examine the internal relations within \( ferri \) as such, then at once a quite different picture emerges. The reason why \( ferri \) cannot, for example, bear the interpretation:

\[FERR- + \text{Dative Singular}\]

NOT simply a matter of morphemes and arrangements of morphemes; the suffix \( -i \) can (as we have seen) be a perfectly good marker of Dative Singular, and both the construction:

\[\text{Noun + Case/Number,}\]

and the particular morphemic representation in question are in themselves perfectly satisfactory. What matters is the specifically morphological block between \( i \) as a 3rd Declension termination (as in \( reg-i \): to the king’) and \( FERR- \) as a 2nd Declension noun: ‘to [the] iron’ could only be \( ferr-o- \). Likewise, the reason why it cannot bear the further interpretation:

\[FERR- + \text{Nominative Plural}\]

(compare \( domin-i \): ‘[the] masters’) is that \( FERR- \) is a 2nd Declension Neuter, whereas as Nominative Plural \(-\text{ii} \) is conditioned by Masculines.

1 P. 59.  
2 See p. 90, above.  
3 P. 91.
7 Item and arrangement versus word and paradigm

We have now completed a survey of the various problematic or less attractive aspects of the Item and Arrangement model, when looked at from the standpoint of a typically inflecting language. Two points about this survey will, however, have been noted. In the first place, the phenomena in question have not yet been related comprehensively to any alternative model. The four main divisions with which we began the discussion were themselves established with IA in mind; for example, fusion and cumulation were taken together because, at the outset, they appear to raise the same problem for the allomorph-relation. Furthermore, although each subsection has led to a number of clues or partial formulations, we have yet to tie these separate remarks together. How far can we in fact do so? And if we do succeed, will the resulting model be free of its own idiosyncratic defects? Or will we find, on comparison, that the Item and Arrangement model is not so unattractive? A supporter of IA is bound to raise such questions at this point.

Secondly, we have pressed ahead with the problems of inflecting languages, while averting our eyes from the agglutinative and other types. But the existence of such types is crucial to our argument. As we made clear in Part 1, universality is not taken to be an absolute requirement for morphological theories. But it would be foolish to deny that it is one sort of virtue. If a theory could in fact apply to languages of every type, with benefit both to the typology and to the individual grammars, that would naturally be the best solution. Now there is no need for these other types to be systematically examined; in a work of this kind, we may assume that specimens are familiar. But the inevitable criticism—that a model ought to be universal, and that one based empirically on ch. 6 is not—must clearly be dealt with.

The argument will accordingly proceed as follows. First we will introduce, in a discursive way, a model which is essentially the opposite of 'Item and Arrangement' (7.1). As one would expect, its main features are not original; indeed they represent a tradition older than that of IA, which the popularity of IA has merely tended to overshadow in recent decades. Hence it will not need more than a general outline at this stage. Before going further, we shall also pause to examine the historical reasons which led to the ascendancy of IA (7.2). We shall then turn to a detailed confrontation, first, of the two extreme models and, secondly, of the two extremes and a number of possible compromises (7.3 and then 7.4.1-3). This will still be confined, for the most part, to the context of inflecting languages. Finally, we shall return to the question of universality (7.5).

7.1 A preliminary version of 'Word and Paradigm'

We begin, then, with the outline of an alternative model. From the arguments in ch. 6, it would appear that there are three requirements which are of central importance.

1. The first is that the model must, for the sake of 6.4.2 or 6.4.3 in particular, assign a central role to the word as well as to the individual primitive elements. From this it does not follow, of course, that the word should itself be a primitive, or that we need fall back on the type of traditional treatment (see 3.2) in which the only morphological analysis is that implied by the listing of word-forms in exemplary paradigms. What is important however is, first, that the word-boundaries must set some limit to the formulation of inflectional rules: specifically a rule at the morphological stage of description should be allowed to refer to any grammatical element within the particular word in question (6.4.2).

---

1 Nor, it is worth remarking, has it been overshadowed in all language areas. Thus for Russian and other Slavonic languages, where a structuralist tradition had been established before IA (cf., in particular, the morphological studies of Karcevskii, 1927, 1932, etc.; Jakobson, 1932; Trubetzkoy, 1934), the continuity is not broken even in the United States (cf. Jakobson's post-war study of Russian (1948) or, most recently, the numerous inflectional studies of Slavonic languages by Bidwell (e.g. Bidwell, 1964)). Here we are concerned, of course, with languages tending towards the inflecting type.

2 It is worth recalling, at this point, the three main features of the IA model which were outlined in ch. 4, pp. 41-4. Of these, feature (II) (the allomorph-relation) has been challenged in various ways throughout 6.2-4; likewise feature (III) (linear arrangement) is challenged in particular by the discussion of words in 6.4.3, and feature (II) (the generalisation of the morpheme) is also thrown into doubt by the association of processes with certain inflectional categories (6.1). Accordingly, no vestige of these IA features remains in the paragraphs which follow.

3 See p. 57.
to none whatever outside it (6.4.3). Secondly, it is important that the traditional division between word-structure (forming a specific level of structural morphology) and sentence-structure (forming a related but separate level of syntax) should in one way or another be maintained. In Item and Arrangement terms both, as we have seen, are a matter of linear arrangements of elements (point III in ch. 4); hence it is at most a difference of size-levels (word as a smaller unit, clause and sentence as larger units) which is involved. But according to our subsequent arguments the linearity of word-structure is both unnecessary, in that the putative ordering of elements is never contrastive in character, and also in one respect inconvenient, in that the ordering may be arbitrary in the typical case of cumulation (6.2.2). Let us therefore turn to the opposite extreme and assume a model in which the structure is entirely non-linear in character. In more traditional terms, for example, a word such as ferra is not grammatically:

\[ \text{FERR-} + \text{Infinitive} + \text{Passive}, \]

(with order specified), but simply:

'The [Present] Infinitive Passive of fero'4

or (since it comes to the same thing):

'The Passive Infinitive of fero'.

The only relationships specified, in other words, would be those between the individual categories or 'accidentia' (Passive Voice, Infinitive Mood) and the verb fero; nothing whatever is said about the actual ordering of exponents. The formal details of such an expression need not concern us, but let us employ the term WORD AND PARADigm

1. By contrast morphophonemic rules, viz., of external sandhi (p. 71, n. 3), do cross word-boundaries; but here, of course, a possible distinction between internal and external sandhi would draw on the same general feature of the model.

2. 'Size-level' in the sense of ch. 2, p. 8. As we have seen, such writers as Halliday (1964) have proposed a hierarchy of size-units in which the word has precisely the same theoretical status as the clause or phrase.

3. 6.4.3, point IV on pp. 68f.

4. I.e. 'Present' Infinitive as the traditional term for Imperfective Infinitive; see Appendix 1.

5. The classical term 'accidentia' (hence 'accidence' in the traditional schoolbooks) is exemplified e.g. by Robins (1967: 57ff.).


(which stands at the head of the present chapter) to refer to any model with grammatical representations of this general sort.1

II. The second requirement is that the structure of the word-form must be supplied by statements of a wholly morphological nature. Let us suppose, for example, that we attempt to handle the form mon-u-i: ('I advised') by a triplet of rules as follows:

(20) The root of the Verb MONEO is of the form mon;

(21) The formative u is selected if (a) the word is characterised by the element Perfective and (b) the Verb belongs to a class with the members MONEO, VETO ('forbid'), etc.

(22) The formative i: is selected if the word is characterised by the elements 1st, Singular, Perfective and Present Indicative.2

Obviously these rules, as they stand, are quite inadequate; they fail to indicate whether the word-form is mon-u-i: (with the root, and then one formative, and then the other), or u-mon-i: (with one formative before the root and one after), or what have you. Now in Item and Arrangement terms the order of morphemic segments would, of course, be specified by the rules of syntax; the syntactic representation of the word has the morphemes in a certain sequence, and the sequence of allomorphs follows automatically (failing some specific adjustment to the contrary) from that. But in a Word and Paradigm model the syntactic elements do not supply the order (point I); it therefore follows that if we want to employ rules which are similar to (20–2), we must first introduce some

1. It should be obvious that the adoption of this term (which originates with Hockett, 1954, and Robins, 1959) implies no commitment to 'paradigm' (μαραδεύσης) in the classical sense. Possibly Hockett himself had such paradigms in mind; he did not, of course, expound his third model in even the most general form. But Robins refers specifically to a model which will absorb some of the insights of morphemics (1959: 116f.) - and in doing so he does, indeed, take the only realistic interpretation of a word-based approach. See also Matthews, 1965b, 1965c - the latter, in particular, for a reconstructed definition of 'Paradigm' and related terms. It must be acknowledged, of course, that true exemplary paradigms (see ch. 3, p. 22) still have their uses for various purposes: e.g. for language-teaching according to Trend (1997).

2. 'Present Indicative', of course, in the sense of the preferred schema in Appendix 1.

3. Compare the putative adjustment of 'Future Participle' to 'Past Participle + Future Participle' (pp. 84f). Another such rule could, for example, permute 'CURR- + Perfective' to Perfective - CURR' for the sake of hukurr- (see p. 135); compare, for example, the 'morpheme transformation' T1 in Cowan & Merrifield, 1968: 287. Likewise, in Lamb's formulation, there would be provision for 'anatisis' (Lamb, 1964a: 66, 1966a: 22); both interstitial and intrastral. Such solutions work, of course; it is only their appropriateness which is in dispute.
general specification of formative ‘slots’ or positions—say by structural formulae of the type:

\text{Root} (\text{First Suffix}) (\text{Second Suffix}) \text{Termination,}\footnote{The informal use of the term ‘slot’ does not imply commitment to specific models, particularly that of Pike (1967, etc.), in which this or similar concepts play a central technical role.}

and then indicate which position each particular formative belongs to. Thus rule (21) would then read:

(23) The First Suffix u is selected if . . . ,

and likewise rule (22):

(24) The Termination i is selected if . . .

In one way or another, a comparable concept of morphological word-structure (a concept of INFLECTIONAL STRUCTURE, to be more precise) would appear to be involved in any Word and Paradigm solution which might be suggested.

III. The final requirement is that the model must allow for processes at both the morpholexical (see 6.1) and the morphophonemic levels (see 6.2.1 and 6.3).\footnote{This formula is to be read in the way which is now usual in linguistics; i.e., the forms which are covered by this statement must have at least a root and a termination and may, in addition, have either a First Suffix or Second Suffix or both. For a textbook introduction to these and other notations see the discussion of abbreviated rules in Koutsoudas, 1966: 9ff.} In the latter case the scope of the process description is already clear enough for our present purposes;\footnote{Pp. 71ff. and 86.} the statements concern essentially rules for the interaction (by assimilation, fusion, epenthesis, etc.) of neighbouring formatives with specific phonological qualities. In the morpholexical case there is, however, one important problem which we will eventually have to consider: namely, should we, in effect, restrict the rule of processes to the points where they are strictly necessary (as, for example, in the treatment of \text{rump}^-, \text{rup}^-, etc.),\footnote{But for further clarification see below, pp. 127ff, and 10.1, in particular pp. 25ff.} or should we extend their application throughout the entire word-form? The nature of the rules is, of course, quite straightforward: in the case of \text{rup}^-, for example, what we would have is a statement essentially of the form:

(25) The final vowel of the root is replaced by the corresponding long vowel if (a) the word is characterised by the element Perfective, and (b) the Verb is a member of the class RUMPO, etc.,

in which an operation is determined by certain grammatical properties of the word in much the way that rule (21), for example, may be said to determine a particular suffix. But why not, in that case, make the rules entirely parallel, i.e., by replacing (21) by an equivalent process statement of the form:

(26) The formative u is suffixed to the root . . . ,

where the only difference lies in the nature of the particular operation which is involved? And if we accept this, then why not go on to handle \text{moni}^+ or \text{ru}^+ as a whole by a further process of suffixing i to the forms which (25) and (26) have provided?

The answer to this last question can, perhaps, be postponed to a later section; it does not seem to have any crucial effect on the validity of the arguments which follow. But the remainder of our three points are of real importance. To allow the types of descriptive statement which have been mooted we are more or less forced to adopt a theory which is oriented towards the word in some significant sense, which employs some concept of inflectional structure that is independent of the patterning of elements in syntax, and which employs a process formulation for at least some purposes. How, then, does such an approach stand up to a wider or more systematic confrontation with Item and Arrangement?

7.2 Earlier motives for ‘Item and Arrangement’

It is as well to remember, first of all, that the concept of the morpheme as developed by the post-Bloomfieldians had its origin in preoccupations which are not shared by the majority of linguists. In particular, the post-Bloomfieldian methodology in general has been sharply criticised by the more recent generative school. How far, then, should the rejection of this outlook in itself compel one to revalue the proposals which ensued?

Of these preoccupations, there are three which appear to have been particularly important. The most explicit was the attempt to bring morphology into line with phonology; just as the PHONEME, as the minimal sound-unit of language, had been defined as a class of ALLOPHONE in certain distributional relationships, so there was to be a minimal unit of grammar, the morpheme, whose allomorphs were to be
related in a similar way. Likewise the syntax or morphotactics of a language was concerned with the arrangement of morphemes in precisely the way that the phonotactics, or 'phonological grammar', was concerned with the arrangement of phonemes. It goes without saying that this attempted parallel seemed, at the level of formulation, terminology, etc., to be entirely successful; by the end of the 1940s one has a virtual isomorphism between two 'Item and Arrangement' models, one for phonology and the other for morphology, which differ only insofar as they are based on different fundamental units. But how much of real substance had, in fact, been achieved?

The answer at the time was already less than wholly favourable. The fact that the phoneme was a sound-unit, and had necessarily to be identified by phonetic and not by wholly distributional criteria, made it impossible for the putative allomorph relationship (in the sense of § 1) to parallel the relation between allophones and phonemes in any significant sense. But in addition the development of phonological theory since this period has led to even stronger doubts. At the beginning of the 1940s it was natural to define the phonemes of a language in terms of non-overlapping classes of sound-segments; familiar efforts in the 1930s had been tending in this direction, and a high degree of clarification and codification had already been achieved. Hence it was at least interesting, whether fruitful or not, to take this definite progress as a source of clues for the exploration of grammar. But what if the phoneme itself is then

---

1 For classificatory definitions of the phoneme see Hockett, 1942 (in *RIL*, 100), Bloch & Trager, 1942: 40; Bloch, 1948: 5; also earlier, and outside the Bloomfieldian school, those of Jones (1933, 1962: 7). For a similar approach to the morpheme see below, p. 113; the parallel is brought out particularly by the similar organisation of chs. 7 and 13 in Harris, 1951a. Finally, for the terminological parallel between phoneme/phoneme and morpheme/morpheme see again the references to Hockett and Nida in p. 42, n. 4.

2 For the term 'morphotactics' see above, p. 89; 'phonotactics' (see ch. 10, p. 219) is from the same source. For phonological grammar see p. 221, n. 2; the attempt to find parallels is still strikingly illustrated by Saporta & Contrares's importation of phrase- structure rules and transformations from the grammar into the phonology (1963).

3 Cf. Harris, 1951a: 8; for 'isomorphisms' between levels compare our previous discussion in ch. 3, p. 23.

4 The spurious nature of this parallel has been argued by Bazell in various places; see in particular Bazell, 1952: 36ff. For objections to a purely 'relational' characterization of the phoneme (proposed as part of Hjelmslev's programme (e.g. Hjelmslev, 1948)) see in particular Bazell, 1954; also Trubetzkoy's comments (1939 (in *HST*, 96f)) on Hjelmslev's earliest distributional approach to consonants and vowels. For the danger of pushing the 'structural' principle too far in this respect see already the well-balanced survey by Pes (1930: 27).

5 For the post-Bloomfieldian school see, in particular, the contemporary codification represented by Hockett, 1942 (and again Joos's comments (*RIL*, 100, 108)).

---

7.2 Earlier motives for IA

rejected? The intervening decades have seen two quite independent theories of phonology (those of the Firthian school and later of the transformational grammarians), both of which have abandoned the search for distributional definitions, and both of which have replaced the segmental sound-unit by primitives of a quite different character. If these developments are accepted then, of course, the parallel in question collapses. From this it would not follow that Item and Arrangement morphology must be rejected too, and still less that a fresh parallel should be sought on some alternative basis; but it does follow that the morphemic concept must be justified in terms which are partly different from those in which it was initially put forward.

The second preoccupation (and the most crucial of all, perhaps?) concerned the search for what have since become known as "discovery procedures". In the preceding section we have considered no more than the model and the format for morphological statements: we have taken for granted that tudi, let us say, is a single grammatical word-form, that its characterisation involves a verb fer- which also appears in ferri, a Perfective element which recurs in reksi: but not in ferri, and so on, and the only question we have asked is how the form tudi and these particular grammatical elements can best be related. But, of course, this is not at all the question which the morphemic approach was originally meant to answer. How (many linguists of the 1940s would have asked) are we supposed to know that these are the grammatical elements which are involved? By what procedures, on the basis of what factual assumptions, and in accordance with what formal system of definitions, have
we established that the sequences of phonemes *tuliː*, *ferriː*, etc. should be isolated and analysed in this way? Furthermore, if we did make explicit the steps by which we arrived at the analysis, then what further statements would be required?

The approach in question was, of course, very largely American in inspiration, and at this point we can no longer ignore the switch from extreme to extreme (from extreme inductivism on the one hand to extreme deductivism on the other) which has been the salient feature of American linguistic methodology since the Second World War. In this book we are clearly biased against the earlier approach; to be specific, we have accepted the Chomskyan position that an issue between one 'analysis' and another (empirical considerations apart) is a matter for evaluation within, if necessary, the total context of the description. Questions concerning 'the way we arrived at an analysis' are accordingly of no serious concern. However, they were of very serious concern indeed to the post-Bloomfieldians; and the question therefore arises whether, among the characteristics of the Item and Arrangement model, there are any which are due not to a more balanced assessment of language-structure, but to specifically methodological priorities of this kind.

In particular, how much is due to the shibboleth of 'circularity' and 'mixing of levels'? As is well known, the phonology of this school and period is distinguished by the attempt to exclude 'grammatical prerequisites': the data for the analysis was considered to be strictly phonetic in character, and the 'rigour' or objectivity of its results would be damaged by reference to constructs on some 'higher' level. But an equally important point concerns the exclusion of what may be called 'syntactic prerequisites' from morphology. The analyst was not considered to know, qua analyst, that *tuliː* or *ferriː* were isolates of any kind, let alone that they belonged to the same paradigm or to classes such as

---

Footnotes:

1. For one extreme see again the general work of Harris (1951d); also the investigation of English by Trager & Smith (1951). For the other extreme see the more recent writings of Chomsky, particularly Chomsky, 1965: ch. 1; but the methodological 'revolution' concerned (see Hockett, 1965: 196; Robin, 1965: 230; Thorne, 1965: 74–4 to name three quite disparate writers) dates essentially from the last years of the 1950s.

2. See again ch. 2, pp. 14f.


4. One of the earliest and clearest statements is that of Hockett (1942 (=RIL, 1973)); see also Bloch, 1948; Harris, 1951a: chs. 3, 4, 7, 8, etc. The term 'grammatical prerequisite' is from the contemporary critique by Pike (1947e, 1952); for the reaction to Pike at the time see the particularly obstinate remarks which Joos (RIL, 96) appended to Bloch, 1941.

---

Finite Verb *verus* Infinitive and the like; the most that he could assume would be a phonemic transcription of (for a living language) a certain corpus of utterances plus, for writers of a less 'rigorous' turn, an indication of whether the meaning of utterances was entirely or only partly different. Now on this basis how could one proceed? Apparently the only way to establish, say, the Hau-, Perfective and 1st Person elements in a form such as *hauisi* would be to begin by taking the whole range of utterances in which the phoneme-sequences *h*, *hay*, *si*, *s*, and so forth appear, to discover which of these segmentations (if any) will lead to the most coherent patterns of recurrence throughout the language, and then to group the segments, with others if necessary, under headings such as 1st Person and so on. In other words, one will follow a dual procedure of segmentation (into *hau*-, *s*, and *i*); and classification of segments (as assigned exclusively to a class labelled Perfective, as assigned exclusively to one labelled 1st Person) which results precisely in the type of analysis which we have criticised earlier in this part. The question is: how far can the crucial features of the allomorph-relation—the insistence on discrete segments on the one hand and discrete assignments to morphemes on the other—be attributed simply to the exigencies imposed by this sort of methodology?

Historically the answer is, of course, extremely difficult to determine. For one thing the model and the procedure of analysis were not always conceptually distinct: it is not obvious, that is to say, that all linguists of this period were prepared to think out where their procedures should lead (what sort of representations of units they wanted, what sort of relations between units were prescribable) before they considered the form which these procedures should take. In addition, is it entirely

---

Footnotes:

1. The extreme position, as Bierwisch points out in his useful survey of definitions of the morpheme (1962), is most consistently taken by Harris (1951a; 1954b); but see also Trager & Smith, 1951: particularly 53f, 68. For the application to morphological analysis specifically see Harris, 1954: 156f, Hockett, 1958, and Harris, 1958 (which seems to be the last major contribution in this vein). For 'recurrent sames of meaning' see, however, Bloomfield, 1926 (=RIL, 16f), 1935: 138ff. and compare, for the post-war period, Nida, 1948: 28ff, 1949: 7 et passim; later, the exclusion of meaning from Harris's morphological work was specifically singled out at the end of Fowler's review (1952: 390). Finally, for an able antidote to the whole idea of a 'bottom-to-top' distributional analysis see Haas, 1954, and the later review (Haas, 1960: particularly 254ff) of Hill, 1958; it is with these criticisms, surely, that the substance of Fowler's review (see above) belongs.

2. I.e., the analyses which would avoid partial fusion (see 6.4.1, p. 69) and avoid extended and overlapping exponence (see 6.4.2, pp. 93ff) in the case of the Perfective.

3. Again, see Chomsky's remarks on this point (1962: n. 25; also in discussion of Smith's paper in the same volume).
clear in which direction the motivation might have led? It is true, as we have said, that the dominant methodology would have tended to encourage the dominant model; conversely, however, the model itself suggested a hypothesis for speech-recognition (see ch. 4 et passim) which, in turn, would insinuate that the methodology might be viable. But the very unclarity of these notions should, perhaps, give particular cause for suspicion. In the present context, the morpheme of the 1940s might well be seen as a compromise between what was operationally feasible (granted, that is, that the objectivity of the analysis was conceived in post-Bloomfieldian terms) and what might, by other criteria, have been materially satisfactory. On the one hand, the emphasis on procedure pulled one towards a morpheme which was ‘visible’ or palpably identifiable in some sense; this is more true the more the procedure was established rigorously on a phonetic basis. On the other hand, it could not be equated with a particular phoneme-sequence as such; to accept that would be to deny, for example, that the u of *momo* and the s of *rekept* reflect the same syntactic element. The way out, obviously, was to say that these are merely the ‘shapes’ which the element assumes in individual instances. In other words, the morpheme itself is not actually ‘composed of’ phonemes; but it can nevertheless be isolated or pinpointed, in any specific context, in terms of a pseudo-unit which is. If the allomorph concept did indeed originate in such a compromise – and if the procedural approach has since turned out to be largely fruitless – we should surely take a very careful look at its continuing popularity.

Finally, one wonders how much may be due merely to a preoccupation with units as such. Let us suppose, for the sake of argument, that we take the concept of linguistic levels, and in particular the levels of phonology and grammar, as in some way ‘given’. In that case, there would be no significance whatever if we then took the term ‘phoneme’ as a cover term for ‘all unstructured elements in phonology’, and likewise ‘morpheme’ as a cover term for ‘any unstructured element in grammar’; in particular, we would not imply that all so-called ‘phonemes’ or all so-called ‘morphemes’ had any further theoretical properties in common.

There might, indeed, be two quite different sorts of phonological primitive (as in the theory of the Firthian school), or at least two different primitives in grammar (as in the sketch of Word and Paradigm). But now let us suppose, by contrast, that we take the definitions the other way round. Let us suppose, in other words, that we merely take ‘phonology’ to mean ‘that level in which we are talking in terms of phonemes’ and ‘grammar’ ‘that level in which we are talking in terms of morphemes’, and that, in general, we take the notion of level a versus level b to rest on the prior distinction of unit a on the one hand and unit b on the other. In that case, questions concerning the properties of the two fundamental units (a and b, phoneme and morpheme) become quite crucial. On what basis, for example, are an inflectional element (3rd Person Singular), a radical (fer-) and a prefixal element in, say, refer to be regarded as instances of the same unit ‘morpheme’, whereas the r, e, f, etc. are instances of a quite different unit ‘phoneme’? What is it, one has to discover, that distinguishes these as entirely different sorts of construct?

The present writer would not, of course, propose either that units should be ‘defined’ (in the strict sense) in terms of levels, or vice versa. But the point is that the second of these two approaches (the unit-based approach, let us say) appears to have been accepted by Bloomfield and by the entire generation of American linguists who followed him. Consider, for example, the feature of language which is normally referred to as that of ‘double articulation’. All that we need show, to establish this point, is that languages exhibit a dual system – or two distinct modes – of syntagmatic and paradigmatic organisation: a phonological mode,

---

3. A common and revealing metaphor; cf., e.g., Bloch, 1947 (= RIL, 244ff.) and, for a striking textbook example, Hockett, 1958: 132 et passim.
4. For the morph as a pseudo-unit see the discussion by Hockett (1961) cited in ch. 10, p. 208, n. 2.
where the organisation is concerned with sound-patterns in some sense, and a grammatical mode, where it is essentially ‘formal’ in character. One needs, in other words, to distinguish the levels themselves in terms of the total linguistic theory. But the fact is that many linguists have seen the phenomenon of double articulation as resting precisely on the minimal units which are said to be involved. The cornerstone of a linguistic model is not, as it were, that there are two sorts of ‘articulation’ as such, but simply that there are two sorts of primitive element, one of which (the morpheme) is in some sense to be regarded as a unit of meaning, whereas the other (the phone) merely serves to differentiate one morpheme from another. The implications are obvious in the light of what we have already said. On the one hand, the central task in phonology is evidently that of ‘defining the phone’ – of hypothesising certain properties of an utterance which the phone (and only the phone) is set up to handle. Likewise, the essential first step in grammar is that of ‘defining the morpheme’; to be precise, one must try to postulate certain further properties of utterances (the recurrent pairing of sound-partials with meaning-partials was, of course, the first to be mooted) which, again, must be handled by constructs of a quite distinctive kind. Only then can grammatical theory as a whole rest on a sufficiently sound basis.

In this way, the two minimal units are literally made the foundation, qua minimal units, of the overall model of language. Moreover there is no doubt that if one looks for a morpheme of this kind (if one assumes that the fundamental unit has, as it were, to be there), then one is quite liable to succeed in finding it. But the question is: was there any better reason why just one type of grammatical element should have been postulated?

7.3 Simplicity versus appropriateness

The topics in the preceding section have been introduced, in effect, in order that we may successfully discount them. On the one hand, it would be misleading to suppose that preoccupations of this kind are still relevant; neither the attempt to model morphology on phonology, nor the emphasis on identifying units in terms of discovery procedures, nor even the unit-based approach in its most general form, exercise the compelling influence which they have sometimes exercised in the past. One should accordingly beware of supposing that because the morpheme represented the best solution in the 1940s, therefore it will still represent the best solution today. But equally, of course, one should not condemn a theory merely because its origins now appear to be partly dubious. The continued predominance of IA may have been helped by a certain inertia; naturally not all linguists would wish to submit an established – and crudely workable – model to an entirely fresh examination. On the other hand, it may also be due to the importance attached to real and permanent advantages.

Of these advantages the most obvious, as we have pointed out in ch. 4, are those of simplicity. Let us therefore begin with a recapitulation of the main points which are involved.

I. The first is, of course, the extreme simplicity of the grammatical representations. If our example *ferr* is represented in Word and Paradigm terms thus:

’T The Passive Infinitive of *fero’; we have an expression of the general form:

‘The $x_1$, ..., $x_n$ of $y$’

where the relationship (‘the $x_i$ of $y$’) is formulated in terms of two primitive classes of elements: that exemplified by Passive and Infinitive on the one hand, and that exemplified by *fero* on the other. Moreover the word as a whole is an instance of yet a third unit, which enters into a quite different sort of relationship with other words and with wider syntags within the sentence. By contrast, the Item and Arrangement representation:

*fer*- + Infinitive + Passive

involves, as we have seen, only one type of unit and a relationship (that of sequence) which will play precisely the same role in the formulation of sentence-structure. It followed that the word (as a special type of complex unit) could also be eliminated from grammatical theory.

II. The second, and most important, concerns the statement of dependence. In a Word and Paradigm solution we are obliged, as we have
said, to introduce an extra level of syntagmatic organisation: namely, the level at which one specifies the actual ordering of the various formative positions. Thus we were unable to interpret the specific rules for monadic (rules (20), (23) and (24)) unless, between the grammatical representation, in which Perfective, etc. are unordered, and the phonological representation, which would be ordered only in terms of phonemes, we could insert our structural formula:

Root (First Suffix) (Second Suffix) Termination,

which supplies the sequence in which mon, u and i; are to be taken. By contrast, the statement of allomorph forms an ‘interlevel’ of the simplest possible kind. The model specifies a unified level of syntactic organisation on the one hand, and a single level of phonological organisation on the other, and the scope of a morphological statement is merely that of coupling a term (or sequence of terms) in the former with a corresponding sequence in the latter. The ordering of sequences in the word-form will then follow directly, as we have seen, from the ordering of the syntactic terms themselves.

III. Finally, we must consider the possibility, at least, of some residual ‘isomorphism’ with phonology. In phonology all models rely, to some degree, on a concept of sequence: since one’s purpose is to account directly for phonetic data, and the data varies continuously along the time-dimension, it is natural for the analysis to display a linearity which will match the linearity of the data wherever convenient. It follows that the conceptual simplicity of a theory will be enhanced if, as in the case of Item and Arrangement, the same linearity is exhibited at the grammatical level as well. Just as, for example, the m of monut: would enter into the classical relationships shown in fig. 7.1:

\[
\begin{align*}
& t \\
& m + o + n + u + i \\
& s \\
& \vdots \\
\end{align*}
\]

Fig. 7.1

with the sequence related closely to the time-dimension via first the allomorph- and then the allophone-relations. Granted there is no reason for requiring that such a parallel must be a feature of the theory: but if it is a feature (as is the case with Item and Arrangement morphology in conjunction with the phonemic model of phonology), then the simplification is not, in itself, to be despised. By contrast, it is of the essence of Word and Paradigm morphology that the axes of fig. 7.2 should be rejected.

Of these three points it is the second, as we have said, which is the most important. Point I is bound to be weaker because, of its very nature, it is concerned solely with the properties of the theory itself. Whichever type of grammatical representation we adopt, the number of specific descriptive constructs (thus Passive, Infinitive and the Verbal element in the example cited) will remain the same. Point II, on the other hand, is plainly concerned both with the properties of the theory and with the properties of the individual descriptions. Consider, again, the illustration which we have taken for discussion: it is true, first of all, that the Word and Paradigm model is complicated by an additional level, but it is also true that the grammar would involve an extra rule (the rule which specifies the inflectional structure), or at least an extra group of descriptive constructs (First Suffix, Termination, etc.), over and above the ones

---

1 See pp. 107-8, above.
2 The overall neatness of this scheme is particularly clear in the passages from Hockett (1955 and 1958: ch. 16) cited earlier (p. 54, n. 4) for his use of the term ‘morphophonemics’. For a more precise distinction between ‘inter-level’ and ‘intra-level’ statement see, however, Wells, 1951: 562. Compare the loosely ‘bridging’ role for related use of the term ‘interlevel’ for statements linking the primary levels of ‘form’ and ‘substance’ (1961: 244, 254).
3 With sequential or syntagmatic relations, in praesentia, to o, n, etc. and paradigmatic or substitution relations, in absentia, to t, s, etc.), I so the 1st Person Singular morpheme, let us say, would contract the entirely comparable relationships shown in fig. 7.2:

\[
\begin{align*}
& \text{1st Plural} \\
& \text{MON} + \text{Perfective} + \text{1st Singular} \\
& \text{2nd Singular} \\
& \vdots \\
\end{align*}
\]

Fig. 7.2

with the sequence related closely to the time-dimension via first the allomorph- and then the allophone-relations. Granted there is no reason for requiring that such a parallel must be a feature of the theory: but if it is a feature (as is the case with Item and Arrangement morphology in conjunction with the phonemic model of phonology), then the simplification is not, in itself, to be despised. By contrast, it is of the essence of Word and Paradigm morphology that the axes of fig. 7.2 should be rejected.

Of these three points it is the second, as we have said, which is the most important. Point I is bound to be weaker because, of its very nature, it is concerned solely with the properties of the theory itself. Whichever type of grammatical representation we adopt, the number of specific descriptive constructs (thus Passive, Infinitive and the Verbal element in the example cited) will remain the same. Point II, on the other hand, is plainly concerned both with the properties of the theory and with the properties of the individual descriptions. Consider, again, the illustration which we have taken for discussion: it is true, first of all, that the Word and Paradigm model is complicated by an additional level, but it is also true that the grammar would involve an extra rule (the rule which specifies the inflectional structure), or at least an extra group of descriptive constructs (First Suffix, Termination, etc.), over and above the ones

---

1 See the recent textbook exposition by Lyons, 1968: 73f.; for earlier sources see Saussure (1916) and Hjelmslev, 1943 (=1953: 22-4). In connection with these references it should, however, be stressed that Saussure’s term ‘associatif’ referred to relations in absentia in general (171, 173-5); it is not until Hjelmslev’s first contribution (1958 (=1959: 153)) that this new term ‘paradigmatique’ becomes linked to the substitution or commutation test. For remarks on the differences between Saussure and Hjelmslev see further Siertsema, 1965: 18-20, and on the ‘psychological’ aspect Lepschy, 1970: 51f.
2 In particular, once the preoccupations discussed in the preceding section (7.2) have been discounted.
which are required for Item and Arrangement. Of course it does not
follow that point I is valueless; as we pointed out in Part I, the simplicity
of the model is one of the various sorts of simplicity which we can reason-
ably consider. But it does follow that point II is initially the better
argument. Moreover if the counter-arguments are sufficient to justify an
independent inflectional structure, then a plea which is based on the
grammatical representations alone is likely to founder also.

Likewise, in part, in the case of point III: the achievement of an
’simorphism’, as such, is a matter of theoretical elegance rather than
descriptive economy. But in addition the argument clearly depends on
the precise phonological theory which is accepted. At one extreme, we
have (intonation and other ineluctable suprasegmentals aside) essentially
the model implied by fig. 7.1; in that case the parallel would indeed be
very close. At the other extreme we have, however, the model of the
Firthian school; although the basic STRUCTURE:S SYSTEM dichotomy recalls
the horizontal and vertical axes of our diagram, the abstraction of
PROSODES (or non place-making elements) on the one hand, and the
specific distinction between structural ORDER and temporal SEQUENCE on
the other, recalls (if anything) the characteristics of Word and Paradigm
rather than its opposite. Furthermore, at least one of the arguments for
introducing prosodies (namely, to avoid ‘segmentalising’ an opposition
which is carried phonetically in various places) seems to involve a princi-
ple similar to that which we ourselves have invoked for extended and
overlapping exponents. For these reasons, any isomorphism with
phonology is bound to be disputed from the purely phonological stand-
point.

Point II, then, provides the most telling argument from simplicity.
But the difficulty is that the argument from appropriateness, which we
introduced hand-in-hand with simplicity at the outset of this part, has in
effect been turned round in its face. The suggestion was, as the reader
will recall, that the illiomorph relation would illuminate the process of

1 In Part III we shall, in fact, adopt a solution which does not involve a rule for
inflectional structure as such (p. 115); but the argument still holds, since the rules
for individual formative are themselves complicated. This loss of descriptive
economy is given particular emphasis, in terms of a comparison between the nota-
tions of specific rule-systems, by Verloren van Themaat (unpublished ms).
2 Ch. 3, p. 23.
3 But see below, p. 136, for the application of this argument to one of our compromise
solutions.
4 For the Firthian theory of structures, systems, prosodies, etc. see again the brief
discussion in ch. 10, p. 211. For order versus sequence see the interpretation placed

speech-recognition; the morph, in other words, could be regarded as an
isolable cue or signalling unit in a precise and non-vacuous sense. But
we have since found that the linguistic evidence for this assertion is ex-
tremely weak. The putative signals are apparently subject to merging at
the phonological level (6.2.1); alternatively they may be separated by
what we are forced to regard as perceptually ‘irrelevant’ material (6.3).
In addition, the signals for morpheme a are often quite inadequately
differentiated from those for morpheme b (6.4.1), and proceed to vary
amongst themselves (6.4.2) under conditions which suggest that entire
complexes of morphs ought, instead, to be taken together (6.4.3). We
have thus not merely failed to find an argument FOR the morphemic
concept, but we have (if we have found anything) found one against it.
How much weight, then, should be given to an ‘appropriateness’ factor
of this kind?

The immediate answer is likely to vary considerably from one linguist
to another. In the first place one cannot, at least at the moment, claim
that anything more than ‘appropriateness’, ‘naturalness’, ‘plausibility’,
etc. is at stake. It may be possible, on purely linguistic evidence, to say
that a certain model cannot yield a satisfactory theory of speech-
recognition; thus a theory based on the Item and Arrangement model, if
taken literally in the way suggested at the beginning of this part, will fail
to account for the implications of morphemic ambiguity or overlapping.
But it is obviously not possible, on this type of evidence alone, to confirm
that a theory is satisfactory. The best evidence for hypotheses of speech-
recognition would plainly be evidence of speech-recognition itself; in
particular, there would be little interest in formulating a theory unless
one could anticipate a substantial body of experimental findings (findings,
for example, on the recognition of grammatical elements in the context
of noise, or in the context of syntactic or semantic incongruity) which it
was intended to account for. Furthermore, it is not necessary for such
hypotheses to be based directly on any model which is linguistically
acceptable. It may be, for example, that the most rudimentary segmen-
tation into ‘stem’ and ‘ending’ (say reksisitis into no more than reks-
and -sisis) would yield a better hypothesis than the finer segmentation
which we have taken for granted. But the finer analysis would still be
justified by linguistic criteria.

1 Cf. Ladeoged & Fromkin, 1968: 123-28, for a particularly strong warning within the
Chomskyan framework.
2 E.g. that of descriptive simplicity; compare discussion of exemplary paradigms in
ch. 3, pp. 129.
In the second place, not all linguists would attach the same importance to the criteria which are involved. The arguments which we have given seem to derive essentially from two major principles, which it might perhaps be useful to label and formulate as follows:

I. The principle of minimal representation: This would state that in representing a sentence, word, etc. one should try to establish the minimum of constructs that are necessary to distinguish it from other forms in the language. A special instance of this principle is well known in phonology. In Latin, for example, one would not assign phonemic status to the known lateral variants [l] (‘clear’ l as in loko) and [r] (‘dark’ l as in hultum) when the invariant l-quality (with ‘clearness’ or ‘darkness’ unspecified) is sufficient to distinguish any of the forms which are relevant. But the principle which is under discussion here is more fundamental than that of invariance as such. In particular, we are concerned with the status of sequence or linear arrangement: is this to be excluded from the grammatical representation of the word (as suggested for the Word and Paradigm model) or is it not? The argument for exclusion is, in part, that the sequence is non-distinctive; if we analyse a word such as ferrum: in morphemic terms then the listing of ‘Items’ alone (Ferr-Infinitive, Passive) is quite enough to distinguish it from any other word in the language. Why go on to specify an ‘Arrangement’ when it has no differentiating role?

II. The principle of direct expenence: This would state, crudely, that if two sentences, etc. require different representations at a given level of description, then these differences in representation should be related as directly as possible to any phonetic differences between the sentences concerned. A classic illustration in phonology is supplied by one of the arguments against ‘junctures phonemes’: in describing, let us say, the phonetics of intervocalic ‘m’ in word-initial versus word-final position (thus terra: marik’e ‘by land and sea’ with bilabial closure; regem et ‘the king and...’ with absence of closure and elision) it would be contrary to this principle if one were to establish a ‘phoneme’ corresponding to the word-boundaries (say terra: marik’e against regem et), to say that this in itself had the single ‘allophone’ zero, and to say that the phonetic features in question were related to the word-structure only in that ‘ would give rise, indirectly, to certain allophones of m, s, etc. Likewise, in morphology, there was the argument against a zero allomorph of Past in English took, Perfective in Latin rupit, and the like. But the cases involving zero are again no more than special instances of the wider principle which is at stake. In particular we have asked why, if a pair of words differ as to one grammatical element, and if the phonological differences reside in a whole set of features f1, f2, etc., it should be thought desirable to select just one f as the direct marker of the elements concerned. For example, why should one say that the grammatical contrast between rego and reksi has as its direct exponent no more than the presence or absence of i?

Principles of this sort would, one may assume, be endorsed by most linguists in one form or another. But the difficulty is that each is liable to conflict with other considerations. Thus we ourselves, while giving general assent to principle II, have in fact adopted analyses of our example reksi: in which the voicelessness of the k is related to the Perfective morpheme in a far more indirect fashion than the following suffixes; this suggests that there are other considerations of appropriateness, associated with the notions of sandhi and assimilation, which can override the criterion of direct expenence in such a case. In addition, are the formulations which we have outlined sufficiently precise for our purposes? Since it is desirable that they should apply for descriptive linguistics in general, and not merely to the fields of morpholexics and morphophonemics in particular, it would perhaps be foolish to attempt any further precision within the context of this monograph. But it seems clear that the wording ‘as directly as possible’ (in principle II) might be somewhat improved.

For these reasons we must observe a reasonable caution in assessing the merits of our two models. But given that we have to choose, there do seem to be grounds for a tentative decision in favour of Word and Paradigm. First, we have already elucidated a general preference for appropriateness as opposed to simplicity: for considerations, that is to say, which involve something other than the literal size or formal make-

---

1 For ‘junctures’ as ‘zero phonemes’ see Harris, 1955: ch. 8. For an attempted analysis of junctures in Latin see Hill, 1954 (and briefly in Hill, 1958: 443); compare the applications to English (e.g. Trager & Smith, 1951: 38ff.; Hill, 1958: 21ff.).
2 See again pp. 61ff., above.
3 The reader will recall our argument on pp. 91ff. The directness of the expence-relataion is relevant, of course, to the problem of evaluating descriptions in general – including those which exemplify the same model and rule-systems; see, for example, ch. 12, p. 284, and my later attempt to capture certain aspects in an evaluation-procedure, pp. 31ff.
up of the description. It is surely more important to work out what we should say about specific morphological oppositions, to see how this can best be conveyed by the different analyses, and then to consider what kind of rule, etc. would be necessary, rather than to begin by considering, in a priori or idealistic terms, how fast we can get from one level to the other. Now our most striking finding is that there is often little - apart from the nature of the Item and Arrangement system itself - which would lead one to prefer the analyses which an IA description assigns. An appeal to formal and conceptual simplicity does not seem an adequate reply to this point.

Secondly, the original argument in favour of the morphemic concept was too strong for it to be merely set aside or forgotten. One motive for the post-Bloomfieldian model consisted, that is to say, in a genuinely factual assertion about language: namely, that there is some sort of matching between minimal 'sames' of 'form' (morphs) and 'meaning' (morphemes). Qua factual assertion this has subsequently proved false; for certain languages, such as Latin, the correspondence which was envisaged apparently does not exist. But at this point one cannot just abandon the argument, and fall back on simplicity instead. One is bound to suspect, in the light of such a conclusion, that the model is in some sense wrong.

Finally, we must not neglect the possibility of relevant psychological evidence. Of course we are a bit late to get it for Latin, and the language that we are likely to get it for (nay English) is unfortunately of a different type; in psycholinguistics, as in linguistics, we must avoid assuming that a single theory applies universally. However, there are various extant languages whose character is sufficiently similar; Italian, for example, exhibits a neat instance of ambiguous formative in the Noun and Modern Greek a great deal of affixed interdependence in the Verb. May one not hope, first, that the perception of morphological units (the transition, in other words, from the phonetic to the grammatical articulation of language) will begin to receive some attention in the psycholinguistic field, and secondly, in view of the blatant differences between languages, that the investigation will proceed on a somewhat wider basis than practitioners of this subject have sometimes thought necessary? Certain tests - tests, for example, which might yield positive evidence against the concept of morphemic signalling-units - seem to an outsider at least to be relatively straightforward. It would be helpful to know if the results are, indeed, as the linguistic evidence might lead one to predict.

7.4 Some possible compromises

However, the purely linguistic argument is not over yet. The two models outlined so far in this part are, as we have made clear, extreme models; our first formulation of Item and Arrangement was the most uncompromising that could usefully be envisaged, and the subsequent sketch of Word and Paradigm was designed on the assumption that all our objections in ch. 6 were justified. But what if only some of these objections are accepted? How far can the general framework be varied between these two poles? And how far would a suggested compromise give rise to further problems of evaluation?

Three suggestions seem to be particularly worth considering. Let us take each in turn and examine it from either or both angles, where necessary.

7.4.1 Morphemic components. The first suggestion is one which arises out of the analysis shown by fig. 6.11: namely that some relations between elements should be sequentially ordered and others unordered. Thus, in the particular example under discussion, the cumulative elements were said to be 'simultaneous':

\[
\begin{bmatrix}
\text{1st Singular}
\end{bmatrix}
\]

but this complex as a whole preceded a non-cumulative element:

\[
\begin{bmatrix}
\text{1st Singular} + \text{Passive}
\end{bmatrix}
\]

(1970). Furthermore, where the perception of grammatical structure has been considered at all, e.g. in the computer simulation of certain limited aspects by Thorne and his associates (Thorne, 1968; Thorne et al., 1968), the recognition of the units as such has been largely taken for granted. Of course, Lamb (see p. 46, n. 1, and references to Part i) has proposed a computer simulation on the basis of an IA-type model; but this has not been tested against alternatives.

2 The reader may recall our remarks on p. 61 (on zero Perfective in /a#96/) and on pp. 63f., on pp. 68, and 70 (on fusion) and pp. 82 and 85 (on 'empty morphs', etc.).
3 Compare ch. 3, p. 33. One may hope, however, that genuinely relevant evidence may come from Russian.
4 For Italian see Matthews, 1970b: 107f.; for Modern Greek, Matthews, 1970b.
5 So far, the perception of phonological units has received quite disproportionate attention; this is brought out very clearly, for example, in the recent survey by Fry.
Such analyses would appear to be closer to the morphemic than to the
Word and Paradigm pole; however, they do take care of the objection to
ordering which is most immediately compelling – namely, the potentially
arbitrary choice between one sequence and another. Are there, then, any
special arguments which apply to such a solution?

General arguments will, of course, come readily to mind; as it stands,
this will not meet the wider (though admittedly weaker) objections to
ordering which were crucial to the discussion which we have just
completed. But there is in addition a more specific objection, which
concerns the precise theoretical status of the elements involved.
According to this treatment, do the ‘simultaneous’ 1st and Singular belong to
the same set of primitives as the sequential Passive, the verb *fer* etc.? The
most obvious interpretation is that they do not: 1st and Singular are
‘features’ or MORPHEMIC COMPONENTS, and the sequence relationship
would obtain between Passive and a further ‘morpheme’ (let us call it *X*)
which may be viewed as a ‘bundle’ consisting of the two components
concerned.1 But such a formulation seems rather inconvenient for the
purposes of syntax. The rules would have to handle components and
morphemes in two different ways: thus one type of operation would be
needed to add a constituent Passive to the verbal syntagm, and quite
another to specify that a constituent, at a given point, must have the
feature of ‘Singularity’. There seems no reason to take a purely morpho-
logical distinction, and project it onto the syntax in this way.

From this viewpoint a partly ordered representation would, indeed,
be less satisfactory than either pure ‘Item and Arrangement’ or pure
‘Word and Paradigm’. The former has a defect, one might argue, in that
there is no provision at all for ‘compositional’ as against ‘sequential’
elements; one consequence is that lexical elements such as *fer* and
inflexional elements such as Singular and Passive are lumped together
in the same primitive category. By contrast the latter has the virtue,
perhaps, that it is precisely the lexical versus inflectional distinction
which would be captured; thus Passive, 1st and Singular would all be
‘simultaneous’ elements (‘components’ in a sense) which form the
domain of a relation (‘the...of...’ in our verbal representations)2 with

1 ‘Morphemic component’ is from Harris, 1951a: ch. 17; compare the Latin Nominal
examples in 306–9. Note, however, that we are not concerned with the notion of
‘long components’ (e.g. a component ‘Singular’ which extends over both the Verbal
and the Subject syntagms); the discussion of that would carry us too far into syntactic
topics. For ‘bundles’ of ‘features’ compare, of course, the Jakobsonian theory of
phonology (e.g. Jakobson et al., 1952: §5); but the metaphor is dangerous (cf. p. 146,
below).

2 Pp. 106 or 117.

converse domain *fer*, etc. But now this compromise solution introduces
a quite different dichotomy, which makes no syntactic sense whatever!
However it does appear to make morphological sense, at least within the
limited context in which it is proposed. For that reason the suggestion
should not be lost sight of entirely; perhaps it might be reformulated in
a more satisfactory manner?1

7.4 Some possible compromises

7.4.2 ‘Item and Process’. The next suggestion is the most familiar
of the three. At two points in the survey section we have introduced the
concept of a synchronic ‘process’: most noticeably in the case of *rump/-
*rupe* and the like but also, in an apparently less contentious fashion, in
the treatment of fusion and other sandhi phenomena.2 Let us assume,
for the moment, that the objection to processes is unconvincing3 but, on
the other hand, that the remaining arguments for a Word and Paradigm
description have been rejected. What sort of model would one then be
led to propose?

The answer is usually presented under the heading of ITEM AND
PROCESS (IP).4 In an Item and Process description the grammatical
representations may, once again, take the form of a linear arrangement;
but the individual morphemes of which it is composed will function in
the morphology in one of two quite different ways. On the one hand, at
least one morpheme *per* word (the lexical element if no other; alterna-
tively the lexical element and one or more inflectional morphemes) will
have a basic form, or set of basic forms, as usual: thus in the case of
*rupe* the element *RUMP-* will have an inherent phonological representa-

1 See the discussion of order in transformational syntax, pp. 140ff, below. But what of
other syntactic theories? In the ‘tagmemic’ school, for example, the variable
linearity of the syntagmeme is often stressed (most firmly, for example, by Merrifield
& Stoudt, 1957: n. 3); could one not also speak of tagmemes which occur simul-
taneously? Pike (1947) does not seem, however, to exploit this possibility; nor Long-
acre, even when handling the clear case of the Latin Noun (1964: 20ff.). Merrifield’s
brief study of Kiowa has formulae which are suggestive, but this is really a problem
of ‘superficies’ (1959). In the present case, would a tagmemic be inclined to speak,
instead of two or more classes simultaneously filling the same slot? A similar query
arises for Halliday’s theory (1961); would one have a separation in ‘order’ with
simultaneity in ‘sequence’? – compare p. 130, n. 4?

6.1, p. 60; 6.2.1, p. 71; also 6.3, p. 80. The Frisianic-type process mooted in
6.3. pp. 85f. will be left out of account in the present discussion.

3 I.e. that discussed on pp. 65f.; compare again ch. 3.

4 The term has already been introduced, p. 65, above. As Hockett (1954) observes,
the approach was in a somewhat loose and unformulated state at that time. For
further clarification I have relied partly on Hockett himself, partly on Pike (1967
[see above, p. 72, n. 1]), and partly on Koutsoudas (1963, 1964, 1966) and other
writers in the transformational school. The details of the transformationalist
approach to morphophonemics will, however, be left until later (ch. 10, passim).
tion *rup* and 1st Singular might, let us say, have the conditioned representation *i*. A morpheme such as Perfective, on the other hand, will have a different role to play. In a word such as *hausi* or *karpsi*: ‘I picked’ one might, admittedly, assign a similar conditioned representation *i*; but in the case of *rupi*, *karsi*: ‘I took care’, etc. the element will merely serve as a ‘trigger’ (to use a metaphor which we introduced briefly in 6.1) giving rise to a modification of the form which precedes it. The description will accordingly be completed by a list of operations first at the morpholexical and subsequently at the morphophonemic level. The former, for example the operation:

(27) Replace the short vowel concerned with the corresponding long vowel,

will not apply unless they are activated by the relevant trigger; at the same time, the rule which supplies the activation must also indicate the precise operand (e.g. the radical vowel *a* in *rup*) which is in question. By contrast the latter, for example the rule for reducing *s-* in the case of *hausi*, *kar* will apply in general to any form which is of the requisite phonological shape.

The precise details of this approach will depend, unfortunately, on the answers to two subsidiary questions. The first is the one which we have already postponed in connection with the Word and Paradigm model: namely, how far should the scope of morphological or morphophonological processes be extended? Once again, should we treat Perfective in the case of *hausi* as a trigger for the further operation:

(28) Suffix *s*,

— and similarly for 1st Singular *i*, etc., or should we continue, as above, to establish basic allomorphs on a par with *far, hais, cair*? The second question concerns the boundary between morpholexics and morphophonemics: how clear is the dividing line between one sort of process and the other? It is evident that the operations, as such, are formally similar: one possibility, accordingly, is that certain processes might be selected partly by a ‘trigger’ in the grammatical representations and partly by reference to the basic shape of the word-form. But these queries aside, the general design of an Item and Process description is reasonably clear. Taking the grammatical representation as the starting-

point, an interpretive procedure would begin (see fig. 7.3) by determining the basic allomorph for every element which has one; the rules which will be relevant at this initial stage are similar, of course, to the ones already discussed under ‘Item and Arrangement’. Having done so, the procedure will then delete the morphemes and write the allomorphs in their place; thus the three representations:

\[
\text{rup} + \text{Perfective} + 1\text{st Singular,} \\
\text{hau} + \text{Perfective} + 1\text{st Singular,} \\
\text{car} + \text{Perfective} + 1\text{st Singular,}
\]

would be rewritten (if we may assume a conservative use of processes) in the form:

\[
\text{rup} + \text{Perfective} + *i, \\
\text{hau} + *s + *i, \\
\text{kar} + *s + *i,
\]

with just one of the nine morpheme-tokens remaining. These may be referred to as the first stage in the basic representations.

The second stage of the procedure would then derive the basic representation proper. On the most straightforward view, the rule for Perfective in *rupi* could be cast quite simply in the form:

(29) If Perfective is preceded by a Verb such as *rup-*., then Perfective itself is deleted and the final vowel of the preceding segment is replaced with the corresponding long vowel,

—an Item and Process equivalent, in other words, to the Word and Paradigm rule sketched in 7.1. This will accordingly be applied to the first stage of the basic representation to yield:

\[
\text{rup} + *i
\]

with the exponents of BUMP- and Perfective merged together, in effect, into yet a fourth type of ‘portmanteau’. If, in addition, the affixes were also to be handled by morphological processes, then the procedure would first apply rule (29) — and a similar rule for the *s* in *haus-s-i*; and

1 P. 63.
2 Rule (10), p. 70, above.
3 For further discussion of this question, which can again arise for WP as well, see ch. 10, pp. 208ff.
4 Compare the procedure outlined in ch. 2, p. 13.
5 See the discussion and rules on p. 53.
6 See (35), p. 158. Rule (29) itself may best be formulated as some kind of transformation, with structure index *rup* (and associated morphological class indices) + Perfective and a structural change involving the deletion of the second term and a special type of substitution on the first: compare, e.g., Koutsoudas, 1964: 34–5 (rules for infixation and suppletion) for examples in a non-technical formulation. But Koutsoudas does not clarify the difference, either substantive or formal, between such morphological transformations and those of the more usual syntactic type.
Finally, the third stage would apply the sandhi-rules in the manner already indicated. In cases such as rupsi: and karpsi: there will, of course, be none that are relevant; the latter is already in its actual form by the first stage of the basic representation, and the latter will reach it at the second stage (see above). It is at this third stage, however, that *haus-s-si: will finally yield hausit. The conversion from a grammatical to a phonological representation will then be complete.

Such an approach is clearly feasible and, in one way or another, has received extensive illustration in the literature. What then of its merits? The advantage over 'Item and Arrangement' is evidently that of greater generality: the first and second stage will handle anything that the allomorph rules can handle while, in addition, the third and second stages will handle a number of things that they cannot. But it is the comparison with 'Word and Paradigm' which is of particular interest at this point. Of the various phenomena which confuse or blur the discreteness of morphs or allomorphs, we can now take care of all but the most recalcitrant. The concepts of sandhi, fusion and so forth can be captured neatly by various sorts of process in morphophonemics, and at the morphological level the most notorious absurdity of Arrangement descriptions can at least be avoided. In addition, the problem of cumulation might again be handled by a separate modification: for example, by introducing morphemic components (a 'bundle' of components acting, if necessary, as a single trigger) in the same way as in the preceding subsection. The question is: how much significance do we attach to the phenomena which remain?

These are, essentially, the phenomena of formative ambiguity, intermorphemic conditioning, and so forth which were discussed in 6.4. Where the word as such is concerned a Process description is admittedly less negative than 'Item and Arrangement': the word-boundaries are morphologically significant (a morpheme in one word would not, one may assume, give rise to an operation affecting another) and, in addition, it is possible to require that only one element per word should be assigned an initial allomorph. But the reasons which are involved would not be those which we ourselves have put forward. The basic insight of 'Word and Paradigm' is the one expressed by the complementary diagrams of

\(^1\) Compare the interpretation of WP rules in 9.3.2.

\(^2\) Compare the root in WP (ch. 9, p. 179).
6.4.1 and 6.4.2: fig. 6.17 (for exponents overlapping within the system) and fig. 6.18 (for syntagmatic overlapping within the word-form). If we want to pursue this insight, then the inflectional structure of the word cannot be partitioned amongst its syntactic elements. If we reject it, then the Item and Process model is simpler and more satisfactory. The choice between simplicity and appropriateness could not now be plainer—assuming, of course, that the appropriateness is genuine!

7.4.3 A sequential treatment of overlapping exponent. Our third suggestion is no more, in effect, than a modification of ‘Word and Paradigm’ in the ‘Arrangement’ direction. Let us suppose that extended and overlapping exponents have been accepted; we may accordingly assume, to take a more complex example, that the form recsiti: ‘You (sg) ruled’ might be analysed as in fig. 7.4:

**Grammatical Representation:**
\[
\text{REG-} + \text{Perfective} + \text{2nd} + \text{Singular}
\]

**Phonological Representation:**
\[
\text{recsiti} + s + is + \text{št}
\]

Fig. 7.4

**Grammatical Representation:**
\[
\text{REG-} + \text{2nd} + \text{Singular} + \text{Perfective}
\]

**Phonological Representation:**
\[
\text{recsiti} + s + is + \text{št}
\]

Fig. 7.5

with extended exponents of Perfective overlapping the cumulative exponent of 2nd and Singular. But it is helpful for this purpose that the grammatical representation should be unordered? If we diagram the elements differently, say with Perfective between or after 2nd and Singular, the analysis is immediately less perspicuous; thus in fig. 7.5 the arrows cross in a quite inelegant way. Admittedly, we are merely speaking in terms of diagrams. But by comparing these analyses have we not found support, if anything, for the introduction of ordering at the syntactic level?

The answer, of course, is that we have not—not yet at least. Granted, the sequence Perfective + 2nd + Singular would be better than 2nd + Singular + Perfective; but it does not follow that either, as a sequence, is better than no sequence at all. We would beg the question, in brief, if we assumed that figs. 7.4 and 7.5 were not simply equivalent! Nevertheless, the retention of ordering may still be considered as a formal possibility. In attempting to formulate the Word and Paradigm model, we assumed (see 7.1, point 1) that the grammatical representation would be of the known traditional type: namely, with no specific connections between the inflectional elements. The Word and Paradigm rules, for example rules (21/23) and (22/24), would accordingly refer to whichever subset of these elements was appropriate. But if such a representation is rejected, then there is no reason why they should not refer to sub-sequences instead. Let us suppose, for example, that the grammatical representation of recsiti: is indeed of the form implied by fig. 7.4. Then a rule which we may cast as follows:

(32) The Termination \( t i \) is selected if the grammatical representation includes the subsequence Perfective + 2nd + Singular,

if taken in conjunction with the rules for \( s \) and \( is \), would assign an analysis which is otherwise identical to one in which the sequencing has been eliminated. Are there good reasons, in that case, for preferring the traditional model?

On the purely morphological side, the arguments appear to be somewhat inconclusive. Let us begin by considering three successive factual hypotheses. The first is that extended exponents are always adjacent; to be precise, if \( m \) is an inflectional morpheme, and \( s_1 \) and \( s_2 \) are segments associated with \( m \) in some word-form \( w \), then a subsequence \( s_1 + s_2 \) in \( w \) implies that \( m \) is also associated with \( s_2 \). Thus fig. 7.4, to take the same example, shows a group of three contiguous exponents of Perfective; the hypothesis would be falsified if, between \( s \) and \( is \) or \( s \) and \( ti \), there had appeared some further segment whose assignment was entirely different.  

1 P. 106.

2 Much of the discussion which follows derives ultimately from an exchange of correspondence with J. Lyons concerning my analysis of Modern Greeks (Matthews, 1969); however, I have no doubt that Lyons himself would give a different emphasis.

3 This restriction, at least, would appear to be implied by the discussion in Pike, 1967, where overlapping of this kind is treated under the general heading of ‘fusion’; see our previous reference in p. 93, n. 3.

4 For fig. 6.17 see p. 91; for fig. 6.18, p. 94.
The second is that the exponents of one morpheme will never be completely surrounded by exponents of another; to be precise, there can be no sequence ́s₁ ... ́s₂ ... ́s₉, where ́s₂ is associated with m₁ and m₂, and both ́s₁ and ́s₉ are associated only with m₁. Thus in fig. 7.4 the exponents of Perfective extend before but not after the exponent of 2nd and Singular; the hypothesis would however be falsified if, let us say, the segments ́ni and ́ni were to appear in reverse order.

The third is that a syntactic category will always be marked at the same relative position in word-structure; to be precise if, in some word ́w, the exponents of m₁ are later than those of m₂, then there will be no further word ́w', in which for some x, y (x and y = m₁ and m₂ or a further pair from the same syntactic classes), the exponents of y are later than those of x. In fig. 7.4, once again, the segments associated with Perfective

**Grammatical Representation:**

\[ X + 3rd + Singular + Perfective \]

**Phonological Representation:**

Fig. 7.6

form a sequence ́s + ́is + ́ti, whose first two members precede the segment associated with 2nd and Singular; the hypothesis would then be falsified if, for example, some further Verb-form had the analysis shown in fig. 7.6 where ́3rd belongs to the same class as 2nd, but the segment ́y, which is associated both with 3rd + Singular and with Perfective, comes before ́s + ́is instead of after.¹

These three hypotheses (assuming, for the moment, that they are true) could be incorporated without formal difficulty into either model; the formulations which we have just given say nothing about the relationship of the actual morphemes. But would restrictions of this kind be ‘natural’ or ‘explicable’ unless the relation was, in fact, sequential? Let us suppose that there are independent syntactic grounds for insisting on the construction:

Perfective + Person + Number.

¹ A weaker version could, of course, be stated in terms of individual morphemes rather than morpheme-classes; i.e. the hypothesis would be falsified by fig. 7.6 as things stand, but not so if all 3rd sg Perfectives had the same pattern. But both could be falsified (as below).

If that were so, then the restriction on exponents would be precisely what we would expect: the structure of the language would be to that extent perverse or arbitrary if, alongside ́rekskisti: (fig. 7.4), one were also to find examples which displayed the morphological analysis of fig. 7.6. But now let us suppose, instead, that there are grounds for requiring that these elements should not be ordered: in other words, that both the relevant constructions (Perfective + Person + Number; Person + Number + Perfective) will lead to comparable syntactic difficulties. In that case, what would be the motive, within the system of the language, for the type of morphological restriction which we have postulated? Surely one would expect cumulation, or alternatively one would expect the segments to display quite random relations; what is the explanation if, in fact, there is a regular sequential pattern?

**Grammatical Representation:**

\[ \text{curr} + \text{Perfective} + 2nd + \text{Singular} \]

**Phonological Representation:**

Fig. 7.7

This argument is interesting and may, to some readers, seem convincing. However, it is open to query from both the factual and the theoretical angle. In the first place, the restrictions as we have stated them are not strictly true; with a reduplicated Perfect, such as ́kukurrasti: ‘You ran’, we would presumably establish the analysis shown in fig. 7.7, where it is evident that all three hypotheses are falsified. Of course, it is possible to weaken the restrictions in such a way that this example can be accommodated: in particular, we could stipulate that lexical morphemes (such as curr-) should be left out of account. But by doing so one will also weaken the argument as a whole. What is there in the nature of lexical morphemes which would explain exceptions of this kind? The Word and Paradigm model will readily suggest an answer: just as curr- would belong to a distinct class of primitives in the grammatical representation: The 2nd Person Singular ...... of curr-,

so the root ́kurr, as a nucleus for the accretion of inflectional formatives, plays a distinct role in the morphology.¹ But why should it be so if the grammatical representation were a straightforward sequence?

¹ For details see again 9.1 and 9.3.1.
Secondly, one cannot ignore the contrary arguments which were suggested earlier. In 7.3 we distinguished two underlying principles; those of ‘direct exponence’ (principle II) and of ‘minimal representation’ (principle I). We also distinguished three points of simplicity; conceptual economy in the grammatical representation (Simplicity point I), avoidance or inclusion of inflectional structure (point II), and potential isomorphism with phonology (point III). It is, as we have pointed out, the total confrontation of appropriateness principles on the one hand, and features of economy on the other, which decides the issue between ‘Item and Arrangement’ (or ‘Item and Process’) and the opposite extreme. But by introducing this compromise model we have clearly split the argument into two parts. On the one hand, the implications of principle II are accepted (as for ‘Word and Paradigm’) despite the most important of the three arguments from simplicity; to allow overlapping exponence in *rekaitis* we would again be obliged, for example, to supply the inflectional structure implied by ‘Termination’ in rule (30). Principle I, on the other hand, is implicitly overruled; the grammatical representations are at least partly sequential (of course, we may also import morphemic components if we like), despite the fact that sequence is non-contrastive. It is the arguments against principle I, or against following principle I in this instance, which are therefore of crucial importance.

In part, of course, these are simply the residual pleas of simplicity; essentially point I and (if the phonology permits) point III. But these are both inherently weak; as we pointed out before, they are concerned with the conceptual elegance of the model and do not affect the practical economy of the description. Furthermore point III can scarcely be valid if, as here, we reject the allomorph-relation; in such circumstances the arrangement of morphemes, unlike the postulated arrangement of phonemes, will no longer be matched with temporal sequence in the way which was originally supposed. How strong, then, is the entirely new argument which we have introduced in this subsection? What we are saying, if we cleave to principle I, is that since ordering cannot have a syntactic and semantic role, then regularities of order must be purely morphological; their explanation must accordingly be sought beneath the level of grammatical elements as such. By contrast, what we were saying above is that since these regularities exist the ordering must be syntactically significant. What we must then explain is why the exponents do, in fact, show a tendency to overlap. On the face of it, these arguments have simply got hold of different ends of the stick; where the first succeeds the second raises a problem, and *vice versa*. Is there any way, in that case, of deciding which is the more appropriate interpretation of the facts?

The answer, if there is one, might perhaps be found in the theory of syntax. In 6.2.2 we pointed out that there might, conceivably, be a syntactic criterion for the ordering of Person and Number; if so an entirely sequential representation would be more appropriate than the inflections themselves suggest. In 7.4.1, again, we argued that occasional morphemic components might be awkward; the syntax, in other words, would incline one towards a representation which was either wholly sequential or wholly non-sequential. Finally we referred, at the outset of this part, to the formal problems which unordered relationships might raise. Can we now cap this line of argument—e.g. by showing that pure ‘Word and Paradigm’ is syntactically inappropriate? Alternatively, can we show the reverse? If either point were established then, for the models which we are comparing in this subsection, there appears to be no morphological argument which would stand up against it.

Let us consider the extreme possibilities. Perhaps the most interesting line of argument against the Word and Paradigm model would be to show that the specific ordering is syntactically or semantically motivated: to show, in other words, that the particular sequence which is established by the inflections (Root, as a whole, before the exponents of Perfective; Aspect, etc. before Person and Number; Person and Number, as in *fer-t-or*, before Passive) can be explained, in part, by the semantic or syntactic properties of the categories concerned. In a sentence such as *senatorum advenerunt* ‘The senators will have arrived’, the Perfective Aspect and Future Tense may be regarded as semantically inherent features of the Verbal piece. The so-called ‘deep structure’, to use a term which has lately become widespread in syntactic theory, might be

---

1 Namely, in an extension of our earlier arguments concerning the *in rekasit*; pp. 109–12, above.
2 I.e. an explanation in terms of morpholexical fusion; but see again p. 93, n. 3.
3 P. 75.
4 See above, pp. 126f.
of Adverbial prefixes which appear either initially or between the normal position for ‘Tense’ and the earlier position for ‘Person’.1 Translated into Latin terms, this is precisely as if the form for ‘[They] will have arrived’ were to display not the ordering of elements ad-vene-er-un (with the prepositional prefix, ad, adjacent to the root, and Person/Number marking, etc. as above), but the order ad-er-un-vene, unt-ad-er-ven, or the like. The feature under discussion is not, then, universal; is it any more, in fact, than a historical feature of Indo-European and certain other families? If not, then surely nothing can be built upon it.

If valid this would (as we said) provide an interesting argument against ‘Word and Paradigm’. What now of the possible case in its favour? The obvious approach would be to show that order is syntactically inconvenient; i.e. that certain rules would be complex or unnatural if the sequencing, as well as the simple presence or absence of morphemes, were to be handled. Consider, in particular, the position of the Person/Number and Passive suffixes. In a sentence of the type statua in urbem ferribatur ‘The statue was being brought into the city’, a Nominal constituent (statua ‘The statue’) is the Subject specifically of a Passive Verb (ferribatur); it follows that the Passive morpheme, though not necessarily a ‘deep’ element of the Verb in the sense which was implied earlier,2 will nevertheless be part of the representation on which agreement or concord rules will operate. But in the morphology it is the exponent of Passive (ur), and not that of 3rd Singular (t), which appears in final position; surely the rule for ‘adding’ 3rd and Singular to the Verbal representation will be more complex (compare again the discussion earlier in this part) if, instead of merely adding them, it must also place them in a position following Tense, etc.?

The difficulty here is that one cannot be certain how strong an argument we have got. The best case of all would be to show that the adding and the placing would require two separate operations; one operation, in other words, which would add 3rd and Singular to the Verbal constit-

---

1 For ‘deep’ versus ‘surface’ see Chomsky, 1965: 16; textbook presentation in Lyons, 1968: 247ff. The precise treatment of Aspect and Tense varies from one account of ‘deep structure’ to another; for example, Fillmore (1966: 224ff; also 1968) takes them out of the Verb and indeed the nuclear sentence ‘proposition’ altogether, and Lyons (1968: 244; recently confirmed privately) has Aspect as a category of the Predicate but Tense, again, as a category of the sentence. However, the argument in this paragraph could presumably be restated for these theories also.

2 6.2.2, loc. cit. For a recent defence of this treatment of concord see Lyons, 1968: 249ff; also 239ff; compare the earlier transformationalist formulations of Postal (1968a; compare 1964a for concord within Noun-phrases) and Koutoudia (1966: ch. 5). The present writer must, however, confess to misgivings; I would certainly hesitate to apply a term such as ‘incorrect’ (Lyons, 1968: 241f.) to all alternative treatments.

3 The argument in this paragraph may be profitably compared with that of Bazell, 1953: 69ff. But again there is the problem of non-universality (see below).
tuent as a whole, and then another which would reorder the elements into their ‘surface’ position. If so, why bother with the second stage? Why not simply ignore the ordering and let the morphology take care of it? However, it seems unlikely that such a clear-cut argument could be substantiated; it may be that the rule required for the unordered representation:

(31) Add to the set of elements in the Verb a pair of Person and Number elements which match the characteristics of the Subject, would be only marginally simpler than, let us say, a corresponding rule:

(32) Add to the end of the Verb, in a position preceding the Passive morpheme where necessary, . . .

—which would be appropriate if it was ordered. The answer is frankly not clear in the present state of our investigations; there are both theoretical and descriptive problems which need to be explored.1

For this reason, it would perhaps be unwise to press any detailed syntactic argument too far. But there appears to be another, more general, argument which would point to the same conclusion. According to the view of language which we have adopted in this discussion, a particular sentence is characterised by a series of related representations:2 thus the phonological representation, ‘surface’ grammatical representation, and so on. In particular, we have now taken for granted that there is more than one representation (‘deep’, ‘surface’ and various possible intermediaries) within the syntax itself. But in that case do we have to discuss the question of ordering from the ‘surface’ standpoint? Let us suppose, for the sake of argument, that there are grounds for establishing ‘deeper’ representations as unordered: e.g. for establishing at least one representation of senatores adveneunt in which elements

1 Briefly, there are two problems: (a) Can a transformational rule, since there are essentially transformations that we are concerned with, refer in its structural description both to a larger constituent (e.g. Verb) and also to the smaller constituents (e.g. the Passive morpheme) of which it is composed? The standard expositions indicate that it cannot (Chomsky, 1965: 197ff.; Bach, 1964: ch. 4; Kostousadas, 1966: 23ff.; Lyons, 1968: 258ff), but, as Hays for example points out (1967: 153), this is a restriction which is not always observed. (b) As a descriptive matter, do we want to refer to the ‘Verb’ constituent in the particular rule concerned? If the answer to (a) is No, then rule (32) will be illegal; hence, if the answer to (b) is Yes, we will have to formulate two distinct transformations, with the first deriving the structures which meet the structural description of the second. On the other hand, if the answer to (b) is No then, regardless of (a), we would manage with one rule which is variously more complex than (31). Unfortunately, the answers are not clear; see also p. 145, below, for what may be yet another complication.

2 Compare ch. 2, p. 12.

such as Future and Perfective are not related sequentially to ADVENTO. If so, then we must ask ourselves a rather different question – Not: Does the ‘surface’ syntax, as such, provide a justification for one model or the other? But instead: At what point, between the ‘deep’ and the morphophonemic representations, should the relevant sequence be introduced? Even the weakest support for ‘Word and Paradigm’ might be enhanced if this standpoint were to be adopted.

Let us examine the stages of such an argument in greater detail. The crucial point, of course, is whether the ‘deep’ or ‘deeper’ representations should indeed be unordered; are there independent grounds (independent, that is, of the arguments surrounding the ‘surface’ level) for suggesting that they should? In the past few years the suggestion has, in fact, become an issue in syntactic theory. Various schemes for unordered, or partly ordered, representations have been put forward, and these have been supported by arguments which vary markedly from one linguist to another; at the time of writing one can only say that no clear end to the discussion is in sight. So far as the reasons are concerned there seem, however, to be four major preoccupations which might be useful to distinguish.

I. First the principle of minimal representation (principle I in our discussion) applies with greater force at the ‘deep’ than at the ‘surface’ level. In the latter case one can order the elements in correspondence with their exponents; granted one does not need to, but perhaps one might as well. At the ‘deep’ level, on the other hand, a discrepancy between order and temporal succession is obviously unavoidable; in senatores adveneunt, for example, the suffix unt is the exponent of a pair of elements, 3rd and Plural, which we have eliminated from the ‘inherent’ or ‘deep’ representation of the Verb. The counter-argument for ordering is therefore weaker: merely that it might be simpler to preserve the sequence to the extent that temporal succession is in fact

[Saunjan & Soboleva, 1963, and Saunjan, 1965] are the usual leading references; see also the well argued but unfortunately unpublished suggestion by Householder (1965b: 11ff.), the categorial-type formulation mooted by Lyons in particular (Lyons, 1966: 227ff.), and my subsequent remarks in Matthews, 1967: 145ff. For further references in the transformational framework see p. 143, nn. 1-2. Outside this particular school one may refer to the concept of the semantic level in Lamb, 1966; Gleason, 1964, and to the more specific proposals by Merrifield (1967) which are illustrated by Cowan & Merrifield (1967); see also Chafe’s recent proposals (illustrated by Chafe, 1970, p. 123).

These last references are particularly relevant in the context of point III, below. Finally, see the admittance of ‘systemic grammar’ by Halliday (1966, 1967: init.).
foreshadowed. But will it be foreshadowed all that frequently? More important, do the elements for which it is foreshadowed form a natural class in comparison with the remainder? If not, do we then impose a sequencing on the remaining elements also? Or do we decide, once again, that apparently similar constructs must nevertheless be formalised in quite different ways?

II. Secondly, how does one select the ordering in cases where the ‘surface’ sequence can vary? As we pointed out in an earlier context, a Latin Subject-Verb sentence could appear in either of the forms Subject-Verb (e.g. *senatores ad eum venirent*) or Verb-Subject (*ad eum* venirent *senatores*); but which form would reflect a hypothetical ‘deep’ word-order? According to all conceptions of ‘deep’ syntactic structure, the relation of the Subject to the Verb should be the same for both sentences. But if the relationship had to be sequential, would the rather weak and marginal criteria which are available (e.g. the frequency of one ‘surface’ order as against the other) be enough to determine which sequence should be chosen? This argument obviously requires further development, but if the choice is largely arbitrary then (as in other cases of that kind) one would prefer a theory in which the problem is eliminated.

III. The third preoccupation concerns the relationship between ‘deep’ syntax and semantics. For various reasons there is at least an ‘intimate connection’ between the two; in particular one group of semanticists have suggested that the ‘deep’ representation of a sentence should alone suffice to distinguish it, so far as its meaning is concerned, from every other sentence in the language. But two questions then arise. First, is there any genuine break between a representation of the ‘meaning’ of a sentence (its semantic interpretation in a sense which is widely current) and its various representations in the syntax? If not, then the ‘deepest’ representation is simply the semantic interpretation itself; surely, at this level at least, there will be no motive for reflecting the ‘surface’ sequence? Secondly, even if there is a definite syntax-semantics division, would the model not be nearer if the ‘deep’ syntax could display ALL AND ONLY (and not merely ALL) the features of the analysis which are relevant to the question of ‘meaning’? This is, of course, a special instance of the principle of minimal representation – but one where its application seems particularly convincing. For semantic purposes we need to know, for example, that *senatores ad eum venirent* involves the association of Perfective Aspect with the Verb *advenire*; surely the most elegant way of expressing this point would be to bring together all such features into a representation, however abstract it may be, from which every other syntactic feature has been excluded? One of the features so excluded would, of course, be the specific sequence relations of Verb + Aspect.

IV. Finally, there is the question of universality. A major object, for many linguists, is that of reducing the substantive differences between languages – of presenting Latin, at some level of analysis, as ‘really’ very similar to English, English as not ‘really’ so different from Navaho, etc. For some elements, a correspondence may of course be established at the ‘surface’ level; thus the role of *et in paxem et kirhenses* is plainly the same as that of and in the translation *bread and circuses*. But for others it can only be established if one is prepared to go much ‘deeper’. ‘Tense’ and ‘Aspect’, for example, are terms which are useful in both Latin and English, but the systems cannot be made identical unless one is prepared to say that the ‘apparent’ or ‘surface’ contrasts (Past/Non-Past, Progressive/Non-Progressive, Perfective/Non-Perfective in the latter; Per-

---

1. Compare Allen, 1956: 161; but in Allen’s example the potentially orderly elements did form a natural class (see below) as against the unordered. For the general argument see Palmer, 1964; and also the special variant presented as Simplicity point III in 7.3, pp. 118f.
2. Compare the argument re morphemic components, pp. 126ff.
3. P. 98.
4. For the charge of arbitrariness compare again ch. 3, p. 28, and ch. 6, p. 74.
5. Lyons, 1968: 269; but one must be careful here, since according to Chomsky’s original formulation (1966: 16; at least) the connection would simply follow from the definition of ‘deep’ and ‘surface’.
6. The leading reference is to Katz & Postal, 1964, and the suggestion is crystallised, though with some reservations, in Chomsky, 1965: 134ff. (but also 224). For a note of caution see the passage cited from Lyons, 1968: 269; in more recent work, Chomsky himself is back-peddling; the ass in question had not been published at the time of writing.

---

1. For ‘semantic interpretation’ see Katz & Fodor, 1963; also the references in p. 142, n. 6, above. The suggestion that the syntax should start directly from semantics rather than ‘deep structure’ first appears (to my knowledge) in Chafe, 1967; see also Chafe, 1970, 1968. For the first published reference within the transformationalist school proper see McCoole, 1968; subsequently cf., e.g., Postal, 1970, and the excellent survey of recent developments by Lyons (in Lyons, 1976: ch. 6). See also Langacker’s textbook (1968: 86ff.); but references to the ‘thought that a speaker wants to express’ (66) are a feature of the popularisation, one hopes, that Langacker would not like his colleagues to take too seriously!
2. The best references are to two papers by Fillmore (1966, 1968) and the relevant chapters of Lyons, 1968 (ch. 7 and 8); see, in particular, Lyons, 1968: 333. But the search for a universal ‘base-component’ is a well-established feature of the generative school; cf. Chomsky, 1955: 117ff., and compare already Lass’s suggestion that two or more languages might share common ‘kernels’ (1975: 405).
3. I.e. the Latin system of Appendix 1; for English cf., e.g., Palmer, 1965.
fective/Imperfective, Present/Past/Future in the former) are merely the reflections of various non-apparent categories (perhaps more similar to English, perhaps more similar to Latin) which form part of the 'deep' syntax in both languages. What, then, if such an approach is accepted? Obviously, at the level at which 'deep' syntax is universal, one would not want to preserve the 'surface' ordering of any particular instance; the whole point is that one has the same system for a language such as Latin (in which the Object tends to precede the Verb, in which the categories of the Verb are 'reflected' mainly by suffixes, etc.), that one has for a language such as English (in which the Object follows the Verb), that one has for a language such as Navaho (in which the categories of the Verb are 'reflected' by prefixes), and so on. In that case, what criterion for ordering can there conceivably be? Just as point I (minimal representation) is enhanced by the relation of 'deep' syntax to semantics (point III), so the argument from arbitrariness (point II) is greatly enhanced if the problem of universals is handled in this way.²

It seems clear from these remarks that points III and IV are particularly important; unfortunately, it has to be confessed that they are also particularly controversial. The present writer, for example, must admit to serious misgivings on both.³ But suppose, as we said, that the case is at least partly convincing; at what point then, in the transition first through the 'surface' syntax and then through the morphology, should the relevant features of order begin to play a part in the analysis? Naturally, where word-order itself is affected (e.g. point II) it would largely be a matter of syntax; a morphological treatment, at any rate, is excluded by definition.⁴ But the handling of elements within the word depends on further decisions which are still uncertain. How near the 'surface', to take one important question, do we want to press the introduction of universals (point IV)? Would it be desirable, for example, to universalise the concord-rules for all languages that have them: e.g. to make the rule for the 3rd Singular Verb in *status in urbe ferrebatur* the same as that for, let us say, Modern Greek *i póli katastráfike* 'The city was destroyed'?⁵ If so, the position of the exponents is an immediate problem; in Modern Greek an exponent of Passive comes before a Subject suffix (as in this example, the *ik* of *katastráfik-e* is the segment in question), whereas in Latin, as we have seen, it is marked after. Would it not, again, be simpler to universalise without order at all? Simpler, that is to say, than to generalise the order in one language (Modern Greek presumably) and supply an extra rule for any grammar which must depart from this pattern? If we did agree to identify such a surface rule across languages, it would seem that our earlier arguments on *status in urbe ferrebatur* would be greatly enhanced.³

Secondly, will the elements of the 'deep' syntax be the same as the ones which we are concerned with? One possibility is that the levels

```
Verb
Motion

1Vth Conj
Perfective
Future
```

will be distinguished by quite different sets of primitives: at the 'deep' level we would not talk, for example, about the Perfective morpheme, but about some underlying conceptual or notional system whose terms are of a fundamentally different kind.⁶ Likewise, let us say, the Singular and Plural morphemes would be quite different constrasts (despite a close correspondence in this instance) from the underlying notional system of Number. In that case, it might be quite natural for the higher representation to be unordered and the lower to be ordered; although the transition has not been studied as thoroughly as one would like,⁷ there seems no

1 Orthographically ἡ πόλις (*the city*) εκκατοστράφης (*was destroyed*); by introducing this example I do not mean to imply, of course, that the role of the Passive in Greek is always identical with that of the Passive in Latin.
2 Following the analysis of Matthews, 1965b, *ik* would be related to Perfective, Past and Passive, ε is a thematic vowel, and the Subject suffix is zero. The analysis in Koutsoudas, 1962, is different, but not in a way which affects our argument.
3 See p. 139.
4 'Conceptual' in the sense of Langacker, 1968 (see p. 143, n. 1): 'notional' in that, e.g. of Lyons (1968). Compare the 'sememes' and 'hypersememes' of Lamb's model, which, since they are at higher strata, belong to quite different vocabularies (see ch. 10, p. 200) from the 'lexemes' of the grammatical level.
5 Though see Cowan & Merrifield, 1968; also the treatment of grammatical exponence implied by Halliday's more recent work (1966, 1967). Both proposals appear, to me, to bear a general similarity to the procedure in morphology suggested by Matthews,

---

¹ See above, p. 139, n. 1.
³ See in particular Matthews, 1972b: 185ff., apropos of universal grammar. But as I remarked at the outset in ch. 1, the field of syntax is at the time of writing very confused.
⁴ But one must not exclude the possibility that some 'stylistic' ordering (but not, say, the ordering of Prepositions or the dominant ordering of Verb and Object) might be handled by rules which refer specifically to the phonology of the word-forms.
clear reason to reject such a formulation. But if the primitives are the same (or at least partly the same) then how precisely would the transition to ordering be accomplished? Where elements are unordered in 'deep' syntax it is customary for them to be formulated as **syntactic features**; in the case of *advenerunt*, for example, one might at some stage have an analysis which would be diagrammed as in fig. 7.8 (i.e. we are concerned with a **Verb**, more specifically a **Verb of Motion**, ..., also a **Verb** in the IVth Conjugation, and so on), where the square brackets enclose a set of properties characterising the word as a whole. How then would we get from this, at the 'deeper' level, to the 'surface' type analysis displayed in fig. 7.9?

![Fig. 7.9](image)

One can hardly say: fig. 7.8 shows a 'bundle' of features; let us therefore take Perfective and Future out of the 'bundle' and put them down, side by side, somewhere else. This would involve a misuse of the metaphor of 'bundles', or a misrepresentation of graphic displays such as figs. 7.8 and 7.9, which seems formally quite improper. But what formulation is one to adopt - given, that is to say, that the 'Perfective' and 'Future' which would usually be called features on one level, are nevertheless the same as the 'Perfective' and 'Future' which appear as morphemes on the next? This is, admittedly, no more than a problem of formalisation; in the present state of syntactic theory it would perhaps be foolish to allow a difficulty of this nature to exclude analyses which are otherwise desirable. But a more interesting question is why one should want, in fact, to introduce the ordering at this stage. If the syntax is basically unordered, then sequence, from the point of view of the 'surface' rules, is obviously a complication. But it is surely a principle in linguistics that complications, of any kind, should be handled at the lowest level which is convenient; in other words, if ordering could be handled either in the morphology or in the 'surface' syntax, then one would choose the former unless there was some good reason to the contrary. This is a principle, at any rate, which has proved fruitful in the past. Is there then any good reason to the contrary? Granted the premise of our present argument (i.e. that elements such as Perfective are unordered in 'deeper' representations), it would seem that the balance of arguments earlier in this subsection would be tipped decisively in favour of the 'Word and Paradigm' solution.

In the light of this discussion, the present writer is inclined to continue with the extreme model which was formulated in 7.1. What is important, however, is that the question of the grammatical representation can be detached (by means of the compromise suggested at the outset of this subsection) and discussed independently of the other issues which dominate this chapter. For the moment, as we said, the debate will continue in the literature; unfortunately there have been too few writers, as yet, who have considered the role of sequence in a sufficiently broad perspective.

### 7.5 Universal and restricted theories

Finally, we have not yet debated what must, for many readers, be the most important objection to the Word and Paradigm model: namely, that it is not suitable for the description of all languages.

A partial application of this principle is made explicit, in particular, by Hockett, 1947 (= *RiL*, 229); but, as Hockett pointed out, it is implicit elsewhere. As with the related principle of minimal representation (see above), I do not know of any linguist who has argued specifically against it - though it may, as I have said, be overruled by other considerations.

Palmer's essay (1944) is a notable, but unfortunately brief exception. See also Lepchey, 1965.

1. A partial application of this principle is made explicit, in particular, by Hockett, 1947 (= *RiL*, 229); but, as Hockett pointed out, it is implicit elsewhere. As with the related principle of minimal representation (see above), I do not know of any linguist who has argued specifically against it - though it may, as I have said, be overruled by other considerations.

2. Palmer's essay (1944) is a notable, but unfortunately brief exception. See also Lepchey, 1965.

3. This point was immediately mentioned by Hockett (1954 (= *RiL*, 386)), with reference to Chinese. Compare much of the earlier discussion of morphology/syntax at the Sixth Congress (Lejeune, ed. 1940; previously referred to in ch. 2, p. 11, n. 2).

4. In his leading article on WP, Rohrbach (1970: 121ff) gives particular attention to this point and discusses, *inter alia*, the problems of taking the word as a basic unit in English. It is essentially his non-universalist conclusion (already referred to in Part I) which I seek to expand on in this subsection.
we will not dispute. First of all, there are many languages in which a
Latin-like 'word' cannot usefully be posited. In the classical isolating
instance, for example, one may if one likes apply the term 'word' to
the minimal grammatical or lexical units; but such a unit has no properly
morphological structure, and does not function in the syntax in the same
way as the words which we are concerned with. The Mayan languages
or Mixtec, again, do not seem to display a viable word-like unit
between the morpheme and the phrase. But even when the word as a
unit is secure, its grammatical character may differ from that of Latin
words as we have described them. In Turkish (where the word is a
phonological reality attested firmly by the incidence of vowel-harmony),
the ordering of elements within it may vary both freely and in accordance
with the meaning; thus, for example, gelir-ler-se or gelir-se-ler 'If
they come'; "Türk-tür-ler 'they are Turkish', "Türk-ler-dir 'they are
the Turks'. Now admittedly one could handle this type of example in
Word and Paradigm terms, but it would seem entirely perverse to do so.
What one has, on the face of it, is an agglutinating system which can best
be handled by establishing differences in 'Arrangements'.

For these and other reasons, let us accept that WP is not universally
applicable. The question at issue, of course, is whether this should be
considered a defect — and not, in the circumstances, a virtue! If IA
provides a Procrustean solution for Latin, and WP a conversely
Procrustean solution for Chinese, Turkish, etc., should we then conclude that BOTH
should be abandoned? Or should we prefer that both be kept? In our
introductory chapters we referred to the various possible aims of a
description and distinguished, in particular, two sorts of general investigation
within which a study such as ours could reasonably contribute. The
first was the investigation of language-universals; it is this aspect which,

1 See, for example, Thompson's discussion of words in Vietnamese (1955: 381); essentially, these seem to be 'lexemes' in the sense of Conklin and others (see ch. 9, p. 161, n. 3).

2 For Mixtec see Pike, 1940. For Mayan I rely on the analysis of Tzotzil by Jacobs & Longacre (1969), which shows considerable permutability of elements or arbitrary
groups of elements within the phrase; see also the constituency-based analysis of the
Tzotzil phrase in Cowan & Merrifield, 1968.

3 Examples taken at random from Lewis's chapter on the Verb (1967: 98, 110). In the
second pair, tür and drx are the same morpheme, disguised by the effects of vowel-
harmony and a devoicing rule.

4 But the precise boundaries of 'WP' and 'non-WP' languages is a matter for decision;
would one want a WP rule-system to allow, for example, for the same formative
to be handled by different operations of suffixation versus prefixation, etc.? I shall
deliberately exclude such identifications of formatives (ch. 9); but I have had several
instructive arguments with J. Lyons on this and similar points.

5 Ch. 3, pp. 31f.
significant universals unless the 'somehow' in point (c) of this assertion can be replaced by a specific and arbitrary model.

But is there in fact a suitable candidate? We have objected to IA in the context of one language at least. We have also agreed that WP, in turn, should be rejected for certain others. How then can we achieve a theory which is both sufficiently specific and of universal application? At this point, there seem to be three directions in which our discussion might proceed. First, we could decide that the investigation is simply not on, i.e. there are no universals at all within the field of our present study. Secondly we could, of course, go back on some of our previous arguments. In justification we would plead that the theory must be universal; we should therefore be prepared to choose whichever of the existing models is the least inappropriate. Finally, we could decide that all our models must, for this reason, be rejected. We must simply hope for some further insight; an insight, that is to say, into a theory which will allow appropriate analyses for every morphological type.

Let us take the second of these alternatives first. If we adopt this view then there are, undoubtedly, additional criteria which would assist our choice. For example, we could determine which model is appropriate for the languages spoken by the greatest number of speakers; alternatively, we could try to consider which, as it were, will give rise to the least inappropriateness or complexity in the case of recalcitrant systems. But the problem is whether the achievement of universality is worth the sacrifice. The most likely candidates are some version of 'Item and Process' (a version, let us say, with allomorph-rules in addition to morphological processes and with morphemic components in addition to morphemes), or, if the inflecting languages are given greater weight, a comparable version of our third compromise (7.4.3). In the former case, the arguments are obvious from what has gone before. By allowing rules for basic allomorphs one would lose nothing so far as the agglutinating languages are concerned; on the other hand, one would fail (accepting, that is, the submissions presented earlier in the chapter) in attempting to capture the true character of an inflecting system. The reader may feel of linguists there is a tendency to neglect such alternatives; see, for example, Moravcsik's just criticism (1967: 224ff) of the papers in Greenberg, ed. 1965. The question here is whether there are any universals of the last and interesting type: I believe not. For an explanation of double articulation see, for example, the brief paragraph in Martinet, 1960: 17-18. For the concept itself see above, p. 115.

7.5 Universal and restricted theories

that the criterion of universality (when added to the criterion of simplicity) is quite sufficient to outweigh the submissions which we have referred to. But Latin will still fit such a model less successfully than, for example, Turkish or English.

In the latter case, by contrast, it would be the agglutinating languages whose descriptions would suffer. The rational of such a choice is obvious and indeed attractive: namely, (a) one will allow for contrasts of order in languages where they appear, and (b) the concept of exponent, being relatively unrestricted, will allow the individual types to be treated as special cases. In particular, a pure agglutinating language is a special case in which there happens to be a one-one correspondence. But the attractions are diminished as soon as one attempts to work out the suggestion in detail. To assign analyses of the more complex type it seems necessary, as we said, to introduce some separate concept of inflectional structure; it follows that even in the simpler cases — the t of fort for example — we cannot write a straightforward rule:

(33) 3rd Singular has the exponent t,

but instead:

(34) 3rd Singular has as its exponent a t in the Termination position, or some statement to that effect. Would this be an appropriate technique for a language where all exponent was of the one-to-one type? The allomorph-rules would seem at the very least to be somewhat simpler.

Similar objections would arise, a fortiori, in the case of the other models which we have discussed. Let us therefore ask again: is universality so important an aim that we can consider it worth this sort of sacrifice? If the universals did (in some sense) exist, then there is no doubt that their discovery should be hailed as an achievement. One must accept that, under certain agreed conditions, it would be meaningful to use a term such as 'discover' in this context. But in the case under discussion all we can say so far is that it is possible to impose a universal model in one way or another. Well so what? There are obviously theories which, by hook or by crook, can be made to apply to all languages. As we

1 Cf. p. 94, n. 3.
2 One may, perhaps, compare Pike's suggestion (1967) that a morpheme of the classical sort should be regarded as a special case within 'matrix theory'. The typological point is, of course, valid - however precisely we choose to make it.
3 Cf. pp. 107-8 and, for the compromise model under discussion, p. 136 and p. 135, rule (30).
4 Compare p. 50, rule (1) in the case of (33); p. 102, rule (19) for (34).
5 Compare the tentative discussion of 'truth' in ch. 3, pp. 35ff.
pointed out in Part 1, the problem of strict adequacy is trivial in the morphological field. The question is whether, for each language, there is a single theory which is always sufficiently appropriate, if appropriate sufficiently simple, etc. It seems hard to say that a model is satisfactory for human language in general, if one can argue that some language taken individually (e.g. Latin in one case or English in another) would be better handled in a different way.

In these circumstances it is natural, perhaps, to turn to the last of our three alternatives, i.e. to assume that the universal model exists but conclude, unfortunately, that it is not amongst the approaches which we have so far had the wit to devise. We should then look (as we remarked) for further insights. But is it likely that progress will come from such an attitude? A genuine theory of universals must apparently have three properties. It must be appropriate and not merely workable for all languages. It must also be maximally restrictive; it must make clear the types of analysis that do not appear as well as allowing for those that do. Finally it should propose a unitary model, in the sense that it is more than the mere summation of various alternatives. This last requirement deserves particular emphasis. It would be quite easy, for example, to assert that a morphological analysis is EITHER of the type suggested earlier in this part (with allomorph-rules applying evenly throughout the structure of the sentence), OR of the type suggested later (with rules constrained within the inflectional structure of the word), and so forth. But this assertion would not amount to a significant theory of universals. For that one must be able to say ‘My theory proposes the same model for each language’, and not merely ‘My theory is so flexible that it allows us to select whichever particular model we please’!

It would, of course, be foolish and arrogant to maintain that these requirements cannot possibly be met. It is conceivable that we have merely failed, as we said, to discover the formal universals of morphology so far. But to suggest that they do not in fact exist (within, that is to say, the conditions of appropriateness which we have set before us) is more reasonable than one might perhaps have preferred to believe. Let us suppose, first of all, that certain implications hold between the features which we examined in ch. 6: for example, that a language which shows extensive patterning of the type exemplified by fig. 6.17 will also be a language without a contrast of order within the word.1 If so, how is one going to meet BOTH the requirement of maximal restrictiveness AND the requirement of a unitary model? If the implications are ignored then one fails to satisfy the former. But if they are not ignored, then one has to say that the presence or absence of one restriction (for example, the absence of a certain restriction on the exponent-relation) is dependent on that of another (for example, the presence of a restriction on grammatical representations). That way lies a theory which is indeed simply an assortment of different or partly different alternatives.

Admittedly we have not considered how far such implications are true. Frankly, it is not the sort of information which it is easy to extract from the published Item and Arrangement descriptions. But leaving this point aside, it still seems possible for the treatment appropriate for one language to be flatly contradictory to the one which is suitable for another. Latin (to put it in morphemic terms) has more than one allomorph of 1st Singular: the o of rego: ‘I rule’, the m of regam ‘I will rule’, and the n of the Perfective reeks ‘I ruled’. Likewise Assamese, to take an agglutinating paradigm, has more than one allomorph of 3rd Person: an ε which appears finally in [tēc] guṭa-b-ε ‘[He neutral] collected’, and an o which only appears in forms such as guṭa-b-ɔ ‘will collect’.2 Now in Latin this is part of a system which also shows extensive overlapping among formatives; since it is nonsense to claim that the morphs are functioning as independent signals, we have suggested, instead, that the exponents of an element should be sought within the word as a whole. Hence the ni, for example, was related directly to Perfective as well as to 1st Singular itself. But the position in Assamese is quite different. First, although the ε/ɔ alternation again appears to be morphological rather than morphophonemic,3 both ε and ɔ are in this case quite unambiguously 3rd Person. Furthermore, this is a language where, as a general rule, paradigms do not show either inflectionally conditioned diversification or syncretism.4 Future, for

---

1 P. 21.
2 In Chomskyan terms, it is not sufficient that the theory should merely achieve 'observational' and 'descriptive' adequacy (cf. ch. 3, p. 20, n. 2).
3 I.e. the diagram showing the different morphemes with exponents i, o, etc. (p. 91).
4 In the terminology of 6.4.2 (p. 92, above).

Assamese forms are from a former student, Mr. D. N. Bhorborah, whose transcription of his own dialect I have also followed; this differs in sufficiently transparent ways from that of Goswami, 1966, which is the only structural monograph on Assamese that I have been able to consult. I need hardly say that if I have made any morphological howlers (through scant acquaintance with the language) it is my own fault entirely.

In particular, no comparable alternation appears with the high vowel as opposed to the mid: thus [tī] guṭa-b-i ‘[You familiar] collected’ and also [tī] guṭa-b-i will collect’.
example, regularly has the basic exponent b; conversely, a b in this position plays no other role in the paradigm. Is it appropriate, therefore, that the facts should be handled in a similar way? Since the system as a whole is different it seems entirely reasonable to accept, for Assamese, the solution which we have rejected for Latin: namely, that the o of gutabo should be related directly to 3rd Person alone, and only incidentally or indirectly to a conditioning morpheme Future.

What we are suggesting, in such a case, is that although the technique which is applied to Latin can (strictly speaking) be applied to Assamese, and vice versa, we should not in fact do so since, on examining the morphology as a whole, it appears that the languages 'work' in largely contrary ways. This may seem strange to linguists brought up in a universalist tradition: but is there, in the end, any real reason why one should expect it to be otherwise? One would be surprised if there were no universals to discover in, let us say, phonology: obvious external factors, in particular the characteristics of the human ear and the human vocal tract, can be expected to restrict all languages in a similar way.

Again one would anticipate that certain features of grammar—both 'substantive' features such as the existence of some means of Number contrast, and 'formal' features such as the availability of some mechanism for coordination—will be found in any system that one studies; but there are many other features for which one would anticipate otherwise. Concord, for example, is not universal; but if we did not know this would we ever expect that it was? It seems easy to imagine a grammar which fulfills the external requirements of human language, and which does not display relations of the concordial kind. Likewise a language could 'manage', as it were, with different systems of morphological exponent. Of course it is these cases where a universal, if genuinely found, is of a particularly interesting and challenging nature. Equally, one should not feel frustrated if the search fails.

In the light of these remarks the concept of a restricted or non-universal theory deserves at least a serious consideration. In linguistics there are, in general, two ways in which it is customary to generalise statements across languages: (a) by prescribing the terms in which a description must be written, and (b) by establishing comparisons be-

1 Note 'basic'; a final m (bilabial nasal) in [əm] guta-tu 'I will collect' is taken to be a sandhi form for [b] (bilabial consonant) + ŋ (nasal vowel). Compare gutabo, etc. and [əm] guta-tũ 'I collected'.
2 Compare ch. 2, p. 8.
3 For 'substantive' and 'formal' universals see again Chomsky, 1965: 28ff.

...tween the descriptions as such. There seems little doubt that both methods are valid and, in addition, that both are required; the problem is to decide in which way any particular generalisation is to be handled. Let us take, for example, the crucial concept of the word. We have agreed, first of all, that it is not universal; accordingly a common view might be that a linguistic theory, in the sense of Part I of this study, should say nothing whatever about it. It is merely something which belongs to the particular languages. But we have also agreed that it is worth discussing in general terms; whatever the situation in Mayan (or indeed Assamese), we want to say that the word in Latin is the same sort of entity as, let us say, the word in Italian. In that case, the normal reaction would be to make a comparative statement of type (b): one would attempt a definition which is based, first, on concepts which do form part of the theory (e.g. that of a constituent or a construction, that of sequence among constituents, and so forth) and, secondly, on configurations of elements, etc. which would or would not be exemplified in any individual grammar. What we are now suggesting, of course, is that the word should be handled by a generalisation of type (a) instead. Its role in inflecting languages would be specified, that is to say, by a theory whose appropriateness does not extend beyond the languages concerned.

This approach is logically feasible. More general statements (e.g. a statement relating the 'word' in Latin to the 'word' in Assamese or English), and further statements of universal application (for example, an overall definition of morphological exponent), could then be handled on the comparative basis. Moreover the recent history of morphology suggests that it is well worth exploring. The word, which was a central concept in the native classical tradition, has been notoriously depreciated in the development of structural linguistics; but according to our arguments its role cannot be captured, within each relevant description, unless the descriptions are constrained by a theory in which 'words'

1 Compare my previous remarks concerning statements in grammars and statements about grammars (Matthews, 1965: init.; 1967a: 15ff.; also briefly in Matthews, 1970a: 186ff.).
2 So, e.g., Halliday, 1961: 252. If Halliday means that the 'word' in language a can in no way be related to the 'word' in language b, then the recent criticisms by Dik (1968: 227) are justified; the fact that a feature is not universal does not mean that it cannot be defined, in general, for those languages that have it. But perhaps Halliday would envisage a comparative definition instead (see also Halliday, 1957: 57 on word-classess).
3 Compare the system of definitions in Matthews, 1965c.
4 It is worth reminding ourselves that in some important works (and recent works; e.g. Chomsky, 1965) the unit is scarcely mentioned.
'clauses', 'sentences', etc. are more than a mere series of units in the same grammatical hierarchy. On a more detailed plane, the universalist investigations since the 1940s have whittled away almost all restrictions on the exponence-relation: but if particular features were to hang together and if, in addition, there were dependencies between the features in morphology (e.g. extensive formative ambiguity) and further features in syntax (e.g. extensive repetition of elements by concordial means), then the typological statements may well be more straightforward if they are based on a number of theories which are individually rich in restrictions, rather than a single theory in which the restrictions are extremely thin. Finally, it has become clear at least that different languages raise quite different problems in morphological analysis. It is therefore possible that they also require quite different sorts of description.

1 Cf. my previous remarks in Matthews, 1956: 142.
2 Cf. Bazell, 1958; compare also Garvin, 1957, though the possible typological value of this point is not explored.

Part III

TOWARDS A GENERATIVE FORMULATION

The linguistic presentation would speak, for example, of base forms...of derived forms... or processes which yield one form out of another. In all these types of presentation, the elements are seen as having histories, so that the relation of an element to sequences which contain it becomes the history of the element as it is subjected to various processes and extensions.

Harris (1951a: 373)