

CONSTRAINTS
ON
ALLOMORPHY
IN
INFLEXION

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Thesis submitted for the degree of
Doctor of Philosophy
of the University of London

July 1981

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CONSTRAINTS ON ALLOMORPHY IN INFLEXION

Andrew Carstairs

ABSTRACT

This thesis is concerned with the search for constraints on the relationship between morphosyntactic properties and their inflexional exponents -- more precisely, constraints on deviation from the maximally simple 'agglutinative' pattern of one exponent to one property and vice versa. Three principal constraints are proposed: the Peripherality Constraint, the Paradigm Economy Hypothesis and the Systematic Homonymy Claim.

The Peripherality Constraint specifies that the realisation of a morphosyntactic property may be 'sensitive to' a property realised more centrally in the word (i.e. closer to the stem) but not to one realised more peripherally, unless it is sensitive in the same way to all the more peripheral properties in the same category.

The Paradigm Economy Hypothesis concerns the upper limit on the number of distinct inflexional paradigms (declension-types or conjugation-types) into which the inflexional resources (affixes, ablaut etc.) of any part of speech in any language may be organised. Given an appropriate definition of 'paradigm', this upper limit is argued to be extremely strict: no more paradigms may occur than are required to put all the inflexions to work. This hypothesis has to be relaxed to permit 'paradigm mixture', but only under narrowly specifiable conditions.

The Systematic Homonymy Claim presupposes a distinction between those homonymies within an inflexional paradigm which are systematic and those which are accidental from the morphological point of view. It is argued that systematic homonymies can occur only under certain morphological conditions, the principal class of systematic homonymies ('syncretisms') being ones where the

morphosyntactic conditioning factors are realised simultaneously with the neutralised properties.

Evidence for these claims is drawn from a number of languages, both Indo-European and non-Indo-European (including Hungarian, Zulu, Turkish, Dyirbal and Fulfulde). Suggestions are made about priorities for future work on the theory of inflexional morphology.

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Acknowledgments

Having been taught linguistics by many people over a number of years at Oxford, London, MIT and London again, I have a problem in deciding who to leave out. I owe a lot to people at all these places. But I would like to thank especially my supervisors at SOAS from 1979 to 1981, Theodora Bynon and Geoffrey Horrocks, who accepted the burden of overseeing work on what probably seemed an eccentric and unpromising topic and who encouraged me unstintingly throughout; and also Paul Kiparsky, who was my adviser during my first attempt to write a PhD thesis on inflexional morphology in 1971-72 and who has made valuable comments on material underlying parts of this one.

I am grateful to several people for advice on particular languages: D.W. Arnott and Mary McIntosh (Fulfulde), Paddy Consideine (Latin), Richard Hudson (Beja), A.K. Irvine (Arabic and Hebrew), R.H. Robins (Yurok), David Rycroft (Zulu) and Peter Sherwood (Hungarian). Naturally they are not responsible for any mistakes, nor for the conclusions which I draw from the relevant facts. Mary McIntosh and David Rycroft have been especially generous with their time and energy in discussing ideas on morphology and criticising drafts of various parts of this thesis. Paddy Consideine kindly read an earlier draft of Chapter VI. Other people who have helped me with comments, criticism or discussion are Waria Amin, C.E. Bazell, Richard Coates, Steve Johnson, Ruth Kempson, Rochelle Lieber, Frans Plank, Martin Prior, N.J. Sims-Williams, Alan Sommerstein, Nigel Vincent and Wolfgang Wurzel.

I have presented papers based on earlier drafts of various chapters (II-V and VIII) at seminars at SOAS, University College London and the University of Sussex, and at Linguistic Association of Great Britain meetings in September 1980 and April 1981. I owe a lot to the discussions which followed these presentations, and also to a series of seminars on morphology run by Dick Hudson at UCL in the summer of 1980.

My flat mates, Allen Morris and Hugh Haward, have put up with a lot of absent-mindedness, moodiness and inattention during my work on this thesis. They have also helped and inspired me in numerous ways, for which I owe them love and gratitude. Finally, I would like to thank the Leverhulme Trust, whose award of a Senior Studentship made it financially possible for me to return to SOAS as a mature graduate student.

CHAPTER I
INTRODUCTION

1.1 Aims

The aim of this thesis is to propose and defend certain generalisations about morphological behaviour. These generalisations are intended to be valid for all languages which exhibit morphological behaviour of the relevant kinds and are therefore, in that sense, claims about linguistic universals. They concern inflexional morphology, and more particularly the relationship between morphological 'expression' and 'content' (or 'signifiant' and 'signifié').

Inflexional morphology has not been a popular topic for linguistic research, at least in the English-speaking world, for several decades. Because of this, I have thought it necessary to devote considerably more space in this introduction to fundamental matters of definition and method than one would expect to see devoted to such matters today in a doctoral thesis on syntactic or phonological theory. And, since the very fact that I have embarked on this topic implies that I believe the recent neglect of inflexional morphology to have been at least partly unjustified or mistaken, it seems appropriate to say something too about the reasons for this neglect and the way in which my present work relates to earlier work on morphology. One purpose of this introduction, then, is to suggest to sceptical readers that there are indeed neglected questions of general linguistic interest worth asking about inflexional morphology; and I hope that the succeeding chapters will reinforce this suggestion, whether or not I succeed in persuading readers of the correctness or plausibility of all the answers I offer to these questions.

In the final chapter I list some topics for future research. I include this list because I regard what I achieve in this thesis as, at best, the laying of some of the foundations for

what I believe will ultimately be an impressive and complex structure, to whose building many linguists will contribute: an adequate theory of morphology. Very probably, some of these foundations will have to be taken up and re-laid. But, in any case, it seems important to indicate what I think the next most pressing questions will be, if the claims made in this thesis are broadly accepted.

1.2 Assumptions and definitions

All languages relate sounds to meanings, and do so partly through attributing significance to the order of meaningful units smaller than the total utterance (or sentence). The first of these remarks is quite banal. The second is somewhat less so; it is not logically necessary that a communication system for use by human beings should be 'articulated' (in Martinet's sense) at two levels, the phonological and the syntactic; but the fact that language is so articulated is one of the few elements of common ground among all serious students of language. It follows that the description of any language will involve a distinction between its phonology on the one hand and what we can loosely call its syntactic-semantic apparatus on the other. By no means all languages, however, display the sort of behaviour which in a traditional grammatical description of Greek or Latin is treated under the heading 'inflexional morphology'. It has, moreover, been notoriously difficult to arrive at a satisfactory general definition of the term 'word', designating the linguistic unit whose internal structure is the subject-matter of morphology. These are two of the reasons why some linguists have not merely neglected morphology as uninteresting but actually denied its existence as a distinct component of grammar altogether.¹ The first assumption that I will make is that this is incorrect, and that in many languages one can identify grammatical units -- 'words' -- with an internal structure which differs more or less from that of sentences and which therefore cannot be described adequately by reference only to the rules of sentence structure or syntax. For arguments to back up this assumption, the sceptic can turn to Peter Matthews's books on morphology (e.g. Matthews

1974: 2-8).

I assume also Matthews's notions of 'morphosyntactic category' and 'morphosyntactic property' (1972^b: 161-162; 1974: 66, 136). Morphosyntactic properties are what inflexions express or realise, such as Masculine Gender, Past Tense or Accusative Case; I regard them as constituting the inflexional 'content' (as opposed to 'expression') referred to in the first paragraph of this introduction. Morphosyntactic categories are classes of contrasting and mutually exclusive morphosyntactic properties, such as, in Latin, Gender, Tense and Case. Each category, together with the properties it contains, is applicable to one or more parts of speech or word-classes. I adopt here, as I do throughout this thesis, Matthews's practice of giving a capital initial letter to the names of morphosyntactic categories and properties. For brevity, I will often omit the word 'morphosyntactic', but all references to categories and properties should be understood as references to morph^osyntactic ones unless I make it plain that I am using these terms in some other way. In particular, I will not use 'category' in the sense of 'word-class' or 'part of speech'.

The set of categories and properties relevant to one language is not necessarily the same as that relevant to the next. This could hardly be otherwise, given that there are 'isolating' languages which have no inflexion at all and consequently no morphosyntactic properties, according to my definition; that is, in an isolating language like Vietnamese, for example, verbal tenses (if they exist) must be purely syntactic or semantic, and cannot be called morphosyntactic. The non-universality of categories and properties, in this sense, is so obvious as to be hardly worth mentioning. But it leads directly to a problem which is far from banal, namely: what are the criteria for identifying the morphosyntactic properties and categories relevant to a given language?

My answer to this question resembles the answer that I

will give to various other fundamental questions of definition. To arrive at a watertight set of criteria would involve discussion of, and decisions about, a number of problems quite far removed from the aim of this thesis, such as the handling of syntactic 'cooccurrence restrictions' in the widest sense (including concord and 'sequence of tenses'), and the distinction between inflexion and derivation. But there are enough clear examples of inflexion, involving morphosyntactic properties that are fairly straightforwardly identifiable, to provide us with a core of material to begin our investigation. Refining the criteria to cope with the more controversial penumbra can wait until we know whether our study of the core material looks like yielding profitable results in the shape of interesting (i.e. readily falsifiable but nevertheless unfalsified) generalisations; and at that stage we can legitimately allow our provisional results to influence our decisions.

Despite the justification just given for doing without a watertight definition of 'morphosyntactic property' at this stage, it may be felt that I ought to supply something firmer than merely an appeal to general agreement about what constitute 'core instances' of inflexion, and that I ought in particular to say something about the notoriously hazy boundary between inflexional and derivational morphology. But the fact that I do not attempt to formulate that distinction precisely here is not a serious deficiency, because (despite the title of this thesis) none of the claims or suggestions I will put forward hinges on where one draws the line between inflexion and derivation, or even on the assumption that there is a line to be drawn.² In other words, none of my generalisations, as presented, depends crucially on excluding 'derivational' phenomena from its scope, and I leave open the possibility that these generalisations may be applicable to morphological behaviour which would traditionally be labelled 'derivational'.

That said, one can nevertheless identify a kind of spectrum of morphological behaviour with 'derivational' and 'inflexional'

extremes. Most linguists will probably agree in calling a morphological process (of affixation, for example) 'inflexional' if it has all the following characteristics:

- (a) it expresses a meaning (or realises a property) which all members of the relevant word-class can manifest (that is, the expression of that meaning is totally 'productive');
- (b) it is in complementary distribution with some other process or processes which realise the same property (that is, allomorphy is involved);
- (c) the property which it realises is one of a finite set (or 'category') of mutually exclusive properties, one of which must be manifested in every word-form belonging to the relevant word-class;
- (d) it does not alter the word-class membership of the forms to which it applies;
- (e) it is syntactically relevant in the sense that the property it realises is involved in quite precisely specifiable 'cooccurrence restrictions' with properties realised elsewhere in the sentence (for example, restrictions due to concord, government or 'sequence of tenses').

In contrast, most linguists will probably agree in calling a process 'derivational' if it has all the following characteristics:

- (f) it is not fully productive (that is, there are some members of the relevant word-class to which it idiosyncratically fails to apply);
- (g) no single property or 'meaning' can be associated with it;
- (h) it alters the word-class membership of the forms to which it applies;
- (j) it is not syntactically relevant in the sense of (e) (except insofar as characteristic (h) implies syntactic relevance).³

The traditional difficulty of demarcation arises from

the fact that few morphological processes display all and only the characteristics (a)-(e) or (f)-(j) respectively, and many display some characteristics taken from both sets. For example, the process of affixing the 'agentive' suffix -er to verbs in English, which would traditionally be called 'derivational', does indeed have characteristics (h) and (j) but lacks characteristic (g) and would seem to possess characteristic (a). It may also lack characteristic (f), if we are prepared to accept in some contexts agent nouns in -er formed even from those verbs for which the usual corresponding agent noun has some other form (e.g. cycle, type). In contrast, the suffixation of -e to form the Plural of Afrikaans nouns, which would traditionally be called an inflexional process, does indeed have properties (a), (b) and (d), since all Afrikaans 'count nouns' (as one might expect) can form a Plural which is syntactically still a noun, but only some of them do so by adding -e; on the other hand, this process lacks characteristic (e), since, perhaps alone among Indo-European languages, Afrikaans has no 'Number concord' of any kind. 'Core' examples of inflexional morphology, I suggest, are ones which share most of characteristics (a)-(e) and lack most of characteristics (f)-(j). The great majority of the morphological examples which I will be discussing will be unequivocally inflexional in this sense; but, again, nothing in the claims and suggestions that I will be putting forward makes it vital that I should avoid straying occasionally towards the derivational end of the spectrum.⁴

There is, however, one characteristic of morphosyntactic properties which must be regarded as necessary. If morphosyntactic properties are what inflexions realise, then a distinction between two properties which is never manifested in any distinction between inflected word-forms is impossible. One may, of course, want to recognise, even in an inflected language, syntactically relevant 'properties' or 'features' which are never expressed morphologically. 'Properties' of this kind will include, for example, many of Fillmore's (1968) 'cases', which are explicitly more abstract entities than the traditional morphological

Cases of a language such as Latin. Under this heading, too comes Dixon's (1972) distinction between instrumental and ergative 'cases' in Dyirbal. Dixon claims that there are good syntactic grounds for distinguishing these two 'cases'. That may be so; but, since there is never any overt morphological distinction between them, we are not entitled to recognise here more than one morphosyntactic Case, any more than the syntactic distinction between the object of transitive verbs and the subject of embedded infinitival sentences in Latin justifies us in recognising more than one morphosyntactic property Accusative, which happens to be manifested by nouns in two distinct syntactic contexts. A necessary condition, then, for a clear example of a morphosyntactic property is that it should have an overt inflexional manifestation in at least some members of the appropriate word-class.

I have talked so far about what I regard as the basic unit of morphological content. What about the basic unit of morphological expression? In discussing the characteristics typical of the two ends of the morphological spectrum (inflexional and derivational), I referred to 'morphological processes' such as affixation which might 'realise' morphosyntactic properties. I will in fact generally refer to morphological 'signifiants' as 'inflexional realisations' or 'inflexional exponents'⁵ of morphosyntactic properties, or sometimes simply as 'inflexions'. These apparently rather cumbersome terms are chosen in preference to, for example, 'morpheme' or 'morph' because they seem appropriate cover terms not only for affixation but also for such processes as infixation, ablaut, consonantal alternation, tonal alternation and reduplication, all of which may play a part in inflexion. For example, in the English word dogs, I would say that the morphosyntactic property Plural is realised by (or has as its inflexional exponent) the suffix -s (or [z]), while in the word men it is realised by ablaut or, more specifically, the substitution of -e- for the Singular form's -a-. My definitions thus do not commit me to trying to identify a Plural 'morpheme' or 'morph' on the level of expression in a word-form such as man, where inflexion does not involve affixation. Another reason

for avoiding the term 'morpheme' is purely practical: it has been used in so many different senses that its use here would carry too much risk of confusion and misunderstanding, even if I defined carefully at the outset the sense in which I intended to use it myself. To a lesser extent, this is also true of the term 'formative', which I likewise avoid.

The term 'allomorphy', which appears in the title of the thesis, is to be understood by reference to the more precise questions which I will be posing presently about the relationship between morphosyntactic properties and their exponents. To anticipate somewhat, I will be looking for evidence of constraints on certain deviations from the simplest conceivable pattern of exponence; and the deviations which I will have most to say about all involve the sort of behaviour that would traditionally be called 'allomorphic'. 'Constraints on allomorphy' is therefore a useful and relatively comprehensible shorthand for what, in my terminology, should more strictly be called 'constraints on deviation from the simplest conceivable pattern of relationship between morphosyntactic properties and their inflexional exponents'.

Although a basic framework of assumptions and definitions, such as I have now erected, is an essential prerequisite for the discussion of my empirical proposals, I would not claim that there is much new in what I have said so far. But, although the terminology I will be using is not original, some of my arguments will be of a rather novel form; so a second prerequisite, before we can get properly under way, is a justification for this novel form of argument. The next section is devoted to that justification.

1.3 Method in morphological research

My aim, as stated at the beginning of the introduction, is to propose and defend certain empirical generalisations about the relationship between morphosyntactic properties and their exponents. Any generalisation is a claim that certain things

are so. But any empirical generalisation -- that is, any generalisation which is not a tautology, or true by definition -- carries with it too the claim that certain things which might have been so are in fact not so. Any serious attempt to generalise about actual morphological behaviour, therefore, commits us also to the study of logically possible morphological behaviour -- what might happen as well as what does.

This may seem a rather surprising suggestion. One might argue; the business of the linguist is to describe and explicate what actually happens in languages, which is a big enough task in all conscience; considering what might happen is surely not for him but rather for the philosopher, the logician or perhaps the science fiction writer. This reaction is understandable but, I think, mistaken. It is instructive to compare the sort of method I am advocating in morphological research with that which has now become commonplace in the domain of syntax. To justify his account of a given syntactic phenomenon in a given language L, the linguist typically works out what his account predicts about the grammaticality or ungrammaticality of suitably-chosen sentences in L, and tests these predictions against the acceptability-judgments of native speakers. But his aim in this is nearly always two-fold; he is interested not only in how to catalogue correctly the facts of L (achieving what Chomsky has called 'observational adequacy') but also, to some degree at least, in how the evidence from L may contribute to syntactic theory so as to predict, and not merely permit, as much as possible of what he has observed in L (in Chomsky's terms, achieving higher levels of adequacy). The ungrammatical, or 'starred', sentences that he cites in his argument therefore fulfil two functions: they illustrate what is ungrammatical for L in particular, but also, insofar as the linguist succeeds in drawing general theoretic conclusions from them, they may illustrate syntactic behaviour which, though logically possible, could not occur in any human language (if the theory is correct).

The suggestion that we should study what is logically pos-

sible but does not occur in inflexional morphology should now begin to seem less surprising. It is simply a suggestion about the analogue in morphology of the second of the two things that the syntactician is doing when studying his 'starred' sentences. The reason why it may have seemed surprising at first is that there is no such direct analogue in morphological research to the first of the syntactician's two aims: to find out what the brute facts are at the level of the individual language. The brute facts of inflexional morphology have been fully described, at least at the level of observational adequacy, for all the languages for which pedagogical grammar-books exist, and when a linguistic field-worker goes to work on a hitherto undescribed language, one can be sure that the 200-page monograph that results will treat pretty fully the grammatical categories which are expressed by inflexion and the shapes of the inflexions which express them, even if the treatment of syntactic processes is sketchy. The fact that the Plural of English tooth is teeth, or that the Dative and Ablative Cases Singular of most Latin 'third declension' adjectives have the same ending, is not 'news', worthy of publication in the linguistic journals, in the same way that, for example, the phenomenon of pied-piping in English syntax is 'news', rightly given considerable prominence by the linguist who first attempted to describe it fully (Ross 1968). But just as pied-piping in English may well acquire a wider relevance by suggesting some general constraint on how syntactic processes operate, so in principle may the morphological facts that we have mentioned acquire a wider relevance if they can be shown to bear on some empirical claim about the extent of possible inter-linguistic variation in inflexional morphology. In principle, the syntactician and the morphologist use facts, once established, in exactly the same way, to discriminate between what is logically possible in language and what is in fact possible. The only difference is that for the syntactician, unlike the morphologist, establishing the facts at the outset is often an adventure in itself, involving acceptability tests on sentences carefully designed so that the pattern of verdicts which emerges may isolate so far as possible just those factors which must be taken account

of in a correct description; moreover, arguments about the facts themselves ('observational adequacy') almost inevitably merge into arguments about the most appropriate way of describing the facts ('descriptive adequacy') and hence the theoretical framework underlying the description ('explanatory adequacy'). For the inflexional morphologist, on the other hand, discovering the relevant facts (or many of them) usually involves no adventure; the facts are boringly accessible, 'captured' exhaustively on the pages of monographs and reference grammars. So the morphologist is not drawn ineluctably into consideration of general linguistic issues, as the syntactician is, by the very difficulty of establishing what his primary data are.

One can sum up with a paradox: progress in arriving at a general theory of inflexional morphology (that is, in establishing what is and is not possible in inflexion in human languages) has been a great deal slower than progress on the corresponding issues in syntax and semantics, largely because morphological facts have for long been so much more fully and accurately observed than syntactic and semantic facts. The morphologist is thus deprived of certain stimuli to theory-creation and theory-testing that the subject-matter of syntax intrinsically provides. But this lack can be remedied. He must constantly exercise his imagination, when looking at a given array of morphological data (say, a verbal Person-Number-Tense paradigm) and ask not only: what do we observe here? but also: what might we have expected to observe here (or: what might we conceivably have observed here) that we do not observe?

The two words 'expected' and 'conceivably' are important. Of course, there is in principle an infinite range of morphological phenomena which logically might occur in a given language but which in fact do not; the linguist needs some method of distinguishing within this range those non-occurrences which are of potential theoretical interest (corresponding to the 'starred' sentences in a disquisition on syntax). To illustrate the sort of method that the morphologist must use, I will start by citing

certain facts about non-occurrences, or 'observational gaps', in Latin inflexional morphology:

- (101) No Latin noun has a Case ending seventeen syllables long.
- (102) No Latin noun has an Accusative ending -at.
- (103) In Latin, there are not two semantically arbitrary classes of nouns, one of which inflects for Number only and the other for Case only; rather, all 'count' nouns (except for a tiny group^{of} indeclinables) inflect for both Number and Case.
- (104) Latin has no Dual Number.
- (105) In Latin, no noun expresses Plurality by means of ablaut.
- (106) In Latin, no noun expresses Plurality by inverting the order of consonants in the stem (as if dominus (Nom Sg) 'lord' had a Nominative Plural "nomidus").

In deciding which of these facts are of linguistic interest, and in what way, a linguist will draw upon his general knowledge of how morphology operates in a variety of languages. Faced with the range of facts cited in (101)-(106), most linguists would, I suggest, agree broadly in allocating them to three broad categories as follows.

- A. Facts of no interest: (101).
- B. Accidental facts about Latin (i.e. facts not reflecting any wider linguistic generalisation): (102), (104), (105).
- C. Facts possibly reflecting some general linguistic principle: (103), (106).

The grounds on which I assign these facts of Latin to one or another of the categories A, B and C will certainly not be the same in detail as those which another linguist will advance if faced with the same task. He might even disagree with my actual categorisation of some fact, on the basis of a wider linguistic general knowledge than I possess. But any practising linguist would, I think, agree about the sort of evidence that is relevant to the categorisation, even if (as the recent history

of linguistics makes likely) he has not devoted much attention to questions of inflexional morphology. I emphasise this probability of agreement about the status of various kinds of observational gap because it is what chiefly guarantees that, even starting as we are with a relatively clean slate on the theory of inflexional morphology, we can nevertheless hope that the questions we ask are ones which other linguists will agree to be worthwhile, and the conclusions we reach will strike other linguists as at least close enough to the truth to deserve consideration and criticism. Thinking systematically about logical possibilities that do not occur need by no means lead to endlessly divergent speculation, but will rather, I believe, make possible cooperative progress in theory-construction founded on new awareness of the potential significance of long-established facts. How far this progress can extend depends mainly on how many of these potentially significant facts turn out to be really significant; but that is, of course, an empirical question, not a methodological one, and indeed the central question of linguistic theory as it applies to morphology.

The reasons for my decisions about the six Latin facts are:

(101') Seventeen-syllable affixes would be intolerably cumbersome. The fact that neither Latin nor (so far as I know) any other language has any can be put down to the banal fact that human language is a communication system, and that in any communication system features which unnecessarily slow down transmission will be avoided. One could imagine, perhaps, a communication medium so 'noisy', in the technical sense, that seventeen-syllable affixes, and the slowness they would entail, would nevertheless be necessary for the accurate understanding of messages received. But neither human speech nor writing is such a medium. This fact about Latin is thus outside the sphere of linguistics, just like (for example) the fact that probably no

one has uttered a sentence 5,000 words long.

- (102') The ending -at is a possible ending in Latin (cf. am-ō 'I love', am-at 'he loves'); moreover, there is no intrinsic reason why it should not function as an Accusative Singular ending, since at least one language (Hungarian) uses it (or, more exactly, an ending phonetically similar to it) to mark the Accusative (or, more exactly, a Case similar enough in function to the Latin Accusative to deserve the same label); cf. Hungarian toll 'pen', tollat 'pen (Accusative)'. The fact that Latin does not use this ending for this purpose can be explained in historical terms, but from the general linguistic point of view it is a pure accident.
- (104') Many languages, of course, have a Dual Number, including some within the Indo-European family whose morphological and (to a lesser extent) syntactic characteristics are quite similar to those of Latin; such as Sanskrit, ancient Greek and Slovenian, and it is clear that the Dual is an ancient feature of Indo-European. From our point of view, then, the absence of a Dual Number in Latin (apart from morphological vestiges in duo 'two', ambō 'both') should probably be regarded as an accident, although we ought not to rule out the possibility of eventually relating the disappearance of the Dual from Latin to some other respect in which Latin differs from those languages which retain it, in such a way that the Latin development will no longer seem arbitrary from the general linguistic point of view.
- (105') Ablaut -- grammatically conditioned stem or root vowel change -- certainly functions as a mark of Plurality in some languages (cf. English tooth/teeth), and it operates in Latin to distinguish some verbal Perfective stems from the corresponding Imperfective⁶ stems (e.g. fēcī 'I made' versus

faciō 'I make'). The fact that ablaut is not used in Latin Plural formation may therefore seem accidental. But it is noticeable that not only ablaut but also reduplication and some other consonantal changes are exploited in Latin for distinguishing Perfective and Imperfective stems and for no other inflexional purpose (e.g. rumpō 'I break', cadō 'I fall' versus rūpī 'I broke', cecidī 'I fell'). We may therefore want to investigate whether there are any wider principles affecting how, in individual languages, particular morphological 'processes' (in Sapir's (1921) sense) are specialised for certain functions.⁷

(103') In a language which normally gives morphological expression to Number distinctions, it is quite possible for some nouns to be exceptions (for example, English sheep, deer). Similarly, in a language where nouns are generally marked for Case, it is quite possible for some nouns to be indeclinable -- to maintain the same shape in all Cases (e.g. Russian pal'to 'overcoat'). In Latin, it is true that all nouns and adjectives fail to distinguish Dative and Ablative Cases in the Plural, and that all Neuters fail to distinguish the Nominative and Accusative Cases, either Singular or Plural; but, again, the category of Case is morphologically irrelevant only to a very few indeclinables (e.g. nefas 'wicked deed'). In none of these languages, however, do we find two distinct classes of nouns, each of which manifests one of the two morphosyntactic categories but not the other; rather, what the English, Russian and Latin examples all seem to suggest is that in each language there is a set of morphosyntactic categories applicable to all members of the class 'noun', and, failing the sort of semantic excuse that 'mass nouns' have, an individual noun may exceptionally ignore (or fail to

express morphologically) one of these categories only if it ignores them all.⁸ Here, therefore, is a non-occurrence in Latin which seems not to be accidental; in view of the similar non-occurrences in other languages, we are entitled to suspect a general linguistic constraint at work.

(106') This fact is in some respects similar to fact (105), which I assigned to category B. Fact (105) -- Latin's failure to use ablaut to mark Plurality -- was deemed accidental from the general linguistic point of view because ablaut is certainly exploited as a Plural marker in other languages, although we left open the possibility that deeper investigation might nevertheless reveal some general principle at work in Latin. Fact (106), on the other hand, is assigned to category C because there is, to my knowledge, no language in which grammatically-conditioned consonantal change takes the form of inversion of the order of the consonants in a root. There is no obvious reason why this should be so; languages tolerate quite radical deformations of the 'basic' form of the root, through infixing and ablaut affecting more than one syllable (the Semitic languages are notorious in this respect). But it seems as if there is a limit to the degree of root-deformation which is permissible for inflexional purposes and that, wherever precisely the limit is to be drawn, the imaginary Latin Plural "nomidus" for dominus would overstep it.

By means of these Latin examples I have tried to demonstrate the legitimacy and usefulness, in morphological investigation, of paying attention to what might occur in individual languages but does not. I have listed certain facts about things which do not occur in the inflexional morphology of Latin, and I have suggested that two of these observational gaps (numbers (103) and (106)) stem not from Latin grammar in particular but from

constraints on language in general -- about the grammatical features which can be expressed morphologically and the morphological processes which can be used to express them. Moreover, I have suggested that most linguists would agree fairly readily about the wider significance of these two particular Latin 'non-facts'. So, thinking about what might happen in Latin but does not has served a useful purpose in drawing our attention to two apparently general facts of morphological behaviour which we might otherwise have overlooked. Throughout this thesis, I will in similar fashion cite as evidence for my conclusions not only the actual morphological behaviour of various languages but also 'non-facts' or observational gaps in their behaviour. The gaps will be used to illustrate behaviour which not merely does not occur but could not, if my empirical generalisations are correct. By the same token, they will illustrate precisely what sort of behaviour we will need to discover in some actual language in order to disprove or undermine my generalisations. But, of course, the more vulnerable a generalisation is to disproof in this fashion, the more valuable it is and the stronger the predictions it entails.

1.4

The status of the generalisations to be proposed

Since I have used the terms 'theory' and 'generalisation' frequently in the previous section, I ought perhaps to say something about the status, as I see it, of the generalisations that I will be putting forward in subsequent chapters. This is particularly so because my own view of what can count as a 'linguistic universal' is different from the one that is perhaps most widely held among linguists today.

I stated at the very beginning of this introduction that my aim was to propose and defend certain generalisations which were intended to be valid for all languages exhibiting inflexional morphology and were therefore claims about linguistic universals. To some readers, this will imply at once that my aim is to construct a 'theory' of inflexional morphology straight away. This is because, ever since Chomsky first propounded the distinction

between 'levels of adequacy' in linguistic theory, many linguists have come to identify the search for generalisations of universal validity about human language with the search for a correct 'theory of language', 'explaining' the grammars of individual languages inasmuch as it specifies a general framework of grammatical organisation (a set of 'formal universals') and probably also certain constraints on how the framework can be filled in (a set of 'substantive universals'). Linguistic 'description', on the other hand, is taken by many to be an intrinsically non-universal pursuit, appropriate to individual languages rather than human language in general.

This set of distinctions has been extremely fruitful, as well as controversial. Only one aspect of it is important here, however: the implications of equating 'universals' with 'explanation'. One can, of course, simply decide by fiat to make this equation; but those who do so clearly see themselves as doing more than merely playing with definitions. In consequence, therefore, they impose what is to my mind an unrealistically heavy burden on anyone looking for linguistic generalisations; they seem to require such generalisations not merely to be accurate but also to be 'explanatory' in something like the common-or-garden sense of the word -- that is, to form part of a coherent account not only of what happens but also of why it happens.⁹ It is clear that there is no obvious place in this approach for a kind of generalisation which is logically perfectly conceivable and, in sciences other than linguistics, surely quite commonplace: a generalisation which is empirically rich, or potentially easy to falsify, but which is explicitly descriptive rather than explanatory, providing an appropriate starting-point for attempts to explain the facts it covers but not providing an explanation in itself, either directly or indirectly through the theory in which it is ensconced. Such a generalisation may ultimately form part of a fully-fledged explanatory theory of the relevant facts, and indeed its originator presumably hopes that it will; but the fact that he himself has not arrived at such a theory need not inhibit him from propounding the generalisation. The

generalisations that I put forward here (except for those in Chapter VIII) are intended to be of this pre-theoretical, non-explanatory kind.

Of course, there is a sense in which any clearly formulated generalisation presupposes a 'theory', in that it rests on a more or less complex set of definitions for the terms in which it is expressed. Let us label this sort of theory a 'theory₁', to distinguish it from 'theories₂' which have a more or less ambitious explanatory purpose. On the basis of this distinction, theories₂ of language or certain aspects of language include, for example, Chomsky's Extended Standard Theory of syntax, Richard Hudson's 'daughter-dependency grammar', Lamb's stratificational grammar and probably also Matthews's Word-and-Paradigm model for inflexional morphology (of which more discussion will follow in section 2.2 of Chapter II). As that incomplete list indicates, one theory₂ may differ radically from another both in what it seeks to explain and in how it seeks to explain it; the important point, however, is that my generalisations, though trivially presupposing a theory₁, do not presuppose any theory₂. The starting-point of this thesis is therefore independent of any of the major theoretical viewpoints advanced in recent years. This does not mean, of course, that the conclusions reached (if they stand up to further investigation) must remain forever neutral between rival theories₂. In principle, my 'pre-theoretical' generalisations can either confirm or disconfirm relevant claims about inflexional morphology flowing from existing theories₂, although in practice, given the recent relative neglect of morphology, the volume of such claims is likely to remain small. A more likely outcome is that an appropriate explanatory theory₂ of inflexion will have to be constructed 'from the ground up', with little help from any existing theories₂ of other domains of grammar.

Given the purely descriptive status of most of my generalisations, their relationship to relevant data is quite direct: if a fact is observed which conflicts with a generalisation, the

need to amend or abandon that generalisation is recognised at once. It is not open to me to make sophisticated distinctions between genuine counter-evidence and mere 'unanalysed phenomena' which it is permissible to ignore (cf. footnote 9 above). Only in Chapter VIII do I venture into the realm of explanation for any distance and, in effect, begin the construction of a theory₂; and the relationship between my claim there and the relevant data is indeed less direct, inasmuch as the nature of the claim is such that its empirical content lies in the appropriateness (or lack of it) of the way it classifies a large body of data, rather than in what it says about individual facts.

1.5 The simplest inflexional pattern

My method of demonstrating that there is order in the apparent inflexional chaos will involve showing that not all logically possible morphological behaviour is actually found in human language. But how can we characterise 'logically possible morphological behaviour'? The answer involves considering what the simplest possible, or most regular, inflexional system would look like. From the standpoint of this maximally simple pattern, we can then consider how languages might logically deviate from it, or, in other words, what complications in the pattern are conceivable. The central empirical question which this thesis is concerned with can then be formulated as: which of these conceivable complications are actually observed to occur in human languages? The size of the class of conceivable but unobserved inflexional complications will be inversely related to the tightness of the constraints to which inflexional behaviour is subject and the strength of the generalisations which we can hope to make about them.

In our hypothetical language with maximally simple inflexion, morphology will, of course, be concerned with the structure of words. Let us leave aside, for the moment, the question of how words are defined, and assume that the division of sentences into words is unproblematical. Let us assume, also, that the grammatical categories and properties relevant to the descrip-

tion of the language can be readily identified on syntactic grounds -- categories such as Tense, Case and Number and properties such as Past, Accusative and Plural. The question then is; in this hypothetical language, what will be the nature of the relationship between word-forms on the one hand and, on the other, the complexes of lexical meanings and morphosyntactic properties that they express or realise? What we are looking for is the simplest possible relationship, where 'simplest' is to be understood in an every-day, non-theoretical sense, not tied to the evaluation measure incorporated in any particular theory of grammar.

Appealing to simplicity in this sense, one might say, amounts to little more than appealing to personal taste, supported perhaps by aesthetic criteria. If so, I would argue that subjective personal taste ultimately underlies the acceptance or rejection of any scientific theory or generalisation. Fortunately, however, we need not get too deeply involved here in fundamental questions of scientific method, because everyone will agree in this instance, I suggest, what sort of relationship between word-form and content must count as the simplest. The simplest relationship imaginable is a perfect one-to-one pairing of lexical meanings and morphosyntactic properties on the one hand with their expressions or realisations on the other. In Saussurean terms, we could say that, within words, each 'signifiant' is unambiguously associated with only one 'signifié', and vice versa. If all human languages behaved like this, scope for variety in inflexional patterning would be quite limited. It would of course be necessary to specify for individual languages what morphosyntactic properties ('signifiés') were relevant and what the actual shapes of their 'signifiants' were. The arrangement of the signifiants within words might also be to a large extent, or even overwhelmingly, language-particular. But it would not be necessary to cope with grammatically conditioned allomorphy, discontinuous morphs, 'portmanteau morphs', 'replacive morphs' or systematic homonymy.¹⁰ Moreover, since syntax already exists as a component of grammar concerned with the arrangement, or order, of items within the sentence, the distinction between

syntax and morphology might well seem otiose. 'Morphology' could just be a name for that branch of syntax concerned with the arrangement of items within words, rather than the arrangement of words themselves; and, insofar as the same principles governed both, we might conclude that there was no need at all to recognise 'morphology', even of this rather attenuated kind, as a separate component of grammar.

1.6 A historical digression: 'Item-and-Arrangement', its rivals and successors

My use of the word 'arrangement' in the last paragraph was deliberate. The sort of morphological pattern just described fits perfectly the model propounded and discussed by Harris, Bloch, Hockett and others in the 1940's and christened by Hockett 'Item and Arrangement' (IA)¹¹. Although it was not presented in those terms, the IA approach to morphology can be seen as a strong, highly restrictive theory of what is possible in morphological behaviour, in that it implicitly treats one-to-one pairings of 'signifiant' and 'signifié' as the normal or ideal state of affairs. Unfortunately, facts of the kind which forced the recognition of grammatically conditioned allomorphy, zero morphs, replacive morphs, Harris's 'morphemic long components' and the like had demonstrated by the early 1950's that the theory implicit in the spirit of IA was too strong.

At that point, I suggest, theoretical research in morphology took a wrong turn. The question which it would have been most fruitful to ask at that point is: how can we accommodate those facts while diluting, or relaxing, that implicit strong theory as little as possible? Instead, linguists in the English-speaking world for the most part turned their attention away from morphology entirely, mainly in the direction of syntax. Those who continued to think about questions of morphological theory were mainly occupied with sketching a rival model, 'Word and Paradigm' (WP), which involved no presumption in favour of one-to-one pairing of morph and function (or morphosyntactic pro-

perty and exponent).¹² They thereby avoided the drawbacks of IA just mentioned. On the other hand, it is difficult to discern in WP any claims about restrictions on inflexional behaviour implicit in its formalism which might take the place of the strong claim implicit in IA. To that extent, WP as presented by Robins and Matthews is a less ambitious and, I would say, more pessimistic theory of morphology. It is true that Matthews, in his discussion of evaluation procedures for choosing between rival grammars within the WP framework, is ready to call one set of criteria 'more realistic' than another on the ground that it defines an 'ideal' of morphological organisation which is closer to how languages actually behave (1972b:320); but he does not explicitly consider what the existence of such an 'ideal' pattern might imply about constraints on the exponence relation or, to put it another way, what sorts of divergence from the ideal are tolerated and what sorts are not.

I have mentioned the great switch in interest from morphology to syntax which followed the publication in 1957 of Chomsky's Syntactic Structures. To some extent, perhaps, this was an inevitable reaction to the widespread earlier habit of discussing linguistic phenomena on which both syntax and morphology have a bearing, such as concord, in almost exclusively morphological terms. For example, Zellig Harris (1951) discussed Person, Number and Gender concord between subjects and predicates in modern Hebrew in terms of 'morphemic long components', and similarly Martinet (1964) described concord as exemplified in expressions such as la grande montagne blanche in terms of 'monèmes' (roughly equivalent to American 'morphemes') with discontinuous exponents ('signifiants discontinus'). This way of approaching concord will probably strike most linguists trained more recently as strange. We are more familiar today with what one might call the classical generative approach, which distinguishes the syntactic side of phenomena such as concord firmly from the 'realisational' side. Syntactic 'features' such as [+ feminine] will be spread around the appropriate constituents by a syntactic rule, which will obey any appropriate general constraints on syntactic rules and itself

potentially furnish evidence for such constraints. On the other hand, the way in which the feature [+ feminine] is realised on each constituent to which it is attached will be a matter for the lexicon, hence by implication unsystematic and unconstrained, or for 'readjustment rules' mediating between syntactic surface structure and phonological interpretation, the common attitude towards which is evident from their MIT nickname 'the garbage component'.

What I have just described is the attitude towards morphology of most generative grammarians in America up to about 1970. But in Europe, even by 1970, Bierwisch (1967), Wurzel (1970) and Kiefer (1970) had begun to explore how to present in generative terms the morphology of languages inflexionally somewhat more complex than English; and after 1970 attitudes changed in America too. There are two main reasons for this. The first is the growth of doubts and disagreements about the power of phonological rules and the abstractness of underlying phonological representations -- a topic to which we will return in the last section of this chapter. The second is the growth of interest in the lexicon, largely due to Chomsky's 'Remarks on nominalization' (1970). The main morphological result of this interest has been a number of theses and articles on English morphology (mainly derivational), including Siegel (1974), Jackendoff (1975), Aronoff (1976), Roeper & Siegel (1978) and Allen (1979); and Lieber (1980) attempts to extend some of the results of this work to inflexional morphology and, in a somewhat more adventurous way than her predecessors, to languages other than English.

Some of the ideas put forward in these studies bear on the question which this thesis is concerned with, and one of them -- the Adjacency Condition -- will be discussed explicitly in the next chapter. But on the whole it seems fair to say that, when linguists of the transformational-generative school turned their attention to morphology again, their first priority was not to investigate constraints on morphological behaviour for their own sake, 'from scratch' as it were, but merely to accommodate

morphology within a theory of grammar in which primacy was given to syntax (or syntax and semantics jointly) as the locus of the most fundamental and interesting constraints on the organisation of human languages. And even though this view has been modified recently, with the attribution of more 'structure' to the lexicon, the preoccupation with derivation and compounding in English has led most 'generative' morphologists to concentrate squarely on the expression side rather than the content side of the morphological 'signe' -- on questions such as the nature and properties of boundaries in English word-formation -- and avoid the sort of question that I posed in the previous section about the extent of actual deviation from the simplest conceivable content-expression relationship.¹³

1.7 Deviations from the simplest pattern: the logical options

After this historical digression, it is time to return to the first question posed in section 1.5 above: what are the logically possible ways in which languages may deviate from the maximally simple morphological pattern I have described? Evidently, any deviation from consistent one-to-one pairing of signifiant and signifié must fall into one of two categories: it must involve either a many-to-one or a one-to-many relationship. Almost equally evidently, the well-known distinction between the two dimensions of linguistic structure, the paradigmatic and the syntagmatic, must be relevant here. Combining these two ideas, we can see that all logically possible instances of deviation from one-to-one patterning must fall into at least one of the following four classes¹⁴:

- (107) Deviation I: One (signifié) to many (signifiants) syntagmatically
- (108) Deviation II: One to many paradigmatically
- (109) Deviation III: Many to one syntagmatically
- (110) Deviation IV: Many to one paradigmatically

For morphological theory, the interest of this classification lies in whether or not it helps to identify logically possible linguistic behaviour which is not actually observed in human

languages. I will argue that it does, although somewhat indirectly.

The first question which arises, clearly, is whether all these types of deviation occur in actual human languages. The answer is yes. It is quite easy to find examples of each:

(111) I: One to many syntagmatically

This type involves what Matthews (1974) calls 'extended exponence'. The first example below is his.

- a. Ancient Attic Greek elelykete 'you (Pl) had loosed'; superficially, at least, the Perfective Tense-Aspect is realised twice in this word-form, namely by the reduplicated prefix -le- and the suffix -k- on either side of the root -ly- 'loose'.
- b. Zulu: úmfána (a)kágézi 'the boy is not washing'. Comparing this with the corresponding Positive sentence úmfána uyágeza 'the boy is washing', and with other Present Tense forms of this and other verbs, we can find evidence for five-fold realisation of the property Negative, namely (1) the optional presence of the Negative prefix a-; (2) the absence of the prefix -ya-; (3) the replacement of u- by ka- as the marker indicating concord with the subject úmfána 'the boy'; (4) the replacement of the low tone on the root -gez- 'wash' by a high tone; (5) the substitution of -i for the final -a.¹⁵

(112) II: One to many paradigmatically

This deviation is exemplified in lexically or grammatically conditioned allomorphy. One could also characterise it as suppletion in inflexion (although the term 'suppletion' is more traditionally restricted to stem alternations, as in go versus went). A standard example is the English nominal property Plural, realised usually by -(e)s but also, for example, by -en in oxen and by vowel change in teeth, men, mice. Less hackneyed examples are:

- a. The Zulu alternation, already mentioned, between

u- and -ka- to mark verbal concord with a Singular subject belonging to the so-called Class 1 ('Class' here being a lexically determined, semantically more or less arbitrary grouping roughly analogous to Gender in Indo-European languages). The grammatical conditioning, as we have seen, involves the absence or presence of the property Negative.

- b. In all Latin verbs, 2nd Person Singular Indicative Active is realised as a suffix -isti in the Present Tense of the Perfective Aspect but by a different suffix -s (with perhaps a preceding vowel) everywhere else.
- c. In Hungarian, the usual mark of the property Plural in nouns is a -k suffix (generally with a preceding 'thematic' vowel or stem change or both), e.g. dal 'song', dalok 'songs'; madár¹⁶ 'bird', madarak 'birds'. But when the noun is also suffixally marked for possession, the -k suffix is replaced by one containing -i-, e.g. dalod 'your song', dalaid 'your songs'; madarunk 'our bird', madaraink 'our birds'.

(113) III; Many to one syntagmatically

This type is exemplified in what Matthews (1974) calls 'cumulative' and 'overlapping' exponence; that is, instances where, in some word-form, more than one morphosyntactic property is realised in one unsegmentable morph or morphological process. Behaviour of this kind is a hall-mark of 'fusional' languages, and Case and Number in Latin nouns furnish a standard example of cumulation. I will therefore give a couple of less obvious examples, deliberately chosen from languages generally labelled 'agglutinating';

- a. In Turkish, Negation is usually expressed in verbs by a suffix -me- (in some phonological environments -miy-)¹⁷, which is accompanied by stress on the preceding syllable, thus:

gelíyorum	'I am coming'	gélmiyorum	'I am not coming'
gelecék	'he will come'	gélmiyecek	'he will not come'
geldík	'we came'	gélmedik	'we did not come'

However, in the Present Aorist Tense, a different pattern emerges:

gelirim	'I come'	gelmém	'I do not come'
gelírsin	'you (Sg) come'	gelmézsín	etc.
gelír	'he/she comes'	gelméz	
gelíriz	'we come'	gélmeyiz	
gelírsiniz	'you come'	gelmézsíniz	
gelírlér	'they come'	gelmezlér	

Here, in contrast to the other Tenses, there is no clear consistent syntagmatic dividing-line between an element realising Negative and an element realising Aorist. It seems necessary to treat the element -méz in gelméz as realising the two properties simultaneously, along with 3rd Person.

- b. In the Zulu example cited at (111 b), the element -ka-, as well as realising Class 1 concord, also helps to realise Negation inasmuch as it contrasts with a Positive prefix u-. Moreover, the comparison of the Active verb-form in úmfána akágézi 'the boy is not washing' with the Passive one in úmfána akágezvá 'the boy is not being washed' illustrates that the suffix -i, as well as realising Negation, also helps to realise Active, inasmuch as it does not appear in the Passive form.

(114) IV: Many to one paradigmatically

This is simply homonymy within inflexional paradigms, which will be discussed in Chapters VIII and IX with numerous examples.

Our aim, as I have said, is to identify the gap between what is logically possible in inflexional morphology and what is actually observed. The fact that examples can be found in natural

languages of all these four logically possible types of deviation from one-to-one content-to-expression pairing may seem to make the prospect of finding such a gap somewhat bleak. But pessimism is premature. We have so far considered individual content-to-expression relationships in isolation. But such relationships do not exist in isolation. In languages other than those of the purest 'isolating' type, most 'morphs' (in the widest sense, including roots, affixes and morphological processes such as ablaut) either may or must occur in word-forms combined with some other morph; and 'units of content' (or 'signifiés') not only combine within words on the 'plane of content' but also contrast paradigmatically with other units of content which, when they are morphosyntactic properties, fall typically into relatively small, relatively clearly delimited closed classes -- Matthews's 'morphosyntactic categories'. The combination of morphosyntactic properties belonging to different categories and the contrast between properties belonging to the same category are, in fact, part and parcel of Deviations III and IV respectively -- that is, those deviations characterised at (113) and (114) as involving overlap (or cumulation) and homonymy. So there is still a wide territory in which to hunt for constraints on inflexional realisations, namely among the possible ways in which inflexional 'signes' can cooccur within word-forms and contrast within inflexional paradigms.

In Chapters II to VIII I will put forward various proposals about constraints on the two paradigmatic deviations -- Deviations II and IV. Deviation I will not be discussed at all in this thesis -- not because I have searched for constraints on it and found none, but because I have not yet begun the search. The other syntagmatic deviation, Deviation III, assumes considerable importance in my discussion of homonymy in Chapter VIII, but again I propose no constraints on it as such. So in the titles of Chapters II, III and VIII, 'one-to-many exponence' and 'many-to-one exponence' are to be understood as referring to the paradigmatic, not the syntagmatic deviations.

1.8 A second digression: phonologically conditioned allomorphy and 'abstractness'

The one-to-many paradigmatic relationship between morpho-syntactic properties and their realisations illustrated in (112) all involve, as I have said, what would traditionally be called lexically or grammatically conditioned allomorphy. But these traditional labels imply a contrast with another kind of allomorphy, namely that which is phonologically conditioned. We therefore need to be able to decide which instances of allomorphy are phonologically conditioned, it seems, in order to exclude them from our search for constraints on sensitivity of the specifically morphological kind. But how do we decide this? One answer which has been explicitly or implicitly given by some generative phonologists is: there is phonologically conditioned alternation between two phonetically different surface forms (in our present context, two phonetically distinct realisations of some morpho-syntactic property) only if the two surface forms are phonologically related in the sense of being derivable by phonological rules from the same underlying phonological representation. Thus, for example, in Anderson's view (1974: chapter 4), to establish that the [iz ~ z ~ s] alternation of the regular English Plural marker is phonologically conditioned involves identifying a single underlying phonological representation from which the three surface alternants are derived by phonological rules. For him, there are only two alternatives (1974: 54): "... we could describe these ... either as suppletive forms from a list, or as phonologically determined variants of a single basic form [my emphasis]"¹⁸.

But the nature of underlying phonological representations is itself a matter of dispute. Since Kiparsky (1968a) first voiced doubts about the justification for some of the highly abstract underlying phonological representations propounded by Chomsky and Halle (1968) in their treatment of English, various attempts have been made to constrain the power of phonological rules within generative grammar. Scholars who have given the highest priority to this, perhaps, are the 'natural generative phonologists' Vennemann, Hooper, Grover Hudson and their followers.

Because they posit underlying phonological representations which are much less abstract than those possible within the Chomsky-Halle framework, much of the allomorphy that Chomsky and Halle account for by phonological rules has to be accounted for by rules of a different kind governing the distribution of distinct representations (that is, in the area which concerns us here, distinct inflexional realisations or 'spell-outs'). Indeed, G. Hudson (1975), supported by Hooper (1976), has gone to the extreme of claiming that all surface alternations, except those which can be assigned to very low-level 'natural' allophony rules, must be regarded as equally suppletive; so, for example, there is a suppletive relationship between the realisation of Plural not only in dogs, geese and oxen but also in dogs, cats and horses. The area of disagreement is therefore considerable (at least superficially), and the debate is by no means resolved. When we investigate possible constraints on morphological sensitivity, must we be inescapably drawn into it?

The answer is no. On one point, all phonologists would agree: there exist distinct 'rival' realisations for some morpho-syntactic properties in some languages which it is clearly impossible to relate phonologically, at least without an absurdly generous notion of what phonological rules can do. Rival realisations of this kind include -(e)s, -en and vowel change as inflexions for Plural in English nouns. Neither Anderson nor any other linguist, to my knowledge, has ever attempted to derive these all from a single representation at the underlying phonological level. These 'rival' alternants thus constitute a quite uncontroversial instance of suppletion in the sense of allomorphy which is conditioned purely lexically or grammatically, not phonologically. Moreover, to establish them as such it has not been necessary to commit ourselves to any view on phonological abstractness in genuinely controversial cases. So, to avoid having to take sides on the phonological issue, my policy throughout this thesis will be to base my arguments so far as possible on examples that are equally uncontroversial. My conclusions will therefore not presuppose a particular view of the suppletive or non-supple-

tive status of more controversial alternations, or of whether (as G. Hudson argues) all alternations should be regarded as suppletive. The logical connexion is, if anything, the other way round. My conclusions may, in principle, have a bearing on the phonological 'abstractness' question, in that if, for example, some restriction emerges on the way in which grammatical conditioning can operate in alternations which are quite clearly suppletive, it will be of interest to see whether 'borderline' alternations -- ones where a single underlying phonological representation is possible but doubtful -- obey the same restriction. If they do, and if alternations closer to the phonological end of the spectrum do not, we will have introduced a useful new tool for phonological analysis. If constraints established for uncontroversially suppletive allomorphy are found to hold right across the spectrum, then Grover Hudson's view of all alternations as suppletive will have received independent support which will be all the more valuable as coming from an investigation whose starting-point is explicitly non-phonological. (This outcome seems to me unlikely; but it is premature to speculate.)

There is an important distinction to be drawn here, however. The fact that I am unwilling to take a stand on whether there is a single underlying phonological representation for the English Plural -(e)s or whether we have here suppletive alternants in Grover Hudson's sense does not mean that I am agnostic as to whether the alternation is phonologically conditioned or not. Clearly it is, in the sense that once we know that an English noun has a regular -(e)s Plural, we know on purely phonological grounds which of the three alternants to choose. I am thus quite ready, in appropriate circumstances, to treat an alternation as phonologically conditioned even if the postulation of a 'single basic form', in Anderson's words, is as problematic as it would be for the Plural markers in the English foxes, oxen and geese. Examples of the sort of alternation that I have in mind are:

(115)

Language	Morphosyntactic properties realised	Alternants	Phonological conditions
a. Hungarian	2nd Sg Indefinite Present Indicative	-ol -(a)sz [-(p)s]	after sibilants and affricates; elsewhere
b. Turkish ¹⁹	3rd Sg Possessive (on nouns)	-i -si	after consonants; after vowels
c. Turkish	Genitive (on nouns)	-in -nin	after consonants; after vowels
d. Fang (Guthrie 1956: 551)	Noun Class 5	a- dz-	before consonants; before vowels
e. Warlpiri (Dixon 1980: 306)	Ergative (on nouns)	-ngku -rlu	after a 2-syllable stem; after a stem of 3 or more syllables

Anderson's account of allomorphy would seem to commit him to finding a single basic underlying representation for each of these alternations, no matter what the cost in arbitrary-seeming 'minor' phonological rules; and indeed Vago (1980) has adopted this sort of approach to the Hungarian example at (115 a). In keeping with my position of neutrality on phonological theory, I take no view on whether this is correct. The point I want to emphasise here is that one can recognise an alternation as phonologically conditioned without committing oneself about the underlying phonological representation of the alternants. This is important, because it will be crucial for my argument in more than one place to be able to distinguish between lexically or grammatically conditioned allomorphy (such as that between the Plural inflexions of foxes, oxen and geese) and phonologically conditioned allomorphy (as exemplified in (115), and as in English foxes, dogs and cats).

Footnotes to Chapter I

1. Some further reasons for the recent neglect of morphology are mentioned in section 1.6.
2. In what I say about the Adjacency Condition in section 2.10, however, I recognise the possibility that a certain constraint may apply to derivation without applying to inflexion.
3. If all derivational morphology involves syntactic transformation (the 'transformationalist' position rejected by Chomsky (1970) in favour of a 'lexicalist' position), then all derivation will by definition be 'syntactically relevant' in one sense. But I am not concerned with that sense here.
4. For further discussion of the distinction between inflexional and derivational (or 'lexical') morphology, see Matthews (1974: Chapter III).
5. For me, in contrast to Matthews, the terms 'realisation' and 'exponent' are merely stylistic variants.
6. I follow Matthews (1972b) (who in turn follows Meillet (1933)) in recognising a category of Aspect in Latin verbs, e.g. Imperfective amō 'I love' versus Perfective amāvī 'I (have) loved'.
7. Lieber (1980: 311-317) has suggested that affixation differs from all non-affixal ('string dependent') morphological processes in such a way that only affixes ('morphemes') will tend to have a single meaning or function within a given language. The facts about reduplication and ablaut in Latin that I have just mentioned run counter to this; so does the fact that the single affix -en in German has a large number of distinct inflexional functions. But the idea that there can be differences of this kind between different processes seems worth exploring.
8. Superficially, this assertion seems to be endangered by the situation in Sogdian, where so-called 'heavy' and 'light' nominal stems seem to belong to quite distinct Case-systems (Sims-Williams 1981). But the very instability of this dual system, whose origin is clearly due to certain phonological innovations, points to its 'unnaturalness' in general linguistic terms.

9. For some linguists, indeed, 'explanation' seems even to take precedence over accuracy, in the sense that unless *prima facie* counter-examples to their generalisations are presented as part of a fully elaborated rival explanatory 'theory', they feel free to ignore them as being mere 'unanalysed phenomena' or 'pre-theoretical observations' (cf. e.g. Lightfoot 1979: 73). In my view, this places the burden of proof on the wrong party.

10. The maximally simple inflexional pattern described here is, in effect, the one we would observe everywhere if what has been called 'Humboldt's Universal' (Vennemann 1972: 183; G. Hudson 1980: 115) were universally complied with.

11. Joos (1957) contains several of the most important papers in which the Item-and-Arrangement approach is applied.

12. On Word-and-Paradigm morphology, see e.g. Robins (1959), Matthews (1972b; 1974) and R.A. Hudson (1972).

13. An exception to the generativists' recent neglect of inflexion in highly-inflected languages is Anderson (1977), which deals with Potawatomi; but since the formal notational framework he puts forward for handling inflexion within generative grammar seems to impose virtually no constraints at all on possible inflexional behaviour, he can be said to share the implicit pessimism about inflexional universals that I attributed to the advocates of WP.

14. My exhaustive four-fold classification of deviations from one-to-one patterning is, so far as I can tell, original. Bally (1944: 143-145) introduces a promising distinction between dystaxie, corresponding to my two syntagmatic deviations, and polysémie, corresponding to my paradigmatic ones; but his subsequent discussion, in terms of lexical rather than grammatical 'signes', is rather elementary and disappointing. Pike (1963) acknowledges syntagmatic many-to-one patterning as characteristic of what he calls 'ideal matrices' (or 'optimal matrices') in morphology, but he does not attempt to classify types of morphological patterning exhaustively. Anttila (1977: 56-57) discusses briefly 'polymorphy' and 'polysemy' as deviations from 'one meaning - one form', but his classification, like Pike's, is not exhaustive.

Under polymorphy he includes allomorphic alternation (i.e. my Deviation II) as well as 'compounds, phrases' (i.e., presumably, Deviation I as it applies to words rather than inflexional realisations), and under polysemy he includes homophony (i.e. Deviation IV) as well as metaphor, metonymy and loan translation (!), but Deviation III seems to have no place in the scheme. It is notable that Bally's 'polysémie' and Anttila's 'polysemy' are by no means the same:

Deviation:	I	II	III	IV
Anttila	polymorphy?	polymorphy	?	polysemy
Bally	dystaxie	polysémie	dystaxie	polysémie

Wheeler's (1980) two Tendencies A and B, which he suggests may contribute to inflexional change by favouring ease of production and ease of perception respectively, correspond roughly to my paradigmatic Deviations IV and II respectively.

15. This example is based on material from Rycroft & Ngcobo (1979) and Doke (1973). Acute accents represent underlying high tones, according to Rycroft's analysis.

16. The acute accent indicates vowel length in Hungarian orthography.

17. For clarity, both here and in all Turkish examples cited in this thesis, I use only front-vowel roots and affixes in their front-vowel shape.

18. Compare also Hyman (1975: 13), discussing go versus went and mouse versus mice: "In both these cases it is not possible to derive one form from the other by means of a general phonological rule. Such cases of irregular allomorphs (known as 'suppletion') therefore differ in a crucial way from the more regular allomorphs derived by phonological rules [my emphasis]". Kenstowicz & Kisseberth (1979) seem to hold the same view (see, for example, page 140 and their conclusion on page 196 about the 'morpheme alternant' theory of underlying representations). By contrast, Chomsky & Halle (1968) seem never to commit themselves so absolutely; and an examination of their sample of 'readjustment rules' for English (pages 238-239), whose function is to convert lexical representations into phonological representations,

suggests that they are willing in principle to countenance the handling of some phonologically conditioned allomorphy by means of rules which do not belong to the phonological component. The Anderson-Hyman view is criticised by Linell (1979) and also (from a somewhat different point of view) by me (Carstairs 1981).

19. In the Turkish examples I ignore the vowel alternation due to vowel harmony, for clarity's sake.

CHAPTER II

A SYNTAGMATIC CONSTRAINT ON ONE-TO-MANY EXPONENCE

2.1 'Pure sensitivity'

This chapter will be concerned with the search for constraints on Deviation II -- the deviation involving what I will call 'sensitivity' on the part of one morphosyntactic property either to other properties realised in the same word-form (where the allomorphy is 'grammatically conditioned') or else to the stem of the word itself (where the allomorphy is 'lexically conditioned'). But where shall we start? This first section is devoted to identifying a class of instances of Deviation II which will provide suitable material for our search to begin with.

Mel'čuk (1976: 73) remarks that, although the term 'suppletion' is generally restricted to the relationship between phonologically dissimilar realisations of the same lexical item (as in go/went, Russian idu/šol 'I am going/(I) was going'), phonologically dissimilar realisations of the same morphosyntactic property seem to stand in just the same relationship. This point is, of course, explicitly emphasised by the proponents of 'natural generative phonology', whom I mentioned in Chapter I. But why is the label 'suppletive' traditionally restricted to (or, at any rate, exemplified by) alternations between roots only, as in the passage from Hyman (1975) quoted in footnote 18 to Chapter I? There must be some difference between the behaviour of roots and that of inflexions which has obscured the parallellism that Mel'čuk noted. The main difference, in fact, seems to be one of frequency and 'ordinariness'. In most languages, root suppletion is unusual, limited to rather few, even if frequently occurring, lexical items. Linguists have therefore tended to see it as a rather marginal phenomenon which they need pay little attention to when constructing a model (or theory) for lexical and morphological description.. But, whether or not this attitude

is justified where root suppletion is concerned, it is clearly quite inappropriate as regards non-phonologically-conditioned inflexional 'suppletion', simply because inflexional alternations of this kind are much too common in even moderately inflected languages to be considered marginal. A terminological distinction has therefore grown up: we tend to speak of suppletion between phonologically dissimilar roots associated with the same lexical item, but grammatically or lexically conditioned allomorphy between phonologically dissimilar realisations of the same morphosyntactic property. It remains to be seen whether there are any grounds for the distinction independent of the factor of 'ordinariness'; I will return to this in Chapter VII to some extent.

Examples of grammatically or lexically conditioned allomorphy were given in (112). One of these examples was the realisation of Plural on English nouns. Although English is generally regarded as being poor in inflexional morphology, it is easy to find further English examples:

(201)	Inflexional property	Suppletive realisations
a.	Past Tense	<u>-ed</u> \emptyset (e.g. <u>put</u>) vowel change (e.g. <u>drove</u>)
b.	Past Participle ¹	<u>-ed</u> \emptyset (e.g. <u>put</u>) <u>-en</u> (e.g. <u>driven</u>) vowel change (e.g. <u>sung</u>)

One might argue that \emptyset and -ed here are not in fact phonologically unrelated; the fact that the former is limited to roots ending in -t may point to an underlying phonological realisation such as /put + d/. But the important point for our present purpose is that there are at least two realisations of Past Tense, namely -ed and vowel change, and three of Past Participle, namely the same two plus -en, which are clearly not phonologically related and whose distribution is determined lexically or grammati-

cally, not phonologically.

As soon as we turn from English to a highly inflected language such as Latin, we find much more elaborate arrays of rival realisations for the same morphosyntactic property, for example:

(202)	Inflexional property	Part of speech	Suppletive realisations
	2 Sg	Verb	-s (-ās, -ēs, -is, -īs) -istī -re (-āre, -ēre, -ere, -īre) -ris (-āris, -ēris, -eris, -īris) -e
	Dat Sg	Noun	-ae, -ō, -ī, -uī, -ū, -ēī
	Infinitive	Verb	-re, -rī, -ī, -isse
	Perfective	Verb	-v- ([w]), -u-, -s-, ablaut, reduplication

Almost equally elaborate allomorphy can be found in Hungarian²:

(203)	Inflexional property	Part of speech	Suppletive realisations
	2 Sg Indef	Verb	-ol, -(a)sz, -j, -ál
	2 Sg	Noun	-od, -ad

Remembering that the first problem that we have set ourselves is to find a starting-point for our search for constraints on Deviation II, what sort of facts out of this array should we concentrate on first?

One point which will strike anyone who knows anything of either Latin or Hungarian is that many of the examples in (202) and (203) are not examples of Deviation II by itself; they also involve Deviation III (many-to-one syntagmatic realisation), identified at (113) with Matthews's 'cumulative' or 'overlapping' exponence. Thus, in (202), the Dative endings listed all realise Singular as well; and in (203) the first two verbal endings given are restricted to the Present Indicative and so may be said to help realise that combination of properties. It is not surprising that many instances of Deviation II should also be instances of Deviation III; after all, many-to-one syntagmatic realisation

of morphosyntactic properties presupposes the impossibility of segmenting the morphological material into one 'morph' per property, which in turn presupposes a kind of mutual sensitivity between the properties so realised. If, however, we wish to concentrate on Deviation II specifically, without the risk of our data being contaminated by any constraints on overlapping or cumulative exponence, we ought to concentrate on instances of Deviation II by itself -- what I will call instances of 'pure' sensitivity. We will consider first a set of hypothetical examples. These hypothetical examples will help us to appreciate the range of potential variety within the realm of pure sensitivity. In the light of this, we can begin to see whether it is plausible to postulate any constraints related to sensitivity alone. But, before we do so, we must establish precisely what is to count as pure sensitivity.

One possible objection to the plan of campaign just outlined is that there can never be any such thing as pure sensitivity -- that we can never find Deviation II unaccompanied by any of the other deviations. Let us suppose that some morphosyntactic property P is sensitive to its grammatical environment in such a way that it is realised as a normally but as b when some other property Q is present, thus:

(204) Property:	P (without Q)	P (with Q)
Realised as:	<u>a</u>	<u>b</u>

In a situation like this, one might say, b realises Q just as much as it does P. But then we have here an instance of many-to-one syntagmatic realisation, in that a single signifiant b realises (or helps to realise) two signifiés, P and Q. The example thus involves Deviation III as well as Deviation II. Yet (the objection continues) all logically possible examples of Deviation II are of this kind, since they all involve an analogue of b; and it is arbitrary to deny to this analogue of b some share in realising the conditioning property (analogous to Q) as well as the property with which it is allegedly primarily associated (the analogue of P).

In answering this objection, I will not deny that the analogue of b in any conceivable example of Deviation II has some share in realising the analogue of Q. But I will argue that we can nevertheless identify circumstances in which it makes sense to talk of 'pure' sensitivity. These will be circumstances in which the conditioning property Q has a 'principal exponent' apart from b, by which I mean that Q is unambiguously realised independently of the sensitive inflexion (b) by some 'morph' (call it x) which also realises Q in some or all environments where P is not present. We can illustrate this on the lines of (204) as follows:

(205) Property:	P (without Q)	Q (without P)	QP
Realised as:	<u>a</u> , <u>b</u> , <u>c</u> , ...	<u>x</u> , <u>y</u> , ...	<u>xb</u>

The point of this restriction is that, although Deviation III is present inasmuch as b realises Q as well as P, what we see here is not complete overlap of the properties P and Q in a single unsegmentable 'morph' but rather the extended realisation of Q by two 'morphs', x and b. Of these, the first serves by itself to realise Q in other environments and, moreover, has no part in realising the sensitive property P. This distinguishability of the two morphs x and b is crucial in allowing us to say that the sensitivity of P to Q in (205) is 'pure'. Our definition of pure sensitivity thus requires us to exclude from consideration for our present purposes all instances of Deviation III where there is complete overlap of the properties concerned, as in the realisation of Case and Number in Latin declension. Any constraints we discover on pure sensitivity may, of course, turn out to apply to sensitivity of other kinds too; but by restricting ourselves in the way I suggest we can be sure that such constraints will be independent of any which intrinsically involve overlap or cumulation.

A couple of examples will illustrate the distinction between pure sensitivity and other types. The French word-forms parlerons [parlə'rɔ̃] '(we) will speak' and parlerions [parlə'rjɔ̃] '(we) would speak' represent the 1st Person Plural of the Future and the Conditional respectively. Comparison with the other Per-

sons of these two Tenses suggests a segmentation into a stem parler- [parlɔr] which is shared by both Tenses and a Personal ending which is not. Clearly the stem affix -er- does not unambiguously realise either Future or Conditional by itself. But the Personal endings do not do so either, since each is shared with some other Tense; -ons with the Present parlons [par'ljɔ̃] '(we) speak' and -ions with the Imperfect parlions [par'ljɔ̃] '(we) were speaking'. Rather, it is the combination of the stem-forming affix and the Personal ending which jointly distinguishes the Future from the Conditional in the 1st Person Plural. So, in this example, the way in which the property-combination 1st Person Plural is realised is certainly sensitive to some other accompanying property, namely one belonging to the category Tense; but we cannot say that either of the two Tenses, Future and Conditional, is unambiguously realised independently of Person, so we cannot call this an instance of pure sensitivity.³ A pure instance is, however, easy to find in Latin. The two forms amās 'you (Sg) love' and amāvisti 'you (Sg) have loved' illustrate sensitivity in the realisation of 2nd Person Singular; -(ā)s⁴ in the Imperfective Present but -isti in the Perfective Present. The ending -isti can therefore be said to share in the realisation of Perfective. Nevertheless, Perfective is unambiguously realised elsewhere, namely in the stem amāv-; after all, amāv- is found in all the Perfective forms of the verb (except the participle) and only there. This, then, is an instance of 'pure' sensitivity, since the property Perfective has a principal exponent which is independent of the Personal ending.

Having thus established what type of behaviour falling under Deviation II we will be examining, it is time to look at the promised hypothetical examples. At (206) is a set of hypothetical verbal endings for Tense, Person and Number:

(206)	Present	Past	Future
Sg 1	en	ok	ain
2	al	il	aip
3	as	or	ur
Pl 1	ant	ont	aint

(206) (continued)

	Present	Past	Future
P1 2	alt	olt	ailt
3	art	ort	airt

Most linguists, when presented with this paradigm, would, I think, find something distinctly implausible about it. Yet a close examination reveals a structure which, although it departs from one-to-one exponent-to-property patterning, does so only in the direction of pure sensitivity and, moreover, in a fashion which can be stated quite succinctly:

(207) a. Order of realisation of categories:⁵

Tense + Person (+ Number)

b. Realisations of Tense:

Present	<u>e</u> in 1st Sg
	<u>a</u> elsewhere
Past	<u>i</u> in 2nd Sg
	<u>o</u> elsewhere
Future	<u>u</u> in 3rd Sg
	<u>ai</u> elsewhere

c. Realisations of Person:

1st	<u>k</u> in Past Sg
	<u>n</u> elsewhere
2nd	<u>p</u> in Fut Sg
	<u>l</u> elsewhere
3rd	<u>s</u> in Pres Sg
	<u>r</u> elsewhere

d. Realisation of Number:⁶

Plural	<u>t</u>
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This statement does, however, provoke a fairly obvious question: what would the hypothetical set of endings look like if no sensitivity were present and if only the 'elsewhere' realisations listed for each property in (207) were to occur? The result would be as in (208):

(208)	Present	Past	Future
Sg 1	an	on	ain
2	al	ol	ail
3	ar	or	air

(208) (continued)	Present	Past	Future
Pl 1	ant	ont	aint
2	alt	olt	ailt
3	art	ort	airt

This in turn suggests a possible explanation for the implausibility of (206): perhaps it is the sheer quantity of sensitivity displayed in (206), as opposed to the maximally perspicuous pattern of (208), which is enough to exclude it (or render it extremely 'costly') as an actual Tense-Person-Number paradigm in an actual language. If so, it is obviously superfluous to look for any deeper or more subtle explanation in terms of general constraints on sensitivity.

This possible explanation can, however, be shown to be false. All we need do to demonstrate this is find a set of Person-Tense-Number forms in an actual language which displays as much sensitivity as (206) or more, from the point of view of the sheer volume of allomorphy. Once again Latin furnishes as set of forms meeting this requirement. Consider (209):

(209)	Imperfective Present	Perfective Present	Imperfective Future
Sg 1	regō 'I rule'	rexī [reksi:]	regam
2	regis	rexi	regēs
3	regit	rexit	regēt
Pl 1	regimus	reximus	regēmus
2	regitis	rexitis	regētis
3	regunt	rexerunt	regent

If we try to draw up for (209) a description of how the relevant morphosyntactic properties are realised, on the lines of (207), the result is unavoidably quite complex, no matter what our view of Latin phonology and of the underlying phonological representations of the various 'morphs'. One version might be as follows:

(210) a. Order of realisation of categories:

Aspect (+ Tense) + $\left\{ \begin{array}{l} \text{Person} \\ \text{Number} \end{array} \right\}$

(i.e. Person and Number are cumulated)

(210) (continued)

b. Realisation of Aspect:

Perfective -v-, -u-, -s-, reduplication,
vowel lengthening

c. Realisation of Tense:

Future a in 1st Sg
u
e elsewhere

d. Realisation of Person-Number:

1st Sg	<u>-ī</u> in Pf Pres	/v <u> </u>	/c <u> </u>
	elsewhere:-	<u>m</u>	<u>ō</u>
2nd Sg	<u>-istī</u> in Pf Pres		
	elsewhere:-	<u>s</u>	<u>is</u>
3rd Sg		<u>t</u>	<u>it</u>
1st Pl		<u>mus</u>	<u>imus</u>
2nd Pl	<u>-istis</u> in Pf Pres		
	elsewhere:-	<u>tis</u>	<u>itis</u>
3rd Pl	<u>-erunt</u> in Pf Pres		
	elsewhere:-	<u>nt</u>	<u>unt</u>

I grant that one could shunt the complexity represented in the final column out of one's morphological into one's phonological description, by dint of positing more abstract underlying phonological representations and several morphologically sensitive 'minor' phonological rules. But one is still left with an irreducible minimum of sensitivity in one's account of how the six properties (or combinations of properties) Perfective, Future, 1st Sg, 2nd Sg, 2nd Pl and 3rd Pl are realised --- just as much as in the hypothetical example at (206), described at (207). Moreover, one could argue that the complexity at (210) involves not merely pure sensitivity but also cumulation of the properties Person and Number, and is therefore exacerbated. So the reason why the hypothetical paradigm at (206) seems so implausible cannot be that the sheer quantity of sensitivity involved is greater than any actual human language will tolerate.

2.2

Excursus: 'pure sensitivity' and Matthews's WP model

I ought, perhaps, to explain why I have not attempted to define 'pure sensitivity', or some other notion which would be

equally useful when exploring constraints on Deviation II, in terms of the framework of definitions introduced by Matthews (1972b:160 ff.) in describing his Word-and-Paradigm model for inflexional morphology. Reasons why one might want to do this, of course, are to avoid introducing new technical terms needlessly and to make it easier, while attempting to break new ground, to profit from earlier discussions and any relevant results already established. I agree that these are both good reasons, other things being equal. But, against this, there are two reasons why I have not adhered more closely to Matthews's model. The first has to do with the difference between Matthews's and my aims, and the second with the actual content of Matthews's definitions.

In section 1.4 I discussed the distinction between 'theories₁' and 'theories₂'. There, I had in mind principally the contrast between my approach and that of many transformational-generative linguists, who tend to doubt the value and interest of any generalisations which are not put forward in the context of an explicit, even if tentative, 'theory₂'. Now, Matthews disagrees with Chomsky and most transformationalists about the status and justification of general linguistic theories; in particular, he does not agree that a model of description must aspire to universal validity (1972b:147-156) and is happy to admit that his own WP framework is not equally suitable for all inflected languages. Clearly, therefore, he does not accept the Chomskyan identification of linguistic theory with a model of the innate linguistic 'knowledge' shared by all human beings. Nevertheless, Matthews's WP framework ^{seems to be} certainly a theory₂ in my sense. It is not a mere assemblage of descriptive generalisations about inflexional behaviour, but a framework for inflexional description, making descriptive generalisations only indirectly to the extent that it is 'adequate' and 'appropriate' (in Matthews's terms) for data from a variety of languages. So for anyone interested (as I am, at present) in establishing descriptive generalisations rather than developing an explanatory theory₂, it would be potentially misleading to adopt any definition of sensitivity which

was too tightly tied to Matthews's framework, to do so would increase the risk of biasing the search for generalisations in the direction of one particular theory₂ in a potentially question-begging fashion. This is a risk that I ought to avoid, even supposing that there happens to be within Matthews's framework a notion which looks to be ideally suited for identifying those instances of sensitivity which it is most profitable to concentrate on at this stage.

My second reason for not adhering more closely to Matthews's model, however, is that there is, in fact, no notion within his framework which would serve our present purpose. Recall that what we are looking for is a way of identifying a class of examples of Deviation II which we can examine with reasonable confidence that any generalisation we discover there concerning the relationships between properties and their exponents will have to do with Deviation II alone, not Deviations I, III or IV. The notions that look most promising in Matthews's system are 'exponence' (1972b:184) and 'formation', which is defined by reference to 'focal terms' (1972b:186). I will discuss each in turn.

It is fairly easy to see that Matthews's 'exponence' will, by itself, be of little help to us. It is a central characteristic of Matthews's framework that 'extended exponence' -- the realisation of a morphosyntactic property in more than one place in the word -- is accepted as normal, not in any way problematic, and the framework imposes on us no obligation to identify one of these realisations as, in some sense, the 'principal' one. To take a Latin example, in amāvī 'I have loved' the Perfective Aspect has as exponents not only the stem-forming affix -v- but also the 1st Person Singular termination -ī, since this termination is peculiar to the Perfective Present Indicative. Yet, if a 'pure' instance of Deviation II is found anywhere, it is surely found in amāvī. There is no synchronic difficulty in determining a boundary between the Perfective stem and the Person-Number ending; and this ending, being distinct from that which occurs in other Moods and Tenses, illustrates a one-to-many

relationship between a property and its realisations in the paradigmatic dimension. So it is clear that we must look elsewhere for a concept which will discriminate between $\text{-}\bar{\text{i}}$ and $\text{-}\bar{\text{a}}\text{v-}$ in the way we want.

The term 'formation' seems more promising. By relating this to a precisely defined notion of 'focal terms', Matthews attempts to capture rigorously that relationship between morphological processes and morphosyntactic properties which is traditionally expressed in statements such as: "vowel lengthening of the root is the formation (for a certain class of verbs) of the Perfective stem"; "the suffixation of $\text{-}\bar{\text{i}}$ to the verbal stem is one of three formations of 1st Singular (the others being the suffixation of $\text{-}\bar{\text{m}}$ and $\text{-}\bar{\text{o}}$)". In $\text{am}\bar{\text{a}}\bar{\text{v}}\bar{\text{i}}$, Matthews's definitions permit one to say (as one would wish) that the suffixation of $\text{-}\bar{\text{v-}}$ is a formation of the Perfective Aspect and that the suffixation of $\text{-}\bar{\text{i}}$ is a formation 1st Singular, but they do not permit one to say that the latter is a formation of the Perfective, because Perfective is not a 'focal term' with respect to 1st Singular forms of verbs. This, in turn, is because there are other 1st Singular suffixes which form complete verb-forms from Tense-Aspect stems according to rules which make no mention of the property Perfective -- for example, the suffix $\text{-}\bar{\text{o}}$ of the Imperfective Future $\text{am}\bar{\text{a}}\bar{\text{b}}\bar{\text{o}}$ or the $\text{-}\bar{\text{m}}$ of the Imperfective Present Subjunctive amem . To say that some morphological process P is an exponent of two properties A and B but is a formation of B only is akin, in Matthews's terms, to saying in Item-and-Arrangement terms that P is the allomorph of B which occurs in the context A. Yet our distinction between 'principal' and other exponents is, in a sense, no more than a restatement of the IA distinction between a morpheme which conditions and one which is conditioned. Could we, then, simply use Matthews's notion of 'formation' to do the job that our term 'principal exponent' is meant to do?

There is an obstacle to this. It can be shown that there are morphological processes which, in Matthews's terms, do count as formations of certain properties even though, in my terms, they

are not principal exponents of them. Consider first the example of the French Conditional parlerions and Future parlerons, discussed earlier. We did not want to call this an instance of 'pure' sensitivity on the part of 1st Plural to the properties Conditional and Future because the Plural suffixes -ons and -ions play an essential part in distinguishing Conditional from Future as well. Yet, within Matthews's framework, the suffixation of -er- certainly counts as a formation of the Conditional and Future stems; for there is no way of forming the stem in any Future or Conditional word-form without invoking a rule which mentions the appropriate Tense, and consequently that Tense is a 'focal term' with respect to that stem.⁷ Here is an instance, then, where reliance on Matthews's notion 'formation of' would require us to include in our study of Deviation II a piece of data which my notion 'principal exponent' would exclude -- and, for reasons already given, it is more appropriate to exclude than include it for our present purposes.

A second example will illustrate a different sort of difficulty. Let us suppose that, in Latin, the Perfective Present Indicative was the only Aspect-Tense-Mood combination in which the exponents of Person and Number took the form of suffixes, these properties being realised everywhere else in prefixes, thus:

(211) Perfective;

Present:		Past:	
Indic	Subjunc	Indic	Subjunc
amāv- <u>i</u>	m-amāveri	m-amāvera	m-amāvisse
amāv- <u>isti</u>	s-amāveri	s-amāvera	s-amāvisse
etc.	etc.	etc.	etc.

Consider now the form amāvī 'I have loved' in this imaginary pseudo-Latin. Just as in real Latin, the suffixation of -v- will count as a formation of the Perfective stem. But, in contrast to real Latin, the suffixation of -i will count as a formation of Perfective too. This is because there are no other 1st Singular suffixes which form complete verb-forms from Tense-Aspect stems according to rules which make no mention of the property Perfective -- indeed, there are, ex hypothesi, no other 1st

Singular suffixes at all. So, in this pseudo-Latin, Matthews would recognise two formations of Perfective in the word-form amāvī: not only the suffix -v- but also the suffix -ī. But, in one respect, pseudo-Latin and Latin are just alike: the suffix -v- occurs in all and only the non-participial Perfective forms of *amo*, while the suffix -ī occurs only in the 1st Singular Present Indicative form. So it still makes sense to call -v- a 'principal exponent' of Perfective; and it still makes just as much sense in pseudo-Latin as in real Latin to regard the idiosyncratic suffixal realisation of 1st Singular and the other Person-Number combinations in the Perfective Present Indicative as a 'pure' instance of Deviation II. So here is a second demonstration that Matthews's term 'formation' is wider than our term 'principal exponent' and, for our present purposes, too wide.

2.3 The Peripherality Constraint: a first statement

I presented at (206) a hypothetical Tense-Person-Number paradigm exhibiting only 'pure' sensitivity. This paradigm looked distinctly implausible: Yet this implausibility could not be due to the sheer quantity of sensitivity involved in it, I claimed, because just as much sensitivity seemed to be involved in the actual Latin verbal paradigm at (209). I will in fact argue that the implausibility of (206) is due to its violation of a constraint on sensitivity which has nothing to do with the volume of sensitivity in a paradigm. I suggest a name for this constraint and offer a first rough characterisation of it as follows:

(212) Peripherality Constraint (first formulation):

The realisation of a property may be sensitive to a property realised more centrally in the word-form (that is, closer in linear sequence to the root), but not to an individual property realised more peripherally (further from the root).

Inasmuch as this constraint refers to other properties realised in the same word-form as the property affected, it relates to the syntagmatic rather than the paradigmatic context of that property; in this respect it differs from the constraints

on one-to-many exponence that will be proposed and discussed in Chapters III-VI. The rest of this chapter will be devoted to justifying and developing the Peripherality Constraint and showing how it restricts the range of possible behaviour involving Deviation II.

2.4 Pure inward sensitivity

I will first show in what respects the hypothetical paradigm at (206) violates the Peripherality Constraint, and then justify the constraint by reference to evidence from actual languages. Consider first the realisations of Tense in (206), stated in (207 b). Tense is more central than Person, since it is realised closer to the root. Yet the realisation of individual Tenses is sensitive to individual properties realised more peripherally (for short, 'more peripheral properties'), namely 1st Person, 2nd Person and 3rd Person. This contravenes directly the Peripherality Constraint as stated at (212). If the Constraint is correct, therefore, we have at least part of an explanation for the implausibility of (206). What of the set of Latin forms at (209), which I introduced to compare with (206)? It may seem to present a clear counterexample to the Peripherality Constraint straight away. The realisation of properties belonging to the cumulated categories Person and Number, described in (210 d), is sensitive to Aspect, or to Aspect and Tense. Since Aspect and Tense are more central than Person and Number, this is quite compatible with the Peripherality Constraint. On the other hand, the realisation of Tense, stated in (210 c), is sensitive to a more peripheral combination of properties, in that the characteristic vowel for the Imperfective Future of regō is different in the 1st Person Singular (regam) from what it is in all the other Person-Number combinations (regēs, reget etc.). There is, fortunately, an independent explanation for this apparently damaging fact; but the explanation presupposes discussion of Deviation IV in Chapter VIII, so I will not be able to present it until Chapter IX. Until then, while recognising it as an apparent piece of direct counter-evidence to the Peripherality Constraint as stated at (212), I will put it on one side.

Turkish does not have arbitrary declension- and conjugation-types of the kind that we are familiar with in Latin, and Hungarian does so only to a relatively small extent. Yet both Turkish and Hungarian display plenty of examples within the verbal inflexional system of inward sensitivity (that is, sensitivity to more central morphosyntactic properties), these more central properties being syntactically or semantically determined ones such as Tenses and Aspects. This is obvious from the following tables, in which Present and Past Tense paradigms are contrasted:

(213)	Turkish <u>gel-</u> 'come'			
		Aorist Simple	<u>di-</u> Past Simple	
	Singular	1 gel-ir-im	gel-di-m	
		2 gel-ir-sin	gel-di-n	
		3 gel-ir	gel-di	
	Plural	1 gel-ir-iz	gel-di-k	
		2 gel-ir-siniz	gel-di-niz	
		3 gel-ir-ler	gel-di-ler	
(214)	Hungarian <u>vár-</u> 'wait'			
		Present Indefinite	Past Indefinite	Conditional Indefinite
	Singular	1 vár-ok	vár-t-am	vár-n-ék
		2 vár-sz	vár-t-ál	vár-n-ál
		3 vár	vár-t	vár-n-a
	Plural	1 vár-unk	vár-t-unk	vár-n-ánk
		2 vár-tok	vár-t-atok	vár-n-átok
		3 vár-nak	vár-t-ak	vár-n-ának

It is not important for our purposes precisely what Tense, Aspect or Mood properties are involved in the distinction between the various columns in (213) and (214). What matters is that (except in the Hungarian Present) these properties are unambiguously realised immediately to the right of the root by an element (-ir- or -di- in Turkish, -n- or -t- in Hungarian) which is constant for all Persons and Numbers and therefore plays no part in realising any individual Person-Number combination.⁸ These elements therefore count as 'principal exponents' of Tense-Aspect-Mood, according to the definition in section 2.1; and the associated Person-Number endings, insofar as their variation

in shape cannot be accounted for phonologically, display 'pure' sensitivity. One does not need to delve deeply into Turkish or Hungarian phonology to determine that phonologically unaccountable sensitivity of this kind exists. For example, the contrast in the 1st Person Plural between the -iz of Turkish geliriz and the -k of geldik cannot be plausibly accounted for either by positing a common underlying phonological representation from which the different surface forms are derived by phonological rules, or by positing distinct underlying representations whose distribution is phonologically determined; rather, there are distinct realisations of 1st Person Plural whose distribution is determined by other morphosyntactic properties realised elsewhere in the word. The same may be said about the contrast between the Hungarian várnának 'they would wait' and vártak 'they waited'. One might argue whether the realisation of Conditional is underlyingly -n- (as suggested in (214)), -na- or -ná- (i.e. /na:/); but the choice between these phonological analyses will not by itself account for the difference between the 3rd Plural endings -(á)nak in the Conditional and -ak in the Past. Rather, one must allow that there are distinct realisations for 3rd Person Plural which are sensitive to properties of Aspect, Mood or Tense, realised principally elsewhere.⁹

Having established that (213) and (214) illustrate pure sensitivity, we must now check whether this sensitivity is consistent with the Peripherality Constraint stated in (212). This means checking whether the properties to which Person and Number are sensitive are more central or more peripheral. In each case, the combination of Tense, Aspect or Mood properties which determines which Person-Number allomorph will be chosen is realised between the Person-Number ending and the root. These facts therefore tend to confirm the Peripherality Constraint.

A further example of pure sensitivity can be found in Zulu, this time in nominal morphology. Zulu, like other Bantu languages, has several noun 'Classes' or Genders; and, within the sentence, many attributive and predicative elements are re-

quired to agree with their subject or head noun by displaying a prefix generally similar in shape to the one on the noun itself. For example, in the possessive construction, the 'possessor' displays a prefix determined by the Class of the 'possessed' noun. In (215) I give a sample of Zulu nouns with the Class-prefix¹⁰ separated from the root by a hyphen, and in (216) I illustrate the possessive construction:

(215)	Singular	Plural	
Class 1/2	um-ntwana	aba-ntwana	'child'
1a/2a	u-thisha	o-thisha	'teacher'
3/4	um-nyango	imi-nyango	'door'
5/6	i-qanda	ama-qanda	'egg'
7/8	isi-hlalo	izi-hlalo	'seat'
9/10	in-tombi	izin-tombi	'girl'
11/10	u-thi	izin-ti	'stick'
14	ubu-so	ubu-so	'face'
15	uku-fa		'death'

(216) Class and Number
of head noun:

a. 3/4 Sg	umnyango womntwana	'the child's door'
5/6 Sg	iqanda lomntwana	' " egg'
7/8 Sg	isihlalo somntwana	' " seat'
11/10 Pl	izinti zomntwana	' " sticks'
14 Sg/Pl	ubuso bomntwana	' " face'
b. 3/4 Sg	umnyango wezintombi	'the girls' door'
5/6 Sg	iqanda lezintombi	' " egg'
7/8 Sg	isihlalo sezintombi	' " seat'
11/10 Pl	izinti zezintombi	' " sticks'
14 Sg/Pl	ubuso bezintombi	' " faces'

There is good evidence elsewhere in Zulu for a phonological contraction rule changing ai to e and au to o --- a rule which has parallels in the Romance languages and in Sanskrit. This suggests that we might derive the forms womntwana, wezintombi etc. etc. in the second column of (216) from an underlying representation incorporating the 'basic' forms of the possessor nouns given in (215), thus:

(217) Class and Number
of head noun:

3/4 Sg	a. wa + umntwana	b. wa + izintombi
5/6 Sg	la + "	la + "
7/8 Sg	sa + "	sa + "
11/10 Pl	za + "	za + "
14 Sg/Pl	ba + "	ba + "

This analysis is supported by what we observe in constructions where the analogue of the possessor (umntwana or izintombi in in (216)) happens to begin with a consonant (e.g. when it is a locative expression), thus:

(218) a. phakathi (kwebhokisi)	iqanda <u>laphakathi</u> (kwe- bhokisi)	'inside (the box)'	'the egg inside (the box)'
b. kwaZulu	izintombi <u>zakwaZulu</u>	'(in) Zululand'	'the girls in Zululand'
c. lapha 'here'	umnyango <u>walapha</u>		'the door here'

It is, in fact, usual to treat the first element in laphakathi (see (218 a)) as a 'possessive concord' morphologically identical to the first element in lomnyango or lezintombi (see (216)), and to analyse the underlying forms of the possessive concords for all the noun Classes as follows:

(219) Class	Singular	Plural
1/2	wa-	ba-
1a/2a	"	"
3/4	wa-	ya-
5/6	la-	a-
7/8	sa-	za-
9/10	ya-	za-
11/10	lwa-	za-
14	ba-	ba-
15	kwa-	

So far, the relationships between morphosyntactic properties and their realisations here seem quite straightforward. Just as we would regard the -a of piccola 'little' in the Italian phrase la piccola finestra 'the little window' as realising

the Gender property Feminine acquired in agreement with the head noun finestra 'window', so we can regard the la- of laphakathi in (218 a) and underlyingly present in lezintombi in (216 b) as realising the Class property 5/6. And just as we can draw up a Gender-Number paradigm for the Italian adjective piccolo as in (220), so we can draw up a Class-Number paradigm for a Zulu noun such as intombi 'girl' exhibiting all the possible possessive concords as in (221):

(220)		Singular	Plural
	Masculine	piccolo	piccoli
	Feminine	piccola	piccole

(221) a. Possessor noun Singular (intombi)

		Number of head noun:	
		Singular	Plural
Class of head noun:	1/2 } la/2a }	wentombi	bentombi
	3/4	wentombi	yentombi
	5/6	lentombi	entombi
	7/8	sentombi	zentombi
	9/10	yentombi	zentombi
	11/10	lwentombi	zentombi
	14	bentombi	bentombi
	15	kwentombi	

b. Possessor noun Plural (izintombi)

		Number of head noun:	
		Singular	Plural
Class of head noun:	1/2 } la/2a }	wezintombi	bezintombi
	3/4	wezintombi	yezintombi
	5/6	lezintombi	ezintombi
	7/8	sezintombi	zezintombi
	9/10	yezintombi	zezintombi
	11/10	lwezintombi	zezintombi
	14	bezintombi	bezintombi
	15	kwezintombi	

No sensitivity is observable in the way the concordial properties are realised, so far; we have had to posit only a single set of prefixes, listed in (219). But consider now the concordial

Class-Number paradigm for a Singular Class 1a noun such as uthisha 'teacher':

(222)		Number of head noun:	
		Singular	Plural
Class of head noun:	1/2 } 1a/2a }	kathisha	bakathisha
	3/4	kathisha	kathisha
	5/6	likathisha	kathisha
	7/8	sikathisha	zikathisha
	9/10	kathisha	zikathisha
	11/10	lukathisha	zikathisha
	14	bukathisha	bukathisha
	15	kukathisha	

The forms we would expect to see here, on the basis of (219) and (221), are: Class 1/2 Sg "wothisha", Pl "bothisha", 7/8 Sg "sothisha" and so on. The forms that we in fact find depart from what we would expect in two ways: firstly, the normal Class-prefix appropriate to Class 1a/2a Sg, namely u-, is replaced by ka-; secondly, the set of concords listed in (219) is replaced by a distinct set, as follows:

(223)		Singular	Plural
Class 1/2		∅	ba-
1a/2a		"	"
3/4		∅	∅
5/6		li-	∅
7/8		si-	zi-
9/10		∅	zi-
11/10		lu-	zi-
14		bu-	bu-
15		ku-	

There are clearly strong resemblances between the two sets of concords at (219) and (223). Nevertheless, the choice between them is certainly not phonologically determined; there is no phonological reason, for example, why the Class 9/10 Plural concord should be za- in izintombi zakwaZulu 'the girls of Zululand' (see 218 a)) but zi- in izintombi zikathisha 'the teacher's

girls'. Clearly, there is sensitivity at work here. The realisation of the concordial Class properties is sensitive to whether or not the noun to which they are attached is Class 1a/2a Sg. Whether this sensitivity is of the 'inward' kind, consistent with the Peripherality Constraint as so far formulated, depends on whether the properties 1a/2a Sg are realised more closely to the root than the concordial prefixes. Fairly evidently, they are. The element -ka-, found throughout the paradigm in (222), is in the pre-stem position habitually occupied by the exponents of the 'possessor' noun's Class and Number; moreover, it is unique to the combination of properties '1a/2a Sg' within the possessive paradigms of Zulu nouns, and so realises it unambiguously. These Zulu facts, therefore, like the Hungarian and Turkish ones we discussed, conform to the Constraint and hence tend to confirm it.

2.5

Outward sensitivity

Can we say, then, that 'inward' sensitivity -- sensitivity of more peripheral properties to more central ones -- is the only kind that exists, and that 'outward' sensitivity never occurs under any circumstances? Even if we continue to neglect the Latin 1st Singular Imperfective Future regam, mentioned earlier, the answer is no. The very facts we have just been considering, involving Zulu possessive concords, contain a clear prima facie example of 'outward' sensitivity. The usual Singular prefix for Class 1a/2a is, as I have said, u-: uthisha 'teacher', ubaba 'my father', udokotela 'doctor'. But this u- is replaced by ka- just when the noun acquires a concordial Class property through participating in a possessive construction¹¹; and, as (223) shows, this concordial property receives, for most Classes, an overt realisation which is more, not less, peripheral than the -ka- which is apparently sensitive to it. Nevertheless, I will argue that a quite restrictive, and so quite powerful, version of the Peripherality Constraint can still be maintained, because the conditions under which 'outward' sensitivity can occur are different from and more restricted than those under which 'inward' sensitivity can occur. To prepare the ground for this

argument, I will now present three more examples of 'outward' sensitivity.

The first of the three examples concerns the realisation of the property Potential in Turkish verbs. The Potential Mood (which can be glossed by English 'can', 'be able to') is usually realised by a suffix -ebil- ((224) below), to which the full range of Tense markers can be added:

(224) gel-dím 'I came'	gel-ebil-dím 'I was able to come'
gel-ecéğ-im 'I will come'	gel-ebil-ecéğ-im 'I will be able to come'
gel-ír-im 'I come (Aorist)'	gel-ebil-ír-im 'I can come'

But when the Negative suffix -me-/-miy- or the peculiar Negative Aorist forms ((113 a) in Chapter I) follow, the suffix -ebil- is replaced by -e-:

(225) gél-me-dim 'I did not come'	gel-é-me-dim (not "gel-ebil- me-dim") 'I could not come'
gél-miy-eceğ-im 'I will not come'	gel-é-miy-eceğ-im (not "gel- ebil-miy-eceğ-im") 'I will not be able to come'
gel-mém 'I do not come (Aorist)'	gel-é-mem (not "gel-ebil-mém") 'I cannot come'

So the property Potential is sensitive to the property Negative even though the property Negative is realised more peripherally.¹²

The remaining two examples were both mentioned as instances of Deviation II at (112) in Chapter I. The first of these, at (112 a), involved the Zulu verbal concord marker for Class I subjects in the Present Negative Tense; it was more fully described at (111 b), since it also involved Deviation I ('extended exponents'). Briefly, again, the usual Class 1 subject concord prefix on verbs, u-, is replaced by -ka- in Negative contexts; and the property Negative is generally realised separately by a prefix a-, preceding, and thus more peripheral than, the subject concord. The second relevant example from (112) is that of the property Plural in Hungarian nouns, mentioned at (112 c). Here,

the usual Plural marker $-k \sim -ok \sim -ak$ ¹³ is replaced by $-((j)a)i-$ when a Personal Possession marker such as $-m \sim -om \sim -am$ 'my' follows:

(226) Base	Unpossessed Plural	With 1st Singular Possessor Singular	Singular Plural
ruha 'dress'	ruhák	ruhám	ruháim (not "ruhákam")
kalap 'hat'	kalapok	kalapom	kalapjaim (not "kalapokam")
ház 'house'	házak	házam	házaim (not "házakam")

This characteristic of Hungarian -- that the Plural suffix on nouns differs in shape according to whether a Possessive suffix follows or not -- is evidently ancient and stable, since it is shared with the related but geographically far distant Ugric languages Ostyak and Vogul, which also display a similar variability in the suffix for the Dual, absent in Hungarian (Gulya 1966: 52, 58-63; Kálmán 1976: 29-32).

To show that some version of the Peripherality Constraint can be defended despite these examples of 'outward' sensitivity, I must at least show that they differ in some common respect from the examples of 'inward' sensitivity that we have so far noticed. This common difference, I suggest, involves a distinction between what one might call 'piecemeal' sensitivity to individual morphosyntactic properties and 'wholesale' sensitivity to whole morphosyntactic categories. In the Peripherality Constraint as so far formulated, no connexion is claimed between the way in which a given property (property \underline{a}) is realised in the context of property \underline{c}_1 belonging to some category C and the way in which \underline{a} is realised in the context of other properties ($\underline{c}_2, \underline{c}_3$ etc.) also belonging to category C. So far as inward sensitivity is concerned, we have found no ground for asserting any such connexion. But in all the examples of outward sensitivity so far observed, a connexion of this kind does appear to exist, in that the exponent of \underline{a} in the context of the more peripheral property \underline{c}_1 to which \underline{a} is sensitive always appears to be the same as its exponent in the context of properties $\underline{c}_2, \underline{c}_3$

and indeed all the properties belonging to category C. This observed connexion could be accidental. But before resting content with that conclusion, we ought at least to explore the possibility that it is not accidental -- in other words, that it represents a special condition on outward sensitivity, in contrast to inward sensitivity. Loosely, what we should explore is the possibility that outward sensitivity may occur, violating the current version of the Peripherality Constraint, only when that violation involves sensitivity to a whole morphosyntactic category rather than to individual properties. Before discussing evidence suitable to test this tentative special condition, I will offer a more formal statement of it, in two versions:

(227) Special condition on 'outward' sensitivity

The realisation of a given property may be sensitive inwards to individual properties (or combinations of them; see Note below) in such a way that the alternant which occurs with property c_1 differs from the one which occurs with property c_2 belonging to the same category; on the other hand, a property may be sensitive outwards to a given property c_1 only if the same alternant occurs with:

- (Version A:) all properties belonging to the same more peripheral category as c_1 .
- (Version B:) all properties belonging to the same more peripheral category as c_1 which have an overt realisation independent of the realisation of the sensitive property itself.

Note: 'Property' is to be understood throughout as including property combinations (e.g. '1st Person Singular'); and 'category', similarly, is to be understood as including category combinations (e.g. 'Person-Number').

The choice between Version A and Version B will occupy us as soon as we consider the Condition in detail. But first I will demonstrate briefly the correctness of exempting inward sensitivity from the Condition, by reference to Latin and Hungarian evidence

showing how properties may be sensitive 'inwards' in different ways to different individual properties within the same category.

If we consider the Latin data illustrated in (209), we observe that several Person-Number combinations are sensitive to the distinction between the individual properties Imperfective and Perfective within the category Aspect in such a way as to show different alternants according to which of the two Aspects is present, thus:

	(228) Present:	
	Imperfective	Perfective
Sg 1	reg-ō	rex-ī
2	reg-is	rex-istī
Pl 2	reg-itis	rex-istis
3	reg-unt	rex-erunt

The exponents of Person-Number are the elements which follow the hyphens; and, although there is partial similarity between (for example) -unt and -erunt, the two exponents of each Person-Number combination here are certainly not identical. So, whatever decision one makes about the underlying phonological representations of these forms, the recognition of morphological sensitivity is inescapable; and, what is most important at present, the sensitivity is to individual properties within the category Aspect rather than to the category as a whole.

The Hungarian data at (214) illustrate similar behaviour. For the 3rd Person Plural Indefinite, the Conditional (whose principal exponent is -n-, -na- or -ná-) selects a different alternant from the Past Indicative (whose principal exponent is -t-); with the former, 3rd Pl Indef is realised as -(á)nak while with the latter it is realised as -ak; várnának 'they would wait' versus vártak 'they waited'. Here, sensitivity is to individual combinations of properties within the categories of Mood and Tense rather than to the categories as a whole. And, of course, it is not only Person and Number which can display inward sensitivity of this kind. Consider the following further data from Latin:

(229)		Mood: Subjunctive	
		Tense: Past	
		Aspect:	
		Imperfective	Perfective
	Singular 1	reg-er-em	rex-iss-em
		2 reg-er-ēs	rex-iss-ēs
		3 reg-er-et	rex-iss-et
	Plural 1	reg-er-ēmus	rex-iss-ēmus
		2 reg-er-ētis	rex-iss-ētis
		3 reg-er-ent	rex-iss-ent

The Person-Number endings here are the same, but the realisation of Past-Subjunctive is sensitive inwards to the two individual Aspects, Perfective and Imperfective, whose principal exponent is more central (namely in the stem, to the left of the hyphen).

I will illustrate now the conditions under which, according to (227), 'outward' sensitivity is permissible. First, consider the facts about the marking of Number and Personal Possession on Hungarian nouns ((112 c) and (226)). As I mentioned, the realisation of the property Plural is sensitive outwards to whether or not some marker of Possession is also present. But in (226) I gave only examples of 1st Person Singular Possession. Something new emerges if we look at a complete paradigm for Possession:

(230)	Possessor	Singular	Plural	
	Sg 1	ruhám 'my suit'	ruhám 'my suits'	(cf. ruhák 'suits')
		2 ruhád	ruháid	
		3 ruhája	ruhái	
	Pl 1	ruhánk	ruháink	
		2 ruhátok	ruháitok	
		3 ruhájuk	ruháik	
	Sg 1	kalapom 'my hat'	kalapjaim 'my hats'	(cf. kalapok 'hats')
		2 kalapod	kalapjaid	
		3 kalapja	kalapjai	
	Pl 1	kalapunk	kalapjaink	
		2 kalapotok	kalapjaitok	
		3 kalapjuk	kalapjaik	

What I want to emphasise here is the fact that, although Plural is realised differently according to whether or not a marker of Possession is present, it is realised in the same way with all Possessors. One could easily imagine a hypothetical set of forms like the following:

(231) Possessor	ruhák	kalapok
Sg 1	ruháim	kalapjaim
2	"ruhákod"	"kalapokod"
3	ruhái	kalapjai
Pl 1	ruháink	kalapjaink
2	"ruhákotok"	"kalapokotok"
3	ruháik	kalapjaik

In this hypothetical set of forms, it is only with certain Possessors (namely, 1st and 3rd Person ones) that the realisation of Plural differs from its 'unpossessed' realisation. There is nothing implausible, in principle, about selective sensitivity of this kind; we have already seen that, in Latin, only four of the six Person-Number combinations are realised differently with the Imperfective and Perfective Present, namely 1st Sg, 2nd Sg and Pl, and 3rd Pl. The fact that Hungarian does not display selective sensitivity here could be a mere fact of Hungarian grammar, accidental from a general linguistic point of view. But, if the condition on sensitivity set out at (227) is correct, then this fact about Hungarian is not accidental, and the sort of behaviour illustrated in the hypothetical examples at (231) is linguistically impossible. This is because in (231) the property Plural is sensitive outwards to properties of Personal Possession in such a way that different alternants for Plural accompany different Persons within the Personal Possession category.¹⁴

Beja, a North Cushitic language of the Sudan and northern Ethiopia, behaves in one respect remarkably like Hungarian. As in Hungarian, there are six Personal Possessive suffixes which may be added to nouns (Hudson 1974: 123). Beja also has a set of three 'inseparable postpositions' meaning 'in, about', 'like', and 'from, by', which may be added to nouns with Genitive inflexion.

Hudson calls these postpositions inseparable because they precede (and are thus, in my terminology, more central than) any Personal Possessive suffixes attached to the same noun. What is interesting about these three 'postpositions' is that, just like the Plural marker in Hungarian, they each have two allomorphs, the choice between them depending on whether a Possessive suffix follows. Thus, 'from, by' may be realised either -' (that is, by an accent on the preceding vocalic mora) or -s-, the latter occurring if and only if there is a following Possessive suffix (preceded necessarily by a Case-Number suffix, which in this instance will be Accusative). Examples are:¹⁵

- (232) a. ti- ?oor-t- i- /
the-girl-Fem-Gen-from
'from the girl'
- b. ti- ?oor-t- ii- s- oo- 'k
the-girl-Fem-Gen-from-Acc-your (Sg)
'from your daughter'
- (233) a. ti- huus- aa-t- e- /
the-knife-Pl-Fem-Gen-from
Pl
'from the knives'
- b. ti- huus- aa-t- ee- s- ee- 'k
the-knife-Pl-Fem-Gen-from-Acc-your (Sg)
'from your knives'

In (227) I set out two distinct versions, labelled A and B, of the special condition on outward sensitivity. Version A was the more restrictive, requiring that, if a property was sensitive to some more peripheral property c_1 , it should be sensitive in just the same way to all properties belonging to the same category as c_1 . Version B, on the other hand, required only that it should be sensitive in the same way to all those properties which both belong to the same category as c_1 and have an independent overt (i.e. non-zero) inflexional realisation. Obviously, Version A is more restrictive than Version B, and, if consistent with the evidence, should be preferred. What conclusion does the evidence in fact suggest?

Let us look again at the property Potential in Turkish verbs, already mentioned at the beginning of this section. As

we saw, Potential has two realisations, -ebil- and -e-, the choice between which is determined by whether or not the property Negative follows. Let us suppose that the property Negative is a member of a two-term morphosyntactic category (let us call it Polarity) whose other member is the property Positive. Version A of (227) cannot then be correct; for it will lead us to predict, incorrectly, that the same realisation of Potential will occur in both Positive and Negative contexts. But the property Positive never has any independent overt realisation of its own; consequently, the Turkish facts, though incompatible with Version A, are compatible with Version B, since the same realisation of Potential does in fact occur in association with all members of the category Polarity whose realisation is independent and overt, namely the one property Negative. Consider too the facts about Class-concord and verbal negation in Zulu mentioned earlier in this section and at (112 a) in Chapter I. To recapitulate, the usual Class 1 Singular Subject Concord verbal prefix u- is replaced by -ka- in Negative contexts. If, as for Turkish, we say that Negative is the second member, alongside Positive, of a two-member morphosyntactic category of Polarity, then the Zulu facts are incompatible with Version A of (227) but compatible with Version B; for, although the Class 1 concord is not realised in the same way with both members of this more peripheral category, it is realised consistently when accompanied by all members of it which have non-zero realisations, namely the one member Negative.

These facts seem to exclude fairly conclusively the possibility that Version A of the special condition on outward sensitivity can be sustained. But they do so only on the assumption that there is, in both Turkish and Zulu, a two-term morphosyntactic category of 'Polarity' of which Positive is a member just as much as Negative is, even though only Negative ever receives any independent overt inflexional realisation. What happens if we question that assumption? If we deny that there is any property Positive (at least, none relevant for morphosyntactic purposes), we are then left with a morphosyntactic category with only one member (Negative). To some people the idea of single-member cate-

gories may seem objectionable on the ground that all grammatical entities, including morphosyntactic properties, exist only inasmuch as they contrast with other entities in systematic ways. But I would like to leave that objection on one side for the moment and concentrate on the consequences of this analysis for outward sensitivity. One important consequence, for our purposes, is quite clear: the Turkish and Zulu facts are now compatible with Version A as well as Version B. This is because we can now say that the realisation of Potential and of Class 1 concord respectively is sensitive not merely to the more peripheral property Negative but rather to a more peripheral category, which we have called 'Polarity', whose sole member happens to be Negative. Given this analysis of the Turkish and Zulu facts, therefore, it is possible to maintain the more restrictive of the two conditions on outward sensitivity -- Version A rather than Version B in (227). I will aim now to produce positive evidence in favour of Version A -- evidence strong enough to outweigh any qualms we may have about recognising such things as one-member morphosyntactic categories.

The logical relationship between Versions A and B is such that any facts which comply with A will comply with B also. It will not be possible, therefore, to produce evidence in favour of A which at the same time conclusively excludes B. It is in principle possible, however, to find evidence in favour of B which conclusively excludes A. What we need to do is to specify the circumstances under which evidence of this kind might be found, then to examine whether, in these circumstances, such evidence is actually found in natural languages. The Turkish and Zulu facts involving negation did not conclusively favour B because the argument drawn from them involved the dubious assumption of a two-member morphosyntactic category of Polarity one of whose members (Positive) never had an independent overt realisation. Any conclusive evidence for B must involve a morphosyntactic category whose existence and membership is less debatable; for example, the category in Zulu whose members are the noun Classes and which is applicable to 'possessor' nouns in possessive cons-

tructions (see section 2.4 above), and the category of Possession in Hungarian whose members are the six Person-Number combinations and which is applicable to 'possessed' nouns (see our discussion of (230) above). What is interesting here about both these sets of properties is that some of them, in some circumstances, lack any independent overt realisation. They therefore supply instances where the choice between Version A and Version B could, in principle, be decided conclusively in favour of Version B. A recapitulation of the facts will clarify this.

In Zulu, 'possessor' nouns regularly acquire, in addition to their own Class prefix, an extra prefix governed by the Class of the 'possessed' noun which they qualify:

(234) (cf. (216))

- a. Class 1/2 Sg umntwana 'child'
 Class 3/4 Sg umnyango 'door'
 Possessive construction:
 umnyango wmntwana 'the child's door'
- b. Class 9/10 Pl izintombi 'girls'
 Class 5/6 Sg iqanda 'egg'
 Possessive construction:
 iqanda lezintombi 'the girls' egg'

If we invoke an independently motivated phonological contraction rule ($a + i \rightarrow e$, $a + u \rightarrow o$), we can set up underlying representations for the extra prefixes as in (219), repeated here for convenience:

(219) Class	Singular	Plural
1/2 } 1a/2a }	wa-	ba-
3/4	wa-	ya-
5/6	la-	a-
7/8	sa-	za-
9/10	ya-	za-
11/10	lwa-	za-
14	ba-	ba-
15	kwa-	

But for 'possessor' nouns which are of Class 1a and Singular (e.g. uthisha 'teacher'), a different set of extra prefixes applies,

as set out in (223):

(223) Class	Singular	Plural
1/2 } 1a/2a }	∅	ba-
3/4	∅	∅
5/6	li-	∅
7/8	si-	zi-
9/10	∅	zi-
11/10	lu-	zi-
14	bu-	bu-
15	ku-	

Of special interest to us now are the five zeros in (223), indicating forms where concord with the 'possessed' noun has no independent overt realisation, even though the morphological relevance of such concord, and so the presence of the relevant morphosyntactic properties, is guaranteed by the overt marking which shows up with possessors of all other Classes. What is crucial is the shape of the inherent Class 1a Singular prefix in these five forms -- forms where the prefix is 'preceded by' a 'zero-marker' of the Class of the possessed noun. We know that the normal Class 1a Singular prefix, in environments where there is no 'extra' prefix and no other special factors affecting its shape, is u-.¹⁶ In contrast, where there is an overt 'extra' prefix, as (for example) with a 'possessed' noun of Class 7/8, the Class 1a Singular prefix is not u- but ka-, thus:

(235) isi-hlalo	si-	ka-	thisha	
7/8-seat	7/8-la/2a-	teacher		
Sg	Sg	Sg		'the teacher's seat'
izi-hlalo	zi-	ka-	dokotela	
7/8-seat	7/8-la/2a-	doctor		
Pl	Pl	Sg		'the doctor's seats'

But what happens when the 'possessed' noun belongs to a Class and Number for which table (223) specifies 'zero', such as umnyango (Class 3/4 Sg) 'door' or amaqanda (Class 5/6 Pl) 'eggs'? In such instances, is Class 1a Sg realised as u-, ka- or in some other way?

If the realisation is u-, then these Zulu data will strongly support Version B rather than Version A, in that the realisation

chosen for environments with 'zero-marked' Possessive concord will be different from the one found with non-zero concords. But we already know from table (222) that the realisation is ka, just as when an overt 'extra' prefix is present, thus:

(236)	um-	nyango	ka-	thisha	
	3/4-	door	la/2a-	teacher	
	Sg		Sg		'the teacher's door'
	ama-	qanda	ka-	dokotela	
	5/6-	egg	la/2a-	doctor	
	Pl		Sg		'the doctor's eggs'

Now, this situation is not incompatible with Version B, since Version B permits but does not require a Class prefix other than ka- in these forms; but, inasmuch as the facts are compatible with the more restrictive Version A, which forbids any prefix other than ka- here, we should choose the version of the Periphrality Constraint which incorporates Version A, other things being equal.

Whether other things are equal depends, of course, on what happens in analogous situations in other human languages. Here, I will do no more than discuss nominal Possession in Hungarian, the second of the two examples mentioned as relevant earlier. The relevant facts are given at (230). The Possessive suffixes for Singular nouns which end in a back vowel are as follows:

(237)		Singular	Plural
	Person: 1	-m	-nk
	2	-d	-tok
	3	-ja	-juk

These are exemplified in the Possessed forms of the Singular of ruha 'dress, suit' in (230): ruhám 'my dress' etc. Now, the Plural stem for the Possessed forms of all nouns also ends in a vowel, since the usual Plural marker -k ~ -ok ~ -ak is replaced by -((j)a)i when a Possession marker follows (see (226)). One might therefore expect the Possessed forms of all nouns in the Plural to show a stem in -((j)a)i followed by the suffixes listed in (237). But this is not the case. As we can see from the Plural columns for both ruha and kalap in (230), the actual

Possessive suffixes for Plural nouns are as follows:

(238)	Singular	Plural
Person: 1	-m	-nk
2	-d	-tok
3	∅	-k

There is a difference between (237) and (238) in the 3rd Person forms. What interests us is the form for the 3rd Person Singular. Here, there is no overt realisation of Person independent of the realisation of the property Plural belonging to the noun. The question, then, is how that property Plural is realised: by a -k suffix, as when a noun is not marked for Possession at all, or by the same -((j)a)i suffix that appears before all overt markers of Possession? If the first answer is correct, then the Hungarian data provide strong support for the looser Version B of the special condition on outward sensitivity and against Version A. But the correct answer is the second, as shown by the forms ruhaj 'his suits/her dresses' and kalapjai 'his/her hats' in (230). Again, therefore, we have evidence which, although consistent with Version B, is exactly what Version A predicts, and so favours the latter.

2.6 The Peripherality Constraint: a revised formulation and some consequences

The Zulu and Hungarian data just discussed give grounds for preferring Version A, the tighter of the two versions of the special condition on outward sensitivity stated at (227). The correctness of this, as of my suggestions about sensitivity in general, depends of course on whether it fits relevant facts from far more languages than the four that I have concentrated on (Zulu, Hungarian, Turkish and Beja). I will in fact have more to say later about the interpretation of the Peripherality Constraint in connexion with paradigms where not all the sensitivity is 'pure'. But I will assume for the moment that Version A is adequately substantiated, and discuss now firstly its consequences for the formulation of the Peripherality Constraint and secondly (very briefly) the potential relevance of this Constraint to syntactic as well as morphological analysis.

The first tentative formulation of the Peripherality Constraint was given at (212). For convenience, I will repeat it here, along with the formulation of the special condition on outward sensitivity at (227):

(212) Peripherality Constraint (first formulation):

The realisation of a property may be sensitive to a property realised more centrally in the word-form (that is, closer in linear sequence to the root), but not to an individual property realised more peripherally (further from the root).

(227) Special condition on 'outward' sensitivity

The realisation of a given property may be sensitive inwards to individual properties (or combinations of them) in such a way that the alternant which occurs with property c_1 differs from the one which occurs with property c_2 belonging to the same category; on the other hand, a property may be sensitive outwards to a given property c_1 only if the same alternant occurs with:

- (Version A:) all properties belonging to the same more peripheral category as c_1 .
- (Version B:) all properties belonging to the same more peripheral category as c_1 which have an overt realisation independent of the realisation of the sensitive property itself.

It would be a straightforward matter to combine (212) and Version A of (227). To some extent, indeed, Version A is foreshadowed in (212) by the reference to 'an individual property realised more peripherally'; by implication, a property might be sensitive to something other than an individual property, such as a group or class of properties. In (227) it is made plain that the group or class in question is a morphosyntactic category (or, more strictly, the set of properties belonging to a morphosyntactic category). But simply to combine (212) and (227) in the manner suggested would not be entirely satisfactory. We have assumed until now that all morphosyntactic properties must belong to some morphosyntactic category, just as (to use standard examples)

Accusative in Latin belongs to the category Case and Past to the category Tense. But we saw in our discussion of certain Negative verb-forms in Turkish and Zulu that, if Version A of (227) were to be preferred to Version B, the morphological facts would prevent us from recognising any morphosyntactic property Positive in paradigmatic contrast with Negative within a category of Polarity. A consequence of this for morphological theory is that we must choose between retaining the formulation of Version A given at (227) at the cost of recognising single-member categories, and revising the formulation so as to cater for the possibility that the conditioning property does not belong to any category at all. I cannot envisage any morphological behaviour that, if observed, would enable us to discriminate between these two alternatives. The choice must be made on grounds of elegance or simplicity rather than empirical evidence. To my mind, the simpler solution is the second -- that of recognising properties which do not belong to any category -- because it enables us to avoid having to incorporate into the description of individual languages entities, such as the category of Polarity in Turkish and Zulu, which do no work in the description except to ensure compliance with a certain theoretical requirement (namely, that all morphosyntactic properties should belong to some category or another). It seems better to apply Occam's Razor, dispensing with both the entities and the requirement, so that a reformulation of Version A will be needed.

Inasmuch as no empirical consequences for morphological behaviour flow from it directly, the choice between the two formulations of Version A may seem unimportant. However, the choice between Version B and Version A (in either formulation) has syntactic consequences which are potentially more substantial. As befits the subject-matter of this thesis, I have concentrated throughout on the morphological aspect of morphosyntactic properties, above all their realisation through inflexions. In general, I have simply taken for granted the pro-

properties relevant to each set of data considered, and, insofar as I have discussed their justification, I have done so on morphological grounds. But, of course, morphosyntactic properties have their syntactic side too. It is reasonable to ask, for example; what constitute the syntactic (as opposed to morphological) grounds for recognising (say) a category Tense or property Future Tense in a given language? Even to begin to try to answer such questions fully here would take us far away from the topic of constraints on sensitivity. Fortunately, there is no need for us to do so. What I want to suggest here is that, if something like Version A of (227) is correct, then some imaginable combinations of syntactic and morphological behaviour which at first sight may seem quite plausible or innocuous are in fact impossible, and morphological evidence can in principle count in favour of one possible syntactic analysis over another in rather surprising ways.

Impossible combinations of morphological and syntactic behaviour, on the assumption of Version A, would occur in situations in which there is conclusive syntactic evidence in favour of a certain property whose existence Version A precludes. In our earlier Turkish and Zulu examples, where Version A precluded the recognition of a property Positive, there was no strong countervailing syntactic evidence. But it is quite easy to devise an imaginary situation where such evidence exists. Let us imagine a language L with, *prima facie*, three Tenses (Past, Present and Future) limited to 'finite verbs', and a 'sequence of tenses' rule affecting the Tense of verbs in subordinate clauses embedded under certain main-clause verbs in the Past tense. (L might, in fact, resemble English, with its restrictions on Tense in 'reported speech'.) Because of this effect on subordinated verbs, there can be no doubt about the syntactic, as opposed to purely semantic, relevance of the property Past Tense. Let us suppose also, however, that L has two Aspects, Perfective and Imperfective, applicable not only to finite verbs but also to non-finite forms such as infinitives and participles; and that the realisation of the Perfective Aspect is sensitive to its grammatical context as illustrated in (239), which also shows the realisation

of the Tenses:

(239) a. Order of realisation of categories:

Verb stem + Aspect (+ Tense)

b. Realisation of Aspect:

Imperfective	-ba-
Perfective	-ka- in Tensed contexts
	-la- elsewhere

c. Realisation of Tense:

Present	-ti
Past	∅
Future	-mi

There are two points to note here. Firstly, there is no independent overt realisation of Past Tense, so its existence rests solely on the syntactic evidence. Secondly, the property Imperfective is sensitive outwards, but this sensitivity is consistent with Version A of (227) because it is the same for all members of the category Tense. Consider now, however, a hypothetical language L' which differs from L superficially in only one small respect, namely that the realisation of the Perfective Aspect is as in (240) rather than (239):

(240) Realisation of Aspect:

Perfective	-ka- in Present and Future contexts
	-la- elsewhere

The set of Perfective endings in the two languages will include the following respectively:

(241)	L	L'
Present	-kati	-kati
Future	-kami	-kami
Past	-ka	-la
	etc.	etc.

The morphological facts of L are compatible with both Version B and Version A of the special condition. The facts of L', on the other hand, are compatible with Version A only if we alter our description in one crucial respect: we posit a category Tense in L' containing only the two properties Present and Future, and no longer including Past. But this alteration runs directly

counter to the syntactic facts which we have postulated for both L and L', which support conclusively the existence of a property Past. A logical consequence of Version A, therefore, is that although L, as described, is a possible human language, L' is not, despite the apparently trivial nature of the difference between them.

In actual languages the facts (both morphological and syntactic) are likely to be less clear-cut than in our hypothetical examples, and the choice between competing analyses less obvious. It is this very fact, however, which makes the Peripherality Constraint (incorporating Version A of the special condition) a potentially useful tool in syntactic analysis, as well as a strong generalisation about inflexion. Suppose the syntactician is hesitating between competing analyses, and one of these analyses crucially involves positing a morphosyntactic category C one of whose constituent properties (call it c_1) sometimes or always lacks an independent overt realisation. If some more central morphosyntactic property displays outward sensitivity, it will be of interest to check how this property is realised in those environments where property c_1 is purportedly present. If the realisation when c_1 is present differs from what it is when other properties c_2, c_3 etc. also purportedly belonging to C are present, then something is wrong with either the syntactic analysis in question or with the Peripherality Constraint as formulated.

I can think of no actual syntactic dilemma which morphological considerations could immediately help to resolve in the fashion just outlined. But the Peripherality Constraint does perhaps bear on one general question of syntactic theory. In some theoretical approaches, if a given syntactic category (such as Tense) is applicable to a given unit of structure (such as Verb Phrase), then every instance of that unit of structure must be specified for some 'value' of that category, or, in our terms, must display some property belonging to that category. (This kind of approach is, I think, characteristic of M.A.K. Halliday's

'systemic' grammar.) Under this approach it would not be possible for a Verb Phrase (using the same example) to be simply unspecified for Tense. Yet, at the rank of the word, what I have proposed implies that such lack of specification is possible. For example, my discussion of the Zulu possessive construction, as exemplified in (216) and (234), does not presuppose any category of what we might call 'Possessorhood' for which all nouns must be specified even when not acting syntactically as a possessor. For example, in (234 a) the category 'Possessorhood' may be said to be applicable to womntwana 'of the child', which displays the property 'Possessor of a Class 3/4 Sg item' in agreement with umnyango 'door', but Possessorhood is not applicable to umnyango itself; we do not have to assign to umnyango a property (say) 'Non-Possessor' coordinate with all the concordial properties, and indeed we must not assign it such a property if Version A is correct. One can summarise the difference between my approach and the approach more or less explicit in systemic grammar by saying that I permit a word-form to be unspecified for some category applicable to its word-class whereas according to the latter all word-forms must be fully specified for all such categories. And, insofar as there is inflexional evidence in favour of my approach, there is, I suggest, inflexional evidence against at least the 'full-specification' aspect of the systemic approach to syntactic properties.

Admittedly, the defender of 'systemic' syntax (or relevantly similar syntactic frameworks) might perhaps argue that the syntactic properties that figure in his analyses are just not the same sort of thing as -- are, in fact, more abstract than -- the morphosyntactic properties which are realised by inflexions, and that my morphologically-based criticism of his syntactic framework is therefore beside the point. I certainly would not claim that the argument is conclusive, especially given the tentative nature of the morphological generalisation on which it is based. I have outlined the argument here mainly in order to illustrate one way in which general conclusions about inflexion might have

at first sight rather unexpected repercussions outside morphology, and how therefore morphological evidence can in principle contribute to syntactic debate.

It is time now to put forward a more precise formulation of the Peripherality Constraint, combining it with the special condition on outward sensitivity -- a reformulation deferred at the beginning of this section. The statement below assumes the possibility that there may be isolated morphosyntactic properties which are not members of any category (although, as I have said, there may be little to choose between this assumption and that of 'singleton' categories). The term 'principal exponent', absent from (212) and (227), is included in order to emphasise a point which has been assumed throughout, namely that the Constraint is for the time being intended to apply only to 'pure' sensitivity -- to instances where Deviation II is contaminated as little as possible by Deviation III. This is a point to which I will return in section 2.9 below, when discussing a potential counter-example to the revised version. Meanwhile, the version I propose is:

(242) Peripherality Constraint (revised version):

The realisation of a property a may not be sensitive to a property c which has a principal exponent more peripheral in the word-form ('outward sensitivity') unless, if c belongs to a category C, the same exponent of a occurs with all other properties belonging to C.

The main substantive difference between (242) and the earlier formulation at (212) (as supplemented by (227)) lies in the phrase "if c belongs to a category C". This allows for the possibility that a morphosyntactic property may be isolated, belonging to no category, like (I have suggested) the property Negative in Turkish and Zulu verb inflexion.

The revised version of the Peripherality Constraint at (242), despite the length of the discussion that has led up to it, cannot be the final version. There is one assumption impli-

cit in (242) which is inconsistent with the inflexional behaviour of some languages; and there are other problems which can be foreseen in applying it. Finally, we have still to deal with the case of Latin regam 'I shall rule', mentioned as a potentially troublesome instance of outward sensitivity in section 2.4. The first set of problems will be the subject of the next three sections; but a solution to the problem of regam must (as I have said) await our discussion of homonymy in Chapters VIII and IX.

2.7 A problem in the operation of the Peripherality Constraint: inconstancy of order in realisation

I will deal first with a problem in applying the revised Peripherality Constraint which is connected with the order of realisation of properties. At (207), (210) and (239) the existence of a heading entitled 'Order of realisation of the categories' implies that, whatever combination of properties from the various applicable categories is chosen, their order of realisation (or at least that of their principal exponents, if any) will be the same. This is indeed much the most frequent state of affairs, seemingly. If we examine a Latin Active verbal paradigm, we can broadly assign each of the categories Aspect, Tense, Mood, Person and Number to one of three positions, thus: Aspect + $\left(\begin{array}{c} \text{Mood} \\ \text{Tense} \end{array} \right)$ + $\left(\begin{array}{c} \text{Person} \\ \text{Number} \end{array} \right)$. There is no combination of properties from these categories which exemplifies a different order of realisation, e.g. $\left(\begin{array}{c} \text{Mood} \\ \text{Tense} \end{array} \right)$ + Aspect + $\left(\begin{array}{c} \text{Person} \\ \text{Number} \end{array} \right)$. On the other hand, in Huave, a language of the Isthmus of Tehuantepec in Mexico, the order of realisation of verbal properties such as 1st Person and Past Tense depends partly on the arbitrary conjugation-type to which the verb belongs and partly on the combinations in which they occur, irrespective of conjugation-type (Stairs & Hollenbach 1969). The question for us is: how does this variability affect the operation of our constraint on outward sensitivity? For example, if a property a is sensitive outwards to a property c₁ belonging to a category C, must a have the same exponent when accompanied by properties c₂ and c₃ also belonging to C even if c₂ and c₃

(unlike c_1) are more central than a ? If so, then the formulation of the Peripherality Constraint given in (242) can stand, in this respect. If, on the other hand, we find that a has the same realisation only when accompanied by those members of C which are more peripheral than it, then (242) needs amending accordingly.

Although the verbal morphology of Huave involves a considerable amount of sensitivity, including some outward sensitivity, the facts there do not point clearly towards either of the two alternatives just presented. This is partly because the relevant contrasts in order of realisation involve not just permutation of prefixes or suffixes on one side or other of the root but permutation around the root, and partly because of difficulty in identifying 'principal exponents', since much of the sensitivity involved is not 'pure'. The first set of data that I will present consists of some partial Tense paradigms belonging to the most productive conjugation-type:

(243) -ndeak 'speak':

	Indicative:			Subordinate:
	Present	Past	Future	
Person: 1	sandeak	tandeakas	sanandeak	nandeak 'that I speak'
2	indeak	tendeak, tindeak	apmendeak, apmindeak	mendeak
3	andeak	tandeak	apmandeak	mandeak

Stairs and Hollenbach analyse the property 1st Person as realised by the prefix sa- in the Present Indicative, by na- in the Future Indicative and Subordinate, and by the suffix -as in the Past Indicative. This realisation seems to involve outward sensitivity, in that the property Future which triggers the realisation -na- for 1st Person is realised more peripherally, not more centrally. One might therefore expect, on the basis of the Peripherality Constraint, to find the same realisation -na- in all Tenses. But, in my terminology, the sensitivity displayed by the 1st Person in the future is not 'pure', since the element sa- which Stairs and Hollenbach regard as being, in effect, the exponent of Future only, appears not to occur with any other Person; so in

the form sanandeak 'I will speak', one could, on the face of it, regard sa- as an exponent of 1st Person just as much as -na- is.¹⁷

The best reason for not doing so, and for following Stairs and Hollenbach in locating the realisation of Person in the second of the two prefixes, seems to come from comparing the Future with the Subordinate forms; the former look as if they are derived from the latter by the addition of a prefix (sa- or ap-), which can therefore perhaps be legitimately regarded as realising only the property Future (albeit in inwardly sensitive fashion).¹⁸ But this analysis at the same time points towards a distinction between Future on the one hand and Present and Past on the other, in that neither Present nor Past has any special morphological connexion with the Subordinate forms of the verb -- a distinction which casts some doubt on the validity of the three-member category Tense implied in (243). The upshot is that, whether or not -na- is the sole exponent of 1st Person in the Future form sanandeak, the fact that the 1st Person Past is tandeakas rather than, say, "tandeakna" may have no relevance for any constraint on outward sensitivity.

Examination of apparent instances of outward sensitivity in the 2nd and 3rd Person forms is equally inconclusive, even when we bring more data into consideration. One of the minority conjugation-types in Huave is one in which Past Tense and all Person markers are consistently suffixed rather than prefixed. In this conjugation, the forms corresponding to those in (243) (except for the Subordinate forms, which apparently have no distinct morphological parallel in this type) are:

(244) witiĩ- 'stand up'

	Present	Past	Future
Persons: 1	witiĩn	witiĩtos	apwitiĩn
2	witiĩr	witiĩtear	apwitiĩr
3	witiĩm	witiĩt	apwitiĩm

By themselves, the forms in (244) seem to indicate inward but no outward sensitivity, in that the Personal endings in the Past are different from those in the Present and Future. But two points

stand out when we compare (244) with (243). Firstly, there is a suffixed -t- in the Past Tense of (244) which seems to correspond to the prefixed t- in (243). Secondly, although the realisation of 2nd and 3rd Person always follows that of Past Tense, the fact that both are prefixed in (243) but suffixed in (244) means that any sensitivity on the part of 2nd Person to Past Tense will be outward in paradigms of the kind illustrated in (243) but inward in (244). A potentially interesting question, then, is whether the realisation of 2nd Person is in fact sensitive outwards in (243) and, if so, whether the same realisation occurs in (244); for, if not, our present formulation of the Peripherality Constraint is at fault. At first sight, there is indeed outward sensitivity in (243), in that 2nd Person has three distinct realisations i-, -e- and -me-, the choice between which is determined by the more peripheral category Tense; moreover, the affix -ear which realised 2nd Person in the Past Tense in (244) differs from all of these, and particularly from the -e- which appears in the Past Tense in (243). But things are not so simple. If the Future Tense -me- in (243) is explicable as derived from the Subordinate Mood, or if we segment the form apm-e- or ap-m-e- rather than ap-me-¹⁹, then it is only the alternation between -e- and i- which is at issue. Here we do indeed seem to have outward sensitivity, and indeed of a kind incompatible with the version of the Peripherality Constraint stated at (242), in that a different realisation occurs in the 'zero-marked' Present Tense from the other Tenses. But the facts themselves are somewhat more complex than so far described, in that . . . forms with -i- such as tindeak 'you spoke' and apmindeak 'you will speak' apparently occur in some idiolects instead of tendeak and apmendeak (Stairs & Hollenbach 1969: 44 note 11). Stairs and Hollenbach do not say which, if either, of these two alternatives is spreading at the expense of the other, nor whether the Subordinate 2nd Person mendeak has a similar variant with -i-; but, given the constraints on sensitivity so far proposed, it is tempting to see the -i- variants as part of a regularising innovation to remove potentially 'illegal' instances of outward sensitivity.²⁰ And, finally, if there is after all no outward sensitivity in the 2nd Person forms

of the prefix conjugation at (243), they can have no bearing on the general question of how constraints on outward sensitivity operate when the order of realisation of categories is not constant.

Our discussion of the Huave example has been inconclusive. In view of the comparative rarity of this kind of variation in order and the difficulty of finding data which tell unequivocally in favour of one version or another of the Peripherality Constraint, I will not continue the search for any more definite conclusions here. But I have, I hope, succeeded in illustrating the sort of considerations which will be relevant. Languages which it will be interesting to examine in future from this point of view, I suggest, are Cheremiss and those other Uralic languages in which the markers of Number, Case and Personal Possession on nouns appear in different orders according to the particular combinations involved.

2.8 A second problem: phonological sensitivity outwards

I will now turn to a quite distinct question. In section 1.8 of Chapter I, I distinguished between grammatically (or morphologically) conditioned allomorphy, which is the type of sensitivity with which we are mainly concerned, and phonologically conditioned allomorphy; and I argued that recognising an alternation as phonologically conditioned need not commit one to any particular view of the phonological representations of the alternants. One illustration of this I drew from the larger and more productive of the verbal conjugation-types in modern Hungarian. There, the two 2nd Person Singular Present Indicative Indefinite inflexions -(a)sz and -ol were deemed not to be rivals because their distribution depended entirely on phonological characteristics of the verb stem; -ol attaches to sibilants and affricates and -(a)sz occurs elsewhere (the choice between -asz and -sz again being determined purely phonologically). Phonologically conditioned alternations of this kind seem to be especially common in Australian languages; for example, the Ergative affix in Dyirbal has a variety of shapes depending on phonological characteristics

of the stem to which it is attached (Dixon 1972: 42), while in Warlpiri (see (115 e)) it is -ngku after disyllabic stems and -rlu [-[u]] after stems of three or more syllables (Dixon 1980: 306).

Establishing the phonological conditions for this kind of alternation has not been particularly important from the point of view of inward sensitivity, because no constraint on sensitivity in this direction has so far been proposed. But with outward sensitivity the position is different. Given that we are exploring the imposition of quite tight restrictions on outward sensitivity within our theory, the question arises: are there any instances of *prima facie* outward sensitivity which do not comply with the Peripherality Constraint but which are explicable in phonological terms? I will first describe two sets of data, in Zulu and Turkish, where this sort of outward phonological sensitivity seems plausible. They do not provide conclusive evidence, however, because an account in terms of straight-forward morphological sensitivity consistent with the revised Peripherality Constraint is not self-evidently wrong; but discussion of them will illustrate the sort of considerations that are relevant. I will then mention some facts in Fulfulde which seem to constitute stronger evidence for outward phonological sensitivity.

In Zulu, the Locative inflexion on nouns consists of a prefix e-, o- or ku- with or without a suffix -ni, thus (Doke 1973: 232-239):

(245)		Locative
umfula	'river'	emfuleni
indlu	'house'	endlini
uThukela	'Tugela River'	oThukela
izingubo	'blankets'	ezingutsheni
umuntu	'person'	kumuntu
abantu	'people'	kubantu

But when either e- or o- is preceded by one of a number of prefixes, an -s- intervenes, thus:

- (246) a. With Possessive concord;
 emfuleni indlu yasemfuleni
 'at the river' 'the house at the river'
- b. With Predicative concord;
 endlini abantu baaendlini
 'in the house' 'the people are in the house'
- c. With conjunctive prefix na- 'and'
 otshanini nasezingutsheni
 'on the grass and on the blankets'
- d. With nga- 'near' and certain other 'adverbial formatives' (in Doke's terminology);
 oThukela ngasoThukela
 'at the Tugela River' 'near the Tugela River'

The problem is how to characterise the environments in which this -s- occurs. Let us assume, to begin with, that the -s- 'belongs to' the Locative affix rather than to the element which precedes it. One possible complication is that some of the environments seem to involve proclitic elements rather than inflexions, e.g. (246 d). But even supposing that this aspect can be dealt with satisfactorily, the question remains whether the inflexional environments can be analysed exhaustively in terms of morphosyntactic categories; for if the -s- occurs after a prefix realising property c_1 in category C but not after the prefixes realising other properties belonging to C, then we have a prima facie counterexample to the Peripherality Constraint. In fact, so far as the Possessive and predicative concords are concerned (246 a and b), the Peripherality Constraint is complied with; the -se- and -so- alternants of the Locative prefix are found wherever any of these concords (all of which are 'non-zero') precedes. But all these concords share a phonological characteristic with the apparently non-inflexional prefixes of (246 c) and (246 d); they end in a vowel. It therefore seems very plausible to account for the distribution of the Locative alternants with and without -s- purely phonologically; the former occur after a vowel within the same 'phonological word', the latter elsewhere. This is the same kind of explanation as Dixon gives for the Ergative -ngku and -rlu in Warlpiri, the only difference being that the

phonological conditioning factor in Zulu is more peripheral,
not more central.

The 3rd Person Possessive affix (or 'izafet') of Turkish furnishes a possible example of phonological conditioning in both directions, although, as I shall argue, the outward sensitivity is better accounted for grammatically than phonologically. We will ignore for present purposes the four-fold vowel harmony to which this and many other Turkish derivational and inflexional affixes are subject; to simplify matters, I will use only examples involving the front unrounded vowel i [i]. Even with this restriction, we find four alternants of the izafet, as in (247):

- (247) a. -i: e.g. ev 'house' ev-i 'his house'
rehber 'guide' telefon rehber-i
'telephone directory'
- b. -in: e.g. ev-in-de 'in his house'
Loc
telefon rehber-in-i kaybettim
Acc
'I've lost the telephone directory'
- c. -si: e.g. bahçe 'garden' bahçe-si 'her garden'
cadde 'main road' İstiklâl Cadde-si
'Independence Street'
- d. -sin: e.g. bahçe-sin-e 'for her garden'
Dat
İstiklâl Cadde-sin-den geldik
Abl
'We've come from Independence Street'

We can describe this inflexion as consisting of -i- preceded or not by -s- and followed or not by -n-. The question now is: under what conditions do this -s- and this -n- appear?

Comparing (247 a, b) with (247 c, d), we notice that the -s- appears with the vowel-final stems bahçe and cadde but not with the consonant-final ones ev and rehber. This points to a generalisation which is in fact almost without exception in Turkish. Yet the alternation between -i(n)- and -si(n)- cannot be explained in terms of any general phonological rules or processes in Turkish such as 'postconsonantal s-dropping' or 'inter-

vocalic s-insertion'; there are plenty of examples of postconsonantal s, both with and without morpheme boundary intervening (e.g. aksi 'perverse', insaf 'justice', geliyor-sun 'you are coming'), and nominal inflexion provides examples of both -y- and -n- as well as -s- serving to separate vowels, depending on the forms concerned:

(248)	ev 'house'	bahçe 'garden'
	3rd Sg Poss ev-i	bahçe- <u>si</u>
	Genitive ev-in	bahçe- <u>nin</u>
	Accusative ev-i	bahçe- <u>yi</u>

The endings -i(n) and -si(n), therefore, cannot plausibly be derived from the same underlying phonological representation; rather, they seem to provide another example of phonologically distinct inflexional variants whose distribution is conditioned by phonological factors more central in the word.

It is not so clear whether the distribution of -(s)i and -(s)in is likewise phonologically governed. The examples in (247 b, d) demonstrate that the occurrence of the n is not restricted to contexts where a vowel follows. What Lewis (1967: 40) says is that "n appears between the suffix of the third person and any case-suffix", that is any of the suffixes listed (in their unrounded front-vowel forms only) in (249):

(249)	Accusative	- <u>(y)i</u>
	Genitive	- <u>(n)in</u>
	Dative	- <u>(y)e</u>
	Locative	- <u>de / -te</u>
	Ablative	- <u>den / -ten</u>

It is certainly true that when -(s)i(n)- precedes an element outside this list, such as -(y)le 'with', the cliticised version of the postposition ile, it is the n-less form which appears, e.g. bahçe-si-yile 'with his garden' like bahçe-yile 'with a garden', not "bahçe-sin-le" like rehber-le 'with a guide'. It seems very plausible, therefore, that what we have here is an instance of morphosyntactic outward sensitivity, whereby an alternant with -n- of the 3rd Sg Poss suffix is chosen just where any overt inflexion of Case follows. This sensitivity complies with the strong

version of the Peripherality Constraint, since there are some grounds for not regarding the endingless 'Absolute' or 'Nominative' as a member of the category Case in Turkish alongside the properties listed in (249).

What may give us pause, however, is the fact that the endings listed in (249) are also the last items in a Turkish noun which are stressable, and thus, since most Turkish nouns are stressed as near the end as possible, they do generally carry the stress. This means that an alternative account of the distributions of -(s)i and -(s)in might refer to stress; specifically, that -(s)in- is always unstressed and always immediately precedes a stressed syllable. This account loses most of its plausibility, however, when we note what happens with the large minority of nouns which are stressed elsewhere than on the final syllable in the basic form and which retain this non-final stress when carrying Possessive and Case affixes. If the 'stress theory' of -(s)i and -(s)in were correct, we would expect these nouns to select the n-less alternant even when inflected for Case, because the Case-ending immediately following would be unstressed (or at any rate would not bear the primary stress). For example, whereas bahçe 'garden' forms bahçe-sin-den 'from his garden',ⁿ with the final affix stressed, in contrast to bahçe-si-yile 'with his garden', we might expect téyze 'aunt' to form not only téyze-si-yile 'with his aunt' but also "téyze-si-den" 'from his aunt'. But, in fact, téyze inflects just like bahçe, despite the difference in stress pattern: we find téyze-sin-den 'from his aunt' just bahçe-sin-dén 'from his garden'. Without a more thorough examination of Turkish phonology, particularly of secondary stress, it would be unwise to rule out entirely an account of the -(s)i/-(s)in alternation in terms of phonological conditioning; but for the time being at least the explanation in terms of outward sensitivity to the category Case seems more attractive. My main purpose in discussing these Turkish and Zulu data, however, has not been to reach any definitive conclusion but to illustrate the sort of arguments which might lead one to postulate a kind of outward sensitivity which is phonological rather than morpho-

syntactic and which is thus outside the scope of the Peripherality Constraint.

Some much less equivocal instances of outward phonological sensitivity can be found in Fulfulde (also known as Fula or Fulani), a language of the Sahel region of west Africa. Verbal morphology in this language is complex, involving a large number of distinct 'Tenses' (where 'Tense' is an ad hoc label subsuming combinations of several categories, including Mood, Voice and Aspect; the details are not important here). Inflected verb-forms (or 'verbal complexes', in the terminology of Arnott (1970) and McIntosh (1981)) generally contain markers of subject and (if transitive) object, and sometimes also an 'anteriority marker'²¹ -no- or -noo- indicating priority in time relative to some other event; what is more, the order in which the subject and object markers appear relative to each other, to Tense affixes and to the anteriority marker varies from one Tense to another and also within Tenses, according to the particular combination of elements in question. What is important for our present purposes, however, is that several suffixes have two alternants, one with a long vowel and one with a short vowel (Arnott 1970: 219, 224-225):

- | | | |
|----------|-----------------------|----------|
| (250) a. | Anteriority marker | noo ~ no |
| b. | Relative Past Passive | aa ~ a |
| c. | Relative Past Middle | ii ~ i |

Examples of these alternations (taken, in fact, not from the Gombe dialect described by Arnott but from the similar Southern Zaria dialect described by McIntosh) are:

- (251) a. Dume ngad-ay- noo- 'on?
 what do- Incompletive-Anterior-you (Pl)
 Active
 'What were you going to do?'
 b. Dume ngad-ay- no- daa?
 what do- Incompletive-Anterior-you (Sg)
 'What were you (Sg) going to do?'
- (252) a. Tokoye suud-aa- 'on?
 where hide-Passive-you (Pl)
 'Where were you (Pl) hidden?'
 b. Tokoye suud-a- daa?
 where hide-Passive-you (Sg)
 'Where were you (Sg) hidden?'

- (253) a. Moye njaaf- ii- mi?
 whom forgive-Middle-I
 'Whom did I forgive?'
 b. Moye njaaf- i- noo- mi?
 whom forgive-Middle-Anterior-I
 'Whom had I forgiven?'

Now, what determines the distribution of the long and short forms?

The Tense suffixes aa ~ a and ii ~ i are subject to a generalisation which Arnott states as follows (1970: 225): "The shorter form occurs regularly when there is a subsequent [i.e. more peripheral] long-vowelled element within the complex ... The longer form occurs in all other complexes". With the Anteriority marker noo ~ no the situation is apparently more complex, since the choice seems to be partly determined by the Tense (1970: 219); but in Tenses belonging to what Arnott calls 'Group B' (Relative Past and Relative Future) the conditioning factor is exactly the same as for aa ~ a and ii ~ i: "the short-vowelled form -no is used whenever the [Anteriority marker] is followed by a long-vowelled element within the complex [Arnott's emphasis]", while the long-vowelled form is found elsewhere. There seems, in fact, to be a general requirement in both the Gombe and Southern Zaria dialects that not more than one long vowel may occur in any verbal complex after the root, and a kind of suffixal precedence hierarchy determines which vowel will win out when a potential clash arises (McIntosh 1981); but, for us, what matters is that the resolution of some clashes involves the phonological sensitivity of one element to a more peripheral one.

A perhaps more interesting Fulfulde example involves what Arnott (1970: 250) calls the Habitual Imperative Singular suffix, -atay, found in forms such as doggatay 'keep on running!', wallatay-mo 'keep on helping him!'. As the second example illustrates, an object-marking suffix such as 3rd Person Sg -mo may follow the Imperative suffix. Imperative forms may in fact be marked suffixally for all Personal objects except 2nd Person (i.e. reflexive) ones. But when we examine all the possible Habitual Imperative Singular forms with suffixal object marking

in the Southern Zaria dialect, we find an apparent instance of outward sensitivity in the 1st Sg form (McIntosh, personal communication):

(254) wall-atay- <u>be</u>	'keep on helping them!'
wall-atay- <u>min</u>	'keep on helping us!'
wall-atay- <u>mo</u>	'keep on helping him!'
but: wall-at- <u>am</u>	'keep on helping me!'

Clearly, unless this outward sensitivity can be shown to be phonological, it constitutes a counter-example to the Peripherality Constraint, since we observe two forms, -atay- and -at-, each of which occurs with different members of the one category Personal Object. But there is in fact evidence for calling this sensitivity phonological, not morphological. The suffix -am is the only one of the four Personal Object suffixes in (254) which begins with a vowel; and one finds elsewhere in the Fulfulde verbal system alternations governed by whether the following suffix is consonant-initial or vowel-initial. There is another vowel-initial object suffix, the 2nd Sg marker -e (which, as already mentioned, is not found with Imperatives); and both -am and -e, as realisations of 1st and 2nd Sg Object, are in complementary distribution with consonant-initial alternants, namely -yam and -ma (or -maa) respectively. Which realisation will be chosen for each Person depends on the Tense, realised more centrally; and the Gombe and Southern Zaria dialects seem to differ in the selection which certain Tenses impose. What is important for our present purposes, however, is that several Tenses have special realisations in the presence of the two vowel-initial suffixes. For example, in what Arnott calls the General Future Active Tense, the -am and -e alternants of 1st and 2nd Sg Object are chosen in the Southern Zaria dialect; and it is precisely with these suffixes that the usual Tense suffix -ay- is replaced by a suffix -Vt-, the quality of the vowel being determined by that of the following syllable²², thus:

(255) 'o-wall-ay- <u>min</u>	'he will help us'
'o-wall-ay-'on [-?on]	'he will help you (Pl)'
'o-wall-ay- <u>be</u>	'he will help them'
'o-wall-ay- <u>mo</u>	'he will help him/her'

(255) (continued)

but:	'o-wall-at-am	'he will help me'
	'o-wall-et-e	'he will help you (Sg)'

In this Tense, it is just those two forms in which the Object suffix begins with a vowel that are the 'odd men out'. This fact supports the idea that, both here and in (254), what the Tense markers are sensitive to is indeed a phonological characteristic of certain of the Object suffixes rather than the morphosyntactic properties which these suffixes realise.²³

What makes this last Fulfulde example especially interesting is the complexity of the realisation process that it seems to involve. The realisation of the Personal Objects is sensitive inwards to morphosyntactic properties of Tense or Mood; but the realisation of these properties is in turn determined partly phonologically by reference to the shapes of the more peripheral Personal suffixes. All this is quite compatible with the Peripherality Constraint, since only phonological, not morphosyntactic, sensitivity 'outwards' is involved. But when we come to construct a theory₂ of inflexion, behaviour such as this will count heavily against treating inflexional affixation as a relatively straightforward process of accretion, working outwards from the centre to the periphery, as seems to be assumed by e.g. Anderson (1977).

2.9 The Peripherality Constraint and Deviation III

So far, in accordance with the plan announced in section 2.1, we have discussed Deviation II (morphological sensitivity, or grammatically conditioned allomorphy) so far as possible in isolation from the other three types of deviation from maximally simple 'one-to-one' morphological patterning. I want to touch now on the interaction between the Peripherality Constraint and Deviation III, and thereby show how the Constraint may be reconciled with some apparent counter-evidence from Finnish.

Let us consider a couple of Finnish nominal paradigms:

	(256) Sg	Pl	Sg	Pl
Nominative	pöytä 'table'	pöydät	tehdas 'factory'	tehtaat
Genitive	pöydän	pöytien	tehtaan	tehtaiden
Partitive	pöytää	pöytiä	tehdasta	tehtaita
Essive	pöytänä	pöytinä	tehtaana	tehtaina
Translative	pöydäksi	pöydiksi	tehtaaksi	tehtaiksi
Inessive	pöydässä	pöydissä	tehtaassa	tehtaissa
Elative	pöydästä	pöydistä	tehtaasta	tehtaista
Illative	pöytään	pöytiin	tehtaaseen	tehtaisiin
Adessive	pöydällä	pöydillä	tehtaalla	tehtailla
Ablative	pöydältä	pöydiltä	tehtaalta	tehtailta
Allative	pöydälle	pöydille	tehtaalle	tehtaille
Abessive	pöydättä	pöydittä	tehtaatta	tehtaitta
Comitative	pöytine ²⁴		tehtaine ²⁴	
Instructive	pöydin		tehtain	

The feature which I want to concentrate on here is the -i- which appears in nearly all Cases of the Plural. In most Cases where Singular and Plural are distinguished (i.e. excluding the Comitative and Instructive), the Plural form differs from the Singular only in having an -i- immediately preceding the Case ending, where the Singular has -a- or -ä- [æ]; and even where the difference is greater than this, as in the Partitive, Illative and (for tehdas) Genitive, the -i- still appears in the Plural form. There therefore seems good ground for calling -i-, in my terminology, a principal exponent of Plural. But the -i- is lacking in the Nominative Plural (pöydät, tehtaat). Must we then say that this is an instance of outward sensitivity? And, if so, unless we can show that 'Nominative' is not a member of the category to which all the other Cases belong, is this not the sort of outward sensitivity that the Peripherality Constraint is supposed to forbid? The answer that I propose involves distinguishing 'pure' sensitivity from the kind of sensitivity implicit in all instances of overlapping exponence.

In the Nominative Plurals tehtaat and pöydät, not only do we find no element corresponding to the usual exponent of Plural;

there is no element specifically identifiable with the Nominative either, since the Nominative Singular lacks any consistent exponent parallel to, say, the Inessive -ssa/-ssä, and among the affixes that one might plausibly consider exponents of it (e.g. -nen, -s), -t does not appear. So there is no ground for segmenting the endings of tehtaat or pöydät into a Plural element and a Nominative element. There are therefore two alternative ways of analysing the inflexion in the forms tehtaat and pöydät: either (a) it realises just one of the two properties Nominative and Plural, the other being realised by zero, or (b) it realises both properties in overlapping fashion. Let us consider first alternative (a). No one has ever seriously proposed treating the -t as a marker of Nominative only, with Plural as zero, and I cannot see any argument in favour of that analysis. On the other hand, it is quite common to find it said in descriptions of Finnish (or of Uralic languages generally) that there is no Nominative 'morpheme' or even, in more abstract terms, no Nominative Case (in contrast to, say, Latin). Now, if we take 'no Nominative morpheme' to mean in our terms 'no morphosyntactic property Nominative belonging to the category Case', then the Finnish facts are perfectly consistent with the Peripherality Constraint; for then the 'Nominative' forms do not properly speaking belong at all in the Case-Number paradigm presented at (256), and, once we remove them, we are left with only one principal exponent of Plural, namely -i-. If, on the other hand, we take 'no Nominative morpheme' to mean 'no overt realisation of the property Nominative', then we do indeed have a prima facie counter-example to the revised Peripherality Constraint, incorporating as it does the strong Version A of the condition on outward sensitivity; for we have a property (Plural) which is sensitive outwards to Case and which, though realised in the same way with all 'non-zero' Cases (thus complying with Version B of the condition) is realised differently with the one 'zero' Case, namely Nominative.

It seems, then, that, if analysis (a) of the Finnish 'Nominative Plural' is correct under this second interpretation, we have found some evidence in favour of the weaker Version B

and against Version A. But I will argue that it is analysis (b), involving overlapping exponence, which is correct; moreover, that instances of overlapping exponence can and should be treated as irrelevant to the Peripherality Constraint.

When we compared Versions A and B in relation to the Hungarian and Zulu data, we assumed tacitly that, if a property such as 'Plural' or 'Class 1a/2a Singular' were realised differently with overt and 'zero' members of some more peripheral category C, the realisation found with 'zero' members would be what one might call the 'ordinary' realisation typical of contexts where no category C property was present. Thus, when we were discussing the phrase um-nyango ka-thisha 'the teacher's door' in Zulu (see (236)), in which the Class 3/4 Sg noun um-nyango 'door' imposes no independent overt Possessive concord prefix on ka-thisha 'of the teacher', what was of interest to us whether the 'possessor' noun stem -thisha would appear with the prefix ka- characteristic of contexts where it follows an overt Possessive concord prefix (one of the non-zero members of the set at (223)), or with the prefix u- characteristic of contexts where there is no Possessive concord. We did not consider the implications of the third possibility -- that -thisha, when qualifying umnyango, might display some third prefix, neither u- nor ka-.

Was this a careless omission? No. Only the two alternatives that we did actually consider are relevant to the choice between Versions A and B of the special condition on outward sensitivity; or, more precisely, only the occurrence of u- rather than ka- would have decided the issue definitively in favour of Version B. This is because of the crucial assumption that the condition on outward sensitivity relates to 'pure' sensitivity, not to instances of mutual conditioning of properties whose exponents completely overlap. Let us suppose that instead of umnyango ka-thisha we observed "umnyango u-thisha". In this hypothetical form, the prefix u- clearly realises the Class property 'Class 1a/2a Singular', since u- is the characteristic mark of that Class

and Number combination nearly everywhere; but there is no ground for saying that u- also realises the property 'Class 3/4 Singular' assigned by concord with umnyango except the purely negative ground that there is no overt realisation of 'Class 3/4 Singular' anywhere else in the word. The choice of u- rather than ka- can hardly be attributed to the mutual conditioning of completely overlapping properties, and there is no choice but to see it as a direct counter-example to Version A. Now let us suppose, by contrast, that in the environment umnyango -thisha the prefix of -thisha is neither u- nor ka- but some third form -- say, "ta-", yielding "umnyango ta-thisha". In this second hypothetical form, "ta-" clearly realises 'Class 1a/2a Singular', just as u- does in the first hypothetical example, since comparison with the other Class-Number prefixes of Zulu will show it to be unique to this particular combination. But, in contrast to the u- of the first example, "ta-" must be regarded as realising the concordial property 'Class 3/4 Singular' too; for, ex hypothesi, it is precisely the property 'Class 3/4 Singular' copied on to -thisha in the Possessive construction which triggers the replacement of the ordinary Class-marker u- by "ta-". This means that the relationship of morphosyntactic properties to their realisations in "umnyango ta-thisha" is crucially different from what it is in "umnyango u-thisha". In "ta-thisha", the realisation of 'Class 1a/2a Sg' is sensitive neither to a more peripheral overt property nor to an unlocatable or 'zero-marked' property belonging to a category other members of which are realised more peripherally, but rather to a property which is realised entirely simultaneously with it, neither more peripherally nor more centrally. Consequently, "ta-thisha" does not constitute a counter-example to Version A, simply because the special condition on outward sensitivity does not 'bite' on it at all.

What is the relevance of all this to our Finnish example? Simply that the Nominative Plurals tehtaat and poydat are in relevant respects similar to the hypothetical Zulu form "ta-thisha". The -t of tehtaat is certainly an exponent of Plural; but it must be regarded as an exponent of Nominative too, since it

is precisely the property Nominative which triggers the realisation -t rather than -i-. To say, as analysis (a) requires, that the -t realises Plural but not Nominative involves a quite arbitrary discrimination between the two properties which jointly identify the morphosyntactic 'slot' where the -t appears. So here, too, we have a property (Plural) sensitive neither to a more peripheral Case nor to an unlocatable or zero-marked one but rather to a Case realised entirely simultaneously with it. This amounts to saying that, if we reject the first version of analysis (a) (according to which there is no morphosyntactic property 'Nominative' at all in Finnish), we are led inescapably to analysis (b), to which, since it involves completely simultaneous exponence, the condition on outward sensitivity is irrelevant.

I have said enough, I hope, to show that the realisation of Plural in the Finnish paradigms at (256) is consistent with the revised Peripheralist Constraint. But one question that remains, perhaps, is why many Uralic scholars have seemingly preferred something closer to analysis (a) than analysis (b) -- have preferred, in fact, to regard the -t of tehtaat as realising Plural but not Nominative. This is due partly, I think, to a tendency to overemphasise the typological consistency of the Uralic languages (or, casting the net more widely, the Uralic and Altaic languages) in contrast to Indo-European, and partly to a difference in terminology. In highly inflected Indo-European languages, we are used to finding overt morphological realisation of a Nominative Case on many nouns, and also concord involving the Nominative as well as other Cases. In an Altaic language such as Turkish, by contrast, there is never any overt realisation of 'Nominative', since in the Singular the 'Nominative' slot is occupied by the bare stem and the Plural suffix is unaffected by Case; moreover, there is no Case concord. There are, in fact, good grounds in Turkish for adopting the first interpretation of our analysis (a) for Finnish -- for saying that there is no morphosyntactic property 'Nominative' at all. Among Uralic languages, Hungarian (for example) is more similar to Turkish than to most Indo-European languages in these respects. But to claim that no

Uralic language has a Nominative 'morpheme' in the sense of 'property' runs counter to not only the sort of Finnish evidence that we have already discussed but also to syntactic evidence. Finnish Case-Number concord, which operates very much like that of Latin, counts in favour of integrating the 'Nominative' with the rest of the Case category; and even if, unlike Latin, Finnish has no clearly identifiable Nominative Singular 'morphemes' in the sense of suffixes characteristic of that Case, this does not affect the argument for regarding -t as an overt exponent of the Nominative in the Plural.

In this Finnish example, then, there is no counterevidence to the Peripherality Constraint because the inflexion which might provide this counterevidence -- the -t of the Nominative Plural -- emerges as an overlapping exponent of more than one property (a 'portmanteau morph'). The fact that the Peripherality Constraint does not 'bite' on examples of overlapping exponence follows from the fact that we are deliberately restricting so far any proposed constraints on Deviation II to instances of 'pure' sensitivity. But we have always envisaged the possibility of extending the proposed constraints to more complex types of sensitivity. In order not to preclude the Peripherality Constraint in advance from such extension, therefore, we need to make explicit the fact that the existence of completely overlapping exponence at some slot in the paradigm does not render the paradigm incompatible with the Constraint. We need also to formulate the Constraint in such a way that one piece of overlap does not, as it were, free the rest of the paradigm -- more precisely, those parts of it where the sensitivity is 'pure' -- from the need to comply with the Constraint. I would like to suggest, therefore, a fairly natural amendment to the formulation of the Peripherality Constraint at (242), and point out some consequences of it. The amended formulation, in which the new material is underlined, is:

(257) Peripherality Constraint (second revised version):

The realisation of a property a may not be sensitive to a property c which has a principal exponent more peripheral in the word-form ('outward

sensitivity') unless, if c belongs to category C, the same exponent of a occurs with all other properties belonging to C whose exponence is not entirely simultaneous with that of a.

With this formulation, we no longer insist on the same realisation for property a with all members of category C, but only with those members of C which have a more peripheral principal exponent.

One consequence of making explicit in this way the compatibility of simultaneous (or fully overlapping) exponence with the Peripherality Constraint is that our attention is drawn to a new possibility for dealing with any apparent instances of outward sensitivity which are at first sight inconsistent with the Constraint. The most straightforward conclusion to be drawn from any such instances is, of course, that the Peripherality Constraint is simply wrong. But another possibility is that the morphological material which we have analysed as involving outward sensitivity, with one property more peripheral than the other, ought rather to be analysed as a simultaneous exponent of both properties concerned. Of course, it would be a mistake to invoke this alternative solely as a device to 'save' the Peripherality Constraint from disproof, without any independent evidence in favour of the 'portmanteau' solution for the problematic forms. But one can envisage situations where relevant independent evidence might be available. Let us suppose that, in a language with a generally transparent 'agglutinating' morphological structure, some phonological or other innovation has the effect of 'splitting' what was previously a single realisation for some property, in such a way that the distribution of the two new alternants involves outward sensitivity of a kind forbidden by the Peripherality Constraint. If the Constraint is correct, we will predict that the exponence relationships within the paradigm concerned will now be reanalysed in such a way that one or both of the alternants is no longer treated as separable from the more peripheral material to which it has apparently become sensitive -- in other words, that it and this more peripheral material are combined into a portmanteau realisation. Once this has happened, we can expect to see a

loosening of the ties between the formerly agglutinated 'morphs' which have become absorbed into this portmanteau realisation and the same morphs in environments which can still, consistently with the Peripherality Constraint, be analysed as 'agglutinating' -- a loosening which will tend to become manifest 'on the surface' through divergence in shape. It remains to be seen whether there are any 'semi-agglutinative' paradigms, or changes involving them, which can in fact be made sense of on these lines. The constellation of inflexional characteristics needed to trigger off the developments I have predicted is perhaps rather unusual, although one might begin by looking at anomalous instances of overlapping exponence within predominantly agglutinating morphological systems, such as the Turkish Aorist Negative paradigm (see (113 a)). I will not pursue this further here. The main point is that the Peripherality Constraint may in principle have a bearing on (and so be empirically testable in) certain at first sight rather unexpected situations potentially involving Deviation III as well as Deviation II.

2.10 Inflexional morphology and the Adjacency Condition

Several linguists in the transformational-generative tradition have turned their attention to morphology in recent years, notably Siegel (1974; 1978), Aronoff (1976)²⁵, Allen (1979), Lieber (1980) and Williams (1981). The questions with which they have been mainly concerned do not overlap much with the subject-matter of this thesis; they have not been concerned with general constraints on the relationship between morphosyntactic properties and their inflexional exponents so much as with derivational morphology, particularly in English, and questions about the phonological boundaries associated with various kinds of affix and about the organisation of the lexicon. But two proposals put forward by Siegel and Allen on the one hand and Williams on the other are potentially relevant to our present topic: the Adjacency Condition and the Atom Condition.

The Adjacency Condition (or Constraint) is formulated by Allen as follows (1979: 49):

(258) Adjacency Condition

No WFR [Word Formation Rule] can involve X and Y, unless Y is uniquely contained in the cycle adjacent to X.

The notion 'cycle' here belongs to a theory of morphology in which words are seen as having a constituent structure representable by means of a bracket notation similar to that which is familiar in syntax, e.g. $[[\text{ornament}]_{N\text{al}}]_A$, $[[\text{dis}[\text{honest}]_A]_A]$, $[\text{un}[[\text{distinguish}]_{V\text{ed}}]_A]_A$. These brackets, of course, define layers of embedding; and for Y to be in the cycle adjacent to X, Y must be separated by only one layer of embedding from X. What this means in practice can be illustrated by examples taken from Siegel (1978). The 'ungrammaticality' or nonexistence of words such as [#]undishonest, [#]undiscrete seems to point to a principle of English morphology blocking the prefix-sequence un-dis-; on the other hand, the existence of words such as undistinguished, undismayed seems to run counter to this. But Siegel points out an apparently consistent difference in the constituent structure of the 'good' and the 'bad' words with un-dis-, illustrated in (259):

(259) [un [[distinguish] ed]] versus
 [#][un [dis [honest]]]
 [un [[dismay] ed]] versus
 [#][un [discrete]]

She suggests that the blocking principle just mentioned (or some more general principle of which it is a consequence) does indeed apply in English morphology, but its application is prevented in words such as undistinguished by the Adjacency Condition, for in these words un- is more than one cycle away from dis-, as is shown by the presence of more than one bracket between these affixes in the word's constituent structure.

Clearly, if the Adjacency Condition is correct, its empirical consequences are extensive. As Allen (1979: 50) puts it: "... given the Adjacency Condition, it becomes impossible for a WFR to refer to any conceivable property of the base at any possible cyclic depth. Rules which crucially involve the notions...

'denominal', 'deverbal' and 'deadjectival' are not allowed within a theory of morphology governed by the Adjacency Condition." This is because a rule to the effect that some affix Y can attach only to deadjectival nouns, for example, would need to be able to 'look beyond' both a noun boundary and an adjective boundary embedded beneath it.

I am not concerned here with how well the Adjacency Condition squares with the facts of derivational morphology, in English or elsewhere, but rather with whether there is any evidence that it constrains inflexion too. It is fairly easy to see what sort of predictions it will yield about property-exponent relationships, and also that these predictions are distinct from those flowing from the Peripherality Constraint. Consider an inflected word of the following structure, where R is a root and A, B and C are affixes each realising some morphosyntactic property:

(260) [[[[R] A] B] C]

The Peripherality Constraint claims that A may not be sensitive to the properties realised by either B or C, unless it is sensitive in the same way to all the properties in the appropriate categories; on the other hand, it does not rule out the possibility that C may be sensitive to A. This latter possibility, however, is ruled out by the Adjacency Condition, which by contrast says nothing about the sensitivity of A to B or vice versa.

The Adjacency Condition has a considerable appeal; and sensitivity to 'adjacent' properties seems certainly to be very much more common, in languages with which I am familiar, than sensitivity to 'distant' or 'remote' ones. But to explore adequately the implications of the Condition for inflexional morphology and its possible relationship to other constraints on inflexional realisation is not feasible within the bounds of the present thesis. All I will do here is cite three examples (from Latin, Attic Greek and Zulu) of inflexional behaviour which is apparently problematic for the Adjacency Condition. After briefly discussing Williams's Atom Condition, I will then point out a

common feature shared by these three examples which may suggest avenues to explore in the course of any further attempt to apply either Condition to inflexion. My discussion will thus be inconclusive but, I hope, constructive.

The Latin example involves 'deponent' verbs. Deponent verbs are verbs such as 'sequor 'follow' which are, in traditional terms, 'Passive in form but Active in meaning'; despite their Passive morphology, they can function like ordinary Active verbs in e.g. taking direct objects (if transitive), although, not surprisingly, they cannot appear in Passive constructions. So, whereas a non-deponent transitive verb like regō 'rule' has two forms for 1st Plural Imperfective Past Indicative, namely an Active one regēbāmus 'we ruled' and a Passive one regēbāmur 'we were ruled', sequor has only one such form, sequēbāmur, with Passive shape but Active syntax and semantics; a 'morphologically Active' form "sequēbāmus" does not occur. Now, in all these forms (and indeed in all Latin verbs except the highly irregular sum 'be') there is an exponent -bā- of Imperfective Past Indicative which intervenes between the root sequ- and the Person-Number affix. So, if we are to bracket sequēbāmur in Siegel's or Allen's fashion, we will get something like [[[[sequ]-ē]bā]mur] (or perhaps [[[sequ]ēbā]mur]), with at least two and perhaps three brackets between the Person-Number affix and the root. Yet there is apparently a dependency between the root, with its idiosyncratic property 'Deponent', and the cyclically non-adjacent Person-Number affix, since this affix is also the sole inflexional exponent in this Tense and Mood of the property Passive. The Adjacency Condition is therefore contravened.

The argument based on the Attic Greek example is rather similar. In Attic Greek, the Present Optative stem of most verbs is formed with a suffix -oi- which follows the root and precedes the Person-Number affixes, thus: [[[lū]oi]mi] 'I loose (Optative)' from lū- 'loose'. On the other hand, so-called 'contracted verbs' are inflected somewhat differently in the Optative. These are verbs whose stems end in a vowel -a-, -e- or -o-, with which

the Optative -oi- coalesces, thus e.g. /t̄ima + oi/ 'honour (Optative)' → t̄imōi-. What is interesting is that the contracted verbs can select in the Singular a different set of Person-Number suffixes from the non-contracted verbs, thus:

(261) Non-contracted:	Contracted:
Sg 1 lū-oi- <u>mi</u> , not "lū-oi- <u>ēn</u> "	t̄imōi- <u>ēn</u> preferred to t̄imōi- <u>mi</u>
2 lū-oi- <u>s</u> , not "lū-oi- <u>ēs</u> "	t̄imōi- <u>ēs</u> preferred to t̄imōi- <u>s</u>
3 lū-oi, not "lū-oi- <u>ē</u> "	t̄imōi- <u>ē</u> preferred to t̄imōi

Again, this seems to involve dependency (or, in my terms, inward sensitivity) which 'sees beyond' the Optative suffix into a non-adjacent cycle.

The Zulu example involves verb-forms with both Causative and Passive suffixes. Let us consider the morphology of the Passive alone first. The Passive suffix is -wa, as indicated in the following examples (where the verb roots are underlined):

(262) Active:	Passive:
uyage <u>za</u> 'he washes'	uyage <u>zwa</u> 'he is washed'
uyab <u>ona</u> 'he sees'	uyab <u>onwa</u> 'he is seen'

But it is characteristic of Zulu phonotactics that -w- can never follow a labial consonant, and when the Passive -wa is added to a verb root whose final consonant (or consonant cluster) is labial, this labial is dissimilated to a corresponding apical:

(263) Active:	Passive:
uyah <u>laba</u> 'he stabs'	uyah <u>latshwa</u> [uja'ʔa:tʃ'wa] 'he is stabbed'
uyab <u>amba</u> 'he catches'	uyab <u>anjwa</u> [uja'ʔa:ndʒwa] 'he is caught'

However, this apicalisation of labials has become at least partly 'morphologised', since it occurs even when the labial consonant is medial or final in the root, thus (Doke 1973: 137; Rycroft & Ngcobo 1979: 65):

(264) uyakh <u>umbula</u> 'he remembers'
uyakhun <u>julwa</u> 'he is remembered'

For our purposes, however, what is most important is that this

'dissimilation at a distance' occurs even when the Causative suffix -is- intervenes between the root and the Passive -wa, thus:

(265) Active:	Passive'
uya-hlab-is-a	uya-hlatsh-is-wa
'he causes to stab'	'he is caused to stab'
uya-bamb-is-a	uya-banj-is-wa
'he causes to catch'	'he is caused to catch'
uya-khumbul-is-a	uya-khunjul-is-wa
'he causes to remember'	'he is caused to remember'

Once again, to rule out ungrammatical Passive forms like "uya-hlab-is-wa" we seem to need a dependency which 'sees beyond' more than one bracket; so again the Adjacency Condition is violated.²⁶

The Atom Condition is rather obscurely formulated by Williams (1981: 253) as follows:

(266) The Atom Condition (AC)

A restriction on the attachment of af_x to Y can only refer to features realised on Y.

From Williams's discussion it emerges that this means something like the following (using terminology deliberately reminiscent of Allen's at (258)):

(267) Atom Condition (unofficial paraphrase):

No WFR can involve a suffix X and any characteristic of the stem to which X is suffixed except the head of that stem or some feature of the head, where 'head' means the rightmost element of the stem.²⁷

It is clear that this Condition, which Williams explicitly propounds as a constraint on inflexion as well as derivation, makes different predictions from either the Adjacency Condition or the Peripherality Constraint. But the point that I want to emphasise here is that, just like the Adjacency Condition, it is incompatible (at least at first sight) with the Latin, Greek and Zulu facts that I have mentioned, because each of these facts involves a dependency, or cooccurrence restriction, between an inflexional suffix and an element which is not the rightmost element in what precedes the suffix.

The proviso 'at least at first sight' is important; Williams in fact weakens his condition enormously by admitting a 'head percolation mechanism' whereby "a feature on a morpheme in head position will be relevant at all further stages of derivation, because it will be inherited in each successive stage of the derivation" (1981: 254)²⁸. In the Latin example, therefore, the feature 'deponent' appropriate to the root sequ- 'follow' would be 'inherited by' the Imperfective Past suffix -ēbā-, and so be available in 'head' position to influence ^{the choice} of -mur rather than -mus as 1st Plural suffix. The trouble with this mechanism is that it seems much too powerful; for any conceivable counterexample to the Atom Condition involving an erstwhile 'head' could surely be handled in just the same way, and the Condition would therefore be stripped of much of its content. One could invoke the 'percolation' mechanism to protect the Adjacency Condition, too, but with similar debilitating consequences. But it is not my intention to discuss such mechanisms in detail here. Instead, I would like to point out the possible relevance of one characteristic which all my three putative counterexamples to the Adjacency and Atom Conditions share; they all involve not a pair of inflexional affixes but rather one inflexional affix and the root (or stem) of the words concerned. Is there any plausible reason why these Conditions might fail to block 'dependencies' in inflexional morphology where one of the dependent items is the root?

There are, in fact, independent grounds for treating 'root inflexion' (ablaut, for example) differently, for some purposes, from affixal inflexion. These grounds, which will be advanced in Chapter VII, have to do with the interaction of stem allomorphy with the 'Paradigm Economy Hypothesis' propounded and defended in Chapters IV-VI. I can see at present no 'reason' for this difference, in the sense of a theoretical₂ 'explanation' for it; but at least one can say that, if a similar proviso concerning roots or stems is needed for two distinct purposes, the proviso appears less arbitrary or 'ad hoc' than if it had to be invoked for one purpose only.

If some version of the Adjacency or Atom Condition turns out to be applicable to inflexion, then we will have found evidence for two different kinds of constraint affecting Deviation II: one involving the linear order of inflexions in relation to each other and to the stem, and one involving morphological embedding or word-internal constituent structure. As I have said, there are objections to both of the constraints on embedding so far discussed; but the counter-examples I have pointed out share a common feature which hints at possible ways of amending these constraints, assuming that they appear worth defending on other grounds.

Footnotes to Chapter II

1. As to (201 b), I am assuming that the relationship between verb root and Past Participle in English is inflexional, not derivational. This will probably be denied by those who say that word-forms belonging to different word-classes cannot be members of the same inflexional paradigm (e.g. Siegel 1974: 17); for participles notoriously share many of the syntactic characteristics of adjectives. But my assumption is not crucial to my argument, simply because, as I said in Chapter I, nothing in my argument hinges on the more fundamental implied assumption that it is possible and useful to distinguish sharply between derivation and inflexion. If past participles are derived rather than inflected forms, our example simply serves to illustrate that some 'derivational properties' are realised sensitively, just as morphosyntactic properties often are, despite (for example) M. Allen's assertion (1979: 3) that suppletion never occurs in derivational morphology.

2. Hungarian has vowel harmony, which affects most inflexional affixes. Only back-vowel versions are given here.

3. I assume here that parlerions is not to be segmented parler-i-ons, with -i- realising a distinct property such as 'Past', realised as [e] (-ais, -ait, -aient) in most other Persons. With an analysis on those lines, the sensitivity involved might indeed be pure.

4. It does not matter for our present purposes whether amās is analysed as containing a 'theme vowel' -a- plus a Personal suffix -s or merely a unitary suffix -ās.

5. 'Order' here (and throughout, unless otherwise specified) refers to linear order, or sequence.

6. It is not important to decide whether in this hypothetical example there is no property 'Singular' at all or whether there is such a property, 'realised by zero'. I will occasionally use the symbol \emptyset , or say that a property has 'zero realisation' in some word-form; but this is always to be taken to mean that the property has no identifiable realisation independent of that of any other properties in its syntagmatic en-

vironment.

7. Matthews might plausibly argue that the formation of the Future and Conditional stems is 'parasitic' on that of the Infinitive, for most French verbs, and involves 'zero formation', just as the formation of Future Participles in Latin is usually parasitic on that of the Past Participle and involves the suffixation of $-\bar{u}r-$ -- a fact he expresses by means of the following rule (1972b: 176):

$$(a) [S^1_{FU-P}] \quad + u:r, S^1_{PA-P}$$

If so, Matthews's French Future stem rule will look something like this:

$$(b) [S^1_{Fut}] \quad \text{Regular Future}; \quad S^1_{Inf}$$

where 'Regular Future' is a 'limitation' indicating that the rule does not apply to a small class of verbs including voir 'see' (Future stem verr-) and envoyer 'send' (Future stem enverr-); and the Conditional stem rule will be similar. But, even with this analysis, Future and Conditional are still inescapably 'focal terms' with respect to the formation of the appropriate stems, because they are mentioned in the 'reference component' of rule (b) and its Conditional analogue (that is, in the part in square brackets at the left).

8. For Turkish, the column headings are taken from Lewis (1967: 136). For Hungarian, they are derived from Bánhidi et al. (1965).

9. Vago (1980) accounts for the $-\acute{a}nak$ ending of the Conditional, as well as the $-(a)nak$ ending of the Present, by means of an n -Suppletion rule sensitive to Tense. He calls this rule 'morpholexical';

there is no motivation in Hungarian for a general process of n -Epenthesis which might be called 'phonological' in the usual sense.

10. The Class-labels are drawn from Rycroft & Ngcobo (1979). They, like Meinhof and many Bantu scholars, treat Plural Classes as distinct from Singular ones -- thus, umtwana 'child' belongs to Class 1 but abantwana 'children' to Class 2. But I

prefer to regard the distinction between umntwana and abantwana as purely one of Number, not Class (as does Doke (1973: 37)), and use the label '1/2' to refer to the Class of them both. In Meinhof's system, the same Class numbers are used in all Bantu languages for Classes whose prefixes are cognate; there are no Classes 12 and 13 in Zulu because the appropriate prefixes have been lost. In Zulu, the prefixes lose their initial vowels in some contexts, and some scholars therefore treat these vowels as outside the Class prefixes proper; but this does not matter for our purposes.

11. A few Class 1a/2a nouns denoting inanimate objects, such as ugwayi 'tobacco', do not replace u- by -ka- in the possessive construction, at least with some native speakers. It is not yet clear how systematic this maintenance of u- is. For our present purposes, its main relevance is in emphasising the lack of any phonological motivation for the u- ~ -ka- alternation in those nouns which display it.

12. There are verb-forms in which the property Negative is realised before (and so more centrally than) the property Potential, e.g. gel-miy-ebil-dim 'I was able not to come'. In these forms, the property Potential is outside the scope of the negation, as the English gloss here indicates. This example raises questions about syntagmatic relationships between properties and about word-internal constituency which I will not attempt to tackle in this thesis; but it does not affect the relevance to the Peripherality Constraint of examples (224) and (225).

13. I use the swung dash ~ from time to time in this thesis to separate alternant realisations of the same property or property combination, irrespective of whether the alternation is phonologically or morphologically conditioned.

14. The details of the morphological analysis of the Hungarian Possessive paradigms have been a matter of controversy among Hungarian scholars for decades. Under some analyses, such as that of Mel'čuk (1973), the need to recognise what I call 'outward sensitivity' disappears. Mel'čuk analyses the -(j)a of a form such as kalapja 'his hat' as a ^amark not of 3rd Sg Possession but of Possession pure and simple; and this mark, he

would claim, appears also in a form such as kalapjaim 'my hats', which must therefore be analysed thus:

kalap-ja- i- m
hat- Possessed-Plural-1st Sg

In my terms, the consequence of this analysis is that the realization of Plural in kalapjaim is sensitive not outwards to the presence of the property 1st Sg but inwards to the property Possessed. The snag with this is that Mel'čuk's Possession 'morpheme' -(j)a is often absent in the Plural (at least 'on the surface'), e.g. in our example (230): alongside ruhája 'her dress' we have ruhaim 'my dresses' etc., not "ruhájaim" etc. But, in any case, no questions are begged if we assume that Mel'čuk's analysis is wrong, since it is only if it is wrong that the Hungarian facts potentially endanger the Peripherality Constraint.

15. The variation in length of the Genitive markers is due to whether a segmental suffix follows: most of them require the long alternant (Hudson 1974: 113). It can thus probably be regarded as an instance of phonological, not morphological, sensitivity -- see section 2.8 below.

16. The shape of any nominal Class prefix may be affected by, for example, a preceding clitic na- 'and' or nga- 'with' (engendering vowel contraction) or by a preceding Negative verb (causing the first vowel of the prefix to drop). But these changes do not cast doubt on what the normal, or basic, shape of the prefix is.

17. Matthews (1972a: 99), in his reanalysis of the Huave facts, makes a similar comment.

18. Matthews (1972a: 113) comments that Stairs and Hollenbach do not succeed in conflating Future-formation with Subordinate-formation. It would be more accurate, I think, to say that they do not explore the possibility. To me, it looks promising.

19. This is not so arbitrary as it may at first seem. Matthews (1972a: 111) argues for treating -m- as a second Future marker (morphophonologically obscured in the 1st Person), so that the exponent of 2nd Person is the vowel -e- alone.

20. Matthews (1972a: 112) speculates that the spread of the -i- variants (if it is occurring) may spring from pressure to, as it were, rectify an exception to the general decline in 'marking relationships' from the top left to bottom right of his Person-Tense matrix at Figure 11. In my terms, this involves attributing the spread of -i- to pressure towards reducing sensitivity in general, in a systematic way, rather than towards removing outward sensitivity in particular. His suggestion would be supported if one could find evidence in morphological matrices elsewhere of similar 'clines' in allomorphy or 'marking relationships'. This certainly seems worth investigating.

21. 'Anteriority marker' is McIntosh's term; Arnott uses 'preterite element'.

22. This kind of inward-operating vowel harmony occurs also in a number of forms where the element following the Tense suffix is a subject rather than an object suffix; see Arnott (1970: 59).

23. Arnott states explicitly that in the Gombe dialect the 1st Sg Object marker in the General Future Active Tense is not the vowel-initial -am but the consonant-initial -yam (1970: 213). If my suggestion about phonological sensitivity is correct, one would expect the form glossed 'he will help me' in the Gombe dialect to show the 'usual' Tense exponent -ay-, not -at-. This is apparently correct; Arnott gives 'o-wall-ay-yam instead of the Southern Zaria 'o-wall-at-am. A similar difference between the two dialects seems to obtain in the Habitual Singular Imperative. Arnott, unlike McIntosh, mentions no -atay- ~ -at- alternation of the kind we find in Southern Zaria; and he confirms (personal communication) that in Gombe one would expect to hear wall-atay-yam rather than wall-at-am for 'keep on helping me!'. The realisation of the relevant Tenses in the two dialects thus seems to involve the same kind of outward phonological sensitivity, but the alternants are differently distributed because the realisations of 1st Sg Object differ in a phonologically relevant manner.

24. On nouns (as opposed to attributive adjectives) the Comitative suffix will never in fact appear 'naked', as here,

but always followed by a Personal Possessive suffix; e.g. pöy-tineen 'with his table(s)'.

25. Aronoff is almost exclusively concerned with derivational, rather than inflexional, morphology. However, he makes one suggestion (1976: 111) which can be readily extended to inflexional morphology and which then bears on our search for constraints on exponence relationships; this is the suggestion that 'allomorphy rules' (which take care of non-phonologically-conditioned allomorphy, in his framework) are always ordered "from the inside out". Unfortunately, this would seem to preclude wrongly all outward morphological sensitivity, even of the kind that the Peripherality Constraint permits.

26. It may seem as if the Peripherality Constraint is violated here too, inasmuch as the 'realisation' of the root itself is sensitive to that of a non-simultaneous and therefore more peripheral property Passive. But this is not so, simply because the Peripherality Constraint deals only with the realisation of morphosyntactic properties, and not roots (or lexical material) as such. This issue will be more fully discussed in Chapter VII.

27. Williams's definition of 'head' is idiosyncratic. It is tied in with his apparent view that suffixation is universally a more important process than prefixation from the point of view of morphological theory, and has quite different properties.

28. Compare the 'feature percolation conventions' discussed by Lieber (1980: 83ff.).

CHAPTER III

PARADIGMATIC CONSTRAINTS ON ONE-TO-MANY EXPONENCE

3.1 Introduction

Inflexional paradigms are familiar to everyone who has studied Latin or Greek in the traditional post-Renaissance European fashion. Someone who knows only that style of linguistic description might well imagine that the notion 'paradigm' would be regarded as indispensable and central by any linguist describing any highly inflected language. But this is not so. In the framework for linguistic description presented by Zellig Harris, one of the foremost American 'structuralists', paradigms are scarcely mentioned at all (Harris 1951). Lounsbury, in his structuralist description of the morphology of the verb in the inflexionally highly complex Iroquoian language Oneida, recognises five 'paradigmatic classes' of verb base which differ according to their influence on the shape of preceding pronominal elements; but 'paradigms' in the sense of lists of inflected forms belonging to one word or lexeme are, for him, purely illustrative, or raw material for the main descriptive task^{of} identifying morphemes and their alternants (Lounsbury 1953).

When transformational-generative grammar appeared on the scene, its initial preoccupation with syntax and phonology provided little incentive to reconsider the status and function of paradigms; the transformationalist approach to phonology did, however, seem at first to supply a motive for maintaining the structuralist exclusion of paradigms from linguistic theory, in that it was thought that a proper understanding of phonological organisation and phonological change obviated the need to invoke explicitly non-phonological factors such as 'paradigm pressure' or 'analogical levelling' to explain 'exceptions' to 'sound laws'. This attitude began to change in the light of the sort of fact pointed out by Wanner (1972) and Harris (1973) in Italian and Spanish: the fact that formal complexity in the operation of apparently well-motivated phonological rules (unexpected ordering, failure of application or both) may be associated with uniformity, or absence

of allomorphy, in the inflexional paradigm thereby produced. Facts such as this led Kiparsky (1971; 1972) to postulate a principle of elimination of allomorphy in paradigms, to be included in the 'evaluation measure' for grammars alongside principles of formal simplicity and maximal transparency (in the technical sense of 'transparency' defined by Kiparsky). Recognising the minimisation of allomorphy as a factor in linguistic change does, however, involve a considerable retreat from the earlier claim of at least some generative phonologists that all instances of what has traditionally been called 'analogical levelling' could be explained in terms of phonological rule loss, rule simplification and rule re-ordering; and Kiparsky himself has more recently been inclined to locate the allomorphy minimisation principle somewhere other than in the phonological 'evaluation measure' (Kiparsky 1974). Halle, by contrast, states (1973: 9): "It is well known that paradigm pressure plays a potent role in the evolution of languages", and adds: "If paradigms can influence the evolution of language then there is every reason to expect that paradigms must appear as entities in their own right somewhere in the grammar". So far as I know, however, neither Halle nor anyone else has yet followed up these remarks with a full account within his framework of the inflexional morphology of any highly inflected language.

Even for Halle, as we have seen, the main evidence for the existence of paradigms as "entities in their own right" has to do with language change. But this provokes a fairly obvious question: is there no substantial relevant evidence of a purely synchronic kind? In other words, if the kind of problem posed by the Italian and Spanish verb paradigms did not exist -- if, for example, velar softening occurred not only in '3rd conjugation' Italian verbs such as leggere 'to read' but also in '1st conjugation' ones such as pagare 'to pay' -- would there no longer be any reason for postulating any non-phonological principles involving paradigms at work in morphology? To answer this, we need to consider what logical possibilities exist, both for synchronic morphological organisation and for morphological change. As soon as we do so, it becomes clear, as I hope to show, that

only a few of the things which logically might happen in 'paradigms' are actually observed to happen. Assuming, then, that these gaps are not accidental, we have independent evidence that paradigms are more than mere lists of word-forms.

3.2 The Free Distribution Characteristic and the Inflectional Parsimony Hypothesis

The Spanish facts described by Harris (1973) had to do with an apparent 'conspiracy' between two phonological rules (truncation and velar softening) to produce a paradigm in which stems remain phonetically constant 'on the surface'¹. Facts of this kind have been seen as evidence for recognising the paradigm as a specifically morphological entity with a place of its own in linguistic 'competence'. But I will argue that, even if there were no such evidence for the surface operation of a paradigmatic levelling principle, the need to assign to paradigms an explicit role in linguistic theory could be demonstrated. Much of the argument will rest on consideration of what might happen in languages but doesn't -- an approach whose legitimacy I defended in Chapter I.

Let us consider a hypothetical language L in which nouns are inflected for two Numbers, Singular and Plural, and six Cases, and in which there is cumulative exponence of Case and Number -- in other words, the realisation of Case and Number involves Deviation III. Suppose further that in L each Case-Number combination may be realised by more than one affix, thus:

(301)	Singular	Plural
Case 1	-s, -m, -∅	-i:, -e:s, -a
2	-e, -m, -∅	-i:, -e:s, -a
3	-m, -∅	-e:s, -a
4	-i:, -is	-rum, -um
5	-i:, -o:	-i:s, -ibus
6	-e, -o:	-i:s, -ibus

Two points can be made immediately. Firstly, there is little doubt that L is a possible human language; there is a close resemblance between the Case-Number affixes listed and those of

Latin, assuming that the six Cases are identified in order as Nominative, Vocative, Accusative, Genitive, Dative and Ablative. Secondly, there is no chance of deriving (morpho)phonologically all the 28 affixes listed from a set of only 12 underlying representations, one for each Case-Number combination, without a phonological theory permitting either extremely abstract underlying representations or 'minor rules' of extremely restricted application or both. I will therefore make the most obvious assumption about L, namely that a single Case-Number combination can indeed be realised by distinct affixes, with distinct underlying representations, even if the details of these phonological representations are not entirely plain.

We will consider now how these affixes in L might be distributed among the nouns to which they are affixed. The interest of this should be clear; if in order to preserve L's status as a possible human language it turns out that we must impose limits on the freedom of distribution of these affixes, we will in effect be constructing general linguistic hypotheses about morphological organisation. We will begin with the possibility implying the least restriction, namely the possibility that any noun may take all of the 28 affixes; that is, that every noun has three forms in free variation for the Case-Number combination 'Singular-1', two for 'Singular-3', and so on. Let us call this the Free Distribution Characteristic. With the Free Distribution Characteristic, a nominal stem re:gi- 'king' in L (preserving the similarity to Latin!) will have two 'Singular-4' forms re:gi: and re:gis, and two 'Plural-5' forms re:gi:s and re:gibus; conversely, a form such as re:gi: will, out of context, be multiply ambiguous, interpretable as 'Singular-4', 'Singular-5', 'Plural-1' or 'Plural-2'.

A priori, we might expect such a degree of ambiguity to be intolerable. But it would be rash to conclude that because of this ambiguity our hypothetical language L, with the Free Distribution Characteristic, is not a possible human language. Languages notoriously tolerate a great deal of ambiguity of different kinds at different levels, and attempts to specify limits to this to-

leration by reference to quasi-quantitative notions such as 'functional yield' have not produced particularly persuasive results². Besides, there are of course numerous languages lacking any morphological distinction of Number or Case, in which the semantic function of such marking is fulfilled by other means; and why could not our language L too use such means to resolve any prima facie ambiguity resulting from the Free Distribution Characteristic? So there does not seem to be any general linguistic principle involving ambiguity which would rule out L, with the Free Distribution Characteristic, as a possible human language. Yet we are still entitled, I think, to feel uneasy. With the Free Distribution Characteristic, L's inflexional morphology loses most of its resemblance to that of Latin. The question, then, is whether there are any languages at all in which, when two or more affixes (or other inflexional devices) are available to realise a given morphosyntactic property or combination of properties, all these affixes collocate freely with all stems of the relevant part of speech. In terms of the discussion in section 1.3 about method in morphological argument, this amounts to asking whether the fact that Latin does not have the Free Distribution Characteristic is an accidental fact about Latin (a category B fact) or is attributable to some general linguistic constraint (a category C fact).

It is certainly true that, in some languages, a few stems display alternative inflexions for some morphosyntactic properties; for example, English fish, Plural fish or fishes; Latin domus 'house', Dative Singular domō or domūī; German Wort 'word', Plural Worte or Wörter; Hungarian szomszéd 'neighbour', Plural with 3rd Singular Possessive szomszédai or szomszédjai 'his/her neighbours'. In some languages, also, there are a few morphosyntactic 'slots' where for all members of the relevant part of speech there is more than one inflexional possibility; for example, the 3rd Plural Perfective Present Indicative in Latin of the 'Golden Age', which may end in -ēre, -erunt or -ērunt (e.g. regō 'I rule'; rexēre, rexerunt or rexērunt 'they have ruled'). Similarly, in Finnish practically all nouns have two or more al-

ternative Genitive Plurals (as Paul Kiparsky has pointed out to me): e.g. peruna 'potato', Gen Pl perunoiden, perunoitten, perunojen, perunien or perunain. But it is equally true that this variability is unusual, in the sense that in most inflected languages most if not all inflected words have only one form for each morphosyntactic slot. The lavish variety of Genitive Plural endings in Finnish may reflect the relatively recent origin of the standard Finnish literary language and its acceptance of features proper to a number of different local dialects, and there are certainly stylistic or 'register' differences between them: "... les génitifs pluriels en -in (tyttöin 'des jeunes filles'), construits sur le thème vocalique d'un grand nombre de noms, ont un accent plus solennel. ... En revanche, à la place des génitifs pluriels en -den, on trouve assez fréquemment -tten (katseitten 'des regards' à côté de katseiden 'id.', sur katse 'regard') qui semble à certains sujets plus naturel que la forme en -d- ..." (Sauvageot 1949: 85-6; cf. also Harms 1957). As for idiosyncratic doublets like fish and fishes, closer examination of most of them reveals that they are not complete synonyms in stable coexistence. Sometimes the two alternatives can be assigned to different dialects or 'registers' (for example, fishes has a rather Biblical ring to my ear, fish being the normal Plural form); sometimes one of the two is in retreat while the other is encroaching (for example, in Hungarian, Plural Possessive forms without -j-, such as szomszédai, are apparently less productive than, and losing ground to, those with -j- (Sauvageot 1971: 146-9, 314-6, 376); sometimes the two forms have clearly distinct lexical meanings (for example, German Worte 'words in the context of an utterance' versus Wörter 'words out of context (e.g. as listed in a dictionary)').

The absence or rarity of genuine alternative forms compatible with the Free Distribution Characteristic is reflected in the traditional terminology of linguistic description and language teaching: "The Genitive Singular of Latin rex 'king' is rēgis", "The Plural of Hungarian fal 'wall' is falak", and so on, where it is clearly presupposed that, with a given stem, a given combination of morphosyntactic properties will be realised in only

one way. It is by no means logically necessary that this should be so. We have already remarked that language has at its disposal devices quite adequate to deal with any ambiguity that the Free Distribution Characteristic might engender. On the basis of the evidence so far presented, therefore, it seems legitimate to hypothesise, as a distinct constraint on inflexional morphology, that no language may possess the Free Distribution Characteristic. More precisely, we might name and formulate a hypothesis on the following lines:

(302) Inflexional Parsimony Hypothesis

Even when there is more than one inflexional realisation available for a given combination of morphosyntactic properties, each stem must select only one of these realisations (unless, where two or more are selected, the overt contrast is associated with some semantic or stylistic function).

The name chosen for this hypothesis is intended to hint at an explanation for it. I suggest that there is at work in morphology, derivational as well as inflexional, a strong tendency to put overt differences to use, to assign them some function -- not to let them go prodigally to waste, as it were. In derivational morphology, the use to which they are put is relatively obvious, namely the distinction of lexical meanings. Thus, in English, one seldom or never finds two nominal forms derived from the same verb which are exactly synonymous. Examples of the kind of meaning differentiation which can occur are:

(303) composer of music	versus	compositor of type
disposal of rubbish, assets		disposition of troops
preference for one al- ternative over another		preferment to a post (usually ecclesiastical)
requirement that a con- dition be met		requisition of property for official purposes

Looking at this tendency from another angle, we can say that a language will not tolerate the coexistence of two derived forms

unless they can be put to distinct semantic uses. This tendency is used by Aronoff (1976: 43-5, 60) to account for the phenomenon of 'blocking', where the nonexistence of some derived word correlates with the existence of some other word with the appropriate meaning.

In inflexional morphology, potential breaches of parsimony arise wherever more than one inflexional resource is available for a single grammatical function. The Inflexional Parsimony Hypothesis predicts that such breaches will not occur, however; that is, that languages will not display the Free Distribution Characteristic. Overwhelmingly the most frequent way of ensuring this is by requiring that each stem display one and only one of the available realisations for each morphologically relevant combination of properties; thus in Latin, for example, we find that the Dative Plural ending -īs is the only one that can cooccur with the stem domin- 'lord', while -ibus is the only one that can cooccur with rēg- 'king'. Alternatively, where more than one realisation is available for some slot, either one of the variants will go out of use (as seems to be happening with Hungarian szomszédai in the face of szomszédjai 'his/her neighbours') or else the difference will be 'functionalised' by the assignment of different meanings or stylistic connotations (as with German Worte versus Wörter). This idea of 'functionalisation' will crop up again later in our discussion of paradigms; and the fact that it does so -- that it has uses going beyond the facts we are currently discussing -- reinforces the case for recognising both it and the associated principle of 'parsimony' as genuine factors in morphological organisation.

3.3 The nonexistence of 'syntactic parsimony'

The Inflexional Parsimony Hypothesis eliminates a vast number of the logically possible ways in which our hypothetical language L could exploit the affixal resources tabulated at (301). Since my concern here is with inflexional morphology, I will for the time being retain the title 'Inflexional Parsimony Hypothesis' even though the derivational facts noted show that the Hypothesis

is almost certainly a special case of a wider morphological principle. But is this principle purely morphological? What analogues, if any, does the Inflexional Parsimony Hypothesis have in syntax? I will argue in this section that there is in fact no corresponding syntactic principle. This conclusion is not vital to the main thread of my argument; nevertheless, the question is of interest because, if my conclusion is correct, the independence of morphology within linguistic theory is reinforced.

We can paraphrase the formulation of the Inflexional Parsimony Hypothesis at (302) by saying that two inflexions cannot be functionally identical (realising the same morphosyntactic property or properties) unless they are distributionally complementary (cooccurring with non-overlapping subsets of the appropriate word-class or part of speech). The question now is: is there any constraint comparable with this in syntax?

It is well known that there are distributional limitations on lexical items which go beyond what is implied by their being members of one or another part of speech. Within the framework of the 'standard theory' of generative grammar, as expounded by Chomsky (1965), these distributional limitations are described in terms of 'strict subcategorization features' and 'selectional features' contained in lexical entries. Some of these limitations are very familiar and are shared by large numbers of lexical items, such as the limitation on intransitive verbs expressed by Chomsky's (1965) feature [+___#]; others, of lesser scope, are more idiosyncratic, like the restrictions on some adjectives in English which bar them from appearing in attributive position (e.g. ill) or in predicative position (e.g. late in the sense of 'deceased'); compare the sentences of (304) with those of (305):

- (304) a. *John is an ill man.
 b. *King George VI has been late for 27 years.³
- (305) a. John is a sick man.
 b. King George VI has been dead for 27 years.

Other notoriously arbitrary distinctions in syntactic behaviour (that is, ones which are inexplicable semantically) between par-

ticular lexical items are illustrated by the ungrammaticality of (307) (in standard British English) in contrast to (306):

- (306) a. He persuaded me to come.
 b. John is likely to leave.
 (307) a. *He convinced me to come.
 b. *John is probable to leave.

But there is an important difference between the ungrammaticality of Latin "rēgī" 'of the king' (Genitive Singular) on the one hand and that of (304) and (307) on the other. Both involve distributional limitations, in some sense, but only the first can be related directly to other, complementary, distributional limitations involving other stems and inflexions. An example of such a complementary limitation is the one which renders ungrammatical "dominis" 'of the lord' (Genitive Singular). The two facts are complementary, because they both involve the selection of the 'wrong' Genitive Singular ending, to wit the one appropriate to the other. In virtue of the Inflexional Parsimony Hypothesis, inflexions appropriate to each Case-Number combination of Latin nouns are in complementary distribution over the whole range of noun stems, and one can identify unequivocally the 'rival' inflexions to which each one is strictly complementary. But words such as ill, late, convince, probable do not have strictly complementary rivals, as (308) shows:

- (308) a. John is ill/sick.
 b. The dead/late king was succeeded by
 his daughter.
 c. He persuaded/convincing me that I should
 come.
 d. It is probable/likely that John will
 leave.

What sort of distributional limitation in syntax would, then, be directly comparable to that involving the Latin Genitive Singular inflexions? What we need to find is an example of some lexical item which is not only subject to apparently arbitrary distributional limitations, as are the adjectives ill and late (in the sense 'deceased'), but is also paired with some exactly synonymous lexical item (or set of lexical items) whose distri-

bution is exactly complementary. This would be the situation if, for example, the noun bucket were barred from appearing in any noun phrase functioning as object of the verbs give, steal, throw and other 'strong' verbs, and the synonymous pail were barred from appearing anywhere else -- were barred, that is, from appearing in any subject noun phrases, or in the objects of prepositions or 'weak' verbs such as receive, drop, clean, or in isolation. But this kind of syntactic complementarity between lexical items does not seem to occur. To return to our metaphor, syntax is prodigal with the overt difference between the synonymous items bucket and pail -- it allows this difference to 'go to waste' in a way which morphology would not tolerate.

It is important to emphasise that the sort of syntactic complementarity just envisaged for bucket and pail is quite distinct from the familiar morphological phenomenon of suppletion. An objector might argue that it is purely by convention that the Present and Past Tense forms go and went in English, or ferō 'I carry' and tulī 'I carried' in Latin, are regarded as alternants of the same lexical item; we could equally well regard them, perhaps, as separate lexical items with peculiar limitations on their distribution. If so, are they not parallel to the imaginary bucket and pail example which, we claimed, was linguistically impossible? The answer is no. It is characteristic of suppletive pairs such as ferō/tulī and go/went that neither member displays a complete range of inflexions; rather, they divide the labour between them, and it is precisely because of the absence of forms such as "wen-ding"⁴, "goed" that go and went are classified as suppletive alternants rather than synonyms. But in our hypothetical bucket/pail situation, both can display the complete range of nominal inflexion -- both can be inflected for Plural and Genitive⁵, since nothing in their mirror-image distribution prevents this. To put it another way, the choice between the suppletive alternants go and went is determined by aspects of their syntactic context which are morphologically relevant to ordinary non-suppletive verbs too, such as the presence or absence of the property Past; whereas the choice in our hypothetical situation between the 'alternants' bucket and pail is determined by factors which have no relevance,

morphological or syntactic, to ordinary nouns at all.

A ^edetermined objector may yet find some ammunition in favour of 'syntactic parsimony' among those languages where some verbs are apparently subcategorised in quite complex ways according to whether or not they can occur with subjects or objects belonging to certain syntactically or semantically definable classes. Languages of this kind include Athapaskan languages such as Navajo and Cherokee, where semantic classes such as 'animate creature', 'round object', 'liquid' correlate with apparently distinct verbs with meanings such as 'throw', 'fall', 'lie' (Davidson, Elford & Hoijer 1963). Suppose that in such a language there are two verbs meaning 'hit', quite dissimilar in shape, one of which is used only with animate objects and the other only with inanimate objects (call them 'hit_a' and 'hit_i'); would this not constitute the sort of complementarity which I have claimed does not exist?

In answer to this, I would first point out that the objector's case falls if it can be shown that the language possesses a morphosyntactic category of Animacy, containing two properties Animate and Inanimate, for which verbs are inflected in agreement with their objects. As soon as that is established -- if we find, for example, that all or most transitive verbs have two forms which alternate in the same way as hit_a and hit_i, and especially if most such paired forms are partially similar phonologically rather than being quite dissimilar in the way that hit_a and hit_i are (ex hypothesi) -- then what we have in hit_a and hit_i is not an example of 'syntactic parsimony' but a straightforward example of morphological suppletion. But suppose these conditions are not fulfilled -- suppose, for example, that only a few verbs can be paired like hit_a and hit_i, and there is no recurring difference in shape which can be identified as a regular inflexional exponent of an Animate-Inanimate distinction? If any language does exhibit this kind of behaviour, then certainly it would seem to provide an instance in syntax of the kind of parsimony that I have claimed to be peculiar to morphology. But, if

such behaviour were found, it would still be worth examining it in the light of a somewhat weaker claim about syntactic parsimony than the one I have advanced so far: not that syntactic parsimony is impossible, but that it is inherently unstable.

What I am suggesting is that there might be a strong general preference for analysing the sort of distributional complementarity between synonyms that we have hypothesised for hit_a and hit_i in morphological rather than syntactic terms -- for analysing them, in other words, not as separate lexical items with syntactically rather than morphosyntactically determined complementarity in distribution, but, instead, as suppletive alternants of the same lexical item, with the corollary that what determines the choice between them must be a morphosyntactic property ('Animate' or 'Inanimate') transferred to the verb through concord with its object, rather than simply some syntactic or semantic property of the object alone, unmediated by concord. This preference, if genuine, will have diachronic consequences. Inasmuch as morphosyntactic properties tend to apply to whole word-classes rather than arbitrary subsets of word-classes⁶, we will expect to find all transitive verbs acquiring morphological marks of the Animate-Inanimate distinction; and, inasmuch as stem suppletion is the exception rather than rule in most inflexional systems, we will expect the suppletive alternations within the group of verbs in question to be replaced by predictable alternations involving some regular process, such as the addition of a particular affix. Alternatively, if such symptoms of 'morphologisation' are absent, we will predict that the instability will be resolved in the opposite direction -- that the complementarity in distribution between hit_a and hit_i, and similar paired or grouped 'synonyms', will become less rigid, thus destroying the crucial characteristic of parsimony. The complementarity will be broken if, for example, we find that hit_i as well as hit_a can be used with animate objects, provided that the object is inert or passive in the manner of an inanimate object. As soon as we observe this kind of behaviour, we can no longer say that hit_a and hit_i are exactly synonymous, as bucket and pail are in our hypotheticalal

version of English, or as the suppletive stems go and wen- are in actual English, because there will be a distinction in meaning between sentences such as 'John hit_a Bill' and 'John hit_i Bill'; the latter will imply or presuppose that Bill is unconscious, while the former will not. And this sort of contrast does in fact seem to be possible in the Athapaskan languages, where Davidson et al. report (1963: 36): "In Dogrib, as in the other [Athapaskan] languages, the same noun may be employed with different classificatory stems, so giving the noun a distinctive denotation. Thus: let'e 'bread' plus the verb niyehfa 'I pick up a category I [solid, compact] object' yields the meaning 'I pick up a loaf of bread'. But let'e plus the verb niyehtsi 'I pick up an object of category V [flat, fabric(like)]' means 'I pick up a slice of bread'".

To sum up: the claim that there is no such thing as syntactic parsimony commits us to the prediction that, when we examine closely any apparent instance of parsimony similar to our hypothetical bucket/pail and hit_a/hit_i examples, we will always find either that the supposedly purely syntactic distinctions determining the distribution of the complementary synonymous forms are in fact morphosyntactic, and consequently that the synonymous forms themselves are in fact to be treated as belonging to single rather than distinct lexical items, or else that the supposed complementarity in distribution of the forms in question is not exact and that the contexts where their distributions overlap show them to be not synonymous after all. There is some evidence, as I have suggested, that this prediction is borne out so far as the prima facie 'syntactic parsimony' of the Athapaskan classificatory verbs is concerned. Whether further evidence will confirm it is an open question⁷. But for the time being it seems reasonable to claim that the Inflexional Parsimony Hypothesis cannot be related directly to any constraint independently needed in syntactic analysis and that what we have here is a constraint peculiar to morphology, reinforcing its distinctness from other areas of grammar.

3.4 The Independent Distribution Characteristic and the notion 'slab'

Our denial of analogues to the Inflexional Parsimony Hypothesis in syntax was essentially a digression from our main task, which is to seek further general constraints on morphological organisation and more specifically on the logically possible ways in which our hypothetical language L could exploit the affixal resources set out in (301). If we assume that the Inflexional Parsimony Hypothesis is correct, what possibility involves the least further restriction on the use of these resources? The Inflexional Parsimony Hypothesis requires that each stem must select only one affix for each Case-Number combination, but it says nothing about the relationship, if any, between the selections made by one stem for different combinations. For example, from the fact that a stem domin- 'lord' has Singular-4 domini; the Inflexional Parsimony Hypothesis predicts nothing about whether the Plural-5 form will be domini:s or dominibus, the two options which (301) seems to make available. Similarly, if we postulate a Singular-4 form domini, the Inflexional Parsimony Hypothesis tells us nothing about whether the Singular-5 form will be domini or domino. Let us suppose that, from the point of view of the general constraints on morphological organisation, both alternatives are possible for both these examples and that there are in general no mutual restrictions linking the choice of one affix with that of another. This amounts to saying that it is possible in L for any noun stem (or, at least, any vowel-final one) to select any possible combination of the affixes listed in the table at (301), provided only that, in accordance with the Inflexional Parsimony Hypothesis, it 'chooses' only one for each of the twelve Case-Number combinations. Let us summarise this by saying that the Case-Number affixes of L have the Independent Distribution Characteristic. Mathematically, this yields $3^4 \times 2^8 = 20,736$ possible affixal patterns for the complete selection. Yet, needless to say, the actual language on which L is based, namely Latin, does not contain anything like 20,736 actual patterns for Case-Number affixation. Instead, we find a vastly more limited range of declension patterns -- traditionally five, although the exact number

that needs to be recognised will depend to some extent on the nature and power of the (morpho)phonological rules which we postulate as operating on the underlying representations of the affixes. Is this degree of restriction in Latin accidental? Almost all linguists, I think, would agree that it is not; that is, that our hypothetical Latin-like language L would not be a possible human language if its nouns selected realisations for each combination of morphosyntactic properties independently.

An intuitively plausible reason for this is not hard to find -- one which casts doubt on whether what we have discovered is a specifically linguistic fact at all. Let us consider what morphological information would have to be specified individually in the lexicon, or learnt individually by the native speaker, for each noun if L possessed the Independent Distribution Characteristic. Clearly, each noun in L would have to be learnt along with no less than twelve 'rules' specifying how each Number and Case combination was to be 'spelt'. If morphological theory has to be constrained so as to exclude such a possibility, it seems natural to explain this constraint by reference to the enormous burden that would be imposed on the memory by the need to learn twelve separate Case-Number rules along with each noun. Is the constraint then a linguistic one at all, strictly speaking? Earlier, when discussing techniques of argument in morphological theory, I suggested that the first of our list of six logical possibilities which did not occur in Latin -- namely, the fact that no Latin noun has a Case ending seventeen syllables long -- was probably best regarded as a fact of no linguistic interest, to be explained rather in terms of elementary properties of any communication system whatsoever. If the individual specification of affixes in a twelve-term Case-Number system is ruled out solely in virtue of human memory limitations, then is not what we have found merely a rather unsurprising consequence of certain facts about human psychology and neurology, and not a specifically linguistic fact at all? If so, then surely linguistic theory as such need not pay any attention to it.

It would be easy to get bogged down here in difficult questions about the relationship between the psychology of language and other branches of cognitive psychology. I would argue, however, that even if many generalisations about human language can be related to wider generalisations covering other aspects of human thought and behaviour too, the wider generalisations will seldom suffice by themselves to explain the relevant linguistic facts entirely. In other words, there will generally be subsidiary questions of a more specifically linguistic kind to do with how the general constraint in question is actually manifested, or complied with, in language. In the present case, the specifically linguistic question is: how far does language go to reduce the potential burden imposed on the the memory by an array of inflexional affixes such as (301)? To put it another way, what is the most complex distribution of Case-Number affixes, from the point of view of the language-learner, that language permits? From the answer to this question we can expect some indication of the actual upper limit to the number of affix-combinations that nouns in L may choose, more or less reduced from the logical upper limit that we have already established by simple arithmetic at 20,736. How precise this indication will be will depend on how tightly languages are actually constrained in this respect.

Before we attempt an answer to this question, it is worth reminding ourselves of one of the main reasons for the recent reawakening of interest among generative linguists in the notion of the morphological paradigm. This reason is the unexpected way in which phonological rules seem to apply or fail to apply in the generation of forms such as those of the Present Tense of regular Spanish verbs -- unexpected, that is, from the point of view of 'classical' generative phonology as expounded by Chomsky & Halle (1968). To account for this, it has appeared necessary to forego the claim that the sole evaluation criterion for phonological descriptions is formal simplicity in the phonological rules, and to recognise an independent evaluation principle favouring the minimisation of allomorphy in paradigms. This,

of course, entails recognising the inflexional paradigm as a genuine linguistic entity, with a role in 'competence', and not merely a grammarian's construct. Evidence of the kind adduced by James Harris (1973) in favour of the linguistic reality of paradigms seems to me quite strong. But such facts as we have so far uncovered in considering the possible distribution of Case-Number affixes in L already provide some confirmation which is quite independent of phonological evidence. If we say that the Independent Distribution Characteristic is impossible in human language and that this impossibility is due in some sense to the burden which a language with the Characteristic would impose on the memory, we are saying in effect that in the nominal Case-Number system of a language such as L, entailment relations hold between the distributions of certain of the affixes, and that these entailment relations are such as to enable the speaker to learn at least part of the affix-pattern for at least some nouns 'en bloc', thereby reducing the number of Case-Number combinations for which the realisations must be learnt individually. Let us use the term 'slab' for those subdivisions of affix-patterns which cohere so as to be learnable 'en bloc' in this way. Now 'slabs', as defined, are just like fragments of inflexional paradigms, in the traditional sense of the term 'paradigm'; so, to say that entailment relations hold between certain affixes in such a way as to define 'slabs' is equivalent to saying that at least fragments of traditional 'paradigms' deserve explicit recognition in linguistic theory. So, even before we have moved on from determining that the Independent Distribution Characteristic does not hold towards determining more precisely what the constraints on affix-distribution in a language such as L are, it is already possible to demonstrate the relevance of some version of the notion 'paradigm' to linguistic theory.

We now return to the question of how tightly the distribution of the affixes in (301) is in fact to be constrained, given that the Independent Distribution Characteristic is excluded. Let us consider one logical possibility which exploits the notion of 'slab' just introduced and which has the effect of reducing

the number of possible affix-patterns consistent with the Independent Distribution Characteristic by a factor of nearly 600. we will assume that the affixes listed indiscriminately in (301) can be arranged into ten partially overlapping slabs, thus:

(309)	Singular	Plural																		
Case 1	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">m</td><td style="border: 1px solid black; padding: 2px;">s</td><td style="border: 1px solid black; padding: 2px;">∅</td></tr> <tr><td style="border: 1px solid black; padding: 2px;">e</td><td style="border: 1px solid black; padding: 2px;">m</td><td style="border: 1px solid black; padding: 2px;">∅</td></tr> <tr><td style="border: 1px solid black; padding: 2px;">∅</td><td style="border: 1px solid black; padding: 2px;">∅</td><td style="border: 1px solid black; padding: 2px;">m</td></tr> </table>	m	s	∅	e	m	∅	∅	∅	m	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">i:</td><td style="border: 1px solid black; padding: 2px;">e:s</td><td style="border: 1px solid black; padding: 2px;">a</td></tr> <tr><td style="border: 1px solid black; padding: 2px;">i:</td><td style="border: 1px solid black; padding: 2px;">e:s</td><td style="border: 1px solid black; padding: 2px;">a</td></tr> <tr><td style="border: 1px solid black; padding: 2px;">a</td><td style="border: 1px solid black; padding: 2px;">∅</td><td style="border: 1px solid black; padding: 2px;">e:s</td></tr> </table>	i:	e:s	a	i:	e:s	a	a	∅	e:s
m	s	∅																		
e	m	∅																		
∅	∅	m																		
i:	e:s	a																		
i:	e:s	a																		
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4	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">i:</td><td style="border: 1px solid black; padding: 2px;">is</td></tr> </table>	i:	is	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">um</td><td style="border: 1px solid black; padding: 2px;">rum</td></tr> </table>	um	rum														
i:	is																			
um	rum																			
5	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">i:</td><td style="border: 1px solid black; padding: 2px;">o:</td></tr> </table>	i:	o:	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">i:s</td><td style="border: 1px solid black; padding: 2px;">ibus</td></tr> </table>	i:s	ibus														
i:	o:																			
i:s	ibus																			
6	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">o:</td><td style="border: 1px solid black; padding: 2px;">e</td></tr> </table>	o:	e	<table style="border-collapse: collapse; margin: auto;"> <tr><td style="border: 1px solid black; padding: 2px;">i:s</td><td style="border: 1px solid black; padding: 2px;">ibus</td></tr> </table>	i:s	ibus														
o:	e																			
i:s	ibus																			

Each slab contains affixes relating to a particular set of Case-Number combinations and contrasts with at least one other 'rival' slab containing an at least partially different set of affixes for the same set of Case-Number combinations. The significance of the slabs is that, if for a given Case-Number combination a given noun chooses affix A in slab X, it must choose affixes belonging to the same slab X for all the other Case-Number combinations in the subset which slab X covers; in other words, no noun can choose affixes out of rival slabs. So, for example, in the light of (309), if a noun in L chooses -e for Singular-2, it must also choose -m for Singular-1; but this choice does not commit it to any particular choice for Cases 4, 5 and 6 in the Singular or any Cases in the Plural. Clearly, if slabs play a role in the distribution of Case-Number affixes in L, the total number of possible inflexional patterns is reduced drastically, namely from more than 20,000 down to $3^2 \times 2^2 = 36$.

The important empirical question now is: is this reduction sufficient to render L a possible human language? If it is sufficient, one can go on to ask whether the notion 'slab' that we have introduced represents some real factor at work in morphological organisation which we can sensibly try to characterise more precisely, or whether it has led us to the right answer only by accident. But if 36 affixal patterns is still too many for L to be a human language, then the notion 'slab' has not led us to

the right answer at all, and it is premature to go on to that further stage of investigation. In trying to answer this question, it seems sensible to look first at the language on which L is modelled, namely Latin. As I have already mentioned, Latin is traditionally regarded as having five 'declensions', within three of which there is a further distinction between Neuters and non-Neuters which affects the the realisation of the Nominative, Vocative and Accusative Cases in the Singular. The exact number of distinct affixal patterns at the level of underlying phonological representations that a contemporary linguist would recognise will depend on his views about Latin phonology and morphophonology, but the total is unlikely to exceed eight -- considerably fewer than the 36 permitted under the 'slab' analysis illustrated at (309). Is the relatively tight restriction on affix distribution on Latin nouns an accident, from the general linguistic point of view? If not, then the notion 'slab' seems inadequate to account for the observed limits on inflexional variety in Latin. But to resolve the question, we clearly need to look at situations in other languages where more than one affix is available to realise given combinations of morphosyntactic properties.

An example from Hungarian is at (310)⁸:

(310)	Present Indefinite:	
	Indicative	Subjunctive
Sg 1	-ok, -om	-ak, -am
2	-(a)sz, -ol	-ő, -ál
3	-ő, -ik	-on, -ék
Pl 1	-unk	-unk
2	-(o)tok	-atok
3	-(a)nak	-anak

The 'mobile' vowels in parentheses are of no interest to us, because their presence is governed by the shape of the stem to which the affixes are attached and the need to avoid certain consonant clusters; a single underlying shape can therefore plausibly be postulated for those affixes with a mobile vowel, or at the very least the distribution of the alternants can be regarded as phonologically determined. But no purely phonological account

of the alternations in the Singular is possible; 'grammatically conditioned allomorphy' must be recognised. If we consider what distribution patterns for the Singular affixes are compatible with the Inflexional Parsimony Hypothesis, it is immediately clear that the choice must lie between two extremes; since there are property combinations with two realisations, there must be at least two distribution patterns, and since there are only six such property combinations, then even if Hungarian has the Independent Distribution Characteristic there can be no more than $2^6 = 64$ distribution patterns. What we in fact find in standard literary Hungarian is the extreme at the low end of the scale: two distribution patterns, or 'conjugations', known as the normal and the ik conjugation, with affixes distributed as follows (ignoring the Plural forms, which are common to both):

(311) Present Indefinite:

	Indicative:		Subjunctive:	
	Normal	<u>ik</u>	Normal	<u>ik</u>
Sg 1	-ok	-om	-ak	-am
2	-ol after sibilants; -(a)sz else- where	-ol	$\underbrace{\begin{matrix} -\emptyset \text{ (brusquer)}^9 \\ -\acute{a}l \text{ (politer)} \end{matrix}}$	
3	$-\emptyset$	-ik	-on	-ék

For a third relevant example I turn to some Zulu facts already discussed in the previous chapter. On the assumption of a synchronic phonological rule of vowel contraction, condensing a + u to o and a + i to e, we need to recognise two alternants for most of the Possessive ConCORDs, manifested by the 'possessor' noun in such phrases as umnyango womntwana 'the child's door' (see (216 a) in Chapter II). These alternants can be listed as follows:

(312)		Singular	Plural
Possessive Concord with a head noun belonging to Class:	1/2 } 1a/2a }	wa-, \emptyset	ba-
	3/4	wa-, \emptyset	ya-, \emptyset
	5/6	la-, li-	a-, \emptyset
	7/8	sa-, si-	za-, zi-
	9/10	ya-, \emptyset	za-, zi-
	11/10	lwa-, lu-	za-, zi-

(312) (continued)		Singular	Plural
Possessive Concord	14	ba-, bu-	ba-, bu-
with a head noun			
belonging to Class:	15	kwa-, ku-	

Here we see fifteen morphosyntactic slots, all but one of which are occupied by two distinct inflexions. The maximum number of distinct inflexional patterns which Zulu nouns could logically exhibit, consistently with the Inflexional Parsimony Hypothesis, is thus $2^{14} = 16,384$. Yet we already know that the number of patterns that Zulu nouns exhibit in fact is just two. Most nouns select the first of each pair of affixes in (312); Class la/2a nouns do so too when they themselves are Plural, but when Singular they select the second of each pair. This statement summarises the discussion of examples (219), (221), (222) and (223) in Chapter II.

In considering the appropriateness of the Independent Distribution Characteristic to human language, we have looked so far at only three topics: Latin noun inflexion, Hungarian verb inflexion and Zulu possessive concords. To generalise on the basis of such slim evidence may seem rash. But it is worth emphasising how exceedingly tightly these three areas of inflexional morphology are organised in comparison with the logical possibilities available. The Independent Distribution Characteristic permits over 20,000 'declensions' for the data from the hypothetical language L, 64 'conjugations' for the data from Hungarian, and over 16,000 possessive paradigms for Zulu. Yet in Latin (of which L is a restricted and simplified version) there are only around eight actual declension-types at the most; in Hungarian there are only two conjugation-types; and in Zulu there are only two possessive paradigms. The number of affix distribution patterns actually found is clearly at or very close to the mathematical minimum consistent with the resources available. Our search for further constraints on the distribution of inflexional affixes has been almost embarrassingly successful, so far. It seems quite justifiable, at this stage, to advance a very strong hypothesis concerning affix distribution patterns, to the effect that in any language a given set of inflexional

affixes must be arranged so as to yield as few inflexional patterns as possible for the lexical items to which they belong. This entails claiming that the inflexional patterns that are most important for linguistic description and that are, in some sense, most real psychologically for native speakers are not merely partial sets or 'slabs' of affixes and corresponding morphosyntactic property combinations, of the kind illustrated in (309), but rather complete paradigms covering all possible morphosyntactic property combinations for the relevant word-class. I will call this claim the Paradigm Economy Hypothesis. The next chapter will be devoted to formulating the Hypothesis more precisely and pointing out some consequences of it.

Footnotes to Chapter III

1. The avoidance of stem allomorphy is, of course, only one of the effects whose achievement has been attributed to a phonological 'conspiracy'. The term was first used widely in connexion with the Yawelmani facts described by Kisseberth (1970), where a number of distinct phonological rules seemed to conspire to prevent the occurrence of certain consonant clusters.
2. For discussion of the 'functional yield' ('realisierte strukturelle Ausnützung', 'rendement fonctionnel') of phonological contrasts, see Mathesius (1931) and, especially, Martinet (1955). For criticism of functional yield as a factor in accounting for phonological changes or the lack of them, see Weinreich et al. (1968: 133-137) and King (1969: 200-201).
3. (305 b) is, of course, acceptable if 'late' is interpreted as 'unpunctual'.
4. Wending does, of course, occur as Present Participle of the obsolescent verb wend, occurring now only in the phrase wend one's way. The important point, however, is that it does not occur alongside going as an alternative Present Participle for go.
5. I ignore here the question whether the Genitive -'s is strictly a nominal inflexion or a noun-phrase clitic (on which see Janda (1980)).
6. This is the first of the characteristics listed as typical of inflexion (in contrast to derivation) in section 1.2.
7. It will obviously be relevant to study in detail the process whereby forms belonging to originally distinct lexical items (such as Present go and Past went) come to be treated as morphologically suppletive. I do not know of any such diachronic study of a particular instance of suppletion.
8. Hungarian examples are cited in Hungarian orthography. Acute accents indicate vowel length, sz represents [s], and ş represents [ʃ]. In the Subjunctive column, the way the affixes are presented presupposes certain assumptions about the phonological shape of the Subjunctive stem, but these assumptions do not affect the argument here.

9. In the 2nd Singular Subjunctive we have, at least at first sight, a counterexample to the Inflexional Parsimony Hypothesis, in that two inflexions are available for the same 'slot' in both conjugations. The Hypothesis will predict this situation to be unstable; either the two alternants \emptyset and -ál will be differentiated functionally or else one will tend to disappear. The situation in fact seems to be that the -ál form, originally proper to the -ik conjugation, is now used in both conjugations to express a politer or less peremptory command than the \emptyset form (Peter Sherwood, personal communication). The Hypothesis's prediction therefore seems to be correct.

CHAPTER IV

THE PARADIGM ECONOMY HYPOTHESIS

4.1 A first formulation

We amassed evidence in Chapter III for a quite tight restriction on the number of distinct patterns into which the inflexional resources available for a given part of speech in some language may be organised. I christened our tentative hypothesis about this constraint the Paradigm Economy Hypothesis. An exact formulation of the Paradigm Economy Hypothesis clearly presupposes a more exact definition of the term 'paradigm'; for, although one of the main themes of our discussion has been the psychological reality of paradigms (or, more tentatively, their theoretical validity), we have not so far said explicitly what a paradigm is. I will offer here a reasonably precise definition, followed immediately by the promised formulation of the Paradigm Economy Hypothesis; I will then discuss certain details of each:

(401) A paradigm for a part of speech N in a language L is a pattern P of inflexional realisations for all combinations of non-lexically-determined morphosyntactic properties associated with N such that some member of N exemplifies P (i.e. displays all and only the realiations in P); except that, if two patterns P_1 and P_2 differ only in pairs of inflexions whose distribution can be accounted for phonologically, P_1 and P_2 count as one paradigm.

(402) Paradigm Economy Hypothesis

When in a given language L more than one inflexional realisation is available for some combination or combinations of non-lexically-determined morphosyntactic properties (some 'slot(s)') associated with some part of speech N, the number of paradigms for N is no greater than the number of phonologically independent ('rival') inflexional realisations available for the slot which has most such realisations.

It should be quite easy to see that the core of the formulation at (402) is the claim that inflexional affixes must be grouped into as few paradigms as is mathematically possible. This formulation therefore predicts precisely the Hungarian and Zulu facts cited in the previous chapter; it predicts that there can be no more than two conjugations of Hungarian verbs using the inventory of Person-Number inflexions at (310), and that there can be no more than two distribution patterns for the Zulu possessive concord markers inventoried at (312)¹.

Two points in the definitions at (401) and (402) call for comment: the term 'non-lexically-determined', and the proviso beginning 'except that ...' in (401). The effect of the term 'non-lexically-determined' in (401) is to allow us to assign two lexical items to the same paradigm even though they (or, rather, their corresponding inflected forms) are not morphosyntactically identical. It requires us, for example, to exclude properties of Gender from those properties whose realisations may help to identify a particular nominal paradigm in a language such as Latin; consequently, it allows us to say that mensa 'table' and nauta 'sailor' exemplify the same paradigm (or belong to the same declension-type) even though the former is Feminine and the latter Masculine. Defining 'paradigm' in this way certainly corresponds to traditional usage. More importantly, the phenomenon in question -- the cross-cutting of classification based on declension- or conjugation-type by classification based on some inherent or lexically-determined morphosyntactic property such as Gender -- is common enough so that to exclude the expression 'non-lexically-determined' in (401) would have embarrassing consequences. If we did exclude it, we would have to say not that mensa and nauta exemplified the same paradigm but that they exemplified two distinct paradigms which happened to be inflexionally identical; and we would have to say the same of (for example) dêmos 'people' and nêsos 'island' in Attic Greek, of d'ad'a 'uncle' and t'ot'a 'aunt' in Russian, and of Tag 'day' and Boot 'boat' in German; since the two nouns in each of these pairs differ in Gender even though the inflexions of both are the same.

A definition which committed us to saying that mensa and nauta in Latin belonged to different paradigms would not merely be infelicitous, however; it would seriously affect the empirical content of the Paradigm Economy Hypothesis. Consider again the Case-Number affixes of our hypothetical language L, set out at (301). I have said that the Paradigm Economy Hypothesis predicts that nouns in L will display not more than three distinct inflexional patterns; but this is so only if there is no lexically-determined category such as Gender applicable to nouns in L, or if Gender is irrelevant to the identification of nominal paradigms. For let us suppose that there are three nominal Genders in L; it follows that instead of twelve Case-Number slots we ought rather to speak of 36 Gender-Case-Number slots. Let us suppose further that each Case-Number inflexion can occur with nouns of any Gender. Now, if the expression 'non-lexically-determined' is removed from (401) and (402), we can no longer properly speak of nominal paradigms pure and simple but must distinguish Masculine, Feminine and Neuter paradigms, since a difference in Gender between two nouns will automatically a difference in paradigm membership. What will the Paradigm Economy Hypothesis predict about this situation? Certainly it will predict that there can be no more than three inflexional patterns for Masculine nouns in L, three for Feminines and three for Neuters, since no Gender-Number-Case slot will contain more than three distinct realisations. On the other hand, there will be nothing to guarantee that the three patterns chosen by one Gender out of the more than 20,000 possibilities will be the same as the three chosen by either of the other two Genders. This version of the Hypothesis, then, will predict that, if L has three Genders, the maximum number of distinct inflexional patterns is not three but nine. In more general terms, omitting 'non-lexically-determined' from (401) and (402) multiplies the permissible number of inflexional patterns associated with some part of speech in any language by the number of properties belonging to any lexically-determined morphosyntactic category applicable to that part of speech; moreover, it renders purely accidental any identity which may be observed between the inflexional

patterns displayed by words which differ in their lexically determined properties. So, provided that the behaviour of the languages that we investigate suggests that an increase in the permitted total of inflexional patterns is unnecessary, and given that we do observe in several languages inflexional identity between nouns of different Genders -- in mensa and nauta and the other pairs mentioned above -- we have reason to prefer versions of the definition of 'paradigm' and of the Paradigm Economy Hypothesis which include the expression 'non-lexically-determined', as (401) and (402) do.

We will see later that lexically determined properties, syntactic and semantic, do seem sometimes to be relevant to paradigm economy; but the circumstances under which their relevance is most apparent can be quite narrowly specified, and this very fact tends to confirm the correctness of not allowing them a role in the identification of paradigms for normal purposes.

The purpose of the proviso at the end of (401) is simply to allow us to treat as a single paradigm a pattern within which there are phonologically determined alternants for some slot, whether or not these alternants can be accounted for in terms of a shared underlying phonological representation. It allows us, for example, to say that the number of paradigms between which Hungarian verbs are divided is only two, even though within the 'normal' conjugation-type, as already mentioned, one must recognise at least two patterns of inflexional realisation, on the basis of the inflexions which appear in the 2nd Singular Indefinite Present Indicative slot: e.g. olvas 'he reads', olvas-ol 'you read', versus ír 'he writes', ír-sz 'you write'. Defining 'paradigm' in this way allows us thus to follow the tradition of Hungarian grammatical description inasmuch as our paradigms and the traditional conjugations correspond. More importantly, it prevents the Hungarian facts from constituting a counter-example to the Paradigm Economy Hypothesis, since, without the proviso, we would need to allocate the three Hungarian verbs olvasni 'to read', írni 'to write' and inni 'to drink' to three distinct paradigms even though they exhibit no more than two distinct realisations

between them in any morphosyntactic slot, thus:

(403) Indefinite Present Indicative:

Realisations available:		<u>írni</u> 'write'	<u>olvasni</u> 'read'	<u>inni</u> 'drink'
2 (ok, om)	Sg 1	ír-ok	olvas-ok	isz-om
2 (sz, ol)	2	ír-sz	olvas-ol	isz-ol
2 (∅, ik)	3	ír	olvas	isz-ik
1 (unk)	Pl 1	ír-unk	olvas-unk	isz-unk
1 (tok)	2	ír-tok	olvas-tok	isz-tok
1 (nak)	3	ír-nak	olvas-nak	isz-nak

But the proviso is not merely an arbitrary device to protect the PEH from disproof. Suppose we abandoned the Paradigm Economy Hypothesis and adopted instead some laxer hypothesis about paradigm organisation to which these Hungarian facts would not constitute even prima facie counter-evidence: this would amount to saying that the phonological predictability of the distribution of the 2 Sg endings -sz and -ol within the írni and olvasni 'paradigms' was morphologically quite irrelevant -- a convenience for learners and speakers of Hungarian, perhaps, but by no means a necessity from the ^opoint of view of morphological theory, since morphological theory would tolerate the existence of these three distinct paradigms in Hungarian even if the distribution of -sz and -ol were phonologically quite unmotivated. The trouble with this laxer hypothesis, then, is that it makes no distinction between the actual Hungarian state of affairs, where phonology comes to the rescue, as it were, over the choice of a 2 Sg ending for 'normal' (i.e. non-ik) verbs, and a hypothetical state of affairs where, shall we say, the alternative 1st Sg endings -ok and -om are scattered among 'normal' verbs in a quite unpredictable fashion.

The drawbacks of the laxer hypothesis are equally evident when we apply it to the nominal Case-inflexions of Dyirbal, as described by Dixon (1972). Superficially, Dyirbal may seem to permit a considerable number of distinct declension-types, and certainly at least one more than the Paradigm Economy Hypothesis predicts it should. The five nouns illustrated in (404) below

each seem to illustrate a distinct inflexional pattern -- a distinct combination of inflexions -- for the three Cases Nominative, Ergative and Simple Genitive, yet none of these three Cases (and none of the Cases not illustrated here) has as many as five distinct inflexional realisations (cf. Dixon 1972: 42):

	(404) Realisations available:			
Nom	1 (base unchanged)			
Erg	4 (- <u>ɟu</u> , - <u>ɟu</u> , - <u>ɟgu</u> , - <u>gu</u>)			
Simple Gen	2 (- <u>ɟu</u> , - <u>u</u>)			
Nom	walguy	biɟdirɪp	gubur	yaɟa
	'brown snake'	'lizard'	'bee'	'man'
Erg	walguyɟu	biɟdirɪpɟu	guburɟu	yaɟaɟu
Simple Gen	walguyɟu	biɟdirɪpu	guburɟu	yaɟaɟu
Nom	yamani 'rainbow'			
Erg	yamanigu			
Simple Gen	yamanipu			

One possible conclusion from these facts is that the Paradigm Economy Hypothesis, as formulated at (402), is simply too strong. But this does not take into account a fact that emerges clearly as soon as we look at a wider sample of Dyirbal nouns: the choice between the various realisations of the Ergative and the Simple Genitive is phonologically determined. As Dixon (1972: 42) puts it:

"ERGATIVE ... involves the addition of:

- (i) -ɟu to a disyllabic stem ending in a vowel;
- (ii) -gu to a trisyllabic or longer stem ending in a vowel;
- (iii) a homorganic stop plus -u to a stem ending in a nasal or -y;
- (iv) -ɟu, together with the deletion of the stem-final consonant, when the stem ends in -l, -r or -ɟ; ...

SIMPLE GENITIVE involves the addition of -u for stems ending in a nasal, and the addition of -ɟu for all other stems. ..."

If we look at these facts in the light of the final proviso of our definition of 'paradigm' at (401), we see that all five inflexional patterns in (404) count as one paradigm; our hypothesis about 'economy' in the number of paradigms into which the infle-

xional resources of a language may be organised does not come into play here at all, because the organisation is already as 'economical' as it could logically be. On the other hand, if we abandoned the final proviso of (401) and so were forced to regard these Dyirbal facts as genuine counterevidence to the strict version of the Paradigm Economy Hypothesis at (402), we would be hard put to it to frame a laxer version of the Hypothesis which discriminated at all, from the point of view of morphological theory, between the actual Dyirbal state of affairs and a hypothetical state of affairs in which (shall we say) the Ergative ending was constant except that stems ending in liquids were divided between two phonologically arbitrary inflexional patterns, some displaying -ru and some -gu. Our present account does discriminate between the two states of affairs, and predicts that the hypothetical one could not exist; it therefore represents the stronger theory, and should be preferred so long as no genuine counter-evidence appears². Some apparently genuine counterevidence, not involving phonologically predictable alternations, will in fact be discussed in Chapter V.

4.2

Some consequences of the Paradigm Economy Hypothesis

One immediate consequence of the Paradigm Economy Hypothesis, which may dispose us in its favour, is that, if the native speaker's knowledge of the inflexional behaviour of a given noun (say) is thought of in terms of lexical assignment to one of a range of nominal paradigms as defined in (401), the Paradigm Economy Hypothesis guarantees that the number of paradigms that the native speaker must learn is no greater than is necessary to accommodate (or 'find work for') the inflexional resources that the language makes available. So, if the function of paradigms, in some extra- or meta-linguistic sense, is to minimise the burden which phonologically unconditioned allomorphy potentially imposes on the memory, then it follows from the Paradigm Economy Hypothesis that paradigms are as efficient in fulfilling this function as they could possibly be. We have thus arrived at a hypothesis about the specifically linguistic mechanism which supplements or, as it were, puts into effect in language

the rather vague general psychological principle which I mentioned in Chapter III as perhaps underlying the inadmissibility of the Independent Distribution Characteristic.

One apparent difficulty with the formulation of the Paradigm Economy Hypothesis at (402) lies in deciding what counts as 'the same' inflexional realisation. When I first introduced our hypothetical Latin-like language L and its nominal inflexions in (301), I chose to assume that the 'rival' affixes for each slot could not plausibly be related by any phonological rules or processes in L. I also tacitly assumed that there was no absolute neutralisation of any underlying distinctions among affixes. Insofar as L is a hypothetical language, I am of course entitled to make whatever assumptions I like about it. But as soon as we try to apply the Paradigm Economy Hypothesis to real languages, we will quickly find ourselves having to make decisions about inflexions which in their phonetic representations are distinct rivals but whose underlying representations are problematic in the sense that it is not obvious whether they should be regarded as distinct at that level too or as derived from some common source, with their phonetic divergence attributable to (morpho-) phonological rules. We may also find instances of the converse: realisations which on the surface are the same but which we may suspect are derived from differing underlying forms by a process of phonological neutralisation. The prediction that the Hypothesis will make about the number of paradigms in such languages will, it seems, depend crucially on the decision taken on (morpho)phonological grounds about the distinctness of underlying representations. And, since there is not yet a universally agreed theory of phonology prescribing a unique phonological analysis for each language, it may seem possible to protect the Paradigm Economy Hypothesis from falsification by rigging the underlying representations of the inflexions appropriately.

The second of the two kinds of phonological dilemma -- that involving neutralisation -- lends itself particularly to this kind of manoeuvre. For example, let us imagine a hypothetical

language I2, with three distinct Case paradigms for nouns (three declensions) as follows (where the affixes are to be understood as being in their 'surface' phonetic representations):

(405)	Decl. 1	Decl. 2	Decl. 3
Case 1	-i	-a	-i
Case 2	-u	-u	-o
Case 3	-un	-un	-o

On the face of it, the Paradigm Economy Hypothesis predicts that I2 will not be a possible human language, because, although there are three paradigms, there are no more than two rival realisations for each Case. But one could in principle save the Hypothesis from disproof by I2 if one posited that in Case 1, for instance, the surface -i in declension 3 was derived phonologically from another underlying segment (say, /e/) and was therefore underlyingly distinct from the -i of declension 1; we would thus have three rival realisations after all for one of the Cases, and three distinct paradigms would therefore be permitted by the Paradigm Economy Hypothesis. And insofar as that manoeuvre is theoretically possible, one might argue, the Hypothesis is invulnerable to empirical disconfirmation and therefore vacuous. So, to preserve the empirical content of the Hypothesis, it seems as if we are ineluctably drawn into the controversy on abstractness in phonology which I set out in Chapter I to avoid.

This kind of objection can, however, be answered satisfactorily. It is certainly a pity that phonological theory is not more firmly established, and as long as there are uncertainties there, there are bound to be uncertainties about the empirical consequences of hypotheses in any other area of language to the extent that they presuppose phonological analyses. But the uncertainty need not render morphological hypotheses vacuous any more than uncertainty in semantics precludes the formulation and testing of empirical hypotheses about syntax. Let us consider again the I2 situation illustrated in (405) and the idea of deriving the -i in Case 1 of declension 3 from /-e/. If in I2 there were instances of [e] contrasting minimally with [i], so that any rule changing /e/ to [i] would have to be hedged about with phonologically ad hoc restrictions, we would need to feel very confident

of the validity of the Paradigm Economy Hypothesis, on the basis of evidence from a variety of languages, in order to be willing to countenance such a desperate ploy in defence of it. Even with such independent confirming evidence, we might choose to abandon the Hypothesis in the light of (405) if it were apparent that the kind of ad hoc restrictions on the /e/-to-[i] rule that would be needed would be unique in L2 and unparalleled in other languages. If, on the other hand, [i] never contrasted with [e] in final position in L2 and there was independent evidence for a vowel-raising rule, it would seem quite reasonable to invoke a phonological explanation for the absence of three distinct Case 1 suffixes in (405) and thus reconcile L2 with the Paradigm Economy Hypothesis. What this hypothetical but (I hope) reasonably realistic example illustrates is that, even if there are no universally agreed limits on what is permissible in phonological representations and rules, it should not be hard in most actual instances to decide whether phonological manoeuvres to account for potential counter-examples to the Paradigm Economy Hypothesis are justifiable or not. It also illustrates how a reasonably well established theory of what is and is not permissible in paradigmatic patterning on purely morphological grounds could in principle help to resolve phonological dilemmas: if, for example, we were firmly convinced of the validity of the Paradigm Economy Hypothesis, on the basis of a wide range of independent morphological evidence, we might be persuaded by the data of (405) that [i] could legitimately be derived from some underlying segment such as /e/ even in the absence of any independent evidence in L2 to that effect and even at the cost of permitting types of phonological rule, or degrees of abstractness in underlying representations, that we would otherwise have preferred to outlaw. There are, no doubt, other kinds of phonological dilemma which a tight, independently justified theory of the paradigm could help to resolve, and the usefulness of such a theory for such purposes is, of course, one of the reasons for wanting to develop it.

The Paradigm Economy Hypothesis also has consequences for phonological change. Paradigms are certainly no more stable over

time than the inflexions which they incorporate, and one could argue considerably less so. For an example of large-scale paradigmatic restructuring in different directions in related languages, one can cite the development of the various types of Indo-European 'i-stem' nouns in Sanskrit, Latin and Greek. In classical Sanskrit, most polysyllabic i-stems follow a Case-Number paradigm very similar to, or perhaps best regarded as morphologically identical with, that of the a-stems, while y-stems tend to resemble more closely the consonant-stems; in classical Latin, all i-stems are paradigmatically quite distinct from a-stems and some have adopted a 'mixed' inflexional pattern close to, yet not identical with, the regular consonant-stem one, to which some original consonant-stem nouns have also been attracted; while in Attic Greek i-stem nouns have developed peculiar affixes of their own (at least 'on the surface'), or else have acquired a new stem-final -d- after the i and thereby passed into the consonantal declension-type. One possible approach to these various developments is simply to account for them piecemeal, in terms of the borrowing of individual affixes by one declension from another, without attempting to find any deeper rationale. But, if the Paradigm Economy Hypothesis is broadly correct, one sort of deeper rationale may be discoverable. Whether the Hypothesis can help to elucidate these changes in practice is, of course, very much an open question at present. At this stage I will do no more than illustrate one sort of change that the Hypothesis will predict.

Let us imagine a third hypothetical language I₃, with a set of Case affixes arranged in three declensions as follows:

(406)	Decl. 1	Decl. 2	Decl. 3
Case 1	-e	-a	-i
Case 2	-u	-u	-o
Case 3	-un	-un	-o

These data are very similar to those of I₂, set out in (405). The only difference is that for Case 1 of declension 1 the realisation is -e, not -i. Because of this, I₃ has three distinct affixes for Case 1 and the existence of three paradigms is compatible with the Paradigm Economy Hypothesis, without the need

for any phonological jiggery-pokery. Let us now suppose that, by a phonological innovation, all e are raised to i (compare the development of [ɛ:] and [e:] to [i] in Hellenistic and Byzantine Greek, and the more recent shift of [e] to [i] in northern Greek dialects). This innovation will transform (406) into (405). If the Paradigm Economy Hypothesis is correct, and if we reject the possibility of reconciling (405) with the Hypothesis by invoking phonological neutralisation, then it follows that the pattern illustrated in (405) must change: either a new rival realisation must be 'invented' for one of the three Cases, so as to permit the maintenance of three distinct declensions, or else the number of declensions must be reduced, with perhaps the loss of one of the rival realisations for some of the other Cases. The prediction that the Paradigm Economy Hypothesis makes is thus reasonably clear; and even though the data in any real language are unlikely to be as clear-cut as in our hypothetical example, the Hypothesis certainly offers a new tool with which to try to make sense of, for example, the emergence of the Latin 'mixed declension' already mentioned. I attempt this in Chapter VI.

4.3 Further questions

Even if a morphological constraint exists of which the Paradigm Economy Hypothesis is a broadly correct statement, it provokes a number of further questions. These include:

(407) Does the existence of n distinct realisations for any combination of morphosyntactic properties suffice to ensure the stable maintenance of n paradigms, or does this depend on the particular combinations concerned (e.g. the most frequent or the least 'marked')?

(408) Can paradigms have variants, or subtypes? This amounts to asking whether, in our formulation of the Paradigm Economy Hypothesis at (402), we were right to say 'the number of paradigms ... is no greater than the number of ... inflexional realisations ...' rather than '... the same as ...'. At first sight, this alteration may seem to have

no empirical consequences, since any data which we might wish to account for by postulating, say, two nominal declension-types of which one has a partially distinct subtype might equally be accounted for by postulating three separate declension-types. But there could, I think, be consequences for what the Hypothesis predicts about morphological change. With three distinct declensions, one would on the face of it expect no particular tendency for nouns to switch from one to another unless the distinctness of some of the inflexions were affected by phonological changes; on the other hand, if the theory permitted us to regard two nearly identical inflexional patterns as variants of a single paradigm, we might expect a drift of lexical items from one variant (presumably the minority or 'marked' one) to the other. I will in fact suggest later two sorts of circumstance in which it is legitimate to acknowledge more than one realisation for the same slot within a single paradigm: one involving lexically determined morphosyntactic properties (with evidence from German, Russian, Dyirbal and Latin) and one involving straightforward exceptionality (with evidence from the history of Latin).

- (409) We have discussed inflexion so far solely in terms of affixes, ignoring the stems to which they are affixed. But not only can stems themselves change, but the boundary between stem and affix may be by no means obvious; for example, Wurzel (1970) and Lieber (1980), who postulate in their treatments of German noun morphology separate processes of inflexional stem formation and 'inflexion proper', draw the line between these processes in untraditional places. What is more, of course, inflexion need not involve affixation at all. How the Paradigm Economy Hypothesis is to be linked with stem alternation is not immediately obvious. I will suggest an answer

to this problem in Chapter VII.

(410) We have not so far consider the relationships between inflexions within a single paradigm -- for example, the constraints (if any) on phenomena such as syncretism. This will be the subject of Chapters VIII and IX.

(411) We have not considered the differences (if any) between paradigms of inflexions which realise single morphosyntactic properties (such as /s ~ z ~ iz/ of English cats, dogs, horses realising Plural, or the Case suffixes of Turkish or Hungarian), and those which realise combinations of such properties -- that is, examples of Deviation III or 'cumulative exponence' such as the /i:s ~ ibus/ of Latin dominīs 'to the lords', rēgibus 'to the kings' realising Dative Plural. I will argue in Chapter VIII that this difference is relevant to the issue of syncretism, just mentioned.

The fact that so many further issues press for attention is a good sign, not a bad one. At the outset of work in a relatively neglected area of linguistic theory, it is something just to be able to identify and formulate specific questions whose resolution seems likely in principle to do most to advance our understanding at this stage. I do not claim that (407)-(411) represents a definitive list of such questions or even that a definitive list is possible; but they all seem worthwhile questions and ones which, in spite of our present state of ignorance, it is not overambitious to tackle. For the present, I will concentrate in Chapter V on some apparent counter-evidence to the Paradigm Economy Hypothesis in German and other languages which will lead to a revised formulation of the Hypothesis and the definitions which underlie it.

Footnotes to Chapter IV

1. It may seem odd that I have chosen in (402) to say "... no greater than the number of phonologically independent ('rival') inflexional realisations ..." rather than "... the same as the number ...". The reason is that, under certain circumstances, I will argue that there can be fewer paradigms than there are inflexions for the most lavishly provided 'slot'. What circumstances these are will be explained in our discussion of certain German and other facts in Chapter V.

2. My analysis of the Dyirbal inflexional patterns at (404) as all belonging to the same paradigm, in the sense of the definition at (401), does not of course rely on deriving all four realisations of the Ergative from a single underlying phonological representation. Dixon attempts to do this, however (1972: 288-289), apparently because (like Anderson (1974) and Hyman (1975)) he is unhappy about recognising phonologically predictable alternations which are not the outcome of phonological or morphophonological rules; but his account, as he admits, involves postulating an otherwise unmotivated difference in phonological behaviour between the Dative suffix -gu and a putative underlying Ergative /-gu/.

CHAPTER V

REVISING THE PARADIGM ECONOMY HYPOTHESIS; MACROPARADIGMS AND SLABS

5.1 Prima facie counter-evidence to the Paradigm Economy Hypothesis

One apparent corollary of the Paradigm Economy Hypothesis has not so far been mentioned; if no more paradigms may exist for any part of speech than there are distinct realisations for that morphosyntactic slot which is most generously provided with realisations, then surely it should be possible to determine unambiguously which paradigm (declension-type or conjugation-type) a word belongs to simply by referring to the form in which that word appears in the inflexionally most diverse slot. More briefly, the slot most generously provided with distinct inflexions ought to have a diagnostic role too; if one wants to indicate as succinctly as possible the inflexional pattern which a given word exhibits -- for example, in a dictionary entry --, it ought not to be necessary to cite more than this one diagnostic form. Yet anyone who has studied even a moderately highly inflected language knows that frequently more than one inflected form or 'principal part' has to be cited in dictionary entries and elsewhere in order to provide an adequate basis for predicting a word's whole inflexional behaviour. Is this not powerful evidence to the effect that the restriction imposed on inflexional organisation by the present version of the Paradigm Economy Hypothesis is much too strong?

In response to this criticism, I would point out first that, from the fact that in some language more than one 'principal part' needs to be cited in order to identify the inflexional pattern of a word clearly, it does not necessarily follow that there must be more distinct paradigms than the present version of the Paradigm Economy Hypothesis allows. Consider a hypothetical language with three nominal declension-types inflected for Case by means of prefixes as follows:

(501)	Type A	Type B	Type C
Case 1	a-	i-	i-
2	e-	a-	e-
3	an-	aN-	en-

where N means a nasal homorganic with any following obstruent, otherwise n.

Here, it is clearly Case 3 which legitimates the maintenance of three distinct declension-types from the point of view of paradigm economy, since Cases 1 and 2 are each realised in only two ways. If, then, there is to be any single inflected form which will indicate unambiguously each noun's inflexional behaviour, it must be the Case 3 form. But consider an inflected form such as antam 'house (Case 3)'. We can determine from this without difficulty that the stem is -tam and that the word does not belong to declension-type C; but we cannot determine which of the other two declension-types it belongs to, since we cannot tell whether the prefix an- represents the /an-/ of Type A or the /aN-/ of Type B. This would not, of course, present any serious practical problem to the writer of a dictionary of our hypothetical language. Two obvious ways of presenting the necessary information on declension-type would be to cite forms of some other Case alongside those of Case 3 or else, while citing Case 3 forms only, to use a 'morpho-phonemic' symbol like our N to distinguish the Type B an- from the Type A one. What is important here is that, even if the first of these alternative courses is adopted, the hypothetical Case-system at (501) will still remain consistent with the Paradigm Economy Hypothesis. For the linguist testing the correctness of the Hypothesis, then, the moral is that even if the inflexional pattern of some language seems to require the citation of more than one 'principal part' in order to identify the inflexional pattern of nouns, verbs or whatever, the facts need not necessarily conflict with the Paradigm Economy Hypothesis, even *prima facie*.

It must be admitted, however, that plenty of instances of the need for multiple 'principal parts' exist which cannot be dealt with so neatly. For example, students of German, Latin and Attic Greek have traditionally had to learn three 'parts' for

German nouns, four 'parts' for Latin verbs and six 'parts' for Greek verbs, e.g.:

(502) a. German:

Nom Sg	Gen Sg ¹	Nom Pl	
Tag	Tages	Tage	'day'
Gast	Gastes	Gäste	'guest'
Mann	Mannes	Männer	'man'
Neffe	Neffen	Neffen	'nephew'
Staat	Staates	Staaten	'state'

b. Latin:

1st Sg Impf Pres Ind Act	Impf Infin Act	1st Sg Pf Pres Ind Act	Supine
amō	amāre	amāvī	amātum 'love'
sonō	sonāre	sonuī	sonitum 'sound'
terreo	terrēre	terruī	territum 'scare'
pōnō	pōnere	posuī	positum 'put'
cado	cadere	cecidī	cāsum 'fall'
spondeo	spondere	sponduī	sponsum 'pledge'

c. Greek:

1st Person Singular Indicative:

Active:				Middle/Pass: :
P Present	Future	Aorist	Perfect	Perfect
horō	ópsomai	éidon	heōrāka	heōrāmai or ōmmai
bállō	balō	ébalon	béblēka	béblēmai
lūō	lūsō	élusa	léluka	lélumai

Passive:

Aorist

ōphthēn	'see'
eblēthēn	'throw'
elúthēn	'loose'

Even without a thorough knowledge of these three languages, it is fairly easy to see that we cannot single out one slot from among those quoted for each language as the one to play the diagnostic role seemingly required by the Paradigm Economy Hypothesis. For example, among the Latin verbs at (502 b) we cannot select the Imperfective Indicative form because amō, sonō and cadō, alike there,

diverge in the Perfective and the Supine; we cannot choose the Perfective because cadō and spondeō, alike in showing initial reduplication there, diverge in the Infinitive; and so on.

I will argue that facts of this kind do indeed require us to revise the present formulation of the Paradigm Economy Hypothesis; but that the revision needed still leaves us with an extremely strong and restrictive hypothesis about paradigm organisation. More specifically, I will argue that all counter-evidence to the present formulation involves:

- either (a) the realisation of lexically determined (as opposed to syntactically acquired) morphosyntactic or 'morphosemantic' properties;
- or (b) a version of the 'slab' notion discussed in section 3.4 in Chapter III;
- or (c) stem allomorphy.

Stem allomorphy in relation to the Paradigm Economy Hypothesis will be dealt with in Chapter VII. Topics (a) and (b) are best discussed in relation to concrete examples of *prima facie* breaches of paradigm economy, and both will be relevant to the next such example: nominal declension in German.

5.2 German nominal declension and the notion 'macro-paradigm'

German nouns are inflected for two non-lexically determined categories: Number (Singular and Plural) and Case (Nominative, Accusative, Genitive and Dative)². There are thus eight morphosyntactic slots to be filled. If we take the Nominative Singular form as the 'base' for Case-Number inflexion, the inflexional resources available for each of these slots may be summarised as follows:

	(503) Singular	Plural
N	∅ (i.e. base unchanged)	∅, -e, u, ue, -r, ur, -s, -n
A	∅, -n	ditto
G	∅, -s, -n, -ns	ditto
D	∅, -e, -n	∅, -n, u, un, -rn, urn, -s

-- where u indicates umlaut (fronting) of a back vowel within the

base. On the face of it, then, the Paradigm Economy Hypothesis predicts that German nouns should be organised into no more than eight paradigms. But, once we examine the phonological contexts of these inflexions, we find that some of the alternations are phonologically determined and that the number of distinct 'rivals' for certain slots is reduced, thus:

(504) Singular	Plural
N \emptyset	
A \emptyset , -n	$\emptyset \sim -e$, $\text{u} \sim \text{ue}$, $-r \sim \text{ur}$, -s, -n
G \emptyset , -s, -n, -ns	
D \emptyset , $\emptyset \sim -e$, -n	$\emptyset \sim -n$, $\text{u} \sim \text{un}$, $-rn \sim \text{urn}$, -s

Broadly speaking, the alternants with -e of the inflexions affected occur after stressed syllables, the e-less ones elsewhere. But the details of the phonological conditioning are not important. What is important is that the slots where there is phonologically determined alternation include the most lavishly supplied ones: the non-Dative Cases of the Plural, where eight realisations are reduced to five. The prediction that flows from the Paradigm Economy Hypothesis is thus tighter than at first appeared: German should have no more than five distinct nominal declension-types. But this prediction is not correct. Does German, then, force the abandonment of the Paradigm Economy Hypothesis?

Before we adopt such a pessimistic conclusion, we ought to look at the German facts a little closer. The mathematical maximum number of paradigms, given the inflexional resources set out at (504), is extremely large: $1 \times 2 \times 4 \times 3 \times 5 \times 5 \times 5 \times 4 = 12000$. Even if (anticipating Chapter VIII) we regard the brace uniting the non-Dative Plural Cases as more than merely accidental, so that only one choice, not three, is involved there, we still arrive at a total of 480. Yet the actual total of distinct paradigms observable in modern standard German is no more than 10^3 :

(505) I	II	III	IV
Sg N Tag 'day'	Gast 'guest'	Mann 'man'	Uhu 'eagle-owl'
A Tag	Gast	Mann	Uhu
G Tages	Gastes	Mannes	Uhus
D Tage	Gaste	Manne	Uhu
Pl NAG Tage	Gäste	Männer	Uhus
D Tagen	Gästen	Männern	Uhus

Principal parts:

Sg G -s	-s	-s	-s
Pl NAG -e	«e	«r	-s
V	VI	VII	VIII

Sg N Hand 'hand'	Rose 'rose'	Mutti 'mummy'	Bär 'bear'
A Hand	Rose	Mutti	Bären
G Hand	Rose	Mutti	Bären
D Hand	Rose	Mutti	Bären
Pl NAG Hände	Rosen	Muttis	Bären
D Händen	Rosen	Muttis	Bären

Principal parts:

Sg G ø	ø	ø	-n
Pl NAG «e	-n	-s	-n
IX	X		

Sg N Dorn 'thorn'	Name 'name'
A Dorn	Namen
G Dornes	Namens
D Dorne	Namen
Pl NAG Dornen	Namen
D Dornen	Namen

Principal parts:

Sg G -s	-ns
Pl NAG -n	-n

Is there any way of reconciling these facts with some version of the Paradigm Economy Hypothesis? I suggest that there is. One point to note is that, as so far formulated, the Paradigm Economy Hypothesis pays no attention to the realisation of lexically determined properties, such as Gender in nouns. There are good reasons for this, discussed in section 4.1 of Chapter IV. Yet German has three Genders, Masculine, Feminine and Neuter. A point

to explore, therefore, is whether any of the ten paradigms in (505) are regularly linked with particular Genders. If so, and if it proves possible to correlate particular pairs or groups of paradigms with particular Gender contrasts, we might consider amending our definition of 'paradigm' or our formulation of the Paradigm Economy Hypothesis so as to permit departures from strict economy which are associated in some way with lexically determined properties.

If we look for Gender biases among the paradigms in (505) we find that they do indeed exist:

(506) I: M, N, no F

II: M, N (rarely, e.g. Kloster, Pl Klöster 'monastery'), no F

III: M, N, no F

IV: M, N, no F

V: F only

VI: F only

VII: F only

VIII: M only

IX: M, N, no F

X: M (rare, e.g. Gedanke 'thought', Buchstabe 'letter of the alphabet'), no N (unless we count Herz 'heart', Acc Sg Herz, not "Herzen"), no F

Comparison of (506) with (505) reveals one striking generalisation: Feminine nouns display the same form throughout the Singular -- that is, they all lack endings such as the Genitive -s and the Acc-Gen-Dat -n. This suggests immediately one way of pairing paradigms on the basis of Gender; a Feminine-only paradigm may be paired with any non-Feminine paradigm which it resembles in all Cases except the oblique (non-Nominative) Singular ones. Acting on this suggestion, we arrive at the following result:

(507) Feminine paradigm V can be paired with Masc-Neut II;

"	"	VI	"	"	"	"	VIII, IX, X;
"	"	VII	"	"	"	"	IV.

How might this be relevant to paradigm economy? What we are groping towards, clearly, is some principle to the effect that two paradigms 'count as one' for the purpose of paradigm economy if

all the inflexional differences between them are associated with a consistent difference in some lexically determined property such as Gender. We might also express this in terms of the realisational functions which inflexions fulfil; apparent breaches of paradigm economy are permissible, perhaps, provided that the paradigms can be grouped or paired, as above, in such a way that the inflexional differences within each pair are assigned a function -- that of realising certain lexically determined properties not taken into account at the first stage of paradigm identification. But whether it is worth pursuing this idea and attempting to formulate it more precisely depends, of course, on how well it seems likely to work in helping to account for actual problematic examples.

Let us invent a new term 'macroparadigm' to denote both pairs or groups of paradigms which result from the process described and individual paradigms which are left unpaired by it. In relation to the German facts, the question now is: granted that there are too many paradigms in German nouns to comply with the Paradigm Economy Hypothesis, can the Hypothesis be reconciled with the facts if we apply it instead to German nominal macro-paradigms? The answer is: not quite. We are still left with seven macroparadigms, as follows:

(508)	I	II M, N ~ V F	III
Principal Sg G	-s	-s ~ \emptyset	-s
parts: Pl NAG	-e	ʷe	ʷr
IV M, N ~ VII F	VIII M ~ VI F	IX M, N ~ VI F	X M, N ~ VI F
-s ~ \emptyset	-n ~ \emptyset	-s ~ \emptyset	-ns ~ \emptyset
-s	-n	-n	-n
Total 'rival'	Sg G	3: -s(~ \emptyset), -n(~ \emptyset), -ns(~ \emptyset)	
inflexions:	Pl NAG	5: -e, ʷe, ʷr, -s, -n	

One possibly rather embarrassing fact here is that the Feminine-only type VI can be paired by our technique with not one but three Masculine or Masculine-Neuter types: VIII, IX and X. It follows that, whereas the assignment of a given noun to its appropriate paradigm (as defined at (401)) is automatic, at least once any phonologically determined alternations have been got out

of the way, the assignment of a noun to a single appropriate macroparadigm may well not be; furthermore, it is not at all obvious what sort of evidence might enable one to resolve the issue. But what is more serious about (508) is that we are still left with seven macroparadigms -- two more than the Paradigm Economy Hypothesis predicts should exist. I will argue in section 5.3, however, that the facts can still be accommodated at a reasonable cost; the cost of recognising the possibility of breaches of paradigm economy in certain circumstances which can be fairly precisely specified by reference to a version of the notion 'slab'. In sections 5.3 and 5.4 I will also cite evidence from other languages for the usefulness of the notions 'slab' and 'macroparadigm', and in section 5.5 I will discuss the effects which the macroparadigm concept has on the empirical content of the Paradigm Economy Hypothesis as formulated at (402). Finally, in section 5.6, I will propose a revision of our definition of 'paradigm', one of whose effects is to resolve the dilemma over which macroparadigm our German type VI belongs to.

Part of the problem is due to the existence of type IX (or IX~VI) as a distinct macroparadigm. One way of solving it, then, might be to show that IX can be amalgamated with some other declension-type on the basis of the sort of argument that we have applied to the Feminines. In the rest of this section, I will argue that, although there is some evidence for a solution on those lines, this evidence is not conclusive; consequently, that part of the problem still remains, and it is still necessary to search for an alternative solution to it.

Type IX contains a fair number of Masculine nouns, such as Staat 'state', See 'lake', Schmerz 'pain', Strahl 'ray', Professor, Vetter 'cousin', as well as a few Neuters such as Bett 'bed', Auge 'eye', Insekt 'insect'. The first point to note about this declension-type is that none of its inflexions are peculiar to it; it is a 'mixed' type in that for every Case-Number combination its inflexion resembles that of one or more of the other declension-types. More specifically, it resembles types I, II and

and III (traditionally known as the 'strong' declension-types) in the Singular and types VI and VIII (traditionally called 'weak') in the Plural. Is there any ground for amalgamating or pairing it with one of these unmixed types? So far, the only condition under which we have considered permitting such amalgamation is if the two paradigms in question can be deemed a macroparadigm -- that is, if the inflexional differences between them can be correlated with a difference in some lexically determined category such as Gender. Clearly, we cannot appeal to Gender as a ground for such amalgamation here, because all the potential new candidates for pairing with IX (where by 'new candidates' I mean 'excluding VI, with which IX has already been paired in (508)') contain Masculines, just as IX does; there is no consistent Gender difference between members of IX and members of I, II, III and VIII comparable with, for example, the difference between II and V. But is there some other lexically determined property which all members of IX share and all members of one of these other four types lack? If so, we will have grounds for recognising a new macroparadigm, and will be one step closer to reconciling the German facts with the Paradigm Economy Hypothesis.

This possibility, which involves attributing morphological relevance to some new lexically determined property, is certainly worth considering, if only because it has, in effect, been seriously advocated by Wurzel (1970). According to him, the paradigm with which IX can be paired is VIII (that of 'weak' Masculines such as Bär 'bear'); and the relevant difference between IX and VIII involves the lexically determined characteristic, or category, of Animacy. For Wurzel, 'mixed' Masculine nouns like Dorn and Staat 'state' which follow paradigm IX in (505) are simply weak nouns (in his feature notation [- stark]) which differ from those such as Bär and Graf 'count' (paradigm VIII in (505)) in being Inanimate ([- belebt]). In other words, Masculines marked [- stark] will end in -(e)n in the Genitive Singular if they are [+ belebt] and -(e)s if they are [- belebt]. Wurzel is not concerned with the issue of paradigm economy in our sense; nevertheless, his invocation of the feature 'belebt' may suggest a

way of justifying a macroparadigm of the kind we seem to need. We cannot call Animate and Inanimate 'morphosyntactic properties' in German, for the simple reason that they play no part in syntax;⁴ but that need not deter us from recognising their morphological relevance, if it exists, and perhaps coining a new term 'morpho-semantic property' to apply to them. The difficulty is that the consistent correlation that Wurzel alleges between the Animate-Inanimate contrast and membership of types VIII and IX respectively does not hold. We have already included three Animates among our examples of nouns belonging to type IX, namely Professor, Vetter and Insekt; conversely, we can find (in standard German) Inanimates of type VIII, such as Diamant 'diamond' and Dividend 'dividend'.

To some extent, Wurzel protects himself against this objection by his decision to ignore the behaviour (or misbehaviour!) of 'foreign' nouns, although this excuse will not do for etymologically native nouns of type IX such as Vetter 'cousin' and Untertan 'subject, vassal'. Moreover, it may be that the exceptions to Wurzel's predictions can be accounted for phonologically (for example, in the Masculine 'mixed' Animates of type IX, such as Professor, the syllable preceding the ending always appears to lack primary stress⁵, unlike in the 'weak' Animates of type VIII). For this reason, I do not rule out entirely the possibility of grouping IX and VIII, together with the Feminine-only VI, into a single macroparadigm. If this is correct, then the task of reconciling the German facts with the Paradigm Economy Hypothesis is eased further. But I prefer an account involving the notion 'slab', for reasons I will explain in the next section.

5.3 The 'mixed' declension-type of Dorn and the notion 'slab' revisited

There is one fact about the 'mixed' declension-type IX that I have mentioned but not so far exploited: the fact that its mixture correlates with Number, in the sense that IX resembles the 'strong' types I, II and III throughout the Singular and the 'weak' type VIII in the Plural. This fact could be a mere accident;

on the other hand, it could be a clue to a more satisfactory way of looking at the sort of breach of paradigm economy that type IX represents.

When I first introduced the notion 'slab', in the course of our search in Chapter III for some upper limit on the number of nominal paradigms in our hypothetical language L below the mathematical limit of more than 20,000, I postulated a state of affairs in which the relevant inflexional resources of L -- the Latin-like Case-Number affixes presented in (301) -- were arranged in 'slabs' corresponding to a partition of the twelve Case-Number slots of L into four subsets. These four subsets, as indicated at (309), were: Cases 1, 2, 3 Singular; 4, 5, 6 Singular; 1, 2, 3 Plural; and 4, 5, 6 Plural. But I did not attempt to develop the idea further or to apply it to any actual linguistic data because it did not seem adequate to reduce the number of possible declension-types in L to anything near the total we observe in an actual human language (Latin) which closely resembled L; it seemed more profitable instead to pass directly to a much more restrictive hypothesis about paradigm organisation, namely the Paradigm Economy Hypothesis.

We noted at the beginning of the present chapter, however, that one corollary of the Paradigm Economy Hypothesis was hard to square with a common characteristic of inflexional behaviour in a variety of languages: the fact that more than one inflected form or 'principal part' of a word must often be cited in order to identify that word's inflexional pattern unambiguously. I quoted examples in German, Latin and Greek of words with more than one 'principal part'. But one relatively obvious question concerning these examples I did not explore: how do the principal parts divide the labour, as it were, of predicting the shape of individual inflected forms within their paradigm? It is by reference to the answer to this question that I will propose a way of reconciling paradigm economy with the German mixed declension-type IX and indeed with all the Latin, Greek and German facts summarised in (502).

It will be helpful to pose the question in more concrete terms, in relation to a particular example of a word with more than one principal part. I will choose the Latin verb amō 'I love' from (502 b). Four principal parts are listed there: the 1st Singular Imperfective Present Indicative Active, the Imperfective Infinitive Active, the 1st Singular Perfective Present Indicative Active and the Supine. Yet the total number of 'parts' or inflected forms of amō (if we treat the three participles as one form each, disregarding their adjectival declension) is no less than 102, arrived at as follows:

(509) 'Finite' forms (i.e. forms inflected for Person):

Imperfective: Active: total 34, viz.

	Present	Past	Future
Indic	6	6	6
Subjunc	6	6	-
Imper	2	-	2

Passive: total 32 (as Active, less Future Imperative)

Perfective (Active only): total 30 (as Imperfective Active, less Imperatives)

'Nonfinite' forms:

Participles: total 3, viz. Imperfective Pres Active
Imperfective Fut Active
Perfective Passive

Infinitives: total 3, viz. Imperfective Active
Imperfective Passive
Perfective (Active only)

Grand total: 102

How, then, do the four principal parts 'predict' the remaining 98 forms? Clearly, the number of logically possible ways in which the labour might be divided among them is massive. For example, one logically possible division of labour might be:

(510) amō 'predicts' 1st Person forms in all Active

Aspects, Moods and Tenses;

amāre 'predicts' all Infinitives and the Passive

Past Subjunctive;

amātum 'predicts' all Participles;

(510) (cont.)

amāvī 'predicts' the rest (i.e. all 2nd and 3rd Person Active forms, all finite Present and Future Passive forms, and the Passive Past Indicative).

But anyone who knows Latin will know that the actual division of labour is quite different from this, and can in fact be much more simply described (at least so far as the 'finite' forms are concerned). For all Latin verbs, the division is in fact as follows:

(511) Except for the Participles:

1st Sg Impf Pres Indic Act and Impf Infin Act jointly predict all Imperfective forms (the Infinitive alone being sufficient for all verbs except a small group in -iō, such as capiō 'take', fodiō 'dig', cupiō 'desire');

1st Sg Pf Pres Indic Act predicts all Perfective forms.

Among the three Participles:

Impf Infin Act predicts the Present Participle;
Supine predicts the remaining two.

What we observe in Latin, then, is a clear division of labour reflecting the division within the category Aspect between the properties Imperfective and Perfective. Logically, this could be a mere accident from the general linguistic point of view; in other words, a language in which the division of labour between the four principal parts was as in (510) rather than as in (511) is perfectly conceivable. But could such a language exist in fact?

In order to answer this question, we need to know what will count as evidence that the Latin-style division of labour reflects some general principle of morphological behaviour and is therefore more than a mere accident. Clearly, we need to find evidence for a division of labour in other languages which is similar in some identifiable respect. Fortunately, it is quite easy to find such evidence.

The most salient difference between (510) and (511) is as follows: in (511) (actual Latin) the main division of labour coincides with a simple contrast between two morphosyntactic properties and, moreover, two properties whose principal exponent in any word-form (if one can be identified) is relatively central; in (510) (a hypothetical pseudo-Latin), on the other hand, the division of labour reflects a rather complex set of contrasts between various combinations of properties, including properties which, in actual Latin at least, are realised peripherally (e.g. 1st Person). What about other instances of inflexional behaviour involving more than one principal part? If we look at the division of labour in Attic Greek between the principal parts listed in (502 c), we find a state of affairs which, to say the least, resembles (511) much more closely than (510); for example, each of the six 1st Singular forms listed can be used to deduce all the remaining seven Person-Number forms which share the same Voice-Aspect-Tense combination, so that we do not need to refer to (say) the 1st Sg Aorist to determine the 3rd Pl of the Perfect -- a logically quite conceivable state of affairs in a pseudo-Greek that one might construct on the lines of our pseudo-Latin at (510).

This resemblance between Latin and Greek might perhaps be put down not to any general morphological principle but rather to the fact that they are relatively closely related Indo-European languages; they might, in other words, have jointly preserved a characteristic which from the general linguistic point of view is merely an accident. But the resemblance certainly extends outside Indo-European. In Turkish, there are thirteen verbs whose 'Aorist' form is not predictable from the 'base' form by any rule (Lewis 1967: 116), and for these verbs two 'principal parts' are cited in the Concise Oxford Turkish Dictionary; but for our purposes the important point is that the second of these principal parts (the Aorist stem) is used to 'predict' all and only the Positive Aorist forms, thus preserving a correspondence with a simple morphosyntactic property contrast. Similarly, in those few Hungarian verbs which are irregular in certain Tenses, and whose behaviour is therefore not entirely predictable from a

single principal part, the 'extra' principal parts are used to predict all and only the forms of a particular Tense or group of Tenses, and not some more complex subset of the verbal paradigm whose specification involves Person and Number too (cf. Bánhidi, Jókay and Szabó 1965: 418-421).

Two tentative conclusions seem to emerge. Firstly, certain morphosyntactic property contrasts, such as ones involving Aspect and Tense, seem to define partitions of verbal paradigms which are morphologically in some sense more fundamental than other property contrasts such as those of Person and Number. Secondly, when (despite the Paradigm Economy Hypothesis) more than one principal part is needed to 'predict' the whole inflexional behaviour of a given verb, the division of labour between the principal parts corresponds to these more fundamental contrasts much more closely than one would expect in the absence of any general principle operating in that direction. The first conclusion is strongly supported by certain facts which are quite independent of paradigm economy: Hooper (1979) reports that, out of a sample of 41 languages investigated, among those 14 which showed verb stem alternations with some clear semantic correlation, 13 had alternations corresponding to Tense or Aspect, one (Acoma) had alternations corresponding to Number but not Tense or Aspect, and none had alternations corresponding to Person.⁶ The second conclusion must await support or disconfirmation from a much wider survey of prima facie breaches of paradigm economy than I have yet carried out. But let us suppose for the moment that it is correct. What, then, are the implications for the linguistic material under discussion -- the German mixed declension-type IX?

The implications have to do with the fact, already noted, that the mixture involved in IX follows the morphosyntactic distinction of Number: type IX is 'strong' in the Singular and 'weak' in the Plural. This fact has to be regarded as purely accidental if we adopt the in any case rather dubious macroparadigm solution for type IX, discussed in the previous section, uniting it with

types VIII and VI on the basis of a combination of phonological and morphosemantic conditioning involving Animacy. But, provided we assume that distinctions of Number are in some genuine sense more fundamental, both in German and also perhaps universally, than distinctions of Case, then we have before us an alternative method of reconciling the existence of declension-type IX with the alleged principle of paradigm economy, and, moreover, a method which puts the Number distinction to use. We can say, in effect, that 'mixed' is not merely a convenient label for this declension-type but may reflect how the native speaker's knowledge of German nominal inflexion is actually organised, since the prima facie breach of paradigm economy observed in declension-type IX, just like the breaches observed in Greek, Latin, Turkish and Hungarian verbs, involves a fundamental morphosyntactic property contrast.

Is there any evidence, then, for the assumption just made about the primacy of Number over Case? I suggest that there is. As is well known, stem allomorphy involving umlaut in modern German nouns consistently reflects Number but not Case, and the same is true in, for example, classical Arabic, where a large proportion of nouns display 'broken plurals' involving internal vowel change but where there are no nouns displaying, for example, the same 'broken' Accusative stem in all Numbers, contrasting consistently with the stems used for the other Cases. I strongly suspect that, if one investigated over a wide range of languages the relative frequency of Number and Case as consistent correlates of stem allomorphy in nouns, in the way that Hooper investigated Tense-Aspect, Number and Person as consistent correlates of stem allomorphy in verbs, Number would win easily.⁷ Secondly, it seems to be generally true that, if a language has linearly separable exponents of Number and Case (that is, if Number and Case are not cumulated as in Latin and German), then Case will be more peripheral than Number; this is Greenberg's (1963) Universal 39. Apart from its synchronic justification, this universal reflects the independent morphological developments in English and the mainland Scandinavian languages, whereby cumulated Genitive Plural suffixes have been replaced by sequences consisting of a Plural

marker followed by a Genitive marker.⁸ The possibility suggests itself, then, of linking 'more fundamental', in the sense relevant to our present discussion, with 'more central', in the sense relevant to the Peripherality Constraint.

It remains to state more precisely the conditions under which morphosyntactic property contrasts may legitimise breaches of paradigm economy, so that the empirical consequences of this revision of the Paradigm Economy Hypothesis may be as clear as possible. I will in fact state the conditions as a 'codicil' to the Hypothesis, invoking the now familiar notion 'slab':

(512) Slab Codicil to the Paradigm Economy Hypothesis

In a given language L some part of speech N may infringe the Paradigm Economy Hypothesis if this infringement involves 'mixed' paradigms (i.e. paradigms containing no inflexional realisation peculiar to them) which are divisible into 'slabs' on the basis of a fundamental morphosyntactic property contrast applicable to N, such that the set of inflexional realisations in each slab is shared with some 'unmixed' paradigm for N in L.

This formulation is vague, of course, to the extent that the notion 'fundamental morphosyntactic property' remains vague. But this is no serious embarrassment so long as there is evidence, such as that already presented here, to the effect that the distinction between 'more fundamental' and 'less fundamental' properties reflects something real in the way inflexion operates. We are entitled, in other words, to defer any attempt to make the Slab Codicil more precise in this respect, because it will automatically become more precise once more is known about the relationships at a relatively abstract level between different morphosyntactic categories, both universally and in the grammars of individual languages. In any case, despite its present vagueness, the Slab Codicil is quite precise enough to discriminate clearly between those breaches of paradigm economy that it renders allowable in a language such as German (given the assumption that Number is more fundamental than Case) and conceivable breaches that are still

disallowed. The latter class is still extremely large. In German, for example, even when the Paradigm Economy Hypothesis is weakened by the addition of the Slab Codicil, it still entails the claim that it is not an accident, from the general linguistic point of view, that there are no nouns which are 'mixed' on the basis of Case rather than Number -- having, say, 'strong' inflexions in the Accusative and Genitive and 'weak' inflexions in the Nominative and Dative. In other words, a pseudo-German in which Dorn is declined as in (513) is still predicted not to be a possible human language:

(513)		I: Strong	VIII: Weak	IX: Mixed
	Singular N	Tag	Bär	"Dorn
		A Tag	Bären	Dorn
		G Tages	Bären	Dornes
		D Tage	Bären	Dornen
	Plural N	Tage	Bären	Dornen
		A Tage	Bären	Dorne
		G Tage	Bären	Dorne
		D Tagen	Bären	Dornen"

Whether this pseudo-German example is in fact impossible -- in concrete terms, whether there are in fact no languages in which 'paradigm mixture' is correlated with less fundamental rather than more fundamental property contrasts -- is, of course, an empirical question, and the strong claim which the Paradigm Economy Hypothesis (as amended by the Slab Codicil) still embodies remains to be tested more widely. But there is a large class of apparent counterexamples which can be identified straight away. Let us continue to discuss the Slab Codicil in relation to Case and Number inflexion in nouns, and continue to assume that Number is always more fundamental than Case. An immediate corollary seems to be that, even if there are nouns which, being 'mixed', have two or more principal parts, these principal parts should all belong to different Numbers; for, if to identify the inflexional behaviour of some noun one crucially needs to specify more than one 'part' belonging to the same Number, this must surely imply 'mixture' which reflects Case distinctions rather than Number

distinctions. Yet it is not true that, in all languages where more than one principal part needs to be specified for nouns, all the parts traditionally cited differ in Number. Does not this show that the Slab Codicil is inadequate to cope with the sort of breaches of paradigm economy for which it was intended?

The first point to be made in reply is that the fact that in some language two principal parts are traditionally cited which share the same Number may be a mere accident of convention, and the same task of 'predicting' the inflexional behaviour in full could sometimes be performed just as well by two principal parts differing in Number. Thus, in dictionary entries for Latin nouns of the third declension, it is customary to specify the Nominative Singular and the Genitive Singular, thereby distinguishing the inflexional behaviour of (for example) cardō (Gen Sg cardinis) 'hinge' and sermō (Gen Sg sermōnis) 'speech'; yet their inflexional behaviour could equally well be predicted by means of (say) the Nominative Plurals (cardinēs, sermōnēs) rather than the Genitive Singulars, so that the identity of Number between the 'parts' Nom Sg cardō and Gen Sg cardinis is not crucial. On the other hand, one finds in Russian a fair number of nouns whose inflexional behaviour seems crucially to require the specification of more than one 'part' in the same Number. For example, the three Masculine nouns stol 'table', vxod 'entrance' and zub 'tooth' differ in their stress patterns in the Plural as follows:

(514) Nom	stol- <u>ý</u>	vxód-y	zúb-y
Acc	"	"	"
Gen	stol- <u>óv</u>	vxód-ov	zub- <u>óv</u>
Dat	stol- <u>ám</u>	vxód-am	zub- <u>ám</u>
Instr	stol- <u>ámi</u>	vxód-ami	zub- <u>ámi</u>
Loc	stol- <u>áx</u>	vxód-ax	zub- <u>áx</u>

The difference in stress pattern is not associated with any consistent contrast in morphosyntactic or morphosemantic properties, and the behaviour of zub is traditionally indicated in dictionaries by citing both the Nom Pl and the Gen Pl forms. There seems no way, within my present framework, to avoid regarding the Plural paradigm of zub as 'mixed', the mixture being governed by Case:

zub 'goes like' vxod (i.e. it has stem stress) in the Nominative Plural, but 'goes like' stol (i.e. it has ending stress) in the other Plural Cases.

My approach to this class of apparent counterexamples involves stem allomorphy. We have so far discussed paradigm economy mainly by reference to inflexional realisations that are affixal in form, and have not considered in any detail precisely what counts as a distinct inflexion for the purpose of defining paradigms or allocating words to them. In Chapter VII I will discuss paradigm economy in relation to stem allomorphy, and will there for a criterion of inflexional distinctness according to which the three Russian declension-types in (514) are not inflexionally distinct at all, and hence are consistent with paradigm economy for reasons which have nothing to do with the Slab Codicil. Stem allomorphy can, however, have no bearing on our hypothetical mixed paradigm at (513) -- the pseudo-German paradigm for Dorn which is mixed on the basis of Case rather than Number -- simply because this pseudo-German Dorn displays no stem allomorphy, any more than the actual German Dorn does. Example (513) therefore remains as an illustration of one of many conceivable patterns of paradigm mixture which are predicted by the Paradigm Economy Hypothesis to be impossible, even when the Hypothesis is relaxed by the addition of the Slab Codicil.

In section 5.2 I pointed out that, although the notion 'macroparadigm' permits a neat pairing of certain German declension-types on the basis of lexically determined distinctions (namely distinctions of Gender), it still leaves us with two paradigms too many, from the point of view of the Paradigm Economy Hypothesis. This is illustrated in (508), which shows that we still need to recognise seven macroparadigms in German even though no morphosyntactic property combination has more than five distinct realisations, seemingly. The argument of this section has had the effect of removing from contention one of the seven declension-types, namely IX, on the ground that it is mixed in a fashion compatible with the Slab Codicil. But we are still left with

six macroparadigms -- one macroparadigm too many. Is there any way of removing one of the remaining six from contention too, so as to reconcile German nominal declension with the Paradigm Economy Hypothesis completely? I will argue that there is, and the declension-type concerned is \times (exemplified in (505) by Name). But the argument in favour of this again involves the relationship between the Paradigm Economy Hypothesis and stem allomorphy, so will be deferred until Chapter VII (section 7.4).

5.4 More on macroparadigms

Modern German nominal declension presents a quite severe test for the Paradigm Economy Hypothesis since, superficially at least, it displays both relatively few distinct inflexions (few in comparison with Greek or Latin, for example) and relatively many distinct paradigms. Yet we have found that even the German facts can be reconciled with paradigm economy, provided firstly that we relax our original highly restrictive hypothesis so as to permit 'mixed' paradigms in certain circumstances (the Slab Codicil) and secondly that we recognise a new notion 'macroparadigm' defined in terms of lexically determined properties as opposed to syntactically determined ones. Before offering a precise formulation of the Slab Codicil, I discussed certain prima facie breaches of paradigm economy in several languages --- particularly in Latin and Greek verbal morphology -- to which the Codicil might be relevant. But I have not so far discussed the 'macroparadigm' notion in relation to any language except German. My first aim in this section, therefore, is to make good this deficiency, buttressing the notion with evidence of its usefulness elsewhere. My second aim is to formulate it more precisely and reformulate the Paradigm Economy Hypothesis in terms of it. I will then be in a position in the next section to reassemble the Paradigm Economy Hypothesis, as hitherto amended, and the various definitions subservient to it, and say something about the difference in empirical content between the original and the new formulations of the Paradigm Economy Hypothesis.

The four languages from which I will mainly draw evidence

for the wider usefulness of macroparadigms are Russian, Dyirbal, Zulu and Latin. Consider first the following Russian nominal paradigms, and in particular the Accusative Case-forms:

(515) a.

Sg Nom	student 'student'	professor 'professor'	ženščina 'woman'
Acc	studenta	professora	ženščinu
Gen	studenta	professora	ženščiny
Dat	studentu	professoru	ženščine
Instr	studentom	professorom	ženščinoj
Loc	studente	professore	ženščine
Pl Nom	studenty	professora	ženščiny
Acc	studentov	professorov	ženščin
Gen	studentov	professorov	ženščin
Dat	studentam	professoram	ženščinam
Instr	studentami	professorami	ženščinami
Loc	studentax	professorax	ženščinax

b.

Sg Nom	akt 'report; act (of play)'	dom 'house'	kvartira 'flat; apartment'
Acc	akt	dom	kvartiru
Gen	akta	doma	kvartiry
Dat	aktu	domu	kvartire
Instr	aktom	domom	kvartiroj
Loc	akte	dome	kvartire
Pl Nom	akty	doma	kvartiry
Acc	akty	doma	kvartiry
Gen	aktov	domov	kvartir
Dat	aktam	domam	kvartiram
Instr	aktami	domami	kvartirami
Loc	aktax	domax	kvartirax

If we ignored the Accusatives, we could say that only three paradigms were exemplified here: one for student and akt, one for professor and dom, and one for ženščina and kvartira. But the difference in the Genitive Plural between ženščina and kvartira, and the differences in both the Singular and Plural Genitives within the other two pairs, prevent us from identifying their

paradigms in this way; in fact, on the basis of the definition of 'paradigm' at (401), we have to say that there are six distinct paradigms here. But, if this is so, the Paradigm Economy Hypothesis is clearly at risk, since no morphosyntactic slot here has anything like as many as six distinct realisations.

The solution to this problem lies in recognising the morphological importance of the semantic distinction between Animate and Inanimate nouns. In all Russian nouns denoting animate creatures, the Accusative Plural is the same as the Genitive Plural, and in the Singular too the Accusative is the same as the Genitive for certain Animate nouns, namely Masculines (not Feminines) belonging to those declension-types which do not supply a distinctive Accusative ending; thus, the Acc Sg is the same as the Gen Sg in the Masculines student and professor (illustrated at (515)) but not in mužčina 'man', a Masculine noun which 'goes like' ženščina, nor in mat' 'mother', a Feminine noun whose Acc Sg is like the Nominative, not the Genitive (i.e. mat', not materi).⁹ In German, we used the macroparadigm concept to unite on the basis of a consistent Gender contrast the paradigms of the Feminines Hand, Rose and Mutti with those of the Masculines Gast, Bär and Uhu respectively (see (508) above). In the present Russian instance, a similar contrast, not in Gender but in Animacy, can be used as ground for amalgamating the three paradigms of (515 a) with those directly below them in (515 b). To put it more precisely, we can say that (for example) student and akt belong to the same macroparadigm because the only inflexional difference between them (namely, in the Accusative) is correlated with a consistent difference in Animacy; so, if paradigm economy applies to macroparadigms rather than to paradigms, the task of reconciling the Russian paradigms at (515) with the Paradigm Economy Hypothesis is simplified considerably.

It will be noticed that the recognition of macroparadigms in (515) does not reconcile these Russian data with the Paradigm Economy Hypothesis altogether; there are still only two, not three, distinct inflexions for even the most generously provided morphosyntactic slots. The most plausible solution in fact involves

data going beyond what I have so far presented; if we take into consideration a fourth paradigm -- that of Neuter nouns such as kreslo (Nom Pl kresla) 'armchair' -- we find that the macroparadigm of professor and dom can be seen as 'mixed' on the basis of of the Singular-Plural distinction (hence consistently with the Slab Codicil), provided only that we recognise another 'macro-paradigmatic' correlation between an inflexional distinction (-ov~∅ in the Genitive Plural) and a lexical property distinction (this time one of Gender, between Masculine and Neuter). But these further ramifications of Russian noun morphology are not important here¹⁰; what is important is the ease and effectiveness with which the macroparadigm concept comes to the rescue, as it were, when the Paradigm Economy Hypothesis is threatened by the data in (515).

The second piece of supporting evidence for the macroparadigm notion comes from Dyirbal, of northern Queensland in Australia, as described by Dixon (1972). In Dyirbal, we find a prima facie instance of 'illegal' paradigm mixture in the partial verbal paradigms quoted below (Dixon 1972: 55, 68, 248):

(516)	'l-stems':		'y-stem'
Stem	balgal 'hit'	wayndil 'go uphill'	baniy 'come'
Unmarked Tense	balgan	wayndin	banipu
Future Tense	balgan	wayndip	banip
'Do-it- quickly'	balgalnbal	wayndigaliy	banigaliy
Purposive Aspect	balgali	wayndili	banigu

The forms relevant to us here are the Unmarked (Present/Past) Tense, the 'Do-it-quickly' stem and the Purposive form. There appear to be two distinct endings for each: -n and -pu for Unmarked Tense, -nbal and galiy for 'Do-it-quickly', and -li and -gu for Purposive. Yet there are more than two distinct paradigms. Balgal and baniy are 'unmixed', the former selecting -n, -nbal and -li and the latter selecting -pu, -galiy and -gu; but wayndil is 'mixed', since it goes like balgal in the Unmarked Tense and Pur-

positive form (wayḡdin, wayḡdili) and like baniy in the 'Do-it-quickly' form (wayḡdigaliy). Dixon makes it clear, however, that the membership of the wayḡdil conjugation-type is not haphazard; it consists, in fact, of just those l-stem verbs (i.e. verbs with an Unmarked form in -n, like balgən 'hits/hit') which are intransitive. We therefore have grounds for uniting the balgal type and the wayḡdil type into a single macroparadigm; for where their inflexional realisations for some non-lexically-determined property differ (namely in the 'Do-it-quickly' form), this difference can be correlated with a difference in some lexically determined category, namely Transitivity. If we relate 'paradigm economy' to macroparadigms rather than to paradigms, therefore, we no longer have here a counterexample to the Paradigm Economy Hypothesis; for the number of macroparadigms here is not three but two, the same as the maximum number of distinct realisations for any one morphosyntactic slot. The usefulness of the macroparadigm notion is therefore demonstrated in a language about as far removed from German and Russian both genetically and geographically as any language could possibly be.

In connexion with our Dyirbal and Russian examples, it is worth pointing out that the identification of macroparadigms in each is unambiguous; that is, having identified some lexically determined property contrast which correlates consistently with some inflexional contrast, we find only one way in which the paradigms can be arranged into macroparadigms. In the Russian example it is never in doubt which paradigms are, as it were, candidates for amalgamation. Seeking an Animate partner in (515 a) for the Inanimate noun akt in (515 b), we can at once rule out Žeňščina, from which akt differs inflexionally in numerous Cases, and also professor, from which akt differs in the Nom Pl in a fashion unrelated to Animacy; student remains as the obvious partner, differing from akt only in the inflexion of the Accusative forms.

In Dyirbal, the only logical alternative to the macroparadigm analysis we have chosen is one uniting the 'mixed' type of

waypdil to that of baniy rather than to that of balgal. Bearing in mind that 80% of Dyirbal intransitive verbs are 'y-stem' verbs like baniy (Dixon 1972: 54), we might consider assigning waypdil and baniy to a single 'Intransitive' conjugation-type, sharing a single 'Do-it-quickly' marker -galiy.¹¹ But then what of the two distinct Unmarked Tense affixes -n and -pu, and the distinct Purposive affixes -li and -gu? Their distribution clearly does not correlate with the Intransitive-Transitive distinction. Could we account for them phonologically? Superficially, in view of Dixon's terms 'l-stem' and 'y-stem' for the balgal and baniy conjugation-types, one could say: -n replaces -pu and -li (or -i) replaces -gu in just those Intransitive verbs whose stem is in -l rather than -y. This sounds superficially parallel to what we have said about the Hungarian 2nd Singular Indefinite Present Indicative: -ol replaces -(a)sz in just those verbs of the 'normal' conjugation whose stem ends in a sibilant or affricate. But there is a difference. In Hungarian, we observe a patent phonological difference in the shapes of the stems to which the relevant affixes are attached. In Dyirbal, on the other hand, there is no -y present 'on the surface' in either the Unmarked Tense or the Purposive forms of baniy, and an -l is present in the Purposive but not the Unmarked form of waypdil. A phonological account of the distribution of the Unmarked Tense forms therefore relies on the possibility of justifying a consistent underlying phonological difference in the stem allomorphs to which the Unmarked Tense affixes are attached -- a hard task, since the sole evidence for this supposed underlying difference resides precisely in the surface forms which it is meant to explain.

On this issue, to appeal to those other forms of 'y-stem' and 'l-stem' verbs in which the y or l is manifest 'on the surface' will settle nothing, because the same facts will be equally compatible with an alternative analysis according to which the 'stem-final' -l- and -y- do not belong to the roots of Dyirbal verbs at all, but are merely stem-forming affixes added to roots before some but not all Tense, Aspect or Mood markers. This alternative analysis square well with the realisation of not only the Unmarked

but also the Future Tense, which shows no sign of a stem-final -l or -y; moreover, treating the -l and -y as something added to verbal roots rather than as part of them absolves us from having to account for why the final segment of verbal 'stems' is subject to a phonological restriction which has no parallel elsewhere in Dyirbal. Under this alternative analysis, then, the l/y contrast defines an arbitrary morphological classification of Dyirbal verbs rather than a phonologically motivated one; consequently, we cannot look to the l/y contrast to yield a phonological motivation for either the -n ~ -nu alternation in the Unmarked Tense or the -li ~ -gu alternation in the Purposive. within a supposed 'Intransitive' macroparadigm lumping wayndil together with baniy. There therefore appears to be good reason to regard our original macroparadigm solution, grouping wayndil with balgal, as the only really plausible one.

Why is it important that in Russian and Dyirbal there is only one way (or only one plausible way) of amalgamating the paradigms into macroparadigms? To put it another way, why is it important that the Russian macroparadigms do not overlap? The potential importance of this will be explained in the next section. Meanwhile, we will find that the same characteristic is shared by the macroparadigm which unites two Zulu noun classes, in our third application of the notion outside German.

In Zulu, as in most Bantu languages, all nouns belong to 'Classes' which play a considerable morphosyntactic role; verbs and predicate adjectives must agree in Class with their subject noun, and attributive adjectives and other modifiers must agree in Class with their head. It is customary to regard Classes as much like Indo-European Genders, only more numerous. But there is a difference. Each Class in Zulu is, broadly speaking, associated with only one nominal prefix (or one pair of prefixes, Singular and Plural)¹² and only one set of concordial elements; whereas in a language such as Latin one Gender may be realised in a variety of ways, depending on the declension-type of the item (noun, adjective, participle or pronoun) which carries it. To

put it another way; in Latin, Gender intersects with declension-type, whereas in Zulu, Class-membership determines declension-type. Indeed, for nouns, whose Class-membership is lexically fixed, Class-membership and declension-type are essentially the same thing. We can therefore think of each Class provisionally as a distinct paradigm, and test whether the organisation of the Classes obeys paradigm economy.

Most Zulu nouns have distinct Singular and Plural forms (the exception being mainly what one would expect on semantic grounds, such as abstracts and mass nouns). The locus of this distinction, and hence the exponent of Number, is the prefix. It is thus appropriate to ask how many distinct prefixes exist for each of these two slots, and then test for compliance with the Paradigm Economy Hypothesis by inquiring whether the number of relevant Classes (i.e. those in which Singular and Plural forms are distinguished) exceeds the number of prefixes for the more generously provided of the two slots Singular and Plural.

The distinct prefixes are as follows:

(517) Singular: um(u)-, u-, i:-, isi-, in-, u:-

Plural: aba-, o-, imi-, ama-, izi-, izin-

There are thus six prefixes for each of the two slots. The mathematical maximum number of distinct paradigms incorporating these prefixes is thus 36, while the maximum as predicted by the Paradigm Economy Hypothesis is six. The actual number (barring a few marginal types¹³) is, however, seven:

(518) Class 1/2 1a/2a 3/4 5/6 7/8 9/10 11/10

Sg um(u)- u- um(u)- i:- isi- in- u:-

Pl aba- o- imi- ama- izi- izin- izin-

None of these Classes is 'mixed', since each has a distinctive prefix in either the Singular or the Plural; yet two Classes (1/2 and 3/4) share a Singular prefix and two (9/10 and 11/10) share a Plural one, so that we have one paradigm too many from the point of view of the Paradigm Economy Hypothesis. How, if at all, can the macroparadigm concept help here?

The fundamental prerequisite for a macroparadigm is a consistent morphosyntactic or-semantic contrast correlated with an inflexional contrast. What we have to look for, then, is a pair of Classes in Zulu which display such a correlation. The natural pairs of Classes to examine first are those which share a prefix for either Singular or Plural: 1/2 and 3/4 (which share um(u)- in the Singular) or 9/10 and 11/10 (which share izin- in the Plural). Both Class 9/10 and Class 11/10 are semantically rather heterogeneous, and certainly no consistent semantic contrast between them seems to stand out (Doke 1973: 53,55). On the other hand, Class 1/2 is entirely homogeneous in that it contains exclusively nouns denoting human beings. What of Class 3/4? Doke remarks (1973: 44): "This is sometimes called the "Tree" or "River" class, but in Zulu [by contrast with its cognate Classes in some other Bantu languages] it is mostly of a miscellaneous nature". The crucial question for us, of course, is whether it contains any nouns denoting humans; for, if it does not, we will have found a straightforward and promising semantic contrast between it and Class 1/2.

The answer to this question is encouraging. Of the small group of Human nouns which belong to Class 3/4 and which therefore display the um(u)-/imi- pattern, Doke (1973: 47) remarks: "It is noticeable that most of such words are compound nouns". Examples are:

(519) umsheshengwana	'sneaking informer'
umbonamathunzi	'pessimist'
umhambuma	'tramp', ¹⁴
umlindankosi	'king's bodyguard'

But a more significant generalisation about these words, I suggest, is that they nearly all have a pejorative connotation, the only common exception being umhlobo 'brother, relative', which sometimes has a Plural imihlobo rather than abahlobo when used in a figurative sense, not implying blood relationship¹⁵. We therefore have some prima facie ground for recognising a 'macro-Class' 1/2/3/4, with inflexion partly reflecting the morphosemantic properties Human and (let us say) Despised, thus:

(520) Class 1/2/3/4

Singular:		um(u)-
Plural: Non-human:		imi-
	Human:	Despised: imi-
		Non-despised: aba-

Any Human noun of the macro-Class which, like umhlobo, forms a Plural with imi- even though it does not carry a pejorative connotation will have to be treated as a lexically marked exception. We will therefore predict that it should be liable to acquire a 'regular' Plural in aba-; and, as we have seen, this prediction seems to be correct. (Doke's (1973: 46) words are: "there is a tendency nowadays to use the [Class 1/2] plural abahlobo".)

An obvious fact about (520) is that the Despised Human Plural prefix is exactly the same as the Non-human Plural prefix. In some sense, therefore, Zulu morphology treats Despised Humans as things. This fact tends to support the recognition of the macroparadigm, I suggest. There is a tendency in at least a few other languages for Human nouns with a pejorative connotation to belong to a Gender or declension-type which is in some way semantically inappropriate. An isolated example of this is the use in German of Mensch as a Neuter noun with the meaning 'hussy', in contrast to its neutral meaning 'human being' when in its normal Masculine Gender. A more systematic instance of the phenomenon, seemingly, is Gender-switch in the Ethiopian language Tigre, whereby diminutives of the opposite Gender to the noun from which they are derived (Masculine for Feminine and vice versa) acquire a pejorative sense (Palmer 1962: 47, 57-9, 61-2). Similarly, in those Polish declensions in which Plural Masculines display a morphological contrast between 'Viriles' (i.e. human males) and 'Non-viriles', a Non-virile inflexion may be deliberately substituted for a Virile one for pejorative effect, e.g. Szwab 'Swabian', Szwabi 'Swabians' versus Szwaby (with Non-virile -y instead of Virile -i) 'Krauts' (Gotteri 1981). I suspect that a careful search would reveal that the phenomenon is quite widespread.

What this means is that the morphosemantic characteristics

of the putative Zulu macro-Class 1/2/3/4 may really be simpler than is indicated in (520); at the point where morphosyntactic properties are realised, it may be appropriate to regard 'Despised Humans' as not really Humans at all, so the fact that their Plural prefix is imi- rather than either aba- or some third possibility (say, izi-) follows automatically from their macro-Class membership. We can say, essentially, that all Human nouns in the amalgamated macro-Class display a Plural prefix aba- except when, as in a few lexicalised instances, they are treated morphologically as Non-humans for pejorative effect.

I remarked earlier that, as in Russian and Dyrbal, there is only one way of amalgamating the Zulu paradigms into macroparadigms. We have, in fact, seriously considered only one such way, for good reason. Not only is Class 3/4 the Class which is morphologically most similar to the all-Human Class 1/2; it is also the only Class which contains no Human nouns at all, except for the predominantly pejorative group we have just discussed. In other words, although only Class 1/2 is exclusively Human, all the other Classes do contain some Human nouns; and there is thus no other obvious candidate on either morphological or semantic grounds for macroparadigmatic amalgamation. This fact indicates that, in adopting the solution I have proposed, one is not merely choosing arbitrarily from among several equally plausible (or implausible) macroparadigmatic possibilities.

I turn now to some Latin evidence for macroparadigms. This evidence will involve us in discussing the relevance of the macroparadigm concept to situations where compliance with the Paradigm Economy Hypothesis is not at issue. Demonstrating such relevance is important. Recall that the macroparadigm concept was first introduced in connexion with German nominal declension, to help account for the superficially far from 'economical' proliferation of declension-types illustrated in (505). The Russian^{Zulu} and Dyrbal facts that we have just discussed also involved prima facie infringements of paradigm economy. Our application of the macroparadigm concept in these four instances has by itself been suc-

cessful enough, to justify its invention, I suggest, in that it has enabled us to reconcile certain seemingly quite damaging facts with a version of the Paradigm Economy Hypothesis which is still extremely strong in its empirical consequences. But it would obviously be more satisfactory if we could show the macroparadigm concept to be useful in morphological description independently of paradigm economy; for, if we could do this, we would improve our chances in the long run of anchoring our approach to paradigms and our claims about their behaviour firmly within a more general framework of definitions and claims about inflexional morphology.

Consider the following Latin nominal paradigms:

(521) a.		Sg	Pl	b.		Sg	Pl
Nom	dominus	dominī		bellum	bella		
	'lord'			'war'			
Voc	domine	dominī		bellum	bella		
Acc	dominum	dominōs		bellum	bella		
Gen	dominī	dominōrum		bellī	bellōrum		
Dat	dominō	dominīs		bellō	bellīs		
Abl	dominō	dominīs		bellō	bellīs		
c.							
Nom	dux [duks]	ducēs		caput	capita		
	'general'			'head'			
Voc	dux	ducēs		caput	capita		
Acc	ducem	ducēs		caput	capita		
Gen	ducis	ducum		capitis	capitum		
Dat	ducī	ducibus		capitī	capitibus		
Abl	duce	ducibus		capite	capitibus		

There is room for argument about precisely how we should analyse the underlying phonological shapes of both stems and endings; but a traditional and relatively uncontroversial analysis would involve recognising four distinct Nom Sg endings, thus:

(522) Nom Sg of	<u>dominus</u> :	-us (/domin + us/)
	<u>bellum</u> :	-um (/bell + um/)
	<u>dux</u> :	-s (/duk + s/)
	<u>caput</u> :	no affix (/kaput/)

Assuming that these four endings are indeed distinct, we will expect to find at least four distinct paradigms among the Latin nouns

which exemplify them. Assuming further that the Nominative Singular is the most generously furnished slot, the Paradigm Economy Hypothesis will lead us to predict that there should be no more than four distinct paradigms among the class of Latin nouns with these four Nom Sg endings. On the basis of the data presented at (521), this seems correct¹⁶. What is the problem, then?

Clearly, there is no breach of paradigm economy here. Yet there is something about (521) which is potentially embarrassing. In the Nominative Singular, as we have seen, there are four distinct endings. If we look at the Nominative Plural, we will probably recognise three: -ī, -ēs and -a, the last of these shared by two of the four nouns (bellum and caput). Yet when we look at the Genitive, Dative and Ablative Cases, the number of distinct inflexions drops to two. Moreover, the inflexions for each slot are distributed in such a way that dominus and bellum share one while dux and caput share the other. The reason why this is potentially embarrassing is that, from the point of view of paradigm economy, it appears at first a mere accident. The existence of four distinct endings in the Nom Sg legitimises, as it were, the existence of four distinct endings in each of the other eleven Case-Number slots. The fact that for six of the eleven slots there are only two distinct endings and that the four nouns 'go together' in pairs as they do is unexplained.

This need not perhaps be regarded as a serious embarrassment. After all, what we have been discussing, and what the Paradigm Economy Hypothesis is meant to impose limits on, is paradigmatic distinctness and not paradigmatic resemblance; there are almost certainly more things to be said about paradigms, and further general constraints to be discovered, which have nothing to do with 'economy'. Nevertheless, it would be very satisfying if the similarities we observe in the Genitive, Dative and Ablative forms could after all be shown to be a consequence of applying some notion we have developed already. I will hope to show, in fact, that they point to the application of the macroparadigm notion

here.

One attribute of the four nouns in (521) has not so far been mentioned: their Genders. In fact, dominus and dux are both Masculine while bellum and caput are both Neuter. Is this an accident of the particular nouns we have chosen, or does it reflect some general association of particular Genders with particular paradigms in Latin? To some extent, both. Although the dominus type is predominantly Masculine, we can certainly find Feminine nouns which belong to it, particularly tree names such as fāgus 'beech', fraxinus 'ash', and the dux type too includes Feminines, such as lux (Gen Sg lūcis) 'light'; but neither of these types includes any Neuters. On the other hand, neither the bellum nor the caput type includes any Masculines or Feminines. We thus have the ingredients we need to set up two macroparadigms in (521), linking dominus with bellum and dux with caput on the ground that their inflexional differences are associated with a consistent distinction between lexically determined properties: non-Neuter versus Neuter. Moreover, there is no doubt about what to link with what; assigning a given noun to its appropriate macroparadigm is as unproblematic as it was in our Russian, Dyirbal and Zulu examples.

The linking of paradigms just achieved will come as no surprise to Latin scholars, who may even wonder what all the fuss is about. They are used to the fact that Neuters in Latin differ from non-Neuters in having a peculiar set of Nominative, Vocative and Accusative endings, and that every Neuter paradigm can be associated with some non-Neuter paradigm on the basis of identity in the endings for all (or nearly all) the other Cases; thus, dominus and bellum would be traditionally described as representing the non-Neuter and Neuter variants respectively of the 'second declension', and dux and caput as both belonging to the 'third declension'. But the fact that the macroparadigm concept yields such uncontroversial results here is a plus, not a minus. It means that what at first may have seemed a rather arbitrary device to protect the Paradigm Economy Hypothesis from disconfirmation by the evidence

from German is after all no more than a notion well established (under whatever name) in the European description of Latin (and also Greek) since before the Renaissance. We have thus succeeded in finding a use for the notion 'macroparadigm' independent of paradigm economy.

Before recapitulating in the next section the stage which our search for constraints on paradigm organisation has reached, it remains to attempt a more precise definition of 'macroparadigm'. The starting point will naturally be the definition of 'paradigm' offered at (401). Building on our first discussion of macroparadigms in connexion with the German facts in section 5.2, an appropriate formulation might be the following:

- (523) A macroparadigm for a part of speech N in a language L is a set of paradigms for N whose inflexional differences can be correlated with some consistent difference in some lexically determined morphosyntactic or morphosemantic category (such as Gender or Animacy in nouns, or Transitivity in verbs); or else any individual paradigm for N in L which cannot be assigned to such a set.

Perhaps the most important single phrase in this definition is 'lexically determined'; for this draws attention to the main difference between paradigms and macroparadigms, according to our definitions. Paradigms are defined at (401) by reference to the inflexional realisations of morphosyntactic properties which are not lexically determined. This allows us to say, for example, that the Latin Masculine noun dominus 'lord' and the inflexionally identical Feminine noun fraxinus 'ash tree' belong to the same paradigm, despite their difference in Gender. On the other hand, our definition of 'macroparadigm' allows us to say that both dominus and fraxinus belong to the same macroparadigm as the Neuter noun bellum, despite the inflexional difference in the Nominative, Vocative and Accusative Cases; and we can say further that this inflexional difference serves a realisational function, inasmuch as Gender, though not realised elsewhere in the inflexion of second-declension nouns, does get realised in the Nom-Voc-Acc of bellum.

At this point, it may seem that our handling of lexically determined properties such as Gender is unnecessarily complex. We seem to have taken care initially to exclude lexically determined properties from any role in the identification of paradigms and consequently in the testing of the Paradigm Economy Hypothesis, only to relax this exclusion later so as to allow such properties a crucial role in the definition of macroparadigms and consequently in the investigation of paradigm economy in relation to macroparadigms. How does this differ empirically from simply removing the specification 'non-lexically-determined' from the definition of 'paradigm' at the outset? The answer is that there is an empirical difference, and quite an important one; demonstrating it will be one of the tasks of the next section.

5.5 The Paradigm Economy Hypothesis revised

There is one obvious gap in the series of definitions and formulations relating to paradigm economy presented so far: we have not yet stated the Paradigm Economy Hypothesis itself in a form incorporating the macroparadigm concept. One purpose of this section is to fill that gap. Another purpose is to defend our at first sight rather roundabout handling of lexically determined properties. The defence will involve discussion of how the empirical content of the Paradigm Economy Hypothesis, as now revised, differs from that of its original version, and also some further discussion of a characteristic emphasised as being shared by our Russian, Dyirbal, Zulu and Latin data: each paradigm can be naturally assigned to only one macroparadigm. The fact that this characteristic is apparently not shared by the German data with which we began leads in the next section to a revised definition of 'paradigm' which has the additional advantage of dispensing with the need for the Slab Codicil.

For the sake of convenience I will restate here the definitions of 'paradigm' and 'macroparadigm' which will underlie our new formulation of the Paradigm Economy Hypothesis:

(524) (cf. (401) in Chapter IV):

A paradigm for a part of speech N in a language L is a pattern P of inflexional realisations for all

combinations of non-lexically-determined morpho-syntactic properties associated with N such that some member of N exemplifies P (i.e. displays all and only the realisations in P); except that, if two patterns P_1 and P_2 differ only in pairs of inflexions whose distribution can be accounted for phonologically, P_1 and P_2 count as one paradigm.

(525) (cf. (523) above):

A macroparadigm for a part of speech N in a language L is a set of paradigms for N whose inflexional differences can be correlated with some consistent difference in some lexically determined morphosyntactic or morphosemantic category (such as Gender or Animacy in nouns, or Transitivity in verbs); or else any individual paradigm for N in L which cannot be assigned to such a set.

Reformulating the Paradigm Economy Hypothesis may now seem at first sight to be a mere straightforward matter of replacing 'paradigm' by 'macroparadigm' in the formulation at (402) in Chapter IV. But this is wrong. Recall our discussion of the four Latin nouns dominus, bellum, dux and caput presented in at (521). The four paradigms illustrated by these nouns belonged to two macroparadigms; and what we would like the Paradigm Economy Hypothesis to predict, intuitively, is that they could belong to no more than two -- that the 'extra' inflexions in the Nominative, Vocative and Accusative associated with Gender distinctions could not, as it were, help to raise the upper limit on the number of distinct inflexional patterns which paradigm economy permits for the inflexional resources in question. But if we simply substitute 'macroparadigm' for 'paradigm' in (402), we will be saying that these extra inflexions do indeed raise the upper limit in this way, since we will be saying that each of the four distinct Nominative Singular inflexions in (522) legitimises a whole distinct macroparadigm, despite its systematic association with a particular Gender (or pair of Genders). So it is clear that our revision of the Paradigm Economy Hypothesis must go further than

this. But it is also reasonably clear what form this revision must take. The factor which determines the upper limit on the number of distinct paradigms must be not the number of rival inflexions pure and simple for the most generously provided slot, but rather the number of rival sets of inflexions, where what defines a 'set of inflexions' is a macroparadigm -- -us and -um together counting as one set of inflexions for Nom Sg in (521a, b) and -s and \emptyset counting as another set in (521c, d), while the two sets of inflexions for Dative Singular (for example) will each have one member only: -ū and -ī. To make it clearer what sort of sets we are talking about, let us call them 'macroparadigmatic inflexional sets', defined as follows:

- (526) A macroparadigmatic inflexional set (or macroinflexion for short) for some morphosyntactic slot is a set of inflexional realisations for that slot consisting of all the realisations appropriate to one macroparadigm.

It follows from this definition and from that of 'macroparadigm' given earlier that a macroinflexion may consist of only one inflexion, namely in those slots where there is no inflexional contrast associated with any contrast in lexically determined properties. It also follows that, if a macroinflexion has more than one member, all its members will realise the same non-lexically-determined (or syntactically determined) morphosyntactic properties, (such as Case and Number) but will differ in what lexically determined properties they realise (such as Gender, in Latin nouns).

We are now in a position to restate the Paradigm Economy Hypothesis in a more satisfactory way, with the innovation underlined for the sake of clarity:

- (527) Paradigm Economy Hypothesis (revision incorporating macroparadigms):

When in a given language L more than one inflexional realisation is available for some combination or combinations of non-lexically-determined morphosyntactic properties (some 'slot(s)') associated with some part of speech N, the number of macro-

paradigms for N is no greater than the number of phonologically independent ('rival') macroinflexional realisations available for the slot which has most such realisations.

Clearly, this reformulation makes a difference to the empirical consequences of the Paradigm Economy Hypothesis. Specifically, for example, it reconciles the Hypothesis with the *prima facie* breaches of paradigm economy illustrated in the Russian, Dyirbal and Zulu data of the previous section, and it goes a considerable way towards reconciling the Hypothesis with the German data of section 5.2, inasmuch as it allows us to take every Feminine-only declension-type in (505) out of contention by lumping it together with some non-Feminine type¹⁷.

The 'macroinflexion' concept also supplies a ready solution to the problem posed for paradigm economy by the Latin fourth declension, mentioned in footnote 16. Once inflexions have been paired on the basis of Gender, it is no longer so clear that the Nominative Singular is inflexionally more diverse in Latin than all other slots; and, in fact, as Risch (1977: 234) in effect shows, at least as good a case can be made for treating as the 'diagnostic' slot for Latin nominal declension the Genitive Plural, in which the fourth and second declensions are inflexionally distinct (-uum versus -ōrum). Admittedly, for Roman infants acquiring a native command of Latin noun morphology, one might expect the choice of such a highly 'marked' slot as diagnostic to present difficulties; but then there is evidence that such difficulties did in fact arise, in that the fourth declension, though inflexionally quite distinctive, seems nevertheless to be unstable and obsolescent from an early stage in the history of Latin (Ernout 1953: 63). But we have here in effect raised again question (407) -- one of the 'further questions' suggested for future study at the end of Chapter IV --, which it will be impossible to tackle adequately within the bounds of this thesis.

Incorporating the macroparadigm notion into the Paradigm Economy Hypothesis weakens the Hypothesis, as we have just noted. But how seriously is it weakened? Bearing in mind that the fun-

damental distinction between paradigms and macroparadigms involves lexically determined morphosyntactic and morphosemantic properties, one could pose this question in another way: how does the reformulation differ in its consequences from merely deleting the term 'non-lexically-determined' in the definition of 'paradigm' incorporated in the original version?

You will recall that I gave reasons in Chapter IV for including the specification 'non-lexically-determined' in the definition of 'paradigm'. One reason was to conform with traditional usage; the definition thereby allows us to say that (for example) the Latin Masculine noun nauta 'sailor' belongs to (or exemplifies) the same paradigm as the inflexionally identical Feminine noun mensa 'table'. But a much stronger reason has to do with the empirical content of the Paradigm Economy Hypothesis. As I put it: "omitting 'non-lexically-determined' from (401) and (402) [the definition of 'paradigm' and the original statement of the Paradigm Economy Hypothesis] multiplies the permissible number of inflexional patterns associated with some part of speech in any language by the number of properties belonging to any lexically determined morphosyntactic category applicable to that part of speech"; thus, in relation to our hypothetical Latin-like language L, if we assume that L, like Latin, has three Genders, omitting the term 'non-lexically-determined' in those formulations increases the upper limit on the number of distinct inflexional patterns in L from three to nine.

Does the 'macroparadigm' formulation of the Paradigm Economy Hypothesis have the same effect? One way of answering this is to consider a hypothetical example and test whether it complies with both versions of the Hypothesis; if it complies with one but not the other, we will have shown that there is indeed an empirical difference between the two versions. The hypothetical example that I will propose makes use of the Singular inflexional resources of L, as set out at (301)¹⁸. These Singular inflexions are grouped below into nine distinct paradigms, three for each of the three Genders, with each paradigm assigned a numeral in

square brackets for convenience in identification, thus:

(528) Masculine:

	[1]	[2]	[3]
Case 1	-s	-m	∅
2	-e	-m	∅
3	-m	-m	∅
4	-i:	-is	-i:
5	-i:	-o:	-i:
6	-o:	-o:	-e

Feminine:

	[4]	[5]	[6]
Case 1	-s	-m	∅
2	-e	-m	∅
3	∅	-m	-m
4	-i:	-i:	-is
5	-o:	-i:	-i:
6	-o:	-e	-o:

Neuter:

	[7]	[8]	[9]
Case 1	-s	-m	∅
2	-e	-m	∅
3	-m	∅	-m
4	-is	-i:	-i:
5	-i:	-i:	-o:
6	-e	-o:	-o:

This pattern is clearly far removed from what we find in Latin, the inspiration for L's inflexions. But the important question here is whether it is consistent with the two versions of the Paradigm Economy Hypothesis that we are currently comparing.

Fairly clearly, the pattern illustrated is consistent with that version of the Paradigm Economy Hypothesis and of the associated definition of 'paradigm' which treats lexically determined and non-lexically-determined morphosyntactic properties on the same footing throughout. The 'slots' for each Gender for which there are most distinct realisations available are the slots for Cases 1 and 2, with three each; and, sure enough, there are only three paradigms per Gender, yielding nine in all, compared

with a mathematical maximum (for the Singular only in L) of $3 \times 3 \times 2 \times 2 \times 2 \times 2 = 144$.

Whether this inflexional pattern is also consistent with the macroparadigm version of the Paradigm Economy Hypothesis, on the other hand, depends on whether we can successfully group the nine distinct paradigms into macroparadigms in such a way that there are no more macroparadigms than there are 'macroinflexions' for the most lavishly provided slot. Given that our aim is to identify, if possible, some empirical difference between the two formulations of the Paradigm Economy Hypothesis, the reader will probably expect me at this point to claim that no such macroparadigm solution is possible. When we attempt a macroparadigm grouping, however, the problem that we encounter is rather the opposite; not that there is no solution but that there are too many. I will not attempt to list all possible solutions, but merely describe two or three, in order to demonstrate that that this *embarras de richesse* indeed exists. I will then argue that this state of affairs does, after all, count in favour of the 'macroparadigm' formulation of the Paradigm Economy Hypothesis.

One fact about (528) which can easily be checked is that, in Cases 1 and 2, the three paradigms [1], [4] and [7] (which are M, F and N respectively) all share the same endings: -s for Case 1, -e for Case 2. Similarly, paradigms [2], [5] and [8] all share -m for these two Cases, and [3], [6] and [9] all share \emptyset . This suggests a possible way of grouping the nine paradigms into three macroparadigms; we can build on the inflexional identities in Cases 1 and 2, and attribute the inflexional diversity in the other four Cases to Gender differences, yielding the following solution:

(529) Macroparadigm solution A for (528)

	Masc	Fem	Neut
Macroparadigm I:	[1]	[4]	[7]
II:	[2]	[5]	[8]
III:	[3]	[6]	[9]

thus:

(529) (continued)

	I	II	III
Case 1	s	m	∅
2	<u>e</u>	<u>m</u>	<u>∅</u>
	M F N	M F N	M F N
3	m ∅ m	m m ∅	∅ m m
4	i: i: is	is i: i:	i: is i:
5	i: o: i: o:	i: i: i: i:	i: o: o:
6	o: o: e	o: e o:	e o: o:

This solution complies with the revised Paradigm Economy Hypothesis, in that the number of macroparadigms does not exceed that of the macroinflexions for the most generously supplied slots -- there being only three distinct macroinflexions for each of the six Cases.

Another solution, however, can be built on another set of shared endings. Again, it is easy to check that paradigms [1], [5] and [9] (M, F and N respectively) share the same endings for Cases 3 and 4, and that the same is true for the complementary threesomes [2], [6], [7] and [3], [4], [8]. This suggests the following macroparadigm analysis:

(530) Macroparadigm solution B for (528)

	Masc	Fem	Neut
Macroparadigm I:	[1]	[5]	[9]
II:	[2]	[6]	[7]
III:	[3]	[4]	[8]

thus:

	I	II	III
	M F N	M F N	M F N
Case 1	s m ∅	m ∅ s	∅ s m
2	<u>e m ∅</u>	<u>m ∅ e</u>	<u>∅ e m</u>
3	m	m	∅
4	<u>i:</u>	<u>is</u>	<u>i:</u>
5	<u>i: i: o:</u>	<u>o: i: i:</u>	<u>i: o: i:</u>
6	o: e o:	o: o: e	e o: o:

Is there any ground for preferring either solution A or solution B over the other? Apparently none at all. But in any case these two

are not the only solutions possible. By exactly similar reasoning we can build on further inflexional similarities (this time in Cases 5 and 6) to arrive at yet a third solution:

(531) Macroparadigm solution C for (528)

	Masc	Fem	Neut
Macroparadigm I:	[3]	[5]	[7]
II:	[2]	[4]	[9]
III:	[1]	[6]	[8]

thus:

	I			II			III		
	M	F	N	M	F	N	M	F	N
Case 1	∅	m	s	m	s	∅	s	∅	m
2	∅	m	e	m	e	∅	e	∅	m
3	∅	m	m	m	∅	m	m	m	∅
4	i: i: is			is i: i:			i: is i:		
5	i:			o:			i:		
6	e			o:			o:		

We have here a third solution which seems no better or worse than solutions A and B. Probably yet more equally plausible ways could be found of grouping the hypothetical paradigms of (528) into macroparadigms; but the important point is by now sufficiently well established, namely that there is no unique macroparadigm solution for these hypothetical data, and indeed none among the possible solutions which stands out as clearly preferable to the others.

Why is this important? The answer is that it shows a respect in which the data of the hypothetical language L, with its nine paradigms set out in (528), differ from the data we drew from the actual languages Russian, Dyirbal, Zulu and Latin. For each of these four sets of data, only one macroparadigm solution was possible (or at any rate plausible); that is, on the basis of the definition of 'macroparadigm' at (525), each paradigm could be assigned to one and only one macroparadigm. This is not by any means a necessary consequence of our definitions. There is nothing in the definition of 'macroparadigm' to forbid macroparadigms to overlap -- that is, nothing to prevent one paradigm from

belonging to more than one macroparadigm. Yet, in the four sets of data mentioned, no macroparadigmatic overlap occurs.

This lack of overlap suggests a method of discriminating between the two formulations of the Paradigm Economy Hypothesis that we are considering, namely a version of the original formulation weakened by the dropping of the term 'non-lexically-determined', and the revised formulation at (527) in terms of macroparadigms. The first of these, even though weaker than the original formulation at (402), still imposes a strong restriction on the number of possible inflexional patterns for L; as we have seen, taking into account the Singular forms alone, it imposes an upper limit of 9 whereas the mathematical upper limit is 144, and if we take into account the Plural inflexions too, the discrepancy is enormously greater: 9, once again, versus a mathematical limit of 20,736. Even so, I take it that most linguists would regard the array of nine paradigms for L set out in (528) as distinctly implausible -- that is, they would regard it as distinctly unlikely that in any natural language such a relatively small wealth of inflexions (no more than three for any slot) could be organised into so many distinct paradigms. Consequently, a theory of inflexion which treats (528) as a possible set-up is deficient, and any plausible method of tightening the theory so as to predict the impossibility of (528) is worth exploring. And the lack of macroparadigm overlap in the examples from our four languages (Russian, Dyirbal, Zulu and Latin) suggests just such a method. The two formulations of the Paradigm Economy Hypothesis will certainly be rendered empirically distinct if we associate with the second (the macroparadigm version) a further empirical claim, on the following lines:

(531) Macroparadigm Uniqueness Claim

When paradigms are assigned to macroparadigms in accordance with the definition of 'macroparadigm', it will be found that each paradigm belongs to one macroparadigm and one only.

The Macroparadigm Uniqueness Claim is clearly incompatible with the hypothetical data from L at (528) since, as we have seen,

each paradigm in L can be assigned with equal justification to at least three macroparadigms and possibly more.

Of course, any generalisation based, as this one is, on evidence from only four languages must be regarded as tentative. But there are two points to be made here. Firstly, our preference for the macroparadigm reformulation of the Paradigm Economy Hypothesis over a weakened version of the original Hypothesis lacking the term 'non-lexically-determined' does not stand or fall by the correctness of the Macroparadigm Uniqueness Claim. There is a world of difference between the sort of macroparadigmatic confusion which we discovered in the nine hypothetical paradigms of (528) and the complete macroparadigmatic perspicuity which the Uniqueness Claim predicts; there is therefore plenty of scope for allowing macroparadigm overlap under certain conditions without opening the floodgates wide. Secondly, one aspect of the traditional terminology of morphological description, at least for Indo-European languages, certainly counts in favour of the Uniqueness Claim. We are used to statements such as: "In Sanskrit, Neuter nouns differ from non-Neuters of the i-stem declension in interpolating an -n- before the ending in certain Cases", or: "In the Latin of the Imperial epoch, Neuters differ from non-Neuters of the fourth declension in preferring -ū over -ūī as a Dative Singular ending". What is significant about these statements is that it is taken for granted that the inflexionally peculiar Neuters belong to some one identifiable declension-type to which non-Neuters also belong. The Macroparadigm Uniqueness Claim does not forbid the existence of, say, a nominal paradigm limited to one Gender. What the Claim does forbid, however, is a situation where such a paradigm can, on the basis of the criteria for identifying macroparadigms, be paired equally readily with more than one other paradigm; or, in other words, it requires that if any pairing is possible it should be unique. And what the traditional terminology of the statements on Latin and Sanskrit indicates is that, in Indo-European languages at least, this requirement seems generally to be met. We do not find, for example, a Neuter-only paradigm in Latin which shares some of the inflexions of 'second-declension' Masculines and Fe-

minines and some of the inflexions of 'fourth-declension' ones; rather, we find two distinct Neuter-only paradigms (those of bellum 'war', already quoted at (521), and of e.g. genu 'knee') which can each be assigned unambiguously to one and only one of the two declensions (the second and fourth respectively).

5.6 'Slabs' again, and a redefinition of 'paradigm'

I have claimed that the Macroparadigm Uniqueness Claim is supported by evidence from Russian, Dyirbal, Zulu and Latin. The Claim does seem at first sight, however, to be incompatible with the set of data which instigated our first introduction of the macroparadigm notion: the facts of German nominal declension presented at (505). The reason is that, when we applied the macroparadigm notion to these facts, we found that there was one Feminine-only declension-type (type VI, exemplified by Rose 'rose') which could be paired according to our criteria with no less than three non-Feminine types -- VIII (Bär 'bear'), IX (Dorn 'thorn') and X (Name 'name'). This multiplicity of possible pairings, indicated in (508) by the occurrence of 'VI' in three distinct macroparadigm labels, seems to involve macroparadigm overlap of a kind expressly forbidden by the Macroparadigm Uniqueness Claim.

I have already suggested that the Macroparadigm Uniqueness Claim could be weakened considerably without rendering empirically equivalent the macroparadigm version of the Paradigm Economy Hypothesis (which I have been advocating) and a version which does not distinguish between lexically determined and syntactically determined properties. It could be that these German facts are simply an indication that the relatively small array of evidence so far considered already requires such a weakening. But it would be disappointing to have to begin to relax our strong claim about macroparadigm overlap so soon; moreover, there is nothing in the German facts to suggest what form this relaxation should take, short of complete abandonment of the Claim.

Fortunately, there is quite a natural way of accounting for the apparent macroparadigm overlap in German which involves

no relaxation of the Macroparadigm Uniqueness Claim at all, but merely a new approach to 'mixed' paradigms. Of the three non-Feminine types in German which are candidates for macroparadigmatic union with type VI, one (namely type X, that of Name 'name') presents independent problems which I have promised to deal with in the course of my account of stem allomorphy in Chapter VII; to anticipate, this will involve claiming that, for purposes of paradigm economy, the Name type is not relevantly different from type I (that of Tag 'day'), with which type VI has no apparent macroparadigmatic connexion. So the immediate problem is reduced to the apparent overlap of only two macroparadigms, in (508) labelled 'VIII~VI' (Bär/Rose) and 'IX~VI' (Dorn/Rose). I will argue that the second of these types can be done away with by virtue of a new treatment of the 'mixed' declension-type IX (that of Dorn); moreover, this new treatment of 'mixture' has the same empirical force as the old treatment and fits in better with other aspects of paradigm economy.

In section 5.3 I argued that the declension-type of Dorn, which infringes the original version of the Paradigm Economy Hypothesis in being 'mixed', with no distinctive inflexions of its own, could be reconciled with our theory if we permitted paradigm mixture in circumstances involving fundamental morphosyntactic property contrasts. I proposed, in fact, that the Paradigm Economy Hypothesis should be relaxed in accordance with the following 'Slab Codicil' ((512) above):

(533) Slab Codicil to the Paradigm Economy Hypothesis

In a given language L some part of speech N may infringe the Paradigm Economy Hypothesis if this infringement involves 'mixed' paradigms (i.e. paradigms containing no inflexional realisation peculiar to them) which are divisible into 'slabs' on the basis of a fundamental morphosyntactic property contrast applicable to N, such that the set of inflexional realisations in each slab is shared with some 'unmixed' paradigm for N in L.

Independent evidence that some such relaxation of the Paradigm Economy Hypothesis was needed could be drawn, I suggested, from

the widespread need to quote more than one 'principal part', in lexical entries and elsewhere, in order to identify completely the inflexional behaviour of a given word; and independent evidence that the Slab Codicil, as proposed, was on the right lines could be found in the fact that each ^{principal part} was typically the basis for predicting the inflexion not of a scatter of forms throughout the paradigm but of a 'slab' of forms associated with some one relatively fundamental morphosyntactic property or combination of properties.

What I will be calling into question now is not the need for the Slab Codicil or its justification, but rather its formulation and the definition of the term 'paradigm' which underlies it. According to all versions of the definition of 'paradigm' so far discussed, the inflexional pattern of Dorn (type IX in (505)) constitutes a paradigm distinct from both the pattern of Tag (type I) and that of Bär (type VIII); what legitimises it, despite its 'mixed' status, is the fact that it is mixed in the fashion specified in the Slab Codicil ('going like' Tag in the Singular and Bär in the Plural). Under our approach so far, the Slab Codicil does not come into play, as it were, until after a set of paradigms has been tested for paradigm economy and been found wanting. But one can equally well take care of 'legitimate' paradigm mixture -- the kind that the Slab Codicil permits -- before the test for paradigm economy is applied. The way to do this is to amend the definition of 'paradigm' in such a way that Dorn ceases to represent a third paradigm on its own, distinct from the two paradigms of which it is a mixture. All we need do in order to effect this amendment is to transfer the important features of the Slab Codicil -- the reference to 'fundamental morphosyntactic property contrast' -- to the definition of 'paradigm' itself. Our current definition is given at (524) at the beginning of section 5.5. I propose now a redefinition as follows:

- (534) A paradigm for a part of speech N in a language L is a pattern P of inflexional realisations for all combinations of non-lexically-determined

morphosyntactic properties associated with N such that some member of N exemplifies P (i.e. displays all and only the realisations in P); except that:

- (a) if two patterns P_1 and P_2 differ only in pairs of inflexions whose distribution can be accounted for phonologically, P_1 and P_2 count as one paradigm;
- (b) if a pattern P is 'mixed' (i.e. contains no inflexional realisation peculiar to it) so as to be divisible into 'slabs' on the basis of a fundamental morphosyntactic property contrast applicable to N such that the set of inflexional realisations in each slab is shared with some unmixed pattern, then P does not count as a paradigm distinct from those unmixed patterns which it resembles.

It should be clear that any inflexional behaviour which can be reconciled with paradigm economy by recourse to the Slab Codicil can just as well be reconciled with it by recourse to this new definition of 'paradigm', for the 'mixed' paradigms to which the Slab codicil would apply will no longer have any independent existence; conversely, the new formulation will withdraw 'paradigmhood' from only those inflexional patterns which, if regarded as distinct mixed paradigms, would have their existence legitimised by the Slab Codicil. So the old and new formulations seem to be empirically identical (or 'notational variants', in Chomskyan terminology). But there is a reason for preferring the new to the old: with the new formulation, German nominal declension at once ceases to be a counterexample to the Macroparadigm Uniqueness Claim. The trouble in German arises because type VI can be paired equally well with type VIII and type IX.¹⁹ But, with the new definition of 'paradigm', the inflexional pattern of Dorn -- that labelled 'type IX' in (505) -- no longer constitutes a distinct paradigm at all. It follows that there remains only one German nominal paradigm with which

type VI can be united to form a macroparadigm, namely type VIII (exemplified by Bär). There is thus no macroparadigmatic overlap in German after all, and the Macroparadigm Uniqueness Claim can still be sustained.

It is, of course, encouraging to find that the Macroparadigm Uniqueness Claim is, after all, compatible with the facts that we have examined not only in Russian, Dyirbal, Zulu and Latin but also in German. But the outcome of our discussion is encouraging for two further reasons. Firstly, the sole macroparadigm to which the German Feminine noun Rose ends up belonging is one which embraces precisely the two declension-types labelled 'weak' in traditional descriptions of German noun morphology -- the Rose type itself and the type of Masculines such as Bär. Our analysis in terms of macroparadigms thus results in not some outlandish lumping together of traditionally disparate declension-types but rather a quite conventional classification for which the labels 'weak' and 'strong' already exist. Moreover, we can reasonably claim that our analysis provides a rationale for this classification in terms of an inflexional theory which is doubtless still inadequate in many respects but which is designed to be applicable to not just Germanic or Indo-European languages but all languages where distinct inflexional patterns need to be recognised within a single part of speech. Secondly, we have found a criterion for discriminating between two methods of treating the kind of paradigm mixture whose existence forces revision of the original strictest version of the Paradigm Economy Hypothesis; we have grounds now for discarding a method involving a qualification or 'codicil' to the Hypothesis itself in favour of a method involving a further revision of the definition of 'paradigm' on which the Paradigm Economy Hypothesis rests. Certainly our new definition of 'paradigm' is more complicated than that with which we started out at the beginning of Chapter IV, and has had moreover to be supplemented by a definition of a new concept, 'macroparadigm'; but this complication, I suggest, is the result not of the piling up of ad hoc revisions to a basically misconceived theory, but simply of the

need to express precisely a basically simple concept ('paradigm economy') whose implementation in the way paradigms are organised is affected by quite a number of other factors: lexical versus syntactic determination of morphosyntactic properties, more versus less fundamental contrasts between them, and phonological versus non-phonological conditioning of inflexional allomorphy.

5.7 Summary of definitions and claims on paradigm economy

In the course of this chapter we have discussed a number of revisions and qualifications of both the Paradigm Economy Hypothesis and the definition of 'paradigm' which underlies it. For ease of reference, I will restate here all the elements in the current version of what one might call the paradigm economy 'package':

- (535) (cf. (534)) A paradigm for a part of speech N in a language L is a pattern P of non-lexically-determined morphosyntactic properties associated with N such that some member of N exemplifies P (i.e. displays all and only the realisations in P); except that:
- (a) if two patterns P_1 and P_2 differ only in pairs of inflexions whose distribution can be accounted for phonologically, P_1 and P_2 count as one paradigm;
 - (b) if a pattern P is 'mixed' (i.e. contains no inflexional realisation peculiar to it) so as to be divisible into 'slabs' on the basis of a fundamental morphosyntactic property contrast applicable to N such that the set of inflexional realisations in each slab is shared with some unmixed pattern, then P does not count as a paradigm distinct from those unmixed patterns which it resembles.
- (536) (cf. (523), (525)) A macroparadigm for a part of speech N in a language L is a set of paradigms for N whose inflexional differences can be correlated with some consistent difference in some lexically

determined morphosyntactic or morphosemantic category (such as Gender or Animacy in nouns, or Transitivity in verbs); or else any individual paradigm for N in L which cannot be assigned to such a set.

(537) (cf. (526)) A macroparadigmatic inflexional set (or 'macroinflexion' for short) for some morphosyntactic slot is a set of inflexional realisations for that slot consisting of all the realisations appropriate to one macroparadigm.

(538) (cf. (531)) Macroparadigm Uniqueness Claim:
When paradigms are assigned to macroparadigms in accordance with the definition of 'macroparadigm', it will be found that each paradigm belongs to one macroparadigm and one only.

(539) (cf. (527)) Paradigm Economy Hypothesis (revision incorporating macroparadigms):
When in a given language L more than one inflexional realisation is available for some combination or combinations of non-lexically-determined morphosyntactic properties (some 'slot(s)') associated with some part of speech N, the number of macroparadigms for N is no greater than the number of phonologically independent ('rival') macroinflexional realisations available for the slot which has most such realisations.

One notion is taken for granted throughout this package: that of 'inflexional realisation'. But the question of what constitutes distinctness between inflexional realisations will occupy us in Chapter VII, section 7.3, where I will discuss paradigm economy in relation to stem allomorphy. The result will be a further improvement of the paradigm economy 'package', accounting both for certain further prima facie instances of paradigm mixture and for certain instances of near-identity between apparently distinct paradigms.

Meanwhile, Chapter VI will be devoted to an extended and detailed examination of paradigm economy in the inflexion of certain Latin nouns. This examination will involve for the first time exploring ^{in detail} some consequences of the Paradigm Economy Hypothesis for morphological change; and my conclusion will be that the relevant changes in Latin support the Paradigm Economy Hypothesis, in that certain quite precise predictions flowing from the Paradigm Economy Hypothesis about the the timing and extent of the changes are borne out by the evidence.

Footnotes to Chapter V

1. I ignore here and throughout this chapter the possibility of omitting -e- in many of the forms in -es, e.g. Staates ~ Staats.
2. My thinking about German inflexion owes much to Wurzel (1970) and to classes on the structure of German conducted by Paul Kiparsky at MIT in 1971, even though I differ from them both in general approach and in the problems I am addressing.
3. I say nothing here about the optional schwa in certain 'strong' Genitives (cf. footnote 1) such as Tages or Tags and Datives such as Tage or Tag; I assume that this can be accounted for by optional phonological processes, and has no effect on the number of paradigms. I also ignore marginal declension-types clearly limited to 'foreign' words such as Studium, Firma; to include them would complicate the argument without affecting the main conclusions.
4. Unless, that is, we count pronominal anaphora relating to sex rather than grammatical Gender (e.g. sie coreferential with das Mädchen 'the girl').
5. If we say simply 'lack stress' rather than 'lack primary stress', we will by implication predict that the Genitive Singular of Untertan will be Untertanen rather than Untertans, since the final syllable -tan does have secondary stress. It may be significant, therefore, that Untertanen does actually exist as an alternative form.
6. Similar conclusions emerge from Rudes's (1980) study of verbal stem suppletion.
7. In Chapter VII we will come across instances of noun stem allomorphy which is not consistently correlated with Number; but this is not direct counter-evidence to what I have just said, because the allomorphy is not consistently correlated with Case either.
8. On the development in Swedish, see Wurzel (1975).
9. Comrie (1978) offers a thorough description and discussion of this phenomenon, not only in Russian but also in other

Slavic languages.

10. The ramifications are in fact more complex still, though still consistent with paradigm economy. Comrie & Stone (1978: 89-91) quote evidence to the effect that the -a Nom Pl in Masculine nouns is especially characteristic of technical terms in technical contexts, e.g. supa 'soups' in caterers' usage instead of the usual supy, and redaktora 'editors' in journalists' usage instead of the usual redactory. This suggests the development of a macroparadigm in which the Masculine Nominative Plural endings -a and -y are correlated at least partially with morpho-semantic properties which one might label 'Technical' and Non-technical'.

11. The label 'Intransitive' would not be altogether appropriate since some 'y-stem' verbs are Transitive (Dixon 1972: 54); but all we need here is some arbitrary label, so that does not matter.

12. As in Chapter II, I continue to use 'Class' to refer to patterns of Singular-Plural pairs rather than to Singular types or Plural types individually.

13. The largest such marginal type is the group of ten or so nouns (nearly all Human) which 'go like' Class 9/10 in the Singular and Class 5/6 in the Plural, e.g. in-doda 'man', Plural ama-doda (Doke 1973: 54; Rycroft & Ngcobo 1979: 62).

14. David Rycroft informs me that 'tramp' is a more appropriate gloss than Doke's 'pilgrim'.

15. The pejorative connotation of Class 3/4 Human nouns was pointed out to me by David Rycroft. The contrast in sense between imihlobo and abahlobo was confirmed by a native speaker, Fr. J. Ngubane.

16. There is a fairly small declension-type displaying a Nom Sg in -us but distinct from the type of dominus, namely that of 'fourth declension' nouns such as fructus 'fruit', tribus 'tribe'. The existence of this type seems to entail a breach of paradigm economy if we treat the Nom Sg as the 'diagnostic' slot. I will return to this in the next section.

17. To reconcile the German data fully with the Paradigm Economy Hypothesis of course requires appeal to the Slab Codicil, as described in section 5.3, as well as some satisfactory account of declension-type X (that of Name), which I have deferred until Chapter VII.

18. The Plural is omitted simply to make the example more manageable.

19. It will be recalled that we have deferred until Chapter VII an account of type X (that of Name).

CHAPTER VI

PARADIGM ECONOMY IN THE LATIN THIRD DECLENSION

6.1 Introduction

One consequence of the Paradigm Economy Hypothesis is that every inflexional paradigm should have at least one inflexion (or, strictly, one macroparadigmatic set of inflexions) peculiar to it; more precisely, that some combination of morphosyntactic properties should be realised in each paradigm in a fashion different from every other paradigm for the relevant part of speech. The Hypothesis is therefore violated if in any language we find paradigms containing no unique realisations for any property combination, or, in other words, no distinctive or peculiar inflexions. Such a state of affairs occurs in the Latin of the 'Golden Age' (around the turn of the era) in a set of nominal paradigms, traditionally included under the heading 'third declension', which are derived historically from a partial merger of Indo-European i-stem and consonant-stem patterns. If we are to maintain the Paradigm Economy Hypothesis, we must demonstrate how these Latin data, at first sight incompatible with the Hypothesis, can be accommodated. The first purpose of this chapter is to offer such a demonstration, and thus to show that Latin does not force the abandonment of the Hypothesis.

The chapter has a second purpose also. The relevant Latin data cover a span of time during which considerable change is visible in the inflexional morphology of the third declension -- more change by far than occurs in the other four nominal declensions traditionally recognised, and indeed more than in any other area of Latin morphology. These data thus offer a relatively rare opportunity to examine over time a fairly rapid succession of morphological innovations whose extent and chronology can be established with fair accuracy and in some detail from actual written evidence, rather than internal reconstruction or comparison. So, for anyone interested in the mechanisms of morphological change, the Latin data present in some ways a more severe

challenge than larger-scale problems of Indo-European comparative linguistics -- problems which, because they extend over more languages and a much longer time-span, generally involve 'proto-languages' whose reconstruction inevitably depends to some extent on a priori assumptions about the very mechanisms of change which it is the historical linguist's first task to elucidate. By contrast, in Latin if nowhere else it ought to be possible to test predictions not merely about general trends in morphological development but also about the precise domain and timing of particular innovations. Consequently, if a theory such as the Paradigm Economy Hypothesis, advanced on the basis of synchronic evidence, turns out not merely to be consistent with but actually to predict some of these diachronic facts, then these facts provide solid independent support for it. I hope to show that the Paradigm Economy Hypothesis does indeed entail predictions of this kind, thus helping to explain certain otherwise apparently arbitrary contrasts in the development of particular subclasses of nouns within the 'third declension', and consequently that the Latin evidence turns out in an at first sight rather unexpected fashion positively to confirm the Hypothesis rather than merely to fail to disconfirm it. The discussion will involve further application of the 'macroparadigm' concept and will also bear on the issue of variation or exceptionality within paradigms, mentioned at (408) at the end of Chapter IV. The argument is quite long and intricate, but a chart summarising what factors I believe to be at work in each stage of the Latin development is provided at Appendix C.

I will rely heavily on the work of earlier Latinists. The relevant Latin facts are conveniently summarised, from a historical point of view, in three handbooks: Ernout (1953) 35-38, 50-56; Leumann (1977) 429-441 (especially 437-441); and Sommer (1948) 362-387 (especially 375-8, 382-4 and 385-6). Another important study is that of Meillet (1906). But for testing predictions relating to detail, summaries are insufficient; we are therefore fortunate in having at our disposal the enormous work of Neue and Wagener (1902), which catalogues occurrences

of relevant variant Case-forms of individual third-declension nouns from the earliest period down to the 'Silver Age' of the second century AD and beyond. Neue and Wagener also quote relevant passages from ancient grammarians, of whom the most important for our purposes^s is Varro (116-27 BC), since he was a contemporary of some of the morphological innovations with which we are concerned.¹ NW is usefully supplemented by Ernout (1918), who examines in detail the manuscript evidence for certain third-declension Case-forms in the usage of Lucretius (first half of the first century BC) and compares it with evidence from inscriptions.

The handbooks rightly recommend caution in evaluating the usage of literary authors, whose work survives not in autograph originals nor even in early copies but only in medieval versions at the end of a manuscript tradition more or less corrupted not only by plain mistakes but also by the normalising urges of successive scribes. Morphological oddities of the kind with which we are here concerned are of course particularly vulnerable to 'correction'. So, since I will be referring more than once to the usage of Lucretius in particular, I need to offer reasons why his manuscript tradition is more trustworthy than that of most classical authors. Fortunately, such reasons can be found. They are stated in Appendix B.

The Latin declensional system has been summarised on American structuralist lines by Hall (1946) and Householder (1947). Two other recent studies of considerable interest are those of Janson (1971) and Risch (1977). Janson explores how far it is possible to account on purely phonological grounds for the synchronic distribution of each third-declension Case-ending around the turn of the era, ignoring its historical origin as a consonant-stem or an i-stem ending. Risch, arranging the six main Latin declension-types according to their mutual resemblance, notes that on this basis they form an intriguing circular pattern; and he uses aspects of this pattern to help explain certain morphological trends in a way which complements the argument based on paradigm economy which I shall be advancing here.²

6.2 The Latin data; a first summary

The threat that the third declension poses to the Paradigm Economy Hypothesis can be seen in the table at (601), which illustrates the sort of attempt that school grammars have traditionally made to pin down the inflexional behaviour in the Golden Age of the great majority of third-declension nouns with a Nominative Singular in -s:

(601)	A	B	C
Sg Nom/Voc	ignis 'fire'	dens 'tooth'	rēx [re:ks] 'king'
Acc	ignem	dentem	rēgem
Gen	ignis	dentis	rēgis
Dat	ignī	dentī	rēgī
Abl	ignī or igne	dente	rēge
Pl Nom/Voc	ignēs	dentēs	rēgēs
Acc	ignīs or ignēs	dentēs (also -īs in some nouns)	rēgēs
Gen	ignium	dentium	rēgum
Dat/Abl	ignibus	dentibus	rēgibus

We find here what appear to be at least three distinct paradigms, in the sense defined in Chapters IV and V; yet despite the prediction made by the Paradigm Economy Hypothesis, there is no combination of the categories Number and Case (for brevity, let's say 'no Case') which is realised by as many as three distinct endings, one for each paradigm. Five Cases (Accusative, Genitive and Dative Singular, and Nominative and Dative-AbIative Plural) have the same ending throughout, so do not contribute at all to distinguishing the three paradigms; this burden falls on the remaining four Cases, none of which, however, is realised in more than two ways. This is illustrated in table (602), which highlights those aspects of table (601) which are most relevant to the Paradigm Economy Hypothesis;³

(602)	A	B	C
Sg Nom/voc	-is	-s	-s
Abl	-ī or -e	-e	-e
Pl Acc	-īs or -ēs	-ēs (in some nouns -īs)	-ēs
Gen	-ium	-ium	-um

As I have already said, Latin third-declension morphology changes considerably over a relatively short space of time between the date of our earliest texts and about 50 AD. The facts as presented above are therefore not the whole story, merely a somewhat oversimplified summary of it relating to a point in time around the middle of the first century BC. To try to present all the relevant facts at once would complicate the picture vastly; instead, I will introduce further relevant facts at appropriate points in the exposition. There is, of course, a danger in this; the reader is entitled to an assurance that I will not regard as 'relevant' only those facts which tend to support the analysis I propose, omitting potential evidence which is either neutral or damaging. I will therefore try to mention any fact which could conceivably be relevant in either direction; and the reader can in any case turn to the passages cited in Ernout, Sommer, Leumann or NW for an account by a writer with no axe to grind on the issue of paradigm economy.

One approach which we might consider adopting in order to reconcile (601) with the Paradigm Economy Hypothesis is that of distinguishing between the declension-types A, B and C on the basis of the underlying representations of the stems concerned; we might then attribute the paradigmatic profusion observable 'on the surface' to phonological interaction between stems and endings. By this means we might reduce the number of paradigms distinct at the underlying phonological level to a number consistent with the Paradigm Economy Hypothesis. Such an approach would accord well with the way that historically-oriented scholars have generally described the third declension; for them, words belonging to declension-type A are i-stems while C consists of

consonant-stems and B contains a mixture of the two. But I will not adopt that approach here. From a synchronic point of view it is hard to find independently-motivated phonological rules which would generate the correct surface forms for (say) declension-types A and C (ignis and rēx) on the assumption that at the underlying phonological level they differed only in some characteristic of their stems and not at all in their Case-Number endings. Rather, the evidence suggests that, apart from one or two quite general and 'natural' processes such as voicing assimilation (yielding rēx [re:ks] from /rēg + s/) and cluster simplification (yielding [s] from /t + s/ in word-final position), phonology plays no role in accounting for allomorphy in the Latin third declension; consequently, the boundary between stem and ending in all the declension-types A, B and C is best taken as falling immediately after the invariant part of the word (thus /ign-is/, /ign-ium/, not /igni-s/, /igni-um/).

This view obviously rests on a certain view of Latin phonology which it would take some time to expound and defend. Fortunately, I do not need to do so because, in foregoing any appeal to stem differences to help account for the superficial discrepancies between the declension-types, I make my job of reconciling the data with the Paradigm Economy Hypothesis harder, not easier. If it should turn out that my restrictive view of Latin synchronic phonology is wrong, there will be fewer, not more, distinct paradigms to account for; so, if (as I shall argue) it is possible to reconcile the Hypothesis with the data even on my restrictive phonological assumptions (in consequence of which types A, B and C are distinct underlyingly as well as superficially), it will a fortiori be possible to do so on the basis of more generous assumptions about how much of the superficial paradigmatic divergence can be accounted for synchronically by phonological rules.

There is an important distinction to be made here. All formulations of the Paradigm Economy Hypothesis in Chapters IV and V allowed for the possibility that the inflexional spell-out

rules themselves might be sensitive to phonological features of the lexical items to which they apply, to account for e.g. the distribution of -(a)sz and -ol as realisations of 2nd Person Singular in Hungarian verbs: the latter after sibilants, the former elsewhere. Such phonological conditioning of the spell-out rules themselves, yielding more than one 'spelling' for some combination of properties, is not the same as the subsequent alteration of the output of the spell-out rules by phonological rules in the usual sense; and it is only the latter, not the former, that I have rejected in connexion with Latin. The former -- phonologically-determined diversity in 'spelling' -- will indeed play a considerable part in my argument.

6.3 Syncope, the Paradigm Economy Hypothesis and the Singular of dens

The paradigms of types A, B and C are represented in the first century BC by a large number of nouns, both Masculine and Feminine (no Neuters), including many in common use, for example:

(603) A	B
cīvis M or F 'citizen'	dens (dent-) M 'tooth'
hostis M or F 'enemy'	mons (mont-) M 'mountain'
ignis M 'fire'	gens (gent-) F 'nation'
amnis M 'stream'	mors (mort-) F 'death'
fīnis M or F 'end'	urbs F 'city'
C	
rēx (rēg-) M 'king'	
dux (duc-) M 'general'	
pēs (ped-) M 'foot'	
lēs (lēg-) F 'law'	
hiems F 'winter'	
comes (comit-) M or F 'companion'	

These three declension-types are specially important to us on two counts: first, because of their frequency, and second, because they manifest the greatest degree of morphological fluidity in classical Latin.

One way of approaching an understanding of the synchronic

state of affairs in the three paradigms is to consider their origin. Broadly speaking, types A and C contain nouns which, insofar as they have an Indo-European etymology, belong to original Indo-European i-stem and consonant-stem types respectively (Leumann 1977: 429-430). This is not to deny that a few nouns (e.g. nāvis 'ship') have 'crossed over'. This is not to say either that all the endings illustrated in (601) for type A are straightforward phonological reflexes of endings which can plausibly be reconstructed for one of the i-stem types in proto-Indo-European, or that all the type C endings are straightforward reflexes of proto-Indo-European consonant-stem ones. If the declension of type C nouns were derived purely phonologically from that of their putative ancestors, we would expect to find among type C nouns forms such as Nom Pl "rēgis" (< *rēg-es) and Dat/Abl Pl "rēgbus" (< *rēg-bhos); similarly, we would expect to find among type A nouns forms such as Gen Sg "ignīs" and per-also Acc Sg "ignim"⁴. There seems, in fact, to have been in pre-Latin a considerable amount of mutual influence in inflexional behaviour between original consonant-stem nouns and original i-stem ones; and the details and (so far as possible) the causes of this mutual influence would need to be explored in any attempt at a complete account of the history of Latin declension. But our present concern is only with how the Latin facts outlined at (601) bear on the Paradigm Economy Hypothesis; and, given this restriction, what matters is not the details of how the 'Golden Age' endings of types A and C arose but the fact that the membership of the two classes in Latin still broadly reflects the Indo-European distinction mentioned.

From comparison with cognate forms in other Indo-European languages, it is clear that many vowels in medial and final syllables in pre- or early Latin underwent diachronic processes of weakening, in particular raising (e.g. a > e, e > i, o > u) or deletion. Some of these innovations appear not yet to have been carried through completely at the time of our earliest surviving texts, and many of them left behind alternations which may or may not indicate their survival as synchronic processes. Pro-

vided we assume that in pre- or proto-Latin stress was word-initial, a common feature of all these weakening processes emerges: they operated only on unstressed vowels. But for our purposes what matters is not the cause of these processes but some of their effects, and particularly the effects of the process of syncope which deleted certain vowels in final syllables.

Many of the members of declension-class B (that of dens) originated in class A, with a Nominative Singular in -is rather than -s. This is clear, again, from both early Latin evidence and Indo-European comparisons. The -t- immediately preceding the ending in many class B nouns derives from an Indo-European abstract-noun-forming suffix ^{*}-tei-/-ti-, which shows up also in the common Greek deverbative suffix -sis (Meillet & Vendryes 1966: 395-397). Latin class B nouns with this old suffix include ars 'art', fors 'chance', gens 'nation', mens 'mind', mors 'death', pars 'part', sors 'lot, fate'. Confirmation is supplied by early Latin survivals of unreduced -tis occasionally in the Nominative Singulars mentis for mens in Ennius and sortis for sors in Plautus and Terence (NW 232). We can conclude that these Nominative Singulars were affected at an early stage in the history of Latin by a process of syncope. I will not try to give a complete account of the environments in which syncope operated, partly because this is a matter of doubt to Latinists (Niedermann 1953: 54-59). But it is clear from the examples quoted that at least some of the effects of syncope can be expressed as follows:

$$(604) i \rightarrow \emptyset / \left\{ \begin{array}{l} r \\ n \end{array} \right\} t \text{ ______ } s$$

The class B noun frons (frond-) 'leaf', for which one ancient commentator on Vergil cites an early Nom Sg frondis (NW 230), justifies us in making the rule slightly more general:

$$(605) i \rightarrow \emptyset / \left\{ \begin{array}{l} r \\ n \end{array} \right\} T \text{ ______ } s \quad \text{where } T = t, d$$

It is theoretically possible that declension-type B (that of dens, gens etc.) should have existed before syncope applied in Latin: that is, that there should already have been nouns with

Nom Sg -s and Abl Sg -e (like type C) but Gen Pl -ium (like type A). But there is no positive evidence for this and some evidence against. One common member of class B in Golden Age Latin, namely dens 'tooth', is known to be derived from an Indo-European consonantal stem, not a stem in -i. We might therefore expect it in Latin to display the same Genitive Plural ending as do the reflexes of other Indo-European nouns with consonantal stems, namely -um, not -ium. The Golden Age Latin Gen Pl of dens is, however, dentium, and on that ground it must be assigned to class B. On the other hand, the 'expected' Gen Pl dentum, which would indicate membership of class C rather than B, is regarded by Varro (NW 378) as the only correct one.⁵ It is therefore very tempting to see declension-type B as an entirely post-syncope development in Latin, consisting on the one hand of those former nouns in -is which underwent syncope and whose relationship with other -is nouns was thereby synchronically obscured, and on the other hand of a certain number of former class C nouns, like dens, which could plausibly be reanalysed as belonging to the newly-created type B. This much, in fact, is common ground among the authors of the handbooks, although the reasons for choice between type A and type C endings in individual Case-forms within the new mixed type B are left more or less vague. It is therefore uncontroversial to assume that, before syncope, A and C existed as distinct declension-types, but not B.

Let us now consider the immediate effect of syncope on members of classes A and C. Before syncope, the endings would have been essentially as follows:

(606) Singular:		Plural:		
	A	C	A	C
Nom	-is	-s	-ēs	-ēs
Acc	-em	-em	-īs	-ēs
Gen	-is	-is	-ium	-um
Dat	-ī	-ī	-ibus	-ibus
Abl	-ī	-e	-ibus	-ibus

After syncope, the resulting pattern would have been as follows⁶
(assuming no further changes):

(607)	A (unsyn- copated)	A (synco- pated)	C
Sg Nom/Voc	-is	-s	-s
Acc	-em	-em	-em
Gen	-is	-is	-is
Dat	-ī	-ī	-ī
Abl	-ī	-ī	-e
Pl Nom/Voc	-ēs	-ēs	-ēs
Acc	-īs	-īs	-ēs
Gen	-ium	-ium	-um
Dat/Abl	-ibus	-ibus	-ibus

For clarity, we can condense table (607) by omitting those Cases which are realised identically throughout, much as we did in (602). It becomes clear when we do so that the syncopated A type is a 'mixed' paradigm of the kind that infringes the Paradigm Economy Hypothesis. If we box together inflexions belonging to the mixed paradigm with those that they resemble in the appropriate unmixed paradigm, the result is as in (608):

(608)	A (unsyn- copated)	A (synco- pated)	C
Sg Nom/Voc	-is	-s	-s
Abl	-ī	-ī	-e
Pl Acc	-īs	-īs	-ēs
Gen	-ium	-ium	-um

However, the state of affairs illustrated in (608) is not what we find in Golden Age Latin. What we find there differs in two main respects. First, the membership of the new class B extends beyond syncopated A-stems to include at least one old consonant-stem noun (dens, already mentioned), as well as nox (noct-) F 'night' which, on the basis of cognates, is almost certainly an original consonant-stem noun in pre-Latin too, and a large number of nouns such as urbs 'city', falx 'scythe', arx 'citadel' whose earlier history is uncertain. Secondly, the distribution of 'consonant-stem' and 'i-stem' endings in the new declension-type B is not precisely as in the column headed 'A (syncopated)' in (608). We

will examine the second of these two discrepancies now; the first will occupy us in the next section.

The e-endings in the Ablative Singular and Accusative Plural of declension-types A and B are newer than the i-endings, and become more frequent in the course of the development of Latin until by about the second century AD it seems reasonable to conclude that the i-endings survive only as a literary affectation in a few authors. One might therefore expect that the replacement of -ī by -e and -īs by -ēs would go hand in hand; that the two 'new' e-forms would spread at the same rate to the same words and that any word which acquired one would at the same time acquire the other. But this is not so. The facts can be summarised as follows:

- (609) In the Golden Age, the number of nouns for which an Accusative Plural in -īs is attested is considerably larger than the number of nouns for which an Ablative Singular in -ī is attested.
- (610) The -ī Ablative Singular is obsolete in the B class, even among old 'i-stem' nouns, long before it disappears in the A class. On the other hand, the -īs Accusative Plural remains alive in both classes for about the same length of time.
- (611) The -ī Ablative Singular seems to disappear altogether, even from A-class nouns, sooner than does the -īs Accusative Plural.

Examples of common A-class nouns for which Accusative Plurals in -īs are attested but Ablative Singulars in -ī are not (that is, which consistently show -e in that Case) are auris 'ear', crīnis 'hair', vallis 'valley'. Examples of B-type nouns which share an Ablative Singular in -e but which diverge in the Accusative Plural are mens 'mind' (Acc Pl mentēs) and gens 'nation' (Acc Pl gentīs). The last pair is particularly striking. Mens and gens are phonologically identical in all conceivably relevant respects, and both were originally syncopated -is nouns containing the old suffix *-ti-; but their divergence is stated firmly by Varro,

in the same passage where he confirms the existence of an old Genitive Plural dentum. This passage is worth quoting in full (NW 378):

(612) ... quid potest similis esse quam mens, dens, gens, cum horum casus patricus et accusativus in multitudine sint dispariles. nam a primo fit gentium et gentis, utrobique ut sit i; ab secundo mentium et mentes, ut in priore solo sit i; ab tertio dentum et dentes, ut in neutro sit i.
 [... what could be more similar than mens, dens and gens, although their Genitive and Accusative Plurals are different? For the first [sic] has [Genitive Plural] gentium and [Accusative Plural] gentis, with -i- in both; the second has mentium and mentēs, with -i- only in the first of the two forms; and the third has dentum and dentes, with -i- in neither.]

This state of affairs is confirmed by the manuscript tradition of Lucretius; the Accusative Plural of gens appears six times as gentis and never as gentēs, while for mens we find five times mentēs and only once mentis (Ernout 1918: 164). Similar confirmation comes from the manuscript traditions of other authors⁷ (NW 388).

The aim of the linguist must be to account for the facts summarised in (609)-(611) in a systematic way so far as possible. Despite the apparently arbitrary divergence between (say) auris and amnis in the Ablative Singular and that between gens and mens in the Accusative Plural, I believe a considerable degree of order is observable. Let us consider again the state of affairs produced by the operation of a phonological rule of syncope but with no further change or restructuring, as illustrated in (608).

A first point to note is that, if the Paradigm Economy Hypothesis is correct, we will predict that some restructuring must take place in order to heal the breach in paradigm economy which the phonological innovation has caused. This prediction is correct. At (610) I mentioned that the -ī Ablative Singular be-

comes obsolete at an early stage in the B class. Evidence for this is supplied by the evidence of Lucretius. In the Accusative Plural, both the -ēs ending (as in mentēs) and the -īs ending (as in gentīs) are common in type B nouns; in fact, Ernout (1918: 163-164) lists 35 -īs and 38 -ēs (counting in four instances of dentēs, which he lists among consonant-stem nouns). But we do not find this even spread repeated when we turn from the Accusative Plural to the Ablative Singular of B-class nouns. Here the -e ending predominates heavily, having 179 occurrences against only seven of -ī; moreover, all seven instances of -ī are in the forms partī from pars 'part', against which there are 44 occurrences of parte.⁸ This contrast in the behaviour of the two Cases seems too striking to be merely accidental; nor can it be put down to any general dislike of -ī-Ablatives on Lucretius's part, since with type A nouns he has a distinct preference for it (39 instances of -ī, not counting vī from vīs 'force', against only ten of -e). Moreover, the Lucretian distribution is not idiosyncratic, since it is confirmed by the evidence from other authors presented in NW; all common nouns of type B, even those such as gens, pars and mons which frequently show -īs in the Accusative Plural, either lack an Ablative Singular in -ī entirely or display it only rarely and in early writers. A final confirmation for the divergence of the two Cases comes from Varro. He recognises as equally correct not only alternative Accusative Plurals such as montēs and montīs from type B nouns, but also alternative Ablative Singulars such as avī and ave from type A nouns (NW 378); yet an Ablative Singular such as fontī rather than fonte from a type B noun fons 'spring' is, for him, definitely odd. The relevant passage is again worth quoting at length (De lingua latina ix 112):

(613) qui dicit hoc monti et hoc fontī, cum alii dicant hoc monte et hoc fonte, sic alia quae duobus modis dicuntur, cum alterum sit verum, alterum falsum, non uter peccat tollit analogias, sed uter recte dicit confirmat.

[When someone says hōc montī and hōc fontī, although others say hōc monte and hōc fonte, just as with

other pairs of usages of which one is correct and one incorrect, the man who makes a mistake does not destroy the regularity but the man who speaks correctly strengthens it.]

Evidently syncope in the Nominative Singular of words such as gens and mens triggered a further change in the Ablative Singular. How is this change most naturally explained? The answer, surely, is in terms of a reanalysis of the whole Singular paradigm of the syncopated class on the lines of type C. Such a reanalysis would have as a consequence just the behaviour we observe in Lucretius: since type C does not provide an Ablative Singular ending -ī, old i-stems which had undergone syncope had to adopt a new ending -e in order to conform to type C, or else preserve their old ending (as pars does sporadically in Lucretius) only as an exceptional characteristic, lexically marked and vulnerable to levelling. On the other hand, those i-stems which did not undergo syncope in the Nominative Singular (the unsyncopated A-stems, in the terminology of table (608)) remain 'unmixed' in their declension-type and were subject to no reanalysis; consequently, for them the Ablative Singular in -ī remained no less regular than before and was under no new pressure to yield to -e.

We have shown, then, that the Paradigm Economy Hypothesis is correct in predicting that some further change should occur once syncope had produced the pattern illustrated in (608). The next question, obviously, is whether the actual change we have just described -- a reassignment to type C of the syncopated A-type nouns in the Singular -- is predictable on the basis of the Hypothesis. The answer is, strictly speaking, no; the Hypothesis requires that, if a phonological change (such as syncope) has the effect of increasing the number of distinct paradigms beyond the 'legal maximum', steps must be taken to reduce the number again, but it does not specify what those steps must be. In practice, however, there was readily available in Latin only one way to achieve the desired effect, so we are entitled to say

that the Paradigm Economy Hypothesis predicts the actual change indirectly. The Hypothesis predicts, in fact, that the new syncopated class, being 'mixed', should be realigned completely in the Singular with some other, already existing, declension-type; and the only obvious candidate as model for such realignment was the type which it had come to resemble in all Singular Cases except the Ablative, namely type C. This, in turn, imposed a requirement on syncopated nouns and on them alone to abandon speedily their inherited Ablative ending -ī in favour of the C-type ending -e.

6.4 Declensional merger in the Plural; the Genitive facts

So far, the Paradigm Economy Hypothesis seems to fit the facts quite well. It not only predicts indirectly the thoroughgoing replacement of -ī by -e in the Ablative Singular of syncopated nouns but also suggests why this replacement failed to spread (or spread so rapidly) to non-syncopated type A nouns⁹. Neither the timing nor the extent of these developments could be accounted for in so precise a fashion by a theory of morphological change which relied only on 'analogy', without the tight restriction on paradigmatic diversity which the Paradigm Economy Hypothesis imposes. But we have looked so far only at the Singular. The situation in the Plural is more complicated. The most natural restructuring, we might think, if there is indeed some constraint on paradigmatic diversity at work, would be for the syncopated class to align itself in the Plural too with the same declension-type as in the Singular. We would expect, then, to find nouns such as gens and mens all acquiring C-type Accusative Plurals in -ēs (replacing -īs) and Genitive Plurals in -um (replacing -ium). But this is not what happens. Rather, as Varro says ((612) above), and as the literary and epigraphic evidence confirm, some syncopated nouns (like mens) do rapidly acquire a C-type Accusative Plural, while others (like gens) retain an A-type one for longer; and all, without exception, retain their original Genitive Plural ending -ium. So does our attempt to reconcile the Latin developments with the Paradigm Economy Hy-

pothesis, so apparently successful in the Singular, founder completely in the Plural? This would be a highly damaging conclusion for the Paradigm Economy Hypothesis. But I will argue that we need not draw it. True, developments did not take place in the Plural paradigms which would have restored paradigm economy by the most obvious method, as just outlined; but there are other theoretically possible ways of restructuring the Plural so as to achieve paradigm economy, and I will argue that the further changes which took place in Latin represent one of these ways. Furthermore, just as in the Singular, if we posit a role for paradigm economy in these changes, some details which would otherwise appear arbitrary turn out to be expected.

Let us look once again at the immediate effect of the syncope innovation, tabulated at (607) and (608). We have seen that the paradigm mixture in the Singular is eliminated through a further innovation in the Ablative which allows syncopated nouns of type A to be assimilated to type C. But the Plural paradigm of the syncopated A class, looked at in isolation from the Singular, remains unmixed, simply because syncope did not occur in any Plural Case-forms of the A type. This lack of mixture in the Plural is illustrated by the fact that, in (608), all the Plural Case-forms of the syncopated A class are in the same box as those of the unsyncopated A class. The significance of this from the point of view of the Paradigm Economy Hypothesis is that it suggests the outline of an explanation for the Latin facts invoking proviso (b) in our latest definition of 'paradigm' ((535) in Chapter V). This proviso, it will be recalled, does the work of our earlier Slab Godicil; it legitimises apparent breaches of paradigm economy if they involve 'mixed' inflectional patterns divisible into 'slabs' on the basis of fundamental morphosyntactic property contrasts. The relevant morphosyntactic category for our Latin data will be Number, and the relevant contrast will be that between Singular and Plural. Applying proviso (b), we might say that the new declension-type B, which came into existence as an indirect result of the phonological innovation of syncope, is tolerated in Latin because in the Singular (thanks

to the change in the Ablative already discussed) it conforms to type C while in the Plural it conforms to type A. Depending on how favourably one regards the Slab Codicil and the amendment to our definition of 'paradigm' which replaces it, this line of argument may seem to reconcile the Latin data with the Paradigm Economy Hypothesis more or less satisfactorily. But a closer examination of the data suggests that this account is wrong, and that what took place in the Plural of declension-types A, B and C is best seen as a much more radical implementation of paradigm economy, namely the creation of one single declension-type.

Let us examine again the Slab Codicil argument just outlined. In one respect, this argument will need qualification straight away. Although old syncopated i-stem nouns constitute a large proportion and probably the great majority of the membership of class B, I have already mentioned one noun which joined class B from the consonant-stem class: dens, whose Genitive Plural always appears as dentium except in Varro who, in the earliest occurrence of the Case-form in surviving Latin texts, specifically prescribes dentum. Clearly, dens was reassigned to a new declension-type at or very soon after Varro's time, and it seems natural to suppose that the motivation for this reassignment was phonological; the Nominative Singular dens looks as if it could have arisen by syncope from an earlier "dentis", and for this reason it was assigned to the new declension-type of gens, mens and the rest. But what are the precise phonological conditions for this declensional reassignment? A natural expectation might be that it would affect all and only those erstwhile consonant-stem (or C-class) nouns whose Nominative Singular looked as if it might have come about by syncope. A difficulty here is that, as I have already said, the precise phonological conditions for the operation of syncope as a phonological innovation are unclear. But this difficulty need not worry us, because the natural expectation just mentioned is not in fact fulfilled, at least not in the form of a straightforward transfer of a class of nouns from one existing inflexional pattern to another.

Compare the Nominative Singular and Genitive Plural columns in the following table:

(614) Third-declension nouns with:

Stem-final consonant-cluster:	(a) Nom Sg in:-		(b) Gen Pl in:-	
	-is	-s	-ium	-um
nasal + stop	(none)	dens gens mens mons etc.	dens gens mens mons etc.	dens (in Varro only) parens (sometimes)
liquid + stop	orbis 'circle' corbis 'basket'	urbs stirps 'stem' arx (arc-) falx (falc-) pars (part-) etc.	orbis corbis urbs stirps arx falx pars etc.	(none)
stop + stop	vectis 'lever' neptis 'grand-daughter'	nox (noct-)	vectis neptis nox	(none)
spirant + stop	hostis 'enemy' vestis 'garment' pestis 'plague' etc.	(none)	hostis vestis pestis etc.	(none)

What we observe is a clear contrast in predictability between the two Cases. Knowing that a third-declension noun belongs to one of types A, B or C and that its stem ends in a consonant cluster whose second member is a stop (which accounts for the great majority of consonant-clusters in terms of frequency of occurrence in Latin), we can tell at once that its Genitive Plural will be in -ium (the only post-Varronian exception, and even that not a consistent one, being parentum from parens 'parent'); but we cannot make any prediction about its Nominative Singular unless we also know that the first member of the cluster is a nasal or spirant (in practice this means n or s respectively). It is precisely the existence of 'minimal pairs' such as orbis/urbs and vectis/nox that has perplexed Latin scholars trying to formulate the conditions for syncope; thus, for example, did orbis undergo syncope to orbs but later have -is 'restored', or

did syncope not occur at all after labials, in which case urbs must be considered an original consonant-stem noun? It is not necessary for our purposes to have an answer to these questions. What is important for us is the hint that table (614) offers about how in Latin the conflict with paradigm economy generated by syncope was resolved in the Plural of those third-declension nouns which belong to our classes A, B and C.

Is the distribution of -ium and -um predictable on the basis of stem phonology everywhere, and not just after the consonant clusters illustrated in (614)? If so, and provided that the Accusative Plural forms are explicable in some compatible fashion, we can regard -ium and -um not as signs of a distinction in declension-class membership but merely as distinct 'spellings' or realisations for the same Case within a single declension-type, the choice between them being determined phonologically. They would thus be handled in exactly the same way as the alternants -(a)sz and -ol for the 2nd Person Singular Present Indefinite Indicative in the 'normal' conjugation of Hungarian verbs. The significance of this for Latin's compatibility with the Paradigm Economy Hypothesis should be clear. The Slab Codicil approach, which we sketched above and which the A, B and C columns in table (601) seem at first sight to support, would yield two distinct declension-types in both Singular and Plural, in an arrangement which we can represent schematically thus:

(615)

	A	B	C
Singular	<input type="text"/>	<input type="text"/>	
Plural	<input type="text"/>		<input type="text"/>

On the other hand, the approach relying on phonological conditioning of spell-out rules would yield only one declension-type in the Plural, thus:

(616)

	A	B	C
Singular	<input type="text"/>	<input type="text"/>	
Plural	<input type="text"/>		

But from (616) it is immediately clear that the distinction between type B (that of dens and gens) and type C disappears, at least at the point at which the Paradigm Economy Hypothesis

applies. What we are left with, then, is only two declension-types -- the permitted maximum under the Paradigm Economy Hypothesis, given that there are no more than two rival realisations for any Case in types A, B and C:

(617)	A	BC
Singular		
Plural		

It is therefore of considerable interest to know the answer to the question I posed at the beginning of this paragraph about the phonological predictability of -ium and -um.

Janson (1971: 136) suggests that their distribution is indeed predictable. After examining Genitive Plural endings in a larger class of nouns than we are concerned with (including third-declension nouns outside our types A, B and C as well as adjectives and participles), he states: "I want to point out that it has been altogether possible to describe the distribution of the endings -ium and -um without reference to the nominative singular; and with the possible exception of some small groups of stems, the description would not have been simpler ... by referring to the nominative. It seems that mostly, there is no correspondence whatsoever between the rules for the two cases." In spite of this encouraging summing-up, the set of generalisations about -ium and -um that Janson presents on the preceding pages is complex and not without exceptions. But for our purposes we may ignore those third-declension nouns which fall outside classes A, B and C; and, when we do so, a good deal of the complexity disappears. In (618) I present a table supplementing (614) by including all remaining stem-final consonant and vowel combinations occurring in types A, B and C:

(618)	Genitive Plural:-	
Stem-final:	<u>-ium</u>	<u>-um</u>
1. V	(none)	bōs 'ox', sūs 'pig', grūs 'crane' %
2. -v- ([w])	ovis 'sheep', avis 'bird', nix (niv-) 'snow', unguis 'nail', anguis 'snake' %	(none)

3. $\check{V}C$	apis F 'bee' (sometimes) %	comes (comit-) lapis (lapid-) 'stone' dux (duc-) pēs (ped-) grex (greg-) 'flock' hiems apis (sometimes)
4. $\bar{V}t$	dōs (dōt-) F 'dowry' līs (līt-) F 'lawsuit' % cīvitās (cīvitāt-) F 'citizenship' (sometimes) etc.	nepōs (nepōt-) M 'grandson' sacerdōs (sacerdōt-) M 'priest' % virtūs (virtūt-) F 'virtue' cīvitās F (some- times) etc.
5. $\left[\begin{array}{l} + \text{consonant} \\ + \text{continuant} \end{array} \right]$	fīnis, collis 'hill', mensis M (some- pellis 'skin', axis times) % 'axle', ensis 'sword'; mensis M 'month' (sometimes)	
6. $\bar{V}C, \widehat{V}VC$	faucēs F Pl 'throat' palūs (palūd-) F 'marsh' (sometimes) fraus (fraud-) F 'deception' (some- times)	rēx (rēg-) laus (laud-) 'praise' custōs (custōd-) 'guard' mercēs (mercēd-) 'wages' palūs, fraus (some- times)

Notes: 1. For expository convenience, the order of these six stem-classes is disjunctive, and not random. Each stem-class excludes any stems which meet its defining phonological criteria (as stated in the left-hand column) but which also meet the criteria for an earlier stem-class. Thus, class 3 does not include e.g. ovis, already dealt with under class 2, and class 6 does not include e.g. līs, dealt with under class 4.

2. The symbol % is used to indicate that the examples listed exhaust or virtually exhaust the stems with the given ending assignable to the given class.

The point at issue, in considering both (614) and (618), is whether the phonologically determined stem-classes listed on the left have any genuine role in determining how the Genitive Plural should

be realised. From (618) it is evident that there is not perfect complementary distribution of -ium and -um with respect to all the phonologically defined stem-classes. In (614) we noted parens as a persistent occasional exception to the -ium rule for stems in nasal plus stop. In (618) we find further exceptionality, or the apparent absence of any clear generalisation, in classes 3-6. On the other hand, three points stand out about the type of exceptionality involved. Firstly, much of it takes the form of not constant violation of the 'rule' (or majority behaviour) for the relevant stem-class but rather vacillation between -um and -ium. Secondly, a distinction between Masculine and Feminine Gender, for the first time in our discussion so far, looks as if it may prove relevant. Thirdly, several of the classes involved are extremely small. Let us now examine these facts and their implications in more detail.

According to (618), apis, fraus and palūs are all attested as having the 'correct' Genitive Plural in -um alongside the 'incorrect' -ium (where 'correct' means 'displayed by the majority of nouns in the relevant phonologically-determined stem-class'). Does this tell us anything about the validity of the stem-classes? If the 'incorrect' forms alone were attested for these 'exceptions', this fact would certainly cast serious doubt on the whole approach. This is because there would seem to be no independent ground for choosing to call (say) palūs an 'exceptional' member of 'the same' declension-class as custōs rather than calling it a member of a distinct declension-class; and if our sole ground for choosing the former description is a desire to claim that declension-types A, B and C share a single paradigm in the Plural, then our argument is in danger of vicious circularity. But the fact that each of these nouns vacillates, with -um attested as well as -ium, tends to confirm the validity of the phonological approach. For, if this approach is correct, a noun such as palūs can preserve its Genitive Plural in -ium only at the cost of being lexically marked as an exception to the relevant spell-out rule for the property-combination Genitive Plural; and, inasmuch as exceptionality is unstable and more or less transient, we will predict

quite correctly that palūs will display a 'regular' Genitive Plural in -um also.

I commented earlier that Gender seemed to be relevant here. It is certainly not the case that all nouns displaying -um are Masculine or that all displaying -ium are Feminine. But any tendency to attribute a Gender-marking function as well as a Case- and Number-marking function to these affixes will have a direct bearing on the application of the Paradigm Economy Hypothesis to our familiar declension-types A, B and C. The reason is that it suggests the possibility of analysing the -um and -ium types as members of a single macroparadigm, under certain conditions.-- namely, where the contrast between -um and -ium correlates exactly with a Gender contrast. These conditions are in fact met in the small class of nouns under 4 in (618) (excluding the abstracts in -tās and -tūs), where Masculines such as nepōs and sacerdōs take -um and the Feminines dōs and līs take -ium. In view of this, it may well be significant that the 'incorrect' or 'exceptional' instances of -ium that we have so far noted all involve Feminine nouns (apis, fraus, palūs). If, as seems plausible, speakers will attribute to a form the status of a pure exception (with all the lexical burden that that imposes) only after exhausting the search for some regularity to legitimise it, then the maintenance of the 'exceptional' -ium forms for these three nouns may have been reinforced, or rendered less costly, by a weak tendency to treat -ium as a marker of Feminine Gender as well as Plural Number and Genitive Case. There may, in other words, have been a weak tendency to regard -um/-ium as a single 'macroinflection' associated with the Masculine-Feminine contrast not only within class 4 in (618) but also within classes 3, 5 and 6. If so, it is not hard to understand why this weak tendency, although adequate to preserve a stable contrast in the Genitive Plural between dōs and nepōs, never developed into a strong regularity: Masculines with -ium in the Genitive Plural were numerous and common, and the cost of restructuring them out of existence would have heavily outweighed the saving in terms of achieving regularity for a group of forms

(fraus etc.) both small in number and already subject to regularisation by other means (namely by acquiring the 'correct' ending -um).¹⁰

We have now, in effect, broached the topic of countervailing weak regularities. These are also relevant, I suggest, to the isolated mēnsis ((618) 5), the large and productive class of nouns in -tās represented by cīvitās in (618) 4, and the smaller but morphologically similar class of nouns in -tūs. All of these vacillate between -um and -ium. Mēnsis is isolated in the sense that it is one of only two nouns in types A, B or C with the stem-final combination -ns- (the other being ēnsis 'sword'). Historically it derives from a consonant-stem, not an i-stem form, so the Genitive Plural in -um is the more ancient. The variant mēnsium can thus be seen as an innovation to bring mēnsis into line with all other A, B and C-type nouns with stem-final consonant-clusters (as illustrated in (614) and at 5 in (618)); the fact that the innovation did not completely replace the now exceptional ending -um in this Masculine noun may perhaps be attributed to the weak tendency to interpret -ium as a Feminine marker. The same explanation will not, of course, do for the large class of Feminine nouns in -tās and the much smaller class in tūs (of which only one, virtūs, appears at all in the Genitive Plural). Whether these are original dental stems in *-tāt- or i-stems in *-tāti- which underwent syncope is uncertain; but if the former is correct, then the inherited Genitive Plural ending will be -um and the roughly equally frequent -ium can be seen as due to the removal of a lexically marked irregularity. That the removal was not complete can this time, perhaps, be attributed to the large size and productivity of the -tās group (which would have helped to make the irregularity easier to learn) and to the possibility of correlating the distribution of -ium and -um once again with a lexically-determined contrast (this time one between Abstracts and Non-abstracts), which would have brought -ium and -um together in a single macroinflection. Frequency of occurrence can perhaps be adduced to account too for the maintenance of an -um ending, alongside -ium, by parens alone among the nouns

within the scope of table (614)¹¹.

Summing up, the vacillations we have noticed in the Genitive Plural formation for particular nouns tend to confirm rather than disconfirm the view of the Plural forms of A, B and C-class nouns as constituting a single macroparadigm with some phonologically conditioned allomorphy rather than two or more distinct macroparadigms -- to confirm, in other words, the approach represented diagrammatically by (616) rather than that represented by (615). Condensing the material presented in (614) and (618), we can state the phonological and syntactic conditions for the allomorphy thus:

(619)	Phonological conditions; following stem-final:-	Realisation of Genitive Plural:
1. V	1. V	-um
	2. /w/	-ium
	3. $\check{V}C$	-um
	4. $\bar{V}t$: Masculines	-um
	Feminines	-ium
	5. $\left[\begin{array}{l} + \text{consonant} \\ + \text{continuant} \end{array} \right]$	-ium
	6. $\bar{V}C$	-um
	7. CC	-ium

- Notes: 1. The numbers of the classes correspond to those in (618) except that 7 covers nouns dealt with in (614).
2. As in (618), the order of the numbered classes is significant.

If this is correct, we can see the beginnings of an explanation for why the Ablative Singular ending $-\bar{i}$ and the Accusative Plural ending $-\bar{is}$ diverged so thoroughly in their distribution, as summarised at (609)-(611); for, on the strength of our argument so far, it seems as if syncope may have provoked a restructuring of the Plural of types A, B and C into a single pattern at a time when there were still two distinct declension-types in the Singular. But before we turn our attention to the Singular once more, it is necessary to look at the other Plural Case with com-

peting realisations: the Accusative.

6.5 Declensional merger in the Plural: the Accusative facts

Our argument so far has been that, although the breach of paradigm economy engendered by syncope was not repaired in the Plural by the same method as in the Singular, it was nevertheless repaired -- by the creation of a single Plural macro-paradigm with the distribution of the two endings -ium and -um accounted for mainly phonologically, as illustrated in (619) (barring the exceptions we have discussed). Can the distribution of the Accusative Plural endings -īs and -ēs be accounted for on a similar phonological basis? On the face of it, the answer is no: we have already quoted at (613) Varro's remark about how the Accusative Plurals of gens and mens differ despite their shared Genitive Plural ending, and have found confirmation of this in Lucretius's usage for B-type nouns. If we look beyond the B-class to the A-class (that is, to a class consisting almost entirely of old i-stem nouns, for all of which the ending -īs is historically 'correct') we see much the same picture: Ernout (1918: 162-163) records 64 instances of -īs against 20 of -ēs from nouns in Lucretius with a Nominative Singular in -is and which fall within our class A, even though all of these habitually display a Genitive Plural in -ium in accordance with the phonological conditions set out in (619). This looks like a serious blow for the single-Plural-paradigm¹² analysis. For if -ium acquired a distribution with a genuine synchronic motivation in terms of stem phonology rather than arbitrary declension-type, why did not -īs do likewise? Why, in fact, did -īs not acquire exactly the same distribution as -ium, thus being extended to old consonant-stem nouns of -CC shape like dens and being maintained in all inherited contexts consistently? If -īs had been distributed exactly like -ium, there would of course have been no problem so far as the single-paradigm analysis was concerned. But identical distribution of the two endings is not the only distributional pattern compatible with that analysis. The question, then, is whether the distribution we actually ob-

serve for -īs during the period with which we are concerned is so compatible. If the answer is yes, there does of course remain the question why -ium and -īs should have diverged as they did. But these two questions -- about compatibility and about causation respectively -- are distinct; and, in the context of our present concern with the Paradigm Economy Hypothesis, it is only the first to which the answer is crucial.

One logical possibility regarding -īs and -ēs is that their distribution is indeed phonologically determined, but on a different basis from that of -ium and -um. If this were so, our problem would again be solved. But this is not so; examples such as the contrast between the already-cited gentīs and mentēs indicate that the search for a phonological rationale will in this instance almost certainly be fruitless. What logical possibility remains which would still be compatible with the single-paradigm analysis? The last resort, seemingly, is to invoke the possibility of exceptions to spell-out rules, as we did in our account of the Genitive Plurals of fraus, palūs and the cīvitās group. Using this approach, we will say that one of the two endings (say, -ēs) is regular for all A, B and C-type nouns, and those that display -īs do so in virtue of some lexical feature which marks them as exceptional to the usual realisation rule for the Accusative Plural. This is equivalent to saying that within the class displaying the single Plural paradigm that we have hypothesised, there is a minority sub-class which receives a distinct realisation from the majority for this particular Case.

This approach looks at first sight quite unattractive. Surely (one might object), once one takes the step of allowing a single paradigm to contain distinct realisations for some property combination without any phonological or macroparadigmatic rationale, one opens the door to the lumping together within one 'paradigm' of quite distinct inflexional patterns solely in order to do away with potential counter-examples to the Paradigm Economy Hypothesis; or, to put it another way, one renders the

criteria for paradigmatic distinctness so uncertain as to reduce the Hypothesis to vacuity. This objection, if valid, is extremely serious. But it need not be valid, if we can find independent criteria for exceptionality. Defending my treatment of fraus etc. as exceptions to the general spell-out rule for the Genitive Plural, I adduced the fact that none of the nouns which break the supposed rules does so consistently (except, perhaps, the Plural-only faucēs 'throat'); rather, they vacillate between the 'exceptional' and the 'regular' endings. This suggests a general criterion, independent of paradigm economy, which must be met before one can legitimately postulate a single paradigm with exceptions instead of distinguishing two or more paradigms. This criterion will require that lexical items with the postulated exceptionality should (a) vacillate, with 'regular' behaviour as an option; or (b) diminish in number, as items defect one by one to the 'regular' type; or (c) cluster in such a way that the 'exceptional' realisation is interpretable as marking some further morphosyntactic or -semantic property beyond those which are generally relevant to the paradigm in question, so that the 'regular' and 'exceptional' inflexions can be regarded as constituting a single macroinflexion. All the 'exceptional' Genitive Plurals that we discussed earlier (except, perhaps, faucium) exemplify one or more of conditions (a)-(c). Thus, for example, parens and nouns of the civitas type exemplify (a); (b) is exemplified by dens, whose Varronian Genitive Plural ending -um, exceptional in terms of the set of rules at (619), is swiftly replaced by -ium; and the nouns in -vīt- exemplify (c) insofar as the distribution of -ium and -um among them acquires a Gender-marking function (Feminine versus Masculine) in addition to realising a specific Case-Number combination.

The question now is whether, in the distribution of -īs and -ēs for the Accusative Plural, one or other of the two realisations meets the criterion for exceptionality just enunciated. The answer is that -īs does so. Logically it would be quite possible for those nouns displaying -īs to constitute a stable and entirely distinct class, with no defections and no

vacillation. But this is not what we find in the Latin data. Rather, we find evidence of item-by-item defection, in that (for example) mens abandons -īs before gens does so; and evidence of widespread vacillation, in that (for example) among the eighteen A-class nouns which appear in Lucretius with an Accusative Plural in -īs, ten also appear with -ēs, while five of the sixteen individual B-class nouns in Lucretius also vacillate (Ernout 1918: 160 (for dentēs) and 162-164). Some interesting evidence for condition (c) is provided by inscriptions. Some inscriptions maintain carefully the ending -īs (spelt IS or EIS) for A-type nouns; for example, the milestone of Popilius (Ernout 1957 no. 131 = CIL i² 638), datable to 132 BC and thus a generation earlier than Varro. But in the long Monumentum Ancyranum of 12 AD, we find signs of a division of function between -īs and -ēs on part-of-speech lines: nominal Accusatives AEDES, FINES and GENTES (cf. Varro's and Lucretius's gentīs!), all from A-type or B-type nouns, contrast with adjectival or participial [CV]RVLÍ¹³, [PLV]RÍ¹³, AGENTÍ¹³, INFERENTÍ¹³, with only one participial Accusative LABENTES going against the trend.

On the basis of this evidence, despite the contrast between the Accusative and the Genitive in how their realisations are distributed, there appears to be justification for analysing the Plurals of our A, B and C classes as all belonging to a single paradigm and so as compatible with the Paradigm Economy Hypothesis. We have thus answered the first of the two questions posed at the beginning of this section -- the question about compatibility. On the second question, about the reason why -īs did not acquire a phonologically-motivated distribution exactly parallel with that of -ium, I shall have less to say. This is no weakness, in the present context. Our concern here is to find out whether breaches in paradigm economy (brought about by, for example, phonological change) are in fact repaired, as the Paradigm Economy Hypothesis claims they must be, or not; but, since the Hypothesis does not predict which method of repair will be chosen when more than one is available, defence of the Hypothesis does not require an explanation for such choices. That said, I sus-

pect that some explanation for the divergence between -īs and -ium can ultimately be found, and that it will have to do not with paradigm economy but with syncretism. In declension-type C there is no distinction between the Nominative and Accusative Plural. In this respect type C resembles all Neuters of whatever declension, the remainder of the third declension (including all Masculines and Feminines with Nominative Singular in neither -is nor -s), and both the fourth and fifth declensions. Types A and B, on the other hand, insofar as they permit a distinction between these two Cases, resemble the first and second declensions, which are the largest and most productive in Latin but which differ considerably from the third declension. For the -īs ending to acquire a stable distribution in parallel with -ium would have meant reinforcing and extending in the third declension a characteristic, namely the absence of homonymy between the Nominative and Accusative in the Plural, which was unusual outside the two dominant declension-types. If we regard the most productive declension-types as the least 'marked' (in a more or less Jakobsonian sense) and therefore perhaps the ones likely to show the greatest number of overt distinctions in inflexional realisation, we have the beginnings of an explanation for why in Latin the -īs/-ēs alternation was not stabilised on a phonological basis, as the -ium/-um alternation in the Genitive Plural was.

Of course, one way of having the best of both worlds -- of promoting syncretism between Nominative and Accusative Plural in types A and B while permitting the stable maintenance of -īs in the Accusative -- would have been to extend the Accusative ending -īs to the Nominative. In fact, there are instances of Nominative Plurals in -īs reliably attested in manuscripts of one early author (Plautus) and in early inscriptions (NW 381). It is perhaps not surprising that any such trend failed to establish itself generally, since it would have involved the replacement of a frequent and well-established Nominative Plural ending -ēs¹⁴. But the fact that -īs Nominatives do occur at all may count as further weak evidence in favour of that trend towards syncretism which we have postulated in order to help explain the failure of

-īs to maintain itself as a regular ending in the way that -ium did.

6.6 Declensional merger in the Singular

At the end of section 6.4 I remarked that, if we could justify the claim that declension-types A, B and C shared a single paradigm in the Plural, we could perhaps find some explanation for the divergence in distribution between the Ablative Singular ending -ī and the Accusative Plural -īs, as summarised in (609)-(611). It is now time to expand on that remark. I have argued that the three superficially distinct declension-types A, B and C are reducible to two paradigms in the Singular and one in the Plural, as illustrated in (617). For the type BC, we know that the regular Ablative Singular ending was -e, and that the irregular one -ī (exemplified in Lucretius only by a few instances of partī alongside the more frequent parte from pars) quickly became obsolete. For type A, on the other hand, it is not immediately obvious which of the two endings, if either, should be counted as the regular one at the turn of the era. Varro (NW 378) regarded both avī, ovī and ave, ove as correct (from avis 'bird' and ovis 'sheep'), and Sommer (1914: 376) claims that the original restriction of -e to consonant stems is "seit Beginn der Überlieferung verschoben". Yet the few instances of A-type Ablative Singulars in Plautus and Terence (on the basis of data from Lodge (1962) and McGlynn (1963)) nearly all show the historically 'correct' ending -ī, as do those in Lucretius (Ernout 1918: 148). Moreover, most of the few exceptions in these three authors are accounted for by stems in -st- (hoste, peste, teste 'witness', veste). These facts combined suggest that, until about the first century BC, the distribution of -ī and -e within the A class was phonologically conditioned, with -e as the regular spelling after -st- and -ī regular elsewhere. If so, we have a clear reaspm for the existence of a divergence between the distributions of -ī and -īs in normal usage up to Lucretius's time: namely, that -ī was analysed by native speakers as a regular Ablative ending for A-type nouns in contrast to BC-type ones, while -īs was by that time analysed as an exceptional or irregular

ending everywhere. Moreover, insofar as these analyses are predictable, and not merely accidental, on the assumption of a need to re-establish paradigm economy after it had been disrupted by syncope, the detailed distributional facts we have noted are not merely compatible with the Paradigm Economy Hypothesis but positively support it.

The account just offered may, however, seem to solve one problem only at the expense of creating another. If the divergence of -ī and -īs is due to the fact that, at the time of Lucretius, -ī was the regular ending in the Ablative Singular of A-type nouns, what are we to say about the distribution of -ī and -e shortly after Lucretius's time, when -ī is seemingly in rapid retreat? Specifically, if a Lucretian Ablative such as fīne alongside fīnī from fīnis 'end' is exceptional, how can we explain the fact that, far from being obsolescent, it gains ground subsequently at the expense of fīnī? Certainly, if the two-paradigm pattern for the Singular of A, B and C-type nouns were stable, as illustrated schematically in (616) and (617), we would expect to observe over time less, not more, vacillation between -ī and -e in class A -- to observe, in fact, the rapid development of a consistent preference for -ī in class A (with perhaps -e as a consistently preferred variant in some phonological contexts) exactly in parallel with the rapid extension of -e at the expense of -ī in class B.

Admittedly, this polarisation did not occur. But the reasons are, I suggest, independent of paradigm economy and do not vitiate my account of the -ī/-īs divergence. The important point is that, although the post-Lucretian developments in the Singular of the A class differ from what we might expect, they differ in the direction of more paradigm economy than the Paradigm Economy Hypothesis requires, not less. By this I mean that the post-Lucretian development of the A class suggests that the two-paradigm state of affairs illustrated in (617) was not maintained; rather, declension-type A and the already unified declension-type BC underwent a further merger. The symptoms which support this dia-

gnosis are clear. Firstly, there is vacillation between -ī and -e in those nouns which maintain -ī in the Golden Age. Secondly, the class of nouns which manifest -ī more often than -e within type A is more or less limited to a small group consisting of amnis 'stream', orbis 'circle', ignis 'fire' and vītis 'vine'; the majority have either defected completely to -e (like auris, vallis and crinis, mentioned in section 6.3 in connexion with fact (609)) or show a preference for -e. Thirdly, a very strong tendency towards a functional distinction between -ī and -e develops in that -ī becomes specialised as an adjectival ending, displacing -e from the Ablative Singular of the great majority of old consonant-stem adjectives in the third declension. The Monumentum Ancyranum illustrates these last two points neatly in that it displays only -e in the Ablative Singular of A-type nouns (AEDE, ORBE) but only -ī in that Case in third-declension adjectives (CONSVIARI, GRANDI, NARBONENSI, PENETRALI). (Ernout 1918: 147). All these facts point to a post-*Lucretian* analysis of types A, B and C as belonging to a common paradigm in which -ī survives only as a lexically marked exception.

A further declensional merger of this kind must, however, have consequences for the inflexional realisations of other Cases in the paradigm. Notably, it must affect the second of the two Singular Cases whose realisations in types A and BC differ, namely the Nominative. Whether my claim about declensional merger will stand must depend heavily on whether I can give a plausible and appropriate account of the distribution of -is and -s in the Nominative before, during and after the postulated merger. This involves reconsideration of tables (614) and (618).

We have already remarked that the persistent 'minimal pairs' such as vectis versus nox and orbis versus urbs in the Nominative Singular cloud the environment for syncope as a historical innovation, but that for the Genitive Plural no such minimal pairs exist, in that, apart from the isolated parentum and Varro's dentum, the distribution of -ium and -um can be accounted for purely phonologically among the nouns of table (614).

Yet, outside the small class of nouns with stems in -ct-, -pt- and -rb- or -rp-, the distribution of -is and -s is just as predictable phonologically as that of -ium and -um. This is partially illustrated by table (618). The nouns given as examples for each stem shape are quoted in the Nominative Singular (except for the lexically Plural faucēs). Examining these forms, we find that each stem shape is associated exclusively or almost exclusively with only one of the two Nominative Singular endings. This is illustrated in (620), in which the stem classes are identified by the same numbers as in (618) but ordered differently:

(620) Phonological conditions: following stem-final:-	Realisation of Nom Sg:
1. V	-s
2. /w/	-is
4. $\bar{V}t$	-s, -is
5. [+ consonant [+ continuant]	-is
3. $\check{V}C$	-s
6. $\bar{V}C$	-s
7. CC	-s, -is (see (614))

The reason for placing 3 after 5 in (620) is to account for canis 'dog' and iuvenis 'young man'. The Genitive Plurals of these two nouns are consistently canum and iuvenum (not "canium" and "iuvenium"). They thus fall outside any of types A, B and C in (601). It is not necessary, however, to assign them to yet another mixed declension-type, because their choice of Nominative Singular and Genitive Plural endings is predictable phonologically. They differ from the majority of -VC stems only in having -is instead of -s in the Nominative Singular, like all ABC nouns with a stem ending in a nasal or a liquid¹⁵.

Exceptions remain, of course. We have already mentioned those -CC- stems (illustrated in (614)) whose tolerance of both -is and -s renders the phonological predictability of the Nominative Singular endings less complete than that of the Genitive Plural ones. These constitute class 7 in (620). Class 4

in (620) must also apparently be split, because of the existence of vītis 'vine' alongside līs, dōs and cīvitās etc. The single-paradigm analysis that we are putting forward for ABC nouns after Lucretius requires us to claim that one of the two Nominative Singular endings within each phonologically defined stem-class is regular and the other exceptional, which in turn requires us to look for independent evidence of exceptionality in the shape of defection or vacillation; alternatively, it requires us to claim that the two endings belong to a single macroinflexion. In class 4 the stability of vītis is an undeniable difficulty (although it may be significant, as Janson points out, that the 'expected' Nominative Singular "vīs" would be homonymous with vīs 'force'). In class 7, on the other hand, the fact that later grammarians found it necessary to condemn forms such as orbs, corbs as errors for orbis, corbis (NW 281, 284) suggests strongly that the -s ending did indeed come to be analysed by speakers as the regular one for all liquid-plus-stop stems among ABC nouns, as it undoubtedly was for those which had undergone syncope. The remaining exceptions observable in (618) are few and isolated: apis (which 'misbehaves' in the Genitive Plural too) and nix, which is historically quite regular as a reflex of a stem in ^{*}-ghw- but which from the synchronic point of view in Latin is perhaps best regarded as a heteroclite oddity like carō 'flesh' (non-Nom-Sg stem carn-) and sanguis 'blood' (non-Nom-Sg stem sanguin-), and hence outside our types A, B and C.

The mention of carō and sanguis recalls a salient characteristic of Masculine and Feminine nouns in the rest of the third declension, outside the types A, B and C which we have been concentrating on, namely the inflexional diversity of their Nominative Singular forms. Moreover, it is these inflexionally diverse Nominative Singulars alone which distinguish the rest of the third declension from types A, B and C; that is, in all other Cases (broadly speaking) these nouns 'go like' rēx. Yet our definition of 'paradigm' does not allow for the recognition of degrees of similarity between paradigms; two paradigms which are alike in all Cases but one are just as much distinct as two which

are alike in no Cases at all, and their similarity appears purely accidental from the point of view of the definitions and claims advanced in this thesis so far. This is an excusable drawback in the present state of knowledge, given that it is generalisations limiting intra-linguistic diversity in inflexional behaviour that we have been seeking, not generalisations about inflexional similarity. Nevertheless, if we can find some way within our present framework of relating the ABC declension-types to the non-ABC types which they so closely resemble, we will have gained something.

I suggest that there is in fact a way of relating these types, on the basis of ideas concerning stem allomorphy which will be developed in the next chapter. To anticipate: I will argue, on the basis of evidence from Sanskrit, Italian and German, that stem allomorphy and affixation are not on a par when it comes to deciding whether two inflexional patterns are distinct or not; two patterns which are superficially distinct may after all count as examples of 'the same paradigm' for paradigm economy purposes if their differences are limited to stems rather than affixes. If this is correct, then it may well be significant that the inflexional diversity in the Nominative Singular just mentioned typically involves a difference between the Nom Sg stem and the stem found in all the other Case-Number combinations. What I am suggesting is that distinct realisations of the Nominative Singular in third-declension Masculine and Feminine nouns outside types A, B and C may after all be legitimately regarded as the same for the purposes of paradigm economy. I have not space within this thesis to develop and test this suggestion adequately, since it obviously depends on the possibility of accounting satisfactorily for the distribution of other Case endings -- particularly the -um and -ium of the Genitive Plural -- throughout the third declension, and its implications for languages other than Latin also need to be explored. But, if this suggestion is on the right lines, two consequences follow. Firstly, and fairly obviously, the inflexional identity of third-declension nouns outside the Nom Sg ceases to appear a mere accident,

since they can all legitimately be regarded as members of the same paradigm. But there is a second consequence which is of more immediate relevance to the problem of the distribution of -is and -s in the Nominative Singular of types A, B and C.

If the Latin speaker was used to a Nom Sg inflexion which was irrelevant to paradigm economy in those Masculine and Feminine nouns of the third declension whose Nominative Singular does not end in -s, it is understandable that he should be relatively sanguine about apparent breaches of paradigm economy of the kind involving 'minimal pairs' such as līs/vītis and urbs/orbis. This is equivalent (or nearly so) to saying that, for Latin speakers, ABC-type Nom Sgs such as urbs, ignis or rex did not really consist of a stem /urb-/, /ign-/ or /re:g-/ plus an inflexional ending /is/ or /-s/ in the way that (say) the corresponding Dative Singulars urbī, ignī and regī consisted of that stem plus the inflexional ending /-i:/; rather, on the strength of the many non-ABC Nom Sgs such as ordō 'order' (Dat Sg ordin-ī) consul 'consul' (Dat Sg consul-ī), ratio 'reason' (Dat Sg ratiōn-ī), pater 'father' (Dat Sg patr-ī), as well as less typical examples such as sanguis 'blood' and carō 'flesh' already mentioned, ABC-type Nom Sgs such as urbs, ignis and rex could readily be treated as unsegmentable wholes, with the consequence that the need for an 'economical' distribution of rival Nom Sg affixes -s and -is would not arise at all. If so, we need not worry too much about the fact that our attempt to account purely phonologically for the distribution of these two affixes in types A, B and C, although certainly not a complete failure (as (620) shows), has been less successful than our earlier attempt in respect of the Genitive Plural affixes -ium and -um.

All in all, our attempt to analyse types A and BC as belonging to a single paradigm -- an attempt rendered necessary by the post-Lucretian encroachment of -e on -ī in the Ablative Singular of type A nouns -- has been reasonably successful. We have said, in effect, that the few phonological environments in which -is and -s occurred with anything like equal frequency

as Nominative Singular endings were, for one reason or another, insufficient to block the drive to paradigmatic merger which is predicted by the Paradigm Economy Hypothesis once the encroachment of -e had begun. What the Hypothesis does not explain, however, is why the encroachment should have begun in the first place. But reconciling the Hypothesis with the facts of Latin does not, of course, require us to erect the Hypothesis as the sole instigator of inflexional change. We are confronted here with a problem analogous to that discussed earlier of the failure of -is to establish itself as an Accusative Plural ending in parallel with the Genitive Plural -ium. In my tentative solution to that problem, I invoked the tendency to syncretise the Nominative and Accusative Plural in Latin outside the first and second declensions. My tentative answer to the problem of the Ablative Singular also involves syncretism, but this time an opposite Latin tendency: a preference for the Ablative and Dative Cases to be distinct in the Singular of Masculine and Feminine nouns (as opposed to Neuters and adjectives), except in the second declension. A tendency of this kind may well have been reinforced by the process already mentioned whereby the -i/-e contrast in the Ablative Singular was 'functionalised' as an exponent of the adjective-noun contrast. One half of this process was already substantially complete by the time of Lucretius, namely the displacement of -e by -i in adjectives (Ernout 1918: 149-152) and, as we have noted, the displacement of -i by -e in nouns, though scarcely begun in Lucretius, is complete in the Monumentum Ancyranum about a century later.

6.7

Summary of chronology

I have traced the development of a portion of the Latin third declension from a pre-syncope state compatible with the Paradigm Economy Hypothesis, through a superficially confused period during which paradigm economy was breached, towards a Golden Age state in which (I have argued) paradigm economy has been restored through the creation of a single paradigm, both Singular and Plural, for ABC nouns but in which relics of the earlier confusion are still clearly visible in the shape of ob-

solescent 'exceptional' Ablatives in \bar{i} and Accusatives in \bar{is} . It may be helpful to recapitulate the various stages which are discernible; this will help us to identify those points in the process at which specific predictions flowing from the Paradigm Economy Hypothesis can be tested. The stages are summarised and the Hypothesis's role identified at Appendix C. Of course, to analyse the developments in terms of five or six discrete stages is to oversimplify, and I have further simplified the picture by omitting the brief appearance (already mentioned) of an occasional Nominative Plural in \bar{is} , and the occasional instances in early Latin of an Ablative Singular in \bar{i} or \bar{id} in G-type or other consonant-stem nouns¹⁶. But the evidence on timing summarised in the last column of Appendix C is generally quite compatible with my analysis in terms of the gradual interaction over about three centuries between paradigm economy on the one hand and three independent innovations or constraints on the other (1, 4 and 7 in the table at Appendix C). The only apparent awkwardness is the fact that Varro, so conservative on the Genitive Plural of dens, is so 'forward-looking' in his attitude to new Ablative Singulars such as ave and ove. But the spread of these new Ablative forms (point 7 in Appendix C) is a cause rather than an effect of pressures to restore paradigm economy, and there is no difficulty in supposing that in this respect Varro's dialect happened to be less conservative than that of Lucretius.

One claim which emerges clearly from Appendix C is the claim that the encroachment of \bar{e} on \bar{i} in the Ablative Singular had two sources, the first triggered by syncope (at 3 in the table) and the second independent of it (at 7 in the table). This claim may seem hard to justify. It does, however, accord well with the clear facts about the divergent distributions of \bar{i} and \bar{is} summarised in (609)-(611). On our analysis, the failure of 'analogy' to ensure parallel distributions is expected. All instances of \bar{is} come to be analysed as exceptional (6 in Appendix C) at a time when nearly all remaining instances of \bar{i} are still analysed as regular, at least by many speakers.

Later, when -ī, too, becomes exceptional throughout the ABC paradigm (7 and 9 in Appendix C), parallel development of -ī and -is is inhibited, I suggest, by the fact that the former has a considerably smaller lexical range (having lost most of its -CC- stems earlier, at stage 3) and that, for reasons to do with syncretistic tendencies rather than with the Paradigm Economy Hypothesis, the adjective-versus-noun functionalisation of the -ī/-e contrast has been much more successful than that of the -īs/-es contrast. Moreover, Lucretius confirms the existence of two distinct triggers for -e-encroachment in that he illustrates what one might call a lull between two bursts of innovation: for him, the replacement of -ī by -e in syncopated A-type nouns (now in paradigm BC) is complete, and the associated encroachment of -ēs upon -īs in the now unified Plural paradigm is well under way (3 and 6), but the further encroachment of -e on -ī in the Singular of A-type nouns (7) is still at an early stage. All these fine details of distribution and chronology can be accommodated neatly in an account which distinguishes sharply between those changes which flow from a need to restore paradigm economy from those which do not. The closeness of fit that we have been able to achieve also illustrates the value of testing claims about the mechanics of linguistic change by reference to periods of linguistic history for which good documentation is available.

A final point to emerge from Appendix C relates to the 'developments and constraints not flowing from the Paradigm Economy Hypothesis' listed in column III. The first of these, syncope, is what sets the machinery in motion. The other two, I have tentatively suggested, have something to do with encouraging or discouraging syncretistic tendencies which are favoured or disfavoured in Latin. It is intriguing, therefore, that both the failure in 4 to implement a prediction flowing from the Paradigm Economy Hypothesis in what at first sight seems the most straightforward way and the introduction in 7 of a new inflexional trend that the Hypothesis does not require can be seen as efforts to remove, or prevent the spread of, what for Risch (1977) are

the only two anomalies within his circular arrangement of Latin declensions by mutual resemblance. Indirectly, this fact provides further support for our paradigm-economy-based account of the historical developments and thus for the Paradigm Economy Hypothesis itself. As I have said, reconciling the Paradigm Economy Hypothesis with the Latin developments does not require us to make paradigm economy responsible for every Latin innovation. We would therefore be entitled to be reasonably content with the account summarised in Appendix C even if 4 and 7 had simply to be regarded as unexplained, arbitrary facts which just happen to complicate the achievement of paradigm economy. But in that case our account would be to a certain extent open to criticism on the ground that, although our predictions listed in column I are all implemented, we have offered no real explanation for why they are not all implemented in what seems the most straightforward way, and the disturbing influences invoked in column III have an ad hoc air. But, assuming that there is something real underlying Risch's arrangement of the Latin declensions, a possible explanation for these disturbing influences comes into view. If so, we can claim all the more strongly to have gone beyond merely reconciling the existence of declension-type B with the Paradigm Economy Hypothesis; for the only factors in addition to paradigm economy which are needed to help account in quite precise detail for the various changes in declension-types A, B and C from the earliest Latin to the first century AD are factors whose existence is independently confirmed, even if their details are still obscure.

6.8 Remaining problems: the *puppis* and *nūbēs* types

Despite the length and complexity of my argument, I have not dealt with all the *prima facie* instances of paradigm mixture which a complete account of paradigm economy in Latin nouns would need to cover. There existed in Latin at the period with which we are concerned three further, smaller, declension-types which also contribute to paradigm mixture within and beyond the third declension, as illustrated in (621):

(621)	D	E	F ('fifth declension')
Sg Nom/Voc	puppis 'stern'	nūbēs 'cloud'	faciēs 'face'
Acc	puppim	nūbem	faciem
Gen	puppis	nūbis	faciēī
Dat	puppī	nūbī	faciēī
Abl	puppī	nūbe	faciē
Pl Nom/Voc	puppēs	nūbēs	faciēs
Acc	puppēs or -īs	nūbēs	faciēs
Gen	puppium	nūbium	faciērum
Dat/Abl	puppibus	nūbibus	faciēbus

Type D differs from type A in (601) in having -im for -em in the Accusative Singular and in preferring more consistently the -i- endings in the Ablative Singular and the Accusative Plural; although type D is not itself mixed, it presents a problem in that type A is a mixture of it and type B. Type E, traditionally assigned to the third declension, is mixed in that its Nominative Singular ending is the same as that of the fifth declension (type F) while its endings for the other Cases are the same as for type C (broadly speaking).

As I have said, all these declension-classes are smaller than any of classes A, B and C -- indeed, much smaller. For this reason, one might argue that it is not necessary to worry too much about the breaches of paradigm economy which they engender. One might argue, perhaps, that it is only when large, thriving paradigms seem to exhibit 'illegal' paradigm mixture that the Paradigm Economy Hypothesis is seriously under threat; it is to be expected, perhaps, that unproductive types which have relatively few members and which are less frequently encountered in everyday language use should slip through the net, as it were. This is not necessarily a mistaken attitude to adopt towards small-scale exceptions to a proposed general constraint. But it is an attitude we should adopt only when we have investigated these apparent exceptions carefully and even then failed to make them fit. We should investigate even small-scale exceptions carefully, partly because a properly rigorous approach

requires us to take seriously any apparent counterevidence to our proposals, but also because there is always a chance that (as with types A, B and C) an apparent counter-example may turn out on closer examination to confirm our proposals after all, in unexpected ways. I will suggest that this is precisely what we find when we examine types D and E closely.

I will start with the puppis type (D in (621)). As I have already said, this type is not itself mixed, since one of its inflexions, namely the Acc Sg -im, is peculiar to it. But it contributes to a prima facie breach of paradigm economy in the third declension in that it renders the paradigm of ignis (type A) mixed; this is because ignis resembles in each Case either puppis on the one hand or rēx and dens (types C and B) on the other.

The group of nouns displaying -im in the Accusative Singular is quite small, and several are exceedingly rare. The complete list is as follows:

(622) a. No alternative Acc Sg in -em attested:

i. Agricultural terms:

buris F 'plough-beam'

rumis F 'teat, dug'

cucumis F 'cucumber' (alternative
Acc Sg, with stem change;
cucumerem)

ii. Terms relating to the body or
bodily states:

rāvis F 'hoarseness'

sitis F 'thirst'

tussis F 'cough'

iii. Rivers:

Tiberis M 'Tiber'

iv. Nautical terms: none

v. Others:

futis F 'pitcher'

(622) (continued)

b. Alternative Acc Sg in -em attested:

i. Agricultural terms:

crātis F 'hurdle'

sēmentis F 'sowing'

messis F 'harvest'

ii. Terms relating to the body or
bodily states:

febris F 'fever'

cutis F 'skin'

iii. Rivers:

Albis M 'Elbe'

Lāris M 'Garigliano'

etc.

iv. Nautical terms:

nāvis F 'ship'

puppis F 'stern'

restis F 'rope, hawser'

v. Others:

clāvis F 'key'

neptis F 'grand-daughter'

pelvis F 'basin'

secūris F 'axe'

strigilis F 'scraper'

turris F 'tower'

This is clearly not a simple list; the nouns in question are arranged in such a way as to bring out certain features. First, all except the river names (622 a.iii and b.iii) are Feminine. Secondly, most of the nouns fall into one of four reasonably clear semantic groupings: rivers, agricultural and nautical terms, and terms to do with the body or bodily states. Thirdly, most of the nouns do not have -im exclusively in the Acc Sg, but at least sometimes appear with -em. I will now try to show how these three features relate to paradigm economy in such a way that the puppis paradigm, like the dens paradigm, emerges as confirming rather than disconfirming evidence for the Paradigm Economy Hypothesis.

I will discuss first the Gender distribution in (622). The Paradigm Economy Hypothesis, in its latest formulation, relates to 'macroparadigms' -- that is, to sets of paradigms (possibly single-member sets) within which all inflexional differences can be correlated with lexically determined contrasts such as contrasts of Gender. Now, in each of the three declension-types A, B and C, both Masculine and Feminine nouns could be found, so that no Gender-linked 'macroinflexions' could be identified except, marginally, in the Genitive Plural, where, for a certain phonologically defined sub-class of types A, B and C taken as a whole, -um was found to be characteristic of Masculines and -ium of Feminines. More specifically, among stems in $-\bar{V}t-$, Masculines (such as nepōs, sacerdōs) selected -um while Feminines (such as līs, dōs) selected -ium, including frequently the abstract nouns in -itās such as cīvitās, torn between what was probably the historically 'correct' Genitive Plural in -um and the synchronically regular one (according to my analysis) in -ium. Here, then, was an instance, unusual among A, B and C-type nouns, where an inflexion could be said to realise Gender as well as Case and Number, and where therefore paradigms which were superficially 'rivals' from the point of view of the Paradigm Economy Hypothesis turned out not to be so, since the inflexions in which they differed could be associated with a lexically determined morpho-syntactic property contrast. The reason why all this is relevant to the puppis paradigm is that here, too, we have an inflexion with a particular association with one Gender; the Accusative Singular -im is found exclusively with Feminine nouns, except for the river names Tiberis etc. which are all Masculine. The Accusative Singular ending in the puppis declension-type therefore appears to have a Gender-marking function which it does not share with the -em ending of types A, B and C.

It may seem that this should be enough by itself to reconcile the existence of the puppis declension-type with the Paradigm Economy Hypothesis. If the Gender-marking function of -ium and -um in the Gen Pl was enough by itself to deal with the

declensional contrast between dōs and nepōs, why need I say more about the contrast between puppis and ignis? Indeed, the fact that I do propose to say more may seem suspicious; for if the Gender argument is not enough here, how can it have been enough, supposedly, to account for the -ium/-um facts? But the two cases are not exactly parallel. I argued that, under certain phonologically defined conditions, the endings -um and -ium correlated with the Gender contrast exactly; for all Masculines the regular ending under those conditions was -um and for all Feminines -ium. But with -im and -em the correlation is not so exact. It is true that all -im nouns (except the river names) are Feminine; but it is not true that, among nouns which resemble puppis (type D) or or ignis (type A) all Feminines take -im, since many -- indeed the great majority -- of the Feminine nouns with a Nom Sg in -is take -em and only -em in the Accusative, and so behave in this respect just like the Masculines in -is (except, once again, the river names). What I suggest, then, is that the macroinflexional 'functionalisation' of the contrast between -im and -em in respect of Gender was only partial, unlike that of the -ium/-um contrast among -v̄t- stems; consequently, although it could mitigate the damage to paradigm economy caused by the separate existence of the ignis and puppis types, it could not repair this damage entirely. In terms of macroparadigms, this amounts to saying that, although part of the criteria for recognising a macroparadigm are met in that the ending -im is consistently associated with the Feminine Gender, not all the criteria are met since the Feminine Gender is not consistently associated with -im; the job is only half done, as it were.

If this account is broadly correct, we will predict that, despite the facts about Gender distribution, one or other of the two declension-types A and D will be seen as 'irregular' synchronically, and nouns belonging to it will tend to defect to the other. This prediction is correct; the third of the three features of the puppis type mentioned in connexion with (622) above is that only a minority of the nouns which belong to it are faith-

ful to -im as their sole realisation of Acc Sg, namely seven of the eight listed in (622a) (the eighth, cucumis, having an alternative Acc Sg ending other than -em). Moreover, of the seven, one (futis) is attested only once, in a remark by Varro linking it etymologically with the verb fundō 'pour' (De lingua latina 5, 25, 119); and one (rumis) is also exceedingly rare, being mentioned only by Varro (this time in his capacity as an agricultural expert, in the De re rustica) and by the elder Pliny, and is each time referred to explicitly as an old-fashioned or obsolete word. Of the remaining five, only two, sitis and tus-sis, are common enough to justify us in attributing any significance to the lack of any attested -em Accusative. In contrast, of the fourteen Feminines in (622b) for which -em is an occasional variant or the usual Accusative ending, several are common in Latin texts of all periods (nāvis, puppis, turris, febris) and none is excessively rare.

The second of the three features of type D noted above was the clustering of most of its members into four semantic groups. The significance of this, I suggest, is similar to that of the Gender specialisation mentioned earlier. The fact that the puppis declension-type is to some extent specialised semantically mitigates the breach of paradigm economy to which it contributes, in that the semantic properties associated with it remove it from direct rivalry with the ignis declension-type for the purposes of the Paradigm Economy Hypothesis; once again, the criteria for recognising a macroparadigm are partially fulfilled. It is true that this semantic specialisation differs from the Gender specialisation in one important respect: the properties involved, unlike the properties Masculine and Feminine, have no independent syntactic role at all. It is not appropriate, therefore, to call them 'morphosyntactic' properties. But we have already come across instances where lexically determined properties whose contrast permits the recognition of a macroparadigm cannot be called syntactic simply because they have no syntactic relevance in the language in question. For properties of this kind we coined the term 'morphosemantic' in Chap-

ter V, section 5.2. The first application of it was to the properties Animate and Inanimate which, according to Wurzel (1970), correlate with the distinction between German Masculine nouns of type VIII (e.g. Bär 'bear') and type IX (e.g. Dorn 'thorn'). Although I tentatively rejected a macroparadigm analysis of types VIII and IX in favour of one involving the notion 'slab', the analysis of the Russian declension-types of дом 'house', professor etc. offered in section 5.4 did crucially involve a 'morphosemantic' category, since the inflexional differences in the Accusative were found to correlate with an otherwise syntactically irrelevant¹⁷ distinction between Animate and Inanimate nouns. So in attributing a morphological relevance to the semantic groupings in (622) (agricultural, nautical and bodily), I am not postulating something quite unparalleled in other languages.

Another example of a morphologically relevant but syntactically irrelevant semantic distinction crops up in a rather unexpected place. It is commonplace that in many languages, although sex has no direct morphological or syntactic relevance, it is the main determinant of Gender for nouns denoting human beings and animals. A less widely noticed correlation involves English nouns with zero Plural marking; nouns such as deer, grouse, snipe, cod which are the same in the Plural as in the Singular nearly all denote creatures which are hunted or caught and eaten by humans (the only exception I am aware of being sheep, which of course is eaten but not hunted)¹⁸. The synchronic relevance of a morphosemantic classification of this kind is, of course, strongly confirmed if it is found to be productive; and, for me at least, the zero-Plural formation is indeed productive. The normal Plural of fox is foxes; but if famine or or a revolution in eating habits turned foxes into a source of food in Britain, then sentences such as 'John's been out looking for fox all day' or 'Mary brought home two fox yesterday' would seem quite natural.

For a final example of a similar correlation, I will re-

vert to Latin. Second-declension nouns in -us are overwhelmingly Masculine; but those few which are Feminine include all the second-declension names of trees (e.g. quercus 'oak', fāgus 'beech', fraxinus 'ash', pōpulus 'poplar', prūnus 'plum'), and, conversely, most Latin tree-names are second-declension Feminines (most if not all the few exceptions being also Feminines in -us, such as fīcus 'fig', pīnus 'pine' and laurus 'laurel', which vacillate between the second and fourth declensions). The property 'Treehood' has no relevance whatever in Latin syntax, as one might expect; but it is plainly relevant in morphology. To dismiss the correlation between Treehood and declension-type just described as a survival with no synchronic relevance to the native speaker of classical Latin merely pushes back in time the point at which a lively and productive morphosemantic correlation existed. In any case, there is evidence that the correlation was indeed productive in Latin, in that, whereas those few Feminine nouns of the second declension which are not tree names (e.g. alvus 'bowels', colus 'distaff') show a definite tendency to switch to the morphologically more appropriate Masculine Gender, the tree names show remarkably little tendency to do so (Ernout 1953: 25; Sommer 1948: 333).

To sum up; there is indeed 'illegal' paradigm mixture in the mutual resemblances of types D (puppis), A (ignis) and C (rēx). But, equally, there is evidence within Latin of partial syntactic and semantic correlations which mitigate the breach of paradigm economy here and evidence of a change which tends to remove it. This change is the gradual defection to type A of that majority of type D nouns which also display an Accusative Singular in -em; and the partial correlations are those involving the Feminine Gender and the three semantic groupings illustrated in (622). And, of course, insofar as the alleged general requirement for paradigm economy supplies an explanation for this change and these correlations, the behaviour of type D tends to confirm the existence of the requirement rather than disconfirm it.

I will turn now to the paradigm of nūbēs 'cloud', given

as E at (621). This paradigm is mixed in that it resembles faciēs (F at (621)) in the Nom Sg and ignis (A at (601)) in all the other Cases. Like the puppis paradigm, therefore, it constitutes prima facie counter-evidence to the Paradigm Economy Hypothesis. I hope to show, however, that the actual behaviour of the small group of nouns which decline like nūbēs is consistent with the precarious status one will predict for them if they are indeed 'outlaws' in terms of a general constraint on morphological organisation. So, once again, insofar as the Paradigm Economy Hypothesis predicts characteristics of the nūbēs group which would otherwise seem coincidental, the behaviour of members of the group actually tends to confirm the Paradigm Economy Hypothesis rather than disconfirm it.

The third declension nouns in -ēs have been the subject of a close and thorough examination by Ernout (1965). Ernout's concluding remarks (page 28) confirm one general prediction which one will be inclined to make on the basis of the Paradigm Economy Hypothesis, namely that they represent an obsolescent type: "Ces mots en -ēs apparaissent en latin comme une survivance plutôt qu'une formation vigoureuse et capable de se développer. Beaucoup ne subsistent que dans des langues techniques, termes du vocabulaire de l'architecture, de la chasse, de la langue rustique, de la langue militaire, ou ne sont guère usités que par les poètes; certains sont défectifs, attestés seulement à certains cas, ou au singulier seul, quelques-uns mêmes ne sont conservés que par des glossateurs. Ils forment dans la troisième déclinaison un petit groupe isolé et par la même soumis à l'influence analogique de groupes plus nombreux et plus puissants, notamment le groupe des noms en -is ... Ils ont laissé peu de traces dans les langues romanes". It remains to be seen, however, whether a more detailed examination will yield conclusions equally reconcilable with the Paradigm Economy Hypothesis.

I will start the more detailed examination by listing all the nouns which Ernout treats in his exhaustive essay, classified in a fashion whose significance will become clear presently:

- (623) a. Alternative Nom Sg in -is attested, preferred or criticised as an error in the Appendix Probi¹⁹:

aedēs F 'temple'	nūbēs F 'cloud'
apēs F 'bee'	plēbēs F 'people'
caedēs F 'slaughter'	sēdēs F 'seat'
canēs F 'dog'	tābēs F 'melting'
clādēs F 'disaster'	torquēs M or F 'necklace'
corbēs M or F 'crow'	vallēs F 'valley'
famēs F 'hunger'	vātēs M 'prophet'
fēlēs F 'wild cat?'	vehēs F 'chariot'
prōlēs F 'offspring'	veprēs M or F 'bramble'
subolēs F 'offspring'	verrēs M 'boar'
luēs F 'liquefaction; plague'	volpēs F 'fox'
mēlēs F 'badger'	

- b. Alternative Nom Sg in -is attested only late, and not in the Appendix Probi:

indolēs F 'native quality'	pūbēs F 'pubic hair'
mōlēs F 'mass; effort'	rūpēs F 'cliff'

- c. Alternative n-stem formation (Nom Sg -ō or -iō) attested or preferred:

ambāgēs F 'circumlo- cation'	contāgēs F 'con- tact'
indāgēs F 'inquiry'	propāgēs F 'off- spring'
compāgēs F 'assemblage'	

- d. Alternative Nom Sg in -s attested or preferred:

corbēs M or F	saepēs F 'hedge'
nūbēs F	trabēs F 'beam'
plēbēs F	

- e. Fifth declension forms (type F at (621)) attested or preferred:

contāgēs F	pūbēs F
famēs F	sordēs F 'filth'
lābēs F 'ruin; stain'	tābēs F
plēbēs F	

(623) (continued)

- f. Other stem variants in common or predominant use:

contāgēs F; contāgia or contactus
 palumbēs F 'dove'; palumbus
 praesaepēs F 'animal park'; praesaepe
 ruēs F 'ruin'; ruīna
 tābēs F; tābum
 vehēs F 'cart'; vehīculum, vectūra

- g. Used mainly or exclusively in the Plural:

ambāgēs F	sordēs F
cautēs F 'rock'	tōlēs F Pl 'goitre'
floccēs Pl 'wine dregs'	veprēs M or F
frācēs Pl 'oil dregs'	

- h. Nominative Singular unattested:

brācēs F 'sprouted barley'	frācēs Pl
cautēs F	repāgēs F 'door-bolts'
contāgēs F	ruēs F
fidēs F 'string of a lyre'	sordēs F
floccēs Pl	tōlēs F Pl

- j. Remainder:

gerrēs F 'small fish (anchovy?)'
 impāgēs F 'border of door-panel'
 Palēs F: name of a pastoral goddess
 strāgēs F 'devastation, carnage'
 struēs F 'heap'

The first point to make about this list is that the groups within it are not all mutually exclusive; for example, plēbēs 'people', since it has Nom Sg forms plēbs and plēbis as well as plēbēs and a fifth declension Abl. Sg plēbē, appears in (623a), (623d) and (623e). Secondly, the list includes rare words for which no more than one or two Case-forms are attested and whose assignment to declension-type E therefore depends more on etymology and stem-formation than on inflexional behaviour. This is quite reasonable for Ernout's purposes, which have nothing to

do with paradigm economy. But the result is that a number of the words in Ernout's list commit no prima facie breach of paradigm economy, since they are not attested both in the Nom Sg (where declension-type E resembles the fifth declension, type F) and in some other Case, where type E nouns resemble type A, B or C. So we can exclude from consideration straight away, for example, the eleven nouns in (623h), for which no Nom Sg is attested.

We are left, then, with the nouns belonging to groups (623a-g) and (623j). If the Paradigm Economy Hypothesis is correct, declension-type E cannot have a stable separate existence in Latin; rather, all words belonging to it must be treated as exceptional members of either class F (the fifth declension), class A (the ignis type) or perhaps class B or C, where 'exceptional' is understood as in our discussion of the Gen and Acc Pl forms in sections 6.4 and 6.5. This means that they must all or mostly show one or more of the outward marks of exceptionality, namely they must (a) vacillate between the 'exceptional' and the 'regular' inflexion, or (b) defect entirely to the regular type, or (c) cluster in such a way that the 'exceptional' inflexion acquires new functions, removing it from strict rivalry with the 'regular' one. Further possibilities are to avoid those slots in the paradigm where an 'irregular' inflexion occurs, and to replace the troublesome word with a derivative displaying a different, unmixed inflexional pattern. In fact, nearly all the nouns in (623a-g) and (623j) display one or other of these characteristics. Those in (623a) show clear signs of vacillating with, or defecting to, type A; those in (623d) defect to type B; and those in (623e) to type F. Group (623e) in fact includes two nouns, famēs and plēbēs, which, according to Ernout, are never inflected like sēdēs or nūbēs at all; rather, they derive from roots which at different times and by different authors are assigned more or less firmly either to one of the third-declension types dealt with earlier (type A or type B), or to the fifth declension (type F). Groups (623c) and (623f) illustrate more radical expedients for avoiding breaches of paradigm economy,

namely the replacement of stems which display 'mixed' inflexional behaviour by stems which do not. In group (623g), the exceptional behaviour is avoided by a complete or near-complete restriction of use to the Plural, where type E nouns are inflected exactly like type ABC, and where therefore no new breach of paradigm economy appears.

There remain groups (623b) and (623j). The dividing line between group (623a) (those for which an alternative A-type Nom Sg in -is is relatively common) and group (623b) (those for which it is relatively rare) is not hard and fast. Group (623a), as given above, covers a wide spectrum of inflexional behaviour, from nouns for which -is is the only ending attested outside specifically linguistic comments by grammarians (such as canēs 'dog') to ones for which the -is variant is scarcely attested at all apart from being castigated in the seventh-century Appendix Probi. Precisely where the line between (623a) and (623b) is to be drawn does not matter, however. The important point is that the existence of groups (623b) and (623j) shows that there are some nouns of type E for which the evidence of defection or vacillation is tenuous or nonexistent, since only one noun in (623b) (namely pūbēs) shows any sign of defecting elsewhere than to type A. The question then is whether this residue is sufficient to render null our whole effort to reconcile type E with the Paradigm Economy Hypothesis. My answer is no. In the first place, (623b) (excluding pūbēs) and (623j) combined contain only eight nouns for which outward marks of exceptionality are lacking, as against forty in the rest of (623) (excluding (623h)) for which such marks exist. Moreover, of these eight nouns, all but two (mōlēs and rūpēs) are rare; the fact that an -is Nom Sg is not attested (or not attested more often) is therefore much less significant than if they were of frequent occurrence. We are left, then, with mōlēs and rūpēs, both relatively common nouns of type E for which evidence of any strong tendency to defect to some un-mixed type is lacking. We have to decide whether these two nouns alone suffice to confirm type E as a stable paradigm, in the face of the evidence for its instability furnished by the great majority

of the examples in (623). It seems much more plausible to say that mōlēs and rūpēs are indeed exceptional, too -- most plausibly, exceptional members of type A -- and that their resistance to absorption into the 'regular' pattern is idiosyncratic, much like the idiosyncratic maintenance by gens 'nation' of an Accusative Plural in -īs at a time when the etymologically parallel mens 'mind' had already defected to -ēs.

I have already said enough, perhaps, to show that the existence of the nouns listed in (623) does not pose as serious a threat to the Paradigm Economy Hypothesis as may at first appear. But there is a further factor, not so far mentioned, which may have slowed down their absorption into other, unmixed, paradigms and which may therefore help to explain why many of the 'exceptional' forms continued in existence as long as they did. This factor is the near-complete identification of type E with the Feminine Gender. The only nouns listed in (623) which are unequivocally Masculine are vātēs 'prophet' and verrēs 'boar', both denoting male creatures; and only three of the rest (corbēs, veprēs and torquēs) are occasionally treated as Masculine. It therefore seems reasonable to see the Nominative Singular -ēs as an indicator of Gender as well as Case and Number -- only partially, not totally, functionalised, however, since while all non-male nouns in -ēs are Feminine, it does not seem possible to identify any independent phonological or morphological conditions under which all Feminine nouns have a Nominative Singular in -ēs. As a Gender-marker, then, the ending -ēs in the Nom Sg has the same status as, according to my account, the Acc Sg ending -im has in type D (the declension-type of puppis), and my remarks earlier about 'partial functionalisation' apply here too.

Notes to Chapter VI

1. I will refer to Neue and Wagener simply as NW. Appendix A summarises the chronology of the Latin authors and inscriptional evidence that I will be citing.
2. I am grateful to Professor C. E. Bazell for drawing my attention to Risch's article.
3. (601) and (602) do not cover all the instances of paradigm mixture in Latin, but they do cover by far the most important in terms of the number and frequency of the nouns involved. I will say something about further instances in section 6.8. I will also say something in section 6.6 about those many Masculine and Feminine nouns of the third declension whose Nominative Singular does not end in -s and which therefore do not contribute to the paradigm mixture illustrated in (601), but whose inflexions in all other Cases nevertheless resemble those of type B or type C.
4. This last expectation presupposes that the regular Latin reflex of proto-Indo-European ^h-im is -im, and that the -em of nouns such as ignem is an analogical innovation (cf. e.g. Niedermann 1953: 44). Meillet (1906: 30-35) has argued that ^h-im in fact yielded -em regularly, and that the Latin -im of forms such as puppim, turrim (discussed in section 6.8 below) is derived from ^h-im. It is not important for our present purposes which view is correct.
5. Even though dentum is not found elsewhere, there is no reason to disbelieve Varro. It so happens that no Genitive Plural of dens, whether dentum or dentium, seems to occur in any Latin text earlier than Varro's time; NW (412) cites no attestation of dentium earlier than Cicero, and the lexicons of Plautus (Lodge 1962) and Terence (McGlynn 1963) confirm its absence from their works.
6. These statements are not strictly true in that the endings are given in (606) and (607) in their Golden Age shapes rather than the shapes they would have had at the time that syncope occurred (thus -ī rather than -ei in the Dat Sg, and -īs rather than -eis in the Acc Pl). But the precise phonological shapes of these endings do not affect the argument, and I have

normalised them to facilitate comparison with (601) and (602).

7. It is theoretically possible that copyists could have made conscious efforts to comply with Varro's dictum, so that the manuscript evidence is not truly independent. But this is unlikely. Varro does not seem to have been a particularly influential grammarian, unlike his successor Priscian; and any conscious Varronian tendencies on the part of scribes ought also to have produced examples of a Genitive Plural dentum, which, as I have said, is never found outside Varro.

8. These figures are derived from Ernout (1918: 148-149). His classification is in terms of 'imparisyllabics' versus the rest, but my total of 179 does not include those imparisyllabics in his list which are not unequivocally of type B, such as lux (lūc-) 'light' and abstract nouns in -tās and -tūs. I will be saying something about the latter in due course.

9. The fact that the Abl Sg ending -e did spread to non-syncopated type A nouns as well as to type B ones (albeit more slowly) is discussed in section 6.6 below, where it is attributed to reasons quite independent of paradigm economy.

10. Unlike palūs and fraus, the noun fauces 'throat', which is grammatically Plural, is consistent in displaying the 'wrong' Genitive Plural ending, namely -ium. This may be taken as counter-evidence to my argument. On the other hand, it may only indicate that the stem-class division in (618) needs further refinement. But the size of the classes in question is already so small that it will probably be fruitless to look for a conclusive answer.

11. Instances of -um rather than -ium also occur with other -nt- stems in dactylic verse. But these can be ignored, since the -ium ending there would be metrically impossible. The choice of -um in these contexts is best regarded as a grammatically 'wrong' choice imposed by the metre. For the same reason we can disregard, for example, an occurrence of partum instead of partium from pars in Ennius.

12. Strictly, I should say 'single-Plural-macroparadigm'; but I will continue to use the term 'paradigm' instead of

'macroparadigm' where no confusion is likely to arise.

13. \acute{I} represents the 'i longa' used in the Monumentum Ancyranum and a few other inscriptions of about the same time to represent [i:]. Ernout and Janson both ascribe a fifth example of adjectival $-\acute{I}S$ to the Monumentum Ancyranum, in a form $OMN\acute{I}S$; but this is apparently a restored reading.

14. Bourciez (1946: 229) argues that $-\underline{I}S$ did indeed oust $-\underline{e}S$ in the spoken Latin of Italy and the eastern Empire, and that this accounts for the Plural $-\underline{i}$ of Italian reflexes of third-declension nouns. To my mind, the Latin evidence for this view is weak; and the Italian $-\underline{i}$ is explained by other Romance scholars as the regular phonological descendant of Latin $-\underline{e}S$, at least at the end of polysyllables (Lausberg 1967: 82). But, even if Bourciez is right, there is no conflict with the single-Plural-paradigm analysis, since this requires only that each dialect of Latin should choose one or other of the two Accusative Plural endings as the regular one, not that all dialects should make the same choice.

15. I do not attach any theoretical importance to the extrinsic ordering in (618), (619) or (620) (at any rate, not yet); it is for expository purposes only, to simplify the statement of the phonological conditioning for the various endings. In fact, there is almost certainly a deeper explanation for some aspects of their distribution, in terms of Latin phonotactics. Nominatives such as "cans", "iuvens" would presumably presuppose underlying representations /kan + s/, /juwen + s/, involving a final consonant cluster not elsewhere found underlyingly in Latin. It will not be at all surprising, in general, to find phonotactic motivation for the operation of many phonologically-sensitive spell-out rules, even if G. Hudson (1975) goes too far in claiming that all productive morphological alternations (except those which acquire some new grammatical function) can be accounted for solely by reference to phonotactics or 'surface phonological constraints' (cf. also Hudson 1980). I will say no more about this issue here.

16. The former could, as I have said, be regarded as an experiment with an alternative way out of the dilemma created

by 4 in Appendix C -- an alternative permitting a two-paradigm solution for the Plural while preserving Nominative-Accusative homonymy. The latter is argued by Ernout (1918) to be dialectal and influenced by Oscan, in which the original consonant-stem Ablative ending gave way to an o-stem one.

17. Corbett (1981) has argued that Animate and Inanimate in Russian must be regarded as genuinely syntactic 'features', and not merely morphologically relevant semantic ones. I do not find his argument particularly convincing; but in any case it does not affect my main point here, namely that morphologically relevant 'properties' may play little or no part in syntax.

18. This fact was pointed out to me by Richard Hudson.

19. The Appendix Probi is a post-classical list of common morphological 'errors' which came to be ascribed (wrongly) to the first-century grammarian Valerius Probus.

CHAPTER VII

TWO QUESTIONS CONCERNING STEM ALLOMORPHY

7.1 Introduction

In Chapter II I suggested that there was a general constraint on Deviation II (or, at any rate, those examples of Deviation II which exhibit 'pure' sensitivity) to the effect that the inflexional realisations of morphosyntactic properties may be sensitive inwards to other individual properties but sensitive outwards only to certain classes of properties, namely classes defined by morphosyntactic categories. I named this constraint the Peripherality Constraint. What is perhaps not immediately obvious is that this constraint, if correct, has direct consequences for our analysis of stem allomorphy. In the second section of this chapter I will explain what these consequences are, and attempt to show that they fit in well with the way stems actually behave. In the third section of the chapter I discuss how stem allomorphy interacts with another proposed constraint on inflexional realisations, namely the Paradigm Economy Hypothesis expounded in Chapters IV - VI. In a sense, my conclusions here merely consolidate previous ones, because they amount to showing that stem allomorphy, as observed, does not conflict with either of the two constraints discussed. But the discussion is worthwhile because those conclusions are not self-evident; and, in the course of it, we will come across a further piece of evidence for the Peripherality Constraint from the history of Italian verbal morphology, and develop a new and, I hope, more satisfactory notion of 'inflexional distinctness' for the purpose of paradigm economy which will help us towards a possible solution of an outstanding problem in German noun inflexion.

First, I should make it clear what sort of phenomenon I will be discussing under the label of 'stem allomorphy'. Traditionally, linguists have used the word 'stem' in three rather different contexts, which can be exemplified by the following three statements about inflexion in Latin:

(701) Mensa 'table' is an a-stem noun.

(702) The Perfective stem of amō 'love' is amāv-.

(703) The stem found in the oblique Singular Cases of iter 'journey' is itiner-.

In use (701), what is emphasised is one phonological characteristic of the item which undergoes inflexion, on the basis of which its inflexional behaviour is predictable within the grammar of the language in question. Often it would be more accurate to say 'was formerly predictable'; synchronically, to my mind, it is dubious to analyse all Case-Number forms of mensa in Golden Age Latin as derived phonologically from underlying representations consisting of /mensa/ (rather than /mens/) plus some affix. If so, 'a-stem noun' is synchronically no more than a label for a phonologically unpredictable declension-type, just like 'first-declension noun'. A more secure example of inflexional choice based on stem phonology is that between the two 2nd Singular Indefinite endings -(a)sz and -ol in Hungarian, which I mentioned first in section 1.8; we might describe those Hungarian verbs of the 'normal' conjugation which choose -ol as 'sibilant-stem verbs'. But phonological stem classification of this kind does not, of course, imply any alternation in the shape of the stem, and it is not this sense of 'stem' which will interest us. Nor will we be primarily concerned with use (702), where what is emphasised is the morphosyntactic function of some partially inflected form; if P is some morphosyntactic property, we typically speak of X' as being the P-stem of some word X when all and only the forms of X which share the property P use X' as a basis for further inflexion. Here, the affix or other morphological process whereby X' differs from non-P forms of X is (in my terms) a principal exponent of P. What is important here, however, is that stem-contrast within inflexional paradigms, in this sense of 'stem', is perfectly compatible with the maintenance throughout the paradigm of a single invariant 'root' or core. For example, amō has distinct Imperfective and Perfective stems am- (or amā-) and amāv- respectively, and we might want to go further and, for example, distinguish within the Perfective between a Present Subjunctive 'stem' amāver(i)- and a Past Subjunctive 'stem' amāviss(e)-;

nevertheless, the whole paradigm of amō shares a single invariant root am-. Amō thus does not display any stem allomorphy of the kind we are concerned with -- that characteristic of use (703).

Where we do find such stem allomorphy is in a noun such as iter 'journey'. On the basis of the behaviour of a large number of nouns of the 'third declension', such as caput 'head' and nōmen 'name' illustrated in (705), we can distinguish a set of Singular Case endings as in (704):

(704) Nom/Voc/Acc	∅	(i.e. no affixal realisation of Case is separable from the rest of the word-form)	
Gen	-is		
Dat	-ī		
Abl	-e		
(705) NVA	caput	nōmen	
G	capitis	nōminis	
D	capitī	nōminī	
Ab	capite	nōmine	

Both nouns in (705) display not only the set of endings listed in (704) but also an alternation between two stem-forms, one for the Nominative-Vocative-Accusative and one for the other three Cases. Historically this is due to a purely phonological process of medial vowel weakening, and synchronically it is not implausible to treat it in the same way, even though surface exceptions exist which will contribute to the 'opacity' of the process (e.g. percutis 'you strike' rather than "percitis" alongside capitis). But no such phonological explanation could conceivably account for the stem alternation we observe in (706), either historically or synchronically (except under an extremely powerful phonological theory with generous tolerance for ad hoc 'rules'):

(706) NVA	iter 'journey'
G	itineris
D	itineri
Ab	itinere

Here we have inescapably two 'stems' in the sense of use (703): allomorphy affecting the core element which is not phonologically

or morphophonologically resolvable. It is stem allomorphy of this kind whose relationship with the Peripherality Constraint and the Paradigm Economy Hypothesis will interest us.

7.2 Stem allomorphy and the Peripherality Constraint

The Peripherality Constraint imposes limits on outward sensitivity in inflexion. The reason why there are questions to be answered about the relationship between this constraint and stem allomorphy is that, if one stem allomorph is found in one part of the paradigm -- that is, with one subset of the morphosyntactic property combinations applicable to the word in question -- and another stem allomorph with another subset, then, insofar as those property combinations are realised more peripherally, some outward sensitivity seems to be involved. And, since the stem is by definition the most central part of the word-form, those property combinations will indeed be realised more peripherally in most instances. So in principle, it seems, stem allomorphy ought to obey the Peripherality Constraint; or, to put it another way, stem allomorphy ought to provide an extensive hunting-ground for counter-examples to the Constraint.

Let us continue to assume, for the moment, that all stem allomorphy within inflexional paradigms is relevant to the Peripherality Constraint in this way. Does stem allomorphy in fact obey the Constraint? It is natural to look first at example (706). At first sight, the facts here do seem compatible with it. Where Case has an independent overt realisation, the same allomorph itiner- is always found; where it has not, as in the Nominative, Vocative and Accusative, the allomorph iter can be regarded as realising Case simultaneously with the root, so 'pure' sensitivity is not involved and the Peripherality Constraint cannot be at issue. But it is easy to find examples which cannot be dealt with so easily. In (707)-(710) I place side by side parts of the inflexional paradigms of two words in Italian, German, Sanskrit and Georgian respectively, inflexionally identical except that one involves stem allomorphy and one does not (or at least none which is not explicable by reference to 'trans-

parent' low-level phonological rules):

(707) Italian: verbal Present Indicatives:

	tenere 'to hold'	temere 'to fear'
Sg 1	teng-o	tem-o
2	tien-i	tem-i
3	tien-e	tem-e
Pl 1	ten-iamo	tem-iamo
2	ten-ete	tem-ete
3	teng-ono	tem-ono

(708) German: verbal Present Indicatives:

	geben 'to give'	beben 'to tremble'
Sg 1	geb-e	beb-e
2	gib-st [gi:pst]	beb-st
3	gib-t [gi:pt]	beb-t
Pl 1	geb-en	beb-en
2	geb-t	beb-t
3	geb-en	beb-en

(709) Sanskrit: nominal Case-Number paradigms:

Sg Nom	rājā 'king'	marut 'wind'
Acc	rājān-am	marut-am
Ins	rājñ-ā	marut-ā
Dat	rājñ-e	marut-e
Abl/Gen	rājñ-aḥ	marut-aḥ
Loc	rājñ- rājan- } _i	marut-i
Voc	rāja	marut
Du NVA	rājān-au	marut-au
IDAb	rāja-bhyām	marud-bhyām
GL	rājñ-oḥ	marut-oḥ
Pl NV	rājān-aḥ	marut-aḥ
A	rājñ-aḥ	marut-aḥ
I	rāja-bhiḥ	marud-bhiḥ
DAb	rāja-bhyaḥ	marud-bhyaḥ
G	rājñ-ām	marut-ām
L	rāja-su	marut-su

(710) Georgian: nominal Case paradigms (Vogt 1971: 21):

Nom	msxal-i 'pear'	kal-i 'daughter'
Dat	msxal-s	kal-s

(710) (continued)

Erg	msxl-ma	kal-ma
Gen	msxl-is	kal-is
Ins	msxl-it	kal-it
Loc	msxl-ad	kal-ad

In all these examples, seemingly, stem alternations can be observed which correlate with the presence or absence of individual properties, not whole categories, realised more peripherally. For example, in the Sanskrit example (709), it is trivially obvious that none of the stem allomorphs rājān-, rājñ-, rājan- or rāja- is associated with all Cases or all Numbers, for that would require there to be no stem allomorphy at all. Nor is any of them consistently associated with particular Cases or Numbers, even¹; for example, rājān- is found in the Accusative Singular and Dual but not in the Accusative Plural. Do we, then, have outward sensitivity here of a kind flatly inconsistent with the Peripherality Constraint?

The answer to this question depends on what scope we ascribe to the Constraint; whether it applies merely to the realisation of morphosyntactic properties, or to the realisation of the lexical content of inflected words as well. As formulated in Chapter II, it applies only to the former; it says nothing about allomorphy in that part of the word-form which 'realises', or expresses, the lexical meaning, except insofar as that part of the word-form may also realise some further morphosyntactic property, distinct from that which conditions the allomorphy. An important question in connexion with (707)-(710), then, is whether or not the stem allomorphy in the left-hand column does help to realise any such further properties. The answer is no. It is true that, in a form such as tengo 'I hold' from (707), the allomorph teng- can be said to help realise the property-combination 1st Person Singular, alongside the -o of the ending, inasmuch as the stem-form teng- is peculiar to the 1st Singular and 3rd Plural (within the Present Indicative). But the crucial point is that what is realised in teng-, along with 1st Singular, is only the lexical content 'hold'; there is no extra morpho-

syntactic property here influencing the choice of the stem allomorph. What this word-form illustrates, then, is not outward sensitivity but merely extended exponence: the property-combination 1st Singular has a principal exponent $-o$ (which by itself realises it unambiguously) and what we might call a 'subsidiary exponent' in the choice of the stem-allomorph teng-. The same goes for all the remaining stem allomorphy in the left-hand column of (707)-(710). In most instances, the suffix alone serves to identify unambiguously the morphosyntactic properties realised, and does so in the allomorphy-free paradigms in the right-hand column²; and in no instances are there more morphosyntactic properties involved on the left than on the right.

Can we then say that all stem-allomorphy involving apparent 'pure' sensitivity escapes the Peripherality Constraint? The answer, clearly, is no, because it is possible for a stem allomorph to be a principal exponent and not merely a subsidiary exponent of some morphosyntactic property. To put it another way, stem-allomorphs can also be stems in the sense of use (702) of the term 'stem' -- let's distinguish it as 'stem₂'. In discussing use (702), I gave the example amav-, the Perfective stem₂ of the Latin verb amō. This stem₂ is formed by suffixation. But in several Latin verbs the Perfective stem₂ is distinguished from the Imperfective one by internal differences -- by stem allomorphy in the sense of (703). Examples of Latin Perfective stems₂ involving stem allomorphy in this sense are given in (711):

(711) Imperfective	Perfective
faciō 'I make'	fēcī 'I have made'
rumpō 'I break'	rūpī 'I have broken'
agō 'I act'	ēgī 'I have acted'

For reasons given in Chapter II, the stem₂ is a principal exponent of the properties Perfective and Imperfective in Latin (assuming that Imperfective exists as a property on a par with Perfective). So, just as the Peripherality Constraint predicts (correctly) that the suffix -āv- of amāvī 'I have loved' will be found with all more peripheral properties (given that there is no whole category to which the realisation of Perfective is sensitive 'out-

wards' in Latin), the Constraint will also predict that the alternation between fac(i)- and fēc- will correspond rigidly to the Imperfective-Perfective distinction in the same way. This prediction is correct. There is thus a crucial distinction between the sort of stem allomorphy illustrated in (711) and that illustrated in (707). The alternation between teng- and tien-/ten-³ is quite compatible with the Peripherality Constraint, for reasons given in the previous paragraph. On the other hand, a logically quite conceivable pseudo-Latin in which faciō has two Perfective stems₂ fēc- and facīv-, distributed as in (712), is ruled out by the Peripherality Constraint, despite the superficial similarity in pattern to (707):

(712) Sg 1	fēc-ī
2	"facīv-istī"
3	"facīv-it"
Pl 1	"facīv-imus"
2	"facīv-istis"
3	fēc-erunt, fēc-ēre or fēc-ērunt

A reader who knows Italian may spot an apparent objection here. If the stem allomorphy in (712) is of a kind incompatible with the Peripherality Constraint, and consequently impossible, what about the actual Italian stem allomorphy illustrated in the Italian Preterite Tense-forms of (713)?

(713) Preterites of:	<u>tendere</u> 'hold' (cf. (707))	<u>fare</u> 'make' (<u>< Latin facere,</u> cf. (711), (712))
Sg 1	tenn-i ['tenni]	fec-i [fe:tʃi]
2	ten-esti [te'nesti]	fac-esti [fa'tʃesti]
3	tenn-e ['tenne]	fec-e ['fe:tʃe]
Pl 1	ten-emma [te'nemmo]	fac-emma [fa'tʃemmo]
2	ten-este [te'neste]	fac-este [fa'tʃeste]
3	tenn-ero ['tennero]	fec-ero ['fe:tʃero]

Does this not show the argument in the preceding paragraph to be invalid? The answer hinges on where the property Preterite is realised in the forms illustrated in (713). Consider first those

forms of the Preterite of tenere which have the allomorph ten- (the 2 Sg and 1 and 2 Pl). This allomorph is clearly not peculiar to the Preterite; it is found elsewhere not only in the Present (as shown in (707)) but also in the Infinitive and the Imperfect (tenevo 'I was holding' etc.). The realisation of Preterite seems therefore to belong in these forms firmly to the distinctive endings -esti, -emmo and -este, which are peculiar to this Tense. The same goes for the stem-allomorph fac- in the Preterite of fare. The Italian form facemmo 'we made', for example, is thus by no means parallel in its realisational structure to the pseudo-Latin "faciv-imus" of (712): in "faciv-imus" the realisation of Perfective resides pretty clearly in in the stem₂ faciv-, given that this stem is ex hypothesi peculiar to the Perfective Aspect, and that the ending -imus is not (compare the 1 Pl Imperfective Present facimus); whereas in facemmo the realisation of Preterite resides in the ending.

Consider now the allomorphs tenn- and fec- in the Preterites at (713). Here there does seem to be more ground for locating the realisation of the property Preterite in the stem rather than the ending; the stems tenn- and fec- are peculiar to the Preterite, whereas the associated Personal endings 1 Sg -i, 3 Sg -e and 3 Pl -ero (all unstressed) are not (or at least not peculiar to the Indicative), being found also in the Past Subjunctive (Preterite Subjunctive?) of tenere and fare and indeed all Italian verbs (tenessi, tenesse, tenessero; facessi, facesse, facessero). Here at least, perhaps, the Preterites of tenere and fare show 'pure' outward sensitivity of a kind incompatible with the Peripherality Constraint. But in one important respect the distribution of the stems tenn- and fec- in (713) differs from that of f^eec- in the pseudo-Latin of (712). This will become clearer if we set the paradigms alongside, indicating the stress on the pseudo-Latin⁴ as well as the Italian forms;

(714) Italian	Pseudo-Latin ^e
Sg 1 <u>féc-i</u>	<u>féc-i</u>
2 <u>fac-ésti</u>	<u>faciv-ísti</u>
3 <u>féc-e</u>	<u>faciv-it</u>

(714) (continued)

Sg 1	Italian	Pseudo-Latin
Pl 1	fac-émmo	facīv-imus
2	fac-éste	facīv-ístis
3	féc-ero	fēc-erunt, fēc-ére, fēc-érunt

The important difference is that the distribution of fec- and fac- in Italian correlates with stress, whereas that of fēc- and facīv- in pseudo-Latin does not. In Italian, fec- is always stressed and fac- is unstressed, but no such generalisation can be made about the pseudo-Latin forms. So in Italian, even if we do regard the stem as a principal exponent of Preterite in feci, face and fecero, the sensitivity involved in the choice between the the stem allomorphs fec- and fac- is not morphological but phonological. What we have in Italian is just another instance, this time involving stems, of the phenomenon we observed in affixes in the Hungarian -(a)sz/-ol alternation and the -si(n)/-i(n) alternation of the Turkish 3 Sg Possessive suffix (section 2.8). The upshot of this discussion is that under no plausible assumption about where (if anywhere) a principal exponent of Preterite can be located in the Italian verb-forms do these verb-forms exhibit 'illegal' outward sensitivity.

It is worth digressing briefly from our discussion of examples (707)-(710) in order to contrast the behaviour of the Italian verbs tenere and fare with that of their Spanish cognates, tener [te'ner] and hacer [a'θer]. In Spanish as in Italian, most verbs show the same stem (in the sense of use (703)) throughout their conjugation, all distinctions of Tense, Mood, Person and Number being realised in suffixes. But, like twelve other common Spanish verbs, tener and hacer have stems peculiar to the Preterite, namely tuv- [tuβ] and hiz [iθ] (spelt hic- before i and e).⁵ But, whereas within the Italian Preterite Indicative there is an alternation of tenn- ~ ten- and fec- ~ fac- which is conditioned phonologically, there is no such alternation within the Spanish Preterite Indicative, where the stems₃ tuv- and hiz- are maintained throughout:

(715) Sg 1	túv-e	híc-e
2	tuv-íste	hic-íste
3	túv-o	híz-o
Pl 1	tuv-ímos	hic-ímos
2	tuv-ísteis	hic-ísteis
3	tuv-iéron	hic-iéron

(The stress is indicated throughout in (715), to draw attention to the fact that it does not trigger any stem allomorphy, as it does in Italian.) Because the peculiar Preterite stem is constant in this way, it qualifies unequivocally as a principal exponent of the property Preterite, just as fēc- in Latin is a principal exponent of Perfective within the paradigm of faciō. It is no surprise, therefore, to find that, whereas the Italian tenn- and fec- are limited to three forms in the Preterite Indicative, the Spanish tuv- and hiz- are found not only throughout that Tense but also throughout three other Tense-Mood combinations which Ramsey labels the Imperfect, Aorist and Future Subjunctives (1st Singulars tuviera, hiciera; tuviese, hiciese; and tuviere, hiciere respectively). Assuming that the morphosyntactic property that I have called Preterite is present in all these forms (an assumption that begs no relevant questions because it is the most exacting one from the point of view of the Peripherality Constraint), this is exactly the pattern of stem allomorphy that the Peripherality Constraint requires; or, to put it another way, a logically quite conceivable form such as "haciese" instead of hiciese in the 'Aorist Subjunctive' would risk incompatibility with the Constraint.

A pseudo-Spanish form "haciese", using the 'ordinary' allomorph hac-, would in fact constitute a closer parallel than hiciese to the morphologically similar Italian Preterite (or Past?) Subjunctive form facessi 'I might make', where we find the 'ordinary' allomorph fac- rather than the Preterite Indicative's peculiar fec-. But my account implies that this divergence between Spanish and Italian is no accident, from a general linguistic point of view. When we compare the Spanish and Italian forms with their Latin morphological ancestor fēcissem (1st Singular Perfective Past Subjunctive of faciō), we see that it is Italian that has

innovated in choosing the reflex of Latin fac- rather than fēc- in facessi, whereas hiciese is almost a direct reflex of fēcissen⁶. But this Italian innovation was virtually unavoidable in order to preserve compliance with the Peripherality Constraint. If Italian had maintained a form "fecéssi" instead of switching to facéssi (and maintained fec- throughout the rest of the Preterite Subjunctive), then a purely phonological account of the distribution of the fec- allomorph among Preterite forms would not have been possible, and it would have been hard to avoid recognising 'illegal' outward sensitivity on the part of a principal exponent of Preterite in the forms where it occurred. Identifying principal exponents, if any, in the various forms of the Preterite Indicative in this hypothetical situation would not be altogether straightforward; but it is sufficient to say that the choice between fec- and fac- in these forms would risk contravention of the Peripherality Constraint. It is not unreasonable, therefore, to regard the need to avoid such contravention as one of the factors influencing the way the Italian conjugation system has developed.

I will now return to examples (707)-(710). In discussing example (711) I pointed out that, although the verbal stem allomorphy illustrated in (707) fell outside the scope of the Peripherality Constraint, that of the Latin fac(i)- versus fēc- did not. One can contrast in a similar way the nominal stem allomorphy illustrated in (709) and (710) with that found in those Arabic nouns which have 'broken' or 'internal' Plurals. In the Sanskrit example at (709), the stem alternation between the Acc Sg rājānam and the Acc Pl rājāṇaḥ is certainly not a principal exponent of the Singular-Plural distinction, since the 'strong' stem rājān- is not limited to the Singular and the 'weak' rājñ- is not limited to the Plural. But in the following Standard Arabic paradigm, the stem allomorphy is clearly what carries the distinction of Number:

(716)	Singular	Plural
Indefinite Nom	rajul-un 'man'	rijāl-un
Gen	rajul-in	rijāl-in
Acc	rajul-an	rijāl-an

(716) (continued)	Singular	Pural
Definite Nom	rajul-u	rijāl-u
Gen	rajul-i	rijāl-i
Acc	rajul-a	rijāl-a

This paradigm contrasts with that of nouns such as muʿallimun 'teacher' or ḥayawānun 'animal', which have so-called 'sound' Plurals sharing a stem₃ with the Singular:

(717)	Singular	Plural	Singular	Plural
Indef Nom	muʿallim-un	muʿallim-ūna	ḥayawān-un	ḥayawān-ātun
Gen	muʿallim-in	muʿallim-īna	ḥayawān-in	ḥayawān-ātin
Acc	muʿallim-an	"	ḥayawān-an	"
Def Nom	muʿallim-u	(as above)	ḥayawān-u	ḥayawān-ātu
Gen	muʿallim-i		ḥayawān-i	ḥayawān-āti
Acc	muʿallim-a		ḥayawān-a	"

In a form such as ḥayawanatun 'animals (Indef Nom Pl)' it seems reasonable to distinguish a Plural suffix -āt- from a Case-Definiteness suffix -un. We can thus speak of a contrast between the ways in which rajulun and ḥayawānun form their Plural stems₂; the first by internal vowel change, the second by suffixation. If this analysis is correct, then it is no accident, in general linguistic terms, that no Arabic noun displays an alternation in the Plural between 'broken' and 'sound' forms; that is, there is no paradigm on the lines of (718):

(718)	Indefinite		Definite	
	Singular	Plural	Singular	Plural
N	rajul-un	"rajul-āt-un"	rajul-u	"rajul-āt-u"
G	rajul-in	rijāl-in	rajul-i	rijāl-i
A	rajul-an	rijāl-an	rajul-a	rijāl-a

Such a paradigm, in contrast to (709), would indeed display outward sensitivity of a kind incompatible with the Peripherality Constraint.

The upshot of this discussion is that the Peripherality Constraint is quite compatible with the examples of stem allomorphy considered, even when the stems are themselves principal exponents of some morphosyntactic property; and the empirical import of the Constraint in relation to stem allomorphy is illustrated by the claims that the pseudo-Latin and pseudo-Arabic

paradigms (712) and (717) are impossible (not merely accidentally nonexistent), and that the Constraint may help to account for the divergent development of the Italian and Spanish Preterites. Another instance where the Peripherality Constraint makes diachronic predictions about stem allomorphy crops up in the Preterites of Germanic strong verbs. I will not attempt to discuss the problem in detail here, but merely set out enough of the facts to indicate why a problem exists and also where a solution reconciling these facts with the Constraint might be sought.

All the older Germanic languages (Gothic, Old Norse, Old English, Old High German, Old Saxon) display stem allomorphy involving ablaut within the Preterite Indicative. Historically, this allomorphy was probably dependent on the position of the accent; but, with the loss of the Indo-European accent in Germanic languages, it lost its phonological conditioning. The sort of morphologically conditioned allomorphy that remained is illustrated by the following paradigms in Gothic and Old English:

(719)	Gothic:	Old English:
Preterite:	greipan 'to seize'	grīpan 'to grip'
Sg 1	graip	grāp
2	graip-t	grip-e
3	graip	grāp
Du 1	grip-u	-
2	grip-uts	-
Pl 1	grip-um	grip-on
2	grip-uþ	grip-on
3	grip-un	grip-on

Now, since both stem allomorphs of the Preterite are peculiar to that Tense in each of the two languages, they must both be treated as principal exponents of Preterite (unless we find good reason to say otherwise); yet, since the realisation of Person-Number, which determines the choice of stem allomorph, is apparently more peripheral (being located in the ending), 'illegal' outward sensitivity seems to be involved. But emphasis must be placed on the word 'apparently'. Two points about (719) stand out. Firstly, in Gothic the allomorphy correlates neatly with Number: graip-

in the Singular, grip- in the Dual and Plural. Yet, since Number as such (independent of Person) has no identifiable principal exponent located more peripherally in any of these Gothic forms, the sensitivity to Number apparently displayed by the property Preterite cannot be called 'pure', and the Peripherality Constraint is therefore not contravened; what we see here, rather, is simultaneous exponence of Tense and Number in the stem. Secondly, the Old English distribution of the stem allomorphs is not parallel with that in Gothic. In fact, Old English differs from Gothic in the 2nd Sg in such a way as to disrupt the convenient correlation with Number just mentioned, so that the recognition of 'illegal' outward sensitivity in the Old English 2nd Sg form at least seems inescapable. This conclusion is inescapable, however, only if the ending -e of the 2nd Sg form grip-e (which is not cognate with the -t of the corresponding Gothic form graiþ-t) really is a principal exponent of the property-combination 2nd Singular. But it cannot be called such, apparently, because -e by itself is not an unambiguous exponent of 2nd Singular anywhere in the paradigm of grīpan; rather, we must say that 2nd Sg has no principal exponent in the form grīpe, being realised equally in the stem and in the ending, so that the Peripherality Constraint is again not violated.

Whether a solution on these lines can be sustained for all the Germanic languages remains to be seen. Certainly, cognates of the Old English 2nd Sg ending -e (which, in Old English at least, is not a principal exponent) seem to be found in just those Germanic languages which share the Old English rather than the Gothic distribution of stem allomorphs (Krahe 1969: 103, 105); and this fact is promising. But a detailed chronological study would be needed of the changes which took place both in stem allomorphy and in endings in order to determine whether the Peripherality Constraint is compatible with these changes and, if so, to what extent it may actually have helped to motivate them.

7.3 Stem allomorphy and the Paradigm Economy Hypothesis

In discussing examples (707)-(710) above, I suggested that

in most of the forms⁷ in the left-hand column the stem was at best a subsidiary exponent of the properties realised in the ending. This suggestion was justified by reference to the right-hand column in (707)-(710); the fact that the same endings show up there without any associated stem allomorphy justifies us in attributing to them the chief role in realising the relevant morphosyntactic properties in both columns, and in most instances the endings will also be 'principal exponents' of those properties in the sense of Chapter II. For example, although in tengo 'I hold' in (707) the inflexional exponent of 1st Sg may be said to extend over the stem as well as the ending, the existence of temo 'I fear', in which the ending alone realises these same properties, encourages us to give functional pride of place to the ending in the form tengo as well. We can thus distinguish two senses of 'inflexional realisation': in terms of the first, the property-combination 1st Person Singular is realised differently in tengo and temo, while in terms of the second it has the same realisation in both forms.

This distinction is of no particular interest for its own sake. It acquires interest from the fact that the term 'inflexional realisation' crops up in the definition of 'paradigm' which underlies the statement of the Paradigm Economy Hypothesis in Chapters IV and V. According to that definition, words belong to the same paradigm if they belong to the same part of speech and have all their inflexional realisations in common. So, assuming the first sense of 'inflexional realisation', tenere and temere in (707) belong to two distinct paradigms, whereas if we assume the second sense, they belong to the same paradigm. At first sight, it may not seem to matter much which we say; after all, there is no sign of any breach of paradigm economy in (707) under either assumption. But the two-paradigm analysis does pose a problem of a slightly more subtle kind. Under my present definition of 'paradigm', no allowance is made for the possibility of recognising degrees of distinctness (conversely, degrees of similarity) between paradigms; if two words differ in only one inflexion out of twenty, they belong to distinct 'paradigms' just

as much as if they differed in nineteen out of twenty. This almost certainly represents something missing from my account. For purposes other than assessing compliance with the Paradigm Economy Hypothesis, at least, we will almost certainly need to recognise that, for native speakers, paradigm A may in a real sense be more similar to paradigm B than it is to paradigm C; for example, the Latin first declension (that of mensa) seems closer to the second declension (that of dominus) than it does to the third (that of rēx and ignis) because of both the shapes of its inflexions and the fact that it supplies the Feminine forms of adjectives for which the second declension supplies the Masculine and Neuter forms.⁸ So the fact that the putative two paradigms in (707) -- that of tenere and that of temere -- are very similar is not in itself a strong argument against treating them as distinct for the purposes of paradigm economy. What is striking, though, is where the similarities and differences respectively are located. All the differences are in fact concentrated in the stems, and the endings in the two 'paradigms' are the same. The reason why this is significant is that under the two-paradigm analysis it has to be regarded as accidental from a theoretical point of view, whereas under the single-paradigm analysis it is just what we would predict. I will explain this in more detail directly.

Once two paradigms are distinguished by distinct inflexions for even a single 'slot', there is no extra 'cost' or complexity, so far as paradigm economy is concerned, in further differentiation in other slots. One might express this by saying that the Paradigm Economy Hypothesis tolerates with ease maximal distinctness between paradigms; and I suspect that a closer study of how paradigm economy operates would reveal an actual pressure towards maximal distinctness, despite the probable psychological reality of paradigmatic resemblance mentioned earlier. So, in Italian, if the two-paradigm analysis of tengo and temo is correct, an opportunity is presented, as it were, for distinctness not only in stem allomorphy but also in the endings. Under this analysis, there would be no extra 'cost' involved if the inflexional patterns

were not as in (707) but as in (720), for example:

(720) Pseudo-Italian: verbal Present Indicatives

Sg 1	teng-o 'hold'	tem-o 'fear'
2	tien-i	tem-i
3	tien-e	tem-e
Pl 1	"ten-emo"	tem-iamo
2	ten-ete	tem-ete
3	teng-ono	tem-ono

Here the distinctness extends beyond stem allomorphy to the inflexional endings, namely in the 1st Person Plural, where I have postulated that tenere retains the historically 'correct' ending -emo (cf. Latin tenemus). Now, as I have said, the fact that (720) is not what we observe in actual Italian is merely accidental from the point of view of paradigm economy under the two-paradigm analysis. But it is what we will predict if the one-paradigm analysis is correct -- in other words, if the stem-allomorphy of tenere is ignored for the purpose of paradigm economy. Under this analysis it is quite natural that the differences between the two inflexional patterns should be restricted to the stems; the fact that there are no differences in the endings simply shows that they belong to the same paradigm, and the stem behaviour is, from this point of view, irrelevant.

If we examine more examples, we find that the one-paradigm analysis looks increasingly appropriate on general grounds. It is quite easy to find supporting evidence, both in Italian and in other languages, for not treating stem allomorphy as part of inflexion for the purpose of defining paradigms. I will present the Italian evidence first. Tenere is by no means unique among -ere verbs in displaying stem allomorphy in the Present Indicative. Consider the following:

(721) Type I: 1st Plural stem = 2nd Plural stem

	rimanere	dolere	tacere
	'remain'	'hurt'	'be silent'
Sg 1	rimang-o	dolg-o	tacci-o ['tattʃo]
2	riman-i	duol-i	tac-i ['ta:tʃi]
3	riman-e	duol-e	tac-e
Pl 1	riman-iamo	dol-iamo	tac-iamo [ta'tʃa:mo]
2	riman-ete	dol-ete	tac-ete
3	rimang-ono	dolg-ono	tacci-ono

Type II: 1st Plural stem = 1st Singular stem

	solere	piacere
	'be accustomed'	'please'
Sg 1	sogli-o ['soʎʎo]	piacci-o ['pjattʃo]
2	suol-i	piac-i ['pja:tʃi]
3	suol-e	piac-e
Pl 1	sogl-iamo [soʎʎa:mo]	piacc-iamo [pjat'tʃa:mo]
2	sol-ete	piac-ete
3	sogli-ono	piacci-ono

If we regard stem allomorphy as part of the associated inflexion, it is by no means obvious how many distinct realisations of (say) the 1st Singular we need to recognise, because the stem allomorphy takes different forms. For example, does piaccio 'I please' count as displaying a different realisation ('gemination + -o') from dolgo ('-g + -o'), and do both in turn differ from soglio ('palatalisation + -o')? The more distinct inflexions we posit for this one Person-Number combination, the more distinct paradigms we must recognise, on this assumption; and the more embarrassing it becomes, then, that all the inflexional differences continue to be concentrated in the stems, just as we found when comparing tenere and temere, and that even when we extend the data thus we do not find any distinct paradigm belonging to an -ere verb where the opportunity is taken, as it were, to introduce some allomorphy into the endings too.⁹

Let us load the dice as heavily as possible in favour of the multi-paradigm analysis of the data of (707) and (721) by treating piaccio, dolgo and soglio as all displaying the same

realisation⁶ of 1st Singular -- we could call it 'heavy stem + -o'. Any stem-shape different from that which appears in the 1st Singular we will call a 'light' stem. But even on this basis we have to recognise a third 'paradigm', alongside those of tenere and temere in (707), namely that of type II verbs in (721). This is because of the stem allomorph used in the 1st Plural. In tenere a light allomorph is used, as in the type I verbs of (721); but in the type II verbs the heavy allomorph is used. Moreover, the contrast between the type I verb tacere and the type II verb piacere shows that it is impossible, at least with some verbs, to determine their type on phonological grounds, since these two verbs are phonologically as similar as they could possibly be in all conceivably relevant respects.¹⁰ So, even when we describe stem allomorphy in a fashion deliberately designed to keep down the number of distinct inflexions that we must recognise, we are still forced by the multi-paradigm approach to acknowledge three paradigms among -ere verbs, thus:

(722) No stem allomorphy (e.g. <u>temere</u>)	Stem allomorphy of:	
	type I	type II
Sg 1 -o	H -o	H -o
2 -i	L -i	L -i
3 -e	L -e	L -e
Pl 1 -iamo	L -iamo	H -iamo
2 -ete	L -ete	L -ete
3 -ono	H -ono	H -ono

where H = heavy stem allomorph
L = light stem allomorph

Under the multi-paradigm approach, we are required to recognise three distinct 'paradigms' because there are three distinct inflexions for the 1st Plural in (722). Now, whereas with only two 'paradigms' (those of tenere and temere) the concentration of the inflexional differences in the stems might conceivably be put down to accident or coincidence, the addition of a third 'paradigm' with the same set of endings again makes this account look even less convincing. It is much more attractive, surely, to say that for the purpose of allocating Italian verbs to paradigms it is the endings alone which count, given that the existence of many allomorphy-free verbs like temere justifies us in regarding

the endings, not the stem allomorphs, as the main exponents of the relevant property combinations.

If it is correct to ignore stem allomorphy in instances like this for the purpose of defining paradigms, we will expect to find other examples of inflected words differing in stem behaviour but not in the affixal part of their inflexions. This expectation is fulfilled. Such an example can be found in the Italian 'fourth conjugation' (verbs with an infinitive in -ire). Defining 'heavy' and 'light' allomorphs in the same way as we did earlier, we can identify three types:

(723)	No stem allomorphy (e.g. <u>partire</u> 'depart')	Stem allomorphy of: type I (e.g. <u>salire</u> 'go up')	type III (e.g. <u>finire</u> 'finish')
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Sg 1	-o	H -o	H -o
2	-i	L -i	H -i
3	-e	L -e	H -e
Pl 1	-iamo	L -iamo	L -iamo
2	-ite	L -ite	L -ite
3	-ono	H -ono	H -ono

as in:

Sg 1	part-o	salg-o	finisc-o
2	part-i	sal-i	finisc-i
3	part-e	sal-e	finisc-e
Pl 1	part-iamo	sal-iamo	fin-iamo
2	part-ite	sal-ite	fin-ite
3	part-ono	salg-ono	finisc-ono

Of these three, only the one labelled 'type III' is new. All but two of its members (uscire 'go out', heavy stem esc-, and udire 'hear', heavy stem od-) exhibit the stem-forming suffix -isc-.¹¹ Within type III the choice between heavy and light stem-allomorphs is phonologically determined: the heavy allomorph occurs in just those forms where the stem is stressed. But whether a verb 'goes like' partire or finire is not phonologically predictable, and a certain number of verbs vacillate between the two (e.g. assorbire 'absorb', 1 Sg assorbisco or assorbo). So, once again, if stem allomorphy is part of inflexion for the purpose of defining

paradigms, there is no escaping the recognition of three distinct paradigms; and once again we are left with the embarrassing coincidence that all the paradigmatic differences are concentrated in the stems.

Outside Italian, it is quite easy to find similar examples. At (709) we have one such in Sanskrit. In marut the only stem alternation, that between marut- and marud-, is attributable to a perfectly general phonological process of voicing assimilation. In rājā, on the other hand, things are not so straightforward. The rājñ- ~ rāja- alternation is historically due to the syllabification of nasals between consonants, and that treatment may perhaps be synchronically appropriate too. The alternation between rājā(n)- and the other stem forms is also phonologically explicable originally; the longer, or 'strong', alternant is found in those forms where the historic Indo-European accent was on the stem, and the shorter, or 'weak' and 'middle', alternants where the accent was on the ending. But by the classical period of Sanskrit literature in the early centuries AD the Indo-European accentual system, still partially maintained in Vedic, had disappeared (Thumb-Hauschild 1958: 208). So from the synchronic point of view the distribution of the 'strong' rājā(n)- stem is not phonologically predictable. On this ground, if stem allomorphy is part of inflexion, marut and rājā must be assigned to different paradigms. But, again, the embarrassing question arises; why are the paradigmatic differences limited to the stems, without affecting the endings? As with Italian, we can extend the data further and thereby increase the embarrassment. For example, the noun panthāḥ 'road', with quite idiosyncratic stem allomorphy, nevertheless displays just the same set of endings as marut:

	(724) Singular	Dual	Plural
NV	panthāḥ	panthān-au	panthān-aḥ
A	panthān-am	"	path-aḥ
I	path-ā	pathi-bhyām	pathi-bhiḥ
D	path-e	"	pathi-bhyaḥ
Ab	path-aḥ	"	"

(724) (continued)

G	path-aḥ	path-oḥ	path-ām
L	path-i	"	pathi-ṣu

Once again, the embarrassment disappears if we say that this kind of stem allomorphy, which has merely a subsidiary role in realising the associated morphosyntactic properties, does not count as inflexion for the purpose of paradigm economy.

I suggest, then, that we abandon the view, implicit in the treatment of paradigm economy in Chapters IV and V, that inflexionally-governed stem allomorphy necessarily counts as part of inflexion for the purpose of paradigm economy. What, then, does count as inflexion for that purpose? I have suggested so far that we exclude the sort of stem allomorphy which is a mere subsidiary exponent of the morphosyntactic properties which condition it, and have appealed to evidence mainly from Italian and Sanskrit. In discussing the Italian paradigms of temere and tenere in (707), for example, I contrasted a subsidiary exponent of Person and Number present only in tenere, namely stem allomorphy, with an exponent shared by both verbs, namely the set of endings -o, -i, -e, -iamo, -ete, -ono. A superficially attractive answer therefore suggests itself to the question about what is to count as inflexion, namely any principal exponent of each morphosyntactic property combination relevant to the word in question, with 'principal exponent' understood as in Chapter II. But there is a serious drawback to that approach, involving the contrasts between rājānaḥ and rājñāḥ and between gibt and gebt in the left-hand columns of (709) and (708) respectively.

The definition of 'principal exponent' in Chapter II was designed to enable us to identify instances of 'pure' sensitivity for the purpose of exploring constraints on Deviation II. A principal exponent of a property in a given word-form was considered to be a part of the realisation of that property in that word-form which sufficed by itself to identify the property unambiguously and which did so elsewhere in the paradigm to which that word-form belonged. For example, in the usage of Chapter II, the

-āv- of amāvī 'I loved' counts as a principal exponent of Perfective in that word-form, in spite of the fact that the 1st Person Singular ending -ī is unique to the Perfective Aspect, because the -āv- not only identifies the Aspect as Perfective by itself but does so in other -- indeed all other -- Perfective forms of amō. In the present section, however, we have not been primarily concerned with identifying 'principal exponents' in this sense. In discussing the form tengo of tenere 'hold', I justified assigning to the ending -o the chief role in realising 1st Person Singular (Present Indicative), or calling it 'the inflexional realisation' for the purpose of defining Italian verbal paradigms, on the basis not of how -o behaves elsewhere in the inflexional pattern of tenere but on the basis of its appearance in other verbs as the sole exponent of 1st Singular without any stem allomorphy. In principle, the two approaches can lead to quite different results: 'principal exponents' identified by comparing the realisation of particular morphosyntactic properties in different parts of the paradigm of the same word (in the sense of Matthews's 'lexeme') need not be the same as 'chief exponents' identified by comparing corresponding forms of different words. In practice, the results may sometimes be the same; for example, -o is not only a chief exponent of 1st Singular in tengo but also a principal exponent, since it is unambiguous on its own and crops up also in the form tenevo, 1st Singular Imperfect of tenere. But sometimes there are indeed differences in practice as well as in theory.

I have placed heavy emphasis on the identity of the endings (as opposed to the stems) in the left-hand and right-hand columns of examples (707)-(710), and have argued from this that it is the endings alone which should 'count' for the purpose of defining paradigms. Yet the endings by themselves are by no means always 'principal exponents' in the sense of Chapter II. In rājñah 'kings (Acc Pl)', -ah is certainly an exponent of Accuative Plural, and is shared with marutah 'winds (Acc Pl)', which I claim to belong to the same paradigm; but to call it a principal exponent would require us to ignore the crucial role of the stem allomorphy in distinguishing the Acc.Pl rājñah from the Nominative Plural

rājānah. True, this distinction is not made in the declension of marut, where the Nominative and Accusative Plural are homonymous; but this does not entitle us to ignore the morphosyntactic importance of the stem contrast in the declension of rājā. Given the sense in which I (following Matthews) am using the word 'realise', to say that it is the ending -ah which principally realises both Nom Pl and Acc Pl in rājā as well as marut is to say that the stem contrast in rājā is merely incidental to how we tell the one Case-form from the other, or even to deny that we can tell them apart at all. This is obviously ludicrous. We must therefore conclude that in the Nominative rājānah and the Accusative rājānah the -ah is not a principal exponent of the Cases at all; more specifically, that there is no principal exponent distinguishable within the inflected form as a whole, since the unambiguous realisation of Nom Pl and Acc Pl is achieved by the stem-form and the ending jointly in each of the two forms.¹² The two forms are therefore similar in relevant respects to the French 1st Plural Future parlerons and Conditional parlerions, discussed and rejected as instances of 'pure' sensitivity in Chapter II, section 2.1. And a precisely parallel argument can be built on a comparison of the German geben and beben patterns in (708); the first exhibits a morphosyntactically disambiguating stem contrast, namely in the forms gibt (3 Sg) and gebt (2 Pl), which the second lacks.

It is time to recapitulate. The discussion of marut and rājā arose from our search for a more precise definition of what counts as an inflexional realisation of a property (or property combination) for the purpose of deciding whether or not two words belong to the same paradigm and hence for the purpose of testing compliance with the Paradigm Economy Hypothesis. I argued in the light of (707)-(710) and further Italian data that, at least under some circumstances, stem allomorphy should be ignored. But under what circumstances? A plausible answer, at first sight, was: when it is the ending, not the stem, which is a principal exponent (in my technical sense) of the properties concerned. But the Sanskrit and German facts demonstrate that this will not work. We are still at a loss, then, for a precise criterion to

determine when two inflexional realisations are distinct for paradigm economy purposes; and this is a potentially serious lack, because, if we can ignore stem allomorphy, what other differences might we not permit ourselves to ignore if it suited us, for the sake of protecting the Paradigm Economy Hypothesis from falsification?

Fortunately, we have by no means exhausted the possible criteria for inflexional distinctness, so it is too early yet to resign ourselves to the gloomy outcome just described. Let us set side by side the pairs of forms from (708) and (709) which have caused our difficulty, with the inflexional realisations of the relevant properties underlined:

(725) a. (cf. (708))

	geben 'give'		beben 'tremble'
3 Sg	<u>gib</u> -t	but:	beb- <u>t</u>
2 Pl	<u>geb</u> -t	but:	beb- <u>t</u>

b. (cf. (709))

	rājā 'king'		marut 'wind'
Nom Pl	rājā <u>n</u> -ah	but:	marut- <u>ah</u>
Acc Pl	rājā <u>n</u> -ah	but:	marut- <u>ah</u>

The fact that on the left two portions in each form are underlined while on the right only one is reflects the difference in what one could call the realisational status of the morphological material. But one characteristic the left- and right-hand forms have in common; the endings are the same even though the stems may vary. We might well try to build on this in our search for criteria for inflexional distinctness. I will propose a new criterion at (726). The rest of this section will be devoted to illustrating how this criterion operates, discussing its effect on the empirical content of the Paradigm Economy Hypothesis, and attempting to demonstrate its advantages in handling certain facts about Sanskrit and Italian.

The criterion for inflexional distinctness that I propose is as follows:

(726) Inflexional Distinctness Criterion for paradigm economy: Two inflexional realisations for some

(726) (continued)

property (combination) are distinct if they are phonologically distinct at the underlying level,¹³ unless the phonological difference between the two realisations is confined to that part of either or both realisations which overlaps (or is simultaneous with) the stem.

This criterion is formulated so as to be compatible with the 'package' of definitions and claims about paradigm economy summarised in section 5.7. According to Chapter V, there are three ways in which a pair of superficially distinct inflexional patterns may count as a single macroparadigm for the purpose of paradigm economy:

- i. they may be inflexionally distinct but subsumed under the same paradigm on the ground that the distribution of the inflexions in which they differ is phonologically predictable, as in the two Hungarian 'normal' verbs ír 'write' (2 Sg Pres Indic Indef ír-sz) and olvas 'read' (2 Sg Pres Indic Indef olvas-ol);
- ii. one of the two patterns may be 'mixed' in such a way that it can be subsumed under two unmixed paradigms in accordance with the second exception clause in the definition of 'paradigm' at (535), e.g. the German declension-type IX exemplified by Dorn (according to the analysis preferred in section 5.3);
- iii. the two patterns may be both inflexionally and paradigmatically distinct but united into a single macroparadigm on the ground that their inflexional distinctness is correlated consistently with some lexically determined morphosyntactic or morphosemantic property contrast, e.g. the German nouns Gast 'guest' (Gen Sg Gastes, Masculine) and Hand 'hand' (Gen Sg Hand, Feminine).

The Inflexional Distinctness Criterion now introduces a fourth possibility; the two superficially distinct patterns may not be inflexionally distinct at all, in the relevant sense. The Cri-

terion in fact permits us to regard the left-hand and right-hand columns in (707)-(710) as inflexionally not distinct and therefore as belonging to the same paradigm. This is because the non-stem part of their inflexions is the same, whether it is a principal exponent of the properties involved (as in Nom Pl marutaḥ and 3 Sg bebt) or not (as in Nom Pl rajanah and 3 Sg gibt).

It is fairly obvious, too, that the Criterion at (726) will have important implications for paradigm economy. Consider the inflexional behaviour of three Sanskrit Neuter nouns:

(727)	Singular	Singular	Singular
NVA	jagat 'world'	nāma 'name'	vāri 'water'
I	jagat-ā	nāmn-ā	vāriṅ-ā
D	jagat-e	nāmn-e	vāriṅ-e
AbG	jagat-aḥ	nāmn-aḥ	vāriṅ-aḥ
L	jagat-i	nāmn- nāman- } i	vāriṅ-i
	Dual	Dual	Dual
NVA	jagat-ī	nāmn- nāman- } ī	vāriṅ-ī
IDAb	jagad-bhyām	nāma-bhyām	vāri-bhyām
GL	jagat-oḥ	nāmn-oḥ	vāriṅ-oḥ
	Plural	Plural	Plural
NVA	jagant-i	nāmān-i	vārīn-i
I	jagad-bhiḥ	nāma-bhiḥ	vāri-bhiḥ
DAb	jagad-bhyaḥ	nāma-bhyaḥ	vāri-bhyaḥ
G	jagat-ām	nāmn-ām	vāriṅ-ām
L	jagat-su	nāma-su	vāri-ṣu

The hyphens are inserted in order to draw attention to the inflexional endings to their right. These are the same in all three columns (except for the phonologically predictable substitution of -ṣu for -su in the Loc Pl of vāri), and the differences between them are limited to the stems to the left of the hyphens; for example, in the Gen Pl jagat exhibits the same stem allomorph as in all other Cases except the NVA Pl, nāma exhibits one shared by most Singular Cases but none of the other Plural ones, and the allomorph of the stem of vāri is shared only by the NVA Pl. Yet,

if we adopt the criterion at (726), all three endings will emerge as belonging to the same paradigm. Consequently, if some Sanskrit noun is found which declines partly like nāma and partly like vāri, then, even if the nāma-like and vāri-like endings are distributed in such a way that the pattern of mixture cannot be correlated with any fundamental morphosyntactic property contrast such as Singular versus Plural, there will be no question of paradigm mixture and hence no breach of paradigm economy. So the fact that a group of nouns which behave like this does in fact exist in Sanskrit does not constitute counterevidence to the Paradigm Economy Hypothesis, and necessitates no special 'escape clause'. The group consists of asthi 'bone', aksi 'eye', dadhi 'sour milk' and sakthi 'thigh' (Whitney 1889: 122), which decline as follows:

(728)	Singular	Dual	Plural
NVA	asthi	asthn- asthan- } i	asthān-i
I	asthn-ā	asthi-bhyām asthi-bhiḥ	
D	asthn-e	"	asthi-bhyaḥ
Ab	asthn-aḥ	"	"
G	"	asthn-oḥ	asthn-ām
L	asthn- asthan- } i	"	asthi-ṣu

The boxes in (728) enclose those forms which resemble vāri; the rest 'go like' nāma.

The example of asthi illustrates how the adoption of the Inflexional Distinctness Criterion as formulated at (726) leads to a weakening of the predictions made by the Paradigm Economy Hypothesis in respect of one type of inflexional pattern; for, if stem allomorphy were allowed to contribute to inflexional distinctness, paradigms like that of asthi would be predicted not to exist. But in respect of other logically conceivable types of inflexional pattern, the Inflexional Distinctness Criterion helps to strengthen the Paradigm Economy Hypothesis, in that the Hypothesis now predicts to be impossible some kinds of pattern which would otherwise be compatible with it. Consider two hypothetical nominal paradigms of the following shape (where

large letters indicate stems, small letters indicate inflexional endings, and stem allomorphs are distinguished by the presence or absence of an apostrophe):

(729) Case 1	R a	T e
2	R b	T'f
3	R c	T g
4	R d	T'd

These paradigms are clearly distinct. But what happens when we introduce a third inflexional pattern exhibited by a stem S, partially similar to both R and T? One conceivable pattern would be as follows:

(730) Case 1	R a	S a	T e
2	R b	S'b	T'f
3	R c	S g	T g
4	R d	S'd	T'd

Now, does this involve a breach of paradigm economy? If we reject the Inflexional Distinctness Criterion and regard stem allomorphy as contributing to inflexional distinctness, the answer is no; this is because Case 2 has three distinct realisations, which we can symbolise b, 'b and 'f. But if we accept the Inflexional Distinctness Criterion, then the difference in stem allomorphy between Rb and S'b (or, better, the difference in the distribution of distinct stem allomorphs within the declensions of R and S) does not render them inflexionally distinct. Consequently, no Case in (730) has more than two distinct realisations, and the mixed behaviour of S ('going like' R in Case 1 and T in Case 3) contravenes paradigm economy. Without the Inflexional Distinctness Criterion, therefore, the Paradigm Economy Hypothesis makes no predictions about (730); with the Criterion, on the other hand, it makes a quite precise prediction, namely that such a pattern could not exist, or, if it did exist, would be under strong pressure to reshape itself into a two-paradigm pattern -- most easily, perhaps, by substituting Sc (with the R-type ending) for Sg (with the T-type ending) in Case 3.

It is an open question, of course, whether it is correct to predict the nonexistence of the sort of pattern illustrated in

(730). In more concrete terms, the sort of pattern which would falsify this prediction is illustrated in the pseudo-Italian 'paradigms' at (731):

(731) Sg 1	tem-o	teng-o	parl-o 'I speak'
2	tem-i	tien-i	parl-i
3	tem-e	tien-e	parl-a
Pl 1	tem-iamo	ten-iamo	parl-iamo
2	tem-ete	"ten-ate"	parl-ate
3	tem-ono	teng-ono	parl-ano

Here, there is no breach of paradigm economy if stem allomorphy is reckoned to contribute to inflexional distinctness, because there are three distinct realisations for 3 Pl (namely -ono, H-ono and -ano); consequently, in the imaginary paradigm in the centre, the pseudo-Italian verb tenere is free to choose a 'first conjugation' ending -ate in the 2 Pl, like parlate 'you speak', instead of choosing -ete, like temete 'you fear', as the actual Italian verb tenere does and as the Paradigm Economy Hypothesis with the Inflexional Distinctness Criterion predicts that it should.

One can summarise the principal claims made in this section by saying that, if stem allomorphy is not counted as part of inflexion for the purposes of paradigm economy, then we have a natural explanation for two sets of facts. Firstly, we can explain the failure of the inflexional patterns with and without stem allomorphy in (707)-(710) to differ in their endings as well as in their stems, and more precisely the absence of pseudo-Italian versions of the stem-changing verb tenere differing from the non-stem-changing verb temere in the sorts of ways illustrated in (720) and (731). Secondly, we can reconcile the Paradigm Economy Hypothesis with the existence of what at first sight seems to be 'illegal' paradigm mixture in Sanskrit nouns like asthi, illustrated at (728).

7.4

The declension of Name in German and related problems

In Chapter V we were able by invoking the Slab Codicil and the notion 'macroparadigm' to reduce the ten apparently distinct

inflexional patterns of German nouns exemplified in (505) to only six macroparadigms. But this was still one more than the number of distinct 'macroinflexions' available for the most generously provided slots -- the non-Dative Cases of the Plural. I deferred until the present chapter the task of completing the reconciliation of the Paradigm Economy Hypothesis with the German data, but indicated that the reconciliation would involve discussion of type X in (505) -- that of Name 'name'. In section 5.3 I also postponed until this chapter an account within my framework of the apparently 'illegal' paradigm mixture of the Russian noun zub 'tooth', which in the distribution of stress between stem and endings resembles partly the ending-stressed type of stol 'table' and partly the stem-stressed vxod 'entrance'.

I will deal with the Russian problem first. All or nearly all nominal inflexions in Russian may be either stressed or unstressed;¹⁴ what determines the distribution of stress between stem and inflexion within the paradigm of a given noun is the identity of the noun itself -- that is, some specification within its lexical entry. The stress behaviour of the noun need not be altogether idiosyncratic; there are in fact a number of regularities to be observed which render the behaviour of many nouns more or less predictable. But the important point is that the difference between, say, the ending-stressed stoly 'tables (Nom)' and the stem-stressed vxody 'entrances (Nom)' at (514) has nothing to do with the ending -y. However one represents this difference phonologically -- whether through contrasting values of an underlying feature [+ stress] on the stem vowel, or through some more abstract mark which will affect the operation of a later stress-assignment rule -- one must inevitably recognise some difference between stol and vxod in the pattern of stem allomorphy that each exhibits. But, since the segmental phonology of their endings is identical (all phonetic differences being accounted for by the perfectly general processes of weakening which unstressed vowels undergo in Russian), this amounts to saying that the phonological difference between the realisations of Nom Pl

in the forms stoly and vxody is limited to the stem, and similarly in all other Cases. It follows that, by the Inflexional Distinctness Criterion, stol and vxod are not inflexionally distinct; consequently, any noun, such as zub, which 'goes like' stol in some Cases and vxod in others is not inflexionally distinct either, and all three belong to a single paradigm for the purpose of paradigm economy. The existence of zub, with an inflexional pattern superficially distinct from those of stol and vxod, therefore does not after all contravene the Paradigm Economy Hypothesis.

The German noun Name and those six or seven nouns which decline like it present a somewhat more complex and more interesting problem, not because there is no plausible way of reconciling their existence with the Paradigm Economy Hypothesis but because there are two ways which, in the present state of knowledge, look about equally plausible. The question therefore arises: is this indeterminacy simply a function of our present ignorance, or do we have here a genuine indeterminacy from the point of view of the native speaker -- that is, an instance where different speakers may in fact organise their linguistic knowledge differently? I will do no more here than describe briefly what sort of evidence counts in favour of each of the two answers; the question whether the indeterminacy is genuine or not is left open.

For convenience, I will set out in full the standard German paradigm of Name, alongside that of a type I noun Tag 'day':

(732) Sg Nom	Name 'name'	Tag 'day'
Acc	Namen	Tag
Gen	Namens	Tag(e)s
Dat	Namen	Tag(e)
Pl NAG	Namen	Tage
Dat	Namen	Tagen

The reason for setting these two types alongside is to help us consider the question: where do the inflexional differences between them reside? The way in which I originally presented the German facts in Chapter V implies a clear answer: the differences reside

at least partly in the endings, because in (505) I attributed to Name a Gen Sg ending -ns distinct from the ending -(e)s of Tag. But this begs the important question of where the boundary between stem and ending is to be placed. In Chapter V I assumed that the appropriate 'base' or stem form for all Cases was identical to the Nominative Singular. But for Name at least there is a plausible alternative. Wurzel (1970) analyses the Gen Sg Namens as underlyingly /nām + n + s/, where between the root /nām/ and the inflexional affix /s/ there intervenes a stem-forming suffix ('Stammbildungselement') /n/. If we do likewise, and once more set the two paradigms of Name and Tag side by side, the following picture emerges:

(733) Sg Nom	Name	Tag
Acc	Namen	Tag
Gen	Namen-s	Tag-(e)s
Dat	Namen- \emptyset	Tag-(e)
Pl NAG	Namen- \emptyset	Tag-e
Dat	Namen- \emptyset	Tag-en

Superficially, the inflexional patterns (to the right of the hyphen in each paradigm) are still distinct. But you may recall that, when introducing the German facts in Chapter V, I remarked that several of the distinct inflexions for several of the slots were in complementary distribution on a phonological basis; specifically, $\emptyset \sim e$ in the Dat Sg and NAG Pl and $\emptyset \sim n$ in the Dat Pl. Moreover, Name and Tag meet the phonological conditions for selecting the first and second alternant out of each of these pairs respectively. Consequently, so far as the endings are concerned, Name and Tag display the same inflexional pattern; the only difference between them is that Name displays two stem allomorphs (Name and Namen-) whereas Tag displays only one. Invoking the Inflexional Distinctness Criterion, then, we can say that they exemplify the same paradigm. Our original declension-type X therefore disappears as a distinct paradigm, being subsumed under type I, and the number of distinct paradigms for German nouns is at last reduced from six to five, the number that the Paradigm Economy Hypothesis predicts.

The fact that many nouns (those of our types II, III and V) distinguish a Plural stem with umlaut from a Singular stem without shows that stem allomorphy is not foreign to German nominal declension. The account of Name just presented involves recognising a further instance of stem allomorphy, this time between the stem for the Nom Sg and the stem for all other Cases. What independent evidence might help to support this account?

It has often been remarked that stem allomorphy within paradigms -- at least, that which has no phonological motivation or clear-cut morphosyntactic function -- tends to be reduced over time. Formulating precise predictions about where and how it will be reduced has been notoriously difficult. For our purposes, however, all that matters is that a tendency to 'levelling', albeit under somewhat hazy conditions, is widespread. If, then, we find evidence for some diachronic change affecting nouns of the Name type which can be seen as levelling of stem allomorphy, then our present analysis, under which Name belongs to the same paradigm as Tag, is supported.

A diachronic change of the appropriate kind would be encroachment of one of the alternants Name and Namen- on the sphere of the other. And such a diachronic change does in fact seem to be occurring. Apart from Name, the commonest nouns which generally or occasionally follow type X are Buchstabe 'letter of the alphabet', Friede 'peace', Funke 'spark', Gedanke 'thought', Glaube 'belief', Same 'seed', Schade 'harm' and Wille 'will'. Yet concerning all these a standard pedagogical work on German grammar (Schulz & Sundermeyer 1964: 103) states: "Folgende Substantive auf -e haben im Nominativ Singular eine jüngere Nebenform auf -en [my emphasis]"; and, as my words 'generally or occasionally' imply, usage is by no means consistent. Three reputable dictionaries (see Appendix D) agree completely on the behaviour of only three of the nine nouns; Gedanke, which follows type X only; Friede, which has a variant Frieden of type I; and Name itself, whose type I variant Namen is variously described as 'weniger gut', 'Austrian' or 'rare'. Funke, Same and Wille

are also said by all three dictionaries to have variant Nom Sg forms in -en, implying type I behaviour, and Glaube is added to the list by one of the three. There is therefore strong evidence for defection from type X to type I. There is also weaker evidence for defection to type VIII: Buchstabe, Funke, Same, Glaube and Wille are all alleged by at least one dictionary to have type VIII forms, although on none of them are all three dictionaries unanimous.

Defection from type X to type I is just what we will expect, on the assumption that types X and I are identical so far as the non-stem parts of their inflexions are concerned and thus, in virtue of the Inflexional Distinctness Criterion, belong to the same paradigm, the difference between them being solely a matter of stem allomorphy. The relative weakness of the tendency to defect to type VIII is also to be expected, on this assumption. Superficially, defection to type I and defection to type VIII both involve a change in only one form (the Nom Sg and the Gen Sg respectively), and so would seem on a priori grounds about equally likely, with the latter perhaps marginally preferred because it involves a change in a more 'marked' form (the Genitive) rather than a less 'marked' one (the Nominative). But, according to our present account, defection to type VIII involves a more thorough-going reanalysis, affecting not only the form which changes 'on the surface' but also the boundary between stem and affix for those forms which do not change, thus:

(734)	Type X		Type VIII
Sg N	Same		Same
A	Samen	⇒	Same-n
G	Samen-s		Same-n
D	Samen		Same-n
Pl NAG	Samen		Same-n
D	Samen		Same-n

Contrast this with the degree of reanalysis involved in defection to type I:

(735)	Type X	Type I
Sg N	Same	Samen
A	Samen	Samen
G	Samen--s	Samen--s
D	Samen	Samen
Pl NAG	Samen	Samen
D	Samen	Samen

It may seem as if we have said enough to feel justified in resting content with an account which reconciles the existence of type X with the Paradigm Economy Hypothesis by dint of amalgamating type X and type I into a single paradigm. But I claimed earlier that there were two roughly equally plausible ways of handling type X. It is time now to describe the second, which involves recognising a new German macroparadigm.

Keller (1978: 417), describing the class of nouns with Nom Pl in -en in sixteenth century German (the ancestors of our types VI, VIII, IX and X) says: "In the gen[itive] of masculines and neuters -en/-ens competed with each other until they sorted themselves out on the basis of animate (a) [Gen Sg -en] and inanimate (b) [Gen Sg -ens]" . Later, describing the membership of our types VIII, IX and X in modern standard German, he says (page 563): "The masc. nouns divide into three subclasses according to the formation of the gen. sg.: (a) gen. sg. -en, only nouns denoting animate beings; (b) gen. sg. -ens, inanimate nouns ending in -e, e.g. Gedanke, Name, Wille; (c) gen. sg. -(e)s, nouns ending in a consonant, ...". Keller's class (c), which is our type IX, no longer concerns us. As for Keller's class (a) (our type VIII), his assertion about Animacy is too sweeping, since we have already seen that type VIII does include a few Inanimates such as Dividend and Diamant. But neither of these ends in -e in the Nominative Singular. Moreover, Keller's assertion about class (b) (our type X) seems correct. It seems possible, then, to amalgamate type X with type VIII on the basis of conditioning partly phonological and partly morphosemantic, thus:

(736) Type VIII/X

Gen Sg:	stems in <u>-e</u> :	Inanimates	<u>-ns</u>
		Animates	<u>-n</u>
	other stems:		<u>-n</u>
Nom Pl:			<u>-n</u>

Bearing in mind that type VIII has already in section 5.2 been amalgamated into a single macroparadigm with type VI (Rose) on the basis of a consistent Gender contrast, this analysis leads us to recognise a single macroparadigm embracing three superficially distinct inflexional patterns whose behaviour is predictable partly morphosyntactically, partly morphosemantically and partly phonologically, thus:

(737) Type VI/VIII/X

Gen Sg: Feminine:		\emptyset
	Masculine: stems in <u>-e</u> :	Inanimates <u>-ns</u>
		Animates <u>-n</u>
	other stems	<u>-n</u>
Nom Pl:		<u>-n</u>

Wurzel (1970) offers essentially the same analysis, despite certain differences in his assumptions and terminology. For him, the -e ending of the Nom Sg is a 'Nominativ-Erweiterung' or 'Nominative extension', and the -en ending of the type VIII nouns is really a 'Stambildungselement' or stem-forming element, not a 'Flexiv' or inflexion proper. But his rules of stem-formation and inflexion for nouns (1970: 47 and 48) cooperate to ensure that all nouns with a 'Nominative extension' receive a stem-forming -en in oblique Cases, but of these only the Inanimates subsequently receive an overt Genitive ending -s.

Keller and Wurzel thus independently describe the present status of type X in a fashion which, when translated into my terminology, implies a macroparadigmatic amalgamation not with type I but with type VIII. Moreover, Keller implies that the Animate-Inanimate distinction is not only relevant to the present synchronic state of the paradigm but was visibly involved in the diachronic changes whereby order was imposed on a state of inflexional confusion around the sixteenth century. It is true that this

involves recognising (in my terms) a macroparadigm of some complexity; but we have no grounds so far, at least, for rejecting it as too complex to learn.

The attractiveness of this account does, however, depend to a large extent on the closeness of the correspondence between the -ns/-n contrast in the Genitive and the semantic contrast between Inanimates and Animates. The -ns type (type X), which contains fewer than a dozen nouns, is indeed exclusively Inanimate. But the -n type (type VIII), though overwhelmingly Animate, is not exclusively so; and, while we might be willing to overlook a few exceptions if they satisfied our usual criteria for exceptionality -- particularly if they displayed a tendency to defect to the 'regular' type --, it is disconcerting to find that the Inanimates of type VIII include nouns which have, to all appearances, defected to type VIII from type X, such as Buchstabe and Same (according to the Sprach-Brockhaus) and Funke (according to both the Sprach-Brockhaus and Cassels). I say 'to all appearances' because I have not investigated carefully an alternative possibility, namely that some or all of these three nouns have a long-standing type VIII variant antedating the obsolescence of type X; but even if they have such a variant, the essential point remains, namely that there are Inanimate members of type VIII and even of that sub-class of type VIII with Nom Sg forms in -e.

A more serious difficulty for this analysis, however, is the fact that it suggests no motive for the shift of type X nouns to type I through the acquisition of a Nom Sg in -n. If type X is securely anchored in an 'economical' array of paradigms on the basis of phonological, morphosyntactic and morphosemantic conditioning I have just described, then it ought to be subject to no pressure to adopt 'new' forms. Yet the replacement of Friede by Frieden, Name by Namen and so on demonstrates that such pressure exists. Our second analysis therefore seems in a sense too successful, since it characterises as unproblematic a declension-type which, on the evidence of current linguistic usage, is indeed problematic and subject to 'regularisation' through assimilation.

lation to a more numerous declension-type.

Let us suppose, then, that the macroparadigmatic analysis of type X, which brings it under the umbrella of type VI/VIII, is no longer correct, and that synchronically in modern standard German type X is to be regarded as merely a sub-type of type I with unusual stem-allomorphy. The fact remains that the Animate-Inanimate correlation of types VIII and X is sufficiently exact to form the basis of part of Wurzel's purely synchronic account, and, according to Keller, it contributed to the original stabilisation of type X in early modern German. The obvious question then arises: at what point did the macroparadigm analysis cease to be feasible? A better way of putting the question might be: what is it about the macroparadigm analysis which makes it too 'opaque' for all or some native speakers to achieve, so that the stem-allomorphy analysis becomes preferable? A superficial answer might be: the sort of macroparadigm presented in (737), with three sorts of conditioning in operation, is too complex to be grasped and internalised. But at what point does a complex analysis become too complex? I will not try to answer this question here. An adequate answer will obviously require a careful study of the inflexional behaviour of the nouns in question over several hundred years and probably also a comparative study of their development in various dialects or Umgangssprachen. Whether the detailed results of such a study will tend to support either of our two approaches to reconciling type X with the Paradigm Economy Hypothesis remains to be seen. But it is legitimate to claim that our relatively cursory examination of type X nouns here does confirm the Hypothesis to this extent, that an inflexional pattern which constitutes a *prima facie* problem for paradigm economy seems to present a problem to speakers of German too; and it would not be surprising to find that different speakers internalised different solutions to it, achieving paradigm economy in different ways.

I have said nothing so far about the inflexionally unique noun Herz 'heart', which resembles Name in all Cases except the Accusative: Herz, not "Herzen". This presents no serious problem.

Herz is Neuter, whereas Name and all other nouns of type X are Masculine. There is therefore no difficulty in assigning Herz and Name to the same macroparadigm, with their inflexional divergence accounted for by the Gender contrast. It is no accident, of course, that the special behaviour of Herz in the Accusative renders its Accusative and Nominative forms homonymous; in no Indo-European language do Neuters distinguish these two Cases morphologically. But that remark brings us to the subject-matter of the next chapter,

Footnotes to Chapter VII

1. It is fortunate for my argument, in fact, that we do not find consistent correlation with Case rather than Number; for that would conflict with some of what is said about 'slabs' in section 5.3.
2. I say 'in most instances' rather than 'in all instances' here because of ambiguous forms such as 3 Sg and 2 Pl bebt in (708) and Nom and Acc Pl marutaḥ in (709). Interestingly enough, the ambiguity is resolved in the left-hand paradigms: gibt versus gebt and rājānaḥ versus rājānaḥ. I will have more to say about this sort of example presently.
3. I assume that the choice between tien- and ten- is phonologically determined on the basis of stress, but the issue is not important here.
4. The pseudo-Latin forms here are of course stressed according to the usual Latin stress rule. This rule, to which there are almost no 'surface exceptions', is: stress the penultimate syllable if that syllable is heavy, otherwise the antepenultimate. For more discussion, see e.g. Allen (1973).
5. I have drawn information on Spanish from Ramsey (1902). The Tense which I call Preterite, to preserve the parallel with Italian, he calls Aorist; quite probably the Tenses in the two languages are not exactly parallel syntactically, but this does not affect my argument here.
6. Almost, not quite, a direct reflex because the diphthong -ie- points to ^h-essem rather than -issem. But the important point is that hiz- is a reflex of Latin fēc- rather than fac-.
7. I say 'in most of the forms' rather than 'in all' because of the facts referred to in footnote 2. I will be returning to these facts below.
8. Risch's (1977) circular arrangement of the Latin declensions seems to presuppose the psychological reality of degrees of similarity between paradigms. Of course, such similarity may be a sign of actual identity at the underlying phonological

level, and Lieber (1980: 127-130) argues briefly for this way of handling the Latin first and second declensions, attributing the surface differences mainly to the contrast between a theme vowel /a/ and a 'theme glide' /w/. I do not think this will work without intolerably arbitrary 'minor' phonological rules and rules of theme vowel distribution; but it is not essential to settle the point here.

9. I am excluding here the highly irregular avere 'have' and sapere 'know', in which the stem and ending coalesce in most Persons of the Present Indicative.

10. The contrast between piacere and tacere in the 1st Plural is confirmed by Garzanti (1963) and Roncari & Brighenti (1940), so seems well established in at least one version of educated standard Italian. Another phonologically similar verb, giacere 'lie', seems to vacillate, and on other verbs the authorities differ as to whether the stem used in the 1st Plural is 'light' or 'heavy'.

11. There is a phonological assumption underlying the third column in (723) which some might question. The stem in finisco [fi'nisko] can only be called the same as that in finisci [fi'niʃi] if we recognise a synchronic rule of velar softening in Italian, or if we regard the distribution of distinct underlying forms /'isk/ and /'iʃ/ as phonologically determined. The latter, at least, is very plausible; but even if we deny it, the main point -- that the multi-paradigm approach requires three paradigms in (723) -- is unaffected.

12. It would be more accurate to say 'the unambiguous realisation of Nom Pl and Acc Pl, insofar as this is achieved, ...'. This is because the Acc Pl of rājā, just like that of marut, is still homonymous with the Gen Sg (rājāḥ and marutaḥ respectively), as (709) shows. But this detail does not affect the point about the morphosyntactic importance of the stem allomorphy in rājā.

13. The Inflexional Distinctness Criterion is, of course, imprecise insofar as disagreements remain about phonological analysis; but, as I have said before, there are enough examples which are phonologically uncontroversial in relevant respects to enable

us to avoid most of the drawbacks of this imprecision, for present purposes. See further sections 1.8 and 4.2.

14. 'Nearly all', because one might argue that the Nom Pl ending -a, when attached to Masculines, is intrinsically stressed.

CHAPTER VIII

A CONSTRAINT ON MANY-TO-ONE EXPONENCE; HOMONYMY, SYNCRETISM AND
ATTRACTION8.1 Introduction: previous approaches to homonymy

So far we have been looking for constraints on Deviation II (instances of paradigmatic one-to-many relationships between morphosyntactic properties and their exponents). I have presented evidence for constraints of two kinds; one involving syntagmatic factors (the Peripherality Constraint) and one involving paradigmatic ones ('paradigm economy' in the sense of the Paradigm Economy Hypothesis). I do not claim to have said all there is to say about these two constraints; indeed, there are almost certainly further constraints on Deviation II whose nature I have not even guessed. But I have at least shown that there are some logically conceivable sorts of behaviour involving Deviation II that seem never to occur, and that the actual inflexional behaviour of a variety of languages is therefore hard to reconcile with the pessimistic view that, so far as Deviation II goes, inflexional morphology is a free-for-all.

I want now to pass to the consideration of Deviation IV (paradigmatic many-to-one relationships between morphosyntactic properties and their exponents, or homonymy within inflexional paradigms). I will present arguments in this chapter to the effect that Deviation IV, as actually observed, is subject to constraints too. This suggestion is less novel, and, superficially at least, we are now entering upon more well-trodden ground. There has been a fair amount of interest in inflexional homonymy in recent decades, focussing on three main issues:

- (a) the parallel between morphological and lexical homonymy on the one hand and phonological 'neutralisation' on the other;
- (b) the limits to ambiguity, and how ambiguity is resolved;
- (c) relationships between morphosyntactic properties

on the 'plane of content' which either favour or inhibit homonymy in their inflexional realisations.

My preoccupation is somewhat different from all these, however, as I shall explain.

Since at least the 1930s linguists have noted certain similarities between 'neutralisation' in phonology and homonymy or 'syncretism' in inflexion. In 1957 the Institut de Linguistique in Paris published the replies of more than forty linguists to a questionnaire devised by Martinet about the notion of neutralisation in morphology and the lexicon. This did not turn out to be a very profitable exercise (see TIL 1957). Insofar as one can generalise about the mass of views expressed¹, it seems fair to say that the main preoccupation was with terminology rather than empirical claims; one finds much discussion of whether this or that morphological or lexical phenomenon resembles phonological 'neutralisation' sufficiently closely to deserve the same label, and of what in general the criteria for applying the term 'neutralisation' outside phonology should be, but one finds little in the way of generalisation about what morphological 'neutralisations' are possible and what are not. This is not because empirical generalisations of this kind were sought and not found, but rather because the linguists replying to the questionnaire did not see it as an opportunity to undertake the sort of investigation that we are engaged in here.

Apart from the criteria for applying the term 'neutralisation', what chiefly interested most respondents was how the ambiguities or potential ambiguities arising from neutralisation are resolved. This is not surprising, in view of the long tradition of linguistic and philosophical debate about the limits to ambiguity of all kinds, lexical and syntactic as well as morphological. The question of how ambiguities are resolved is explicitly to the fore in the study of 'neutralisation' in Kasem by Callow (1968), who is strongly influenced by the ideas of Pike, and is also prominent in Pike's own (1965) work on morphological 'matrices' in German. More recently, Frans Plank (1979; 1980) has applied

a range of data, particularly from late Latin and Romance languages, to the question: can one attribute the nonexistence of certain inflexional homonymies or syntactically ambiguous constructions (or, diachronically, their removal through morphological or syntactic innovation) to the necessity to avoid certain intolerable ambiguities? Insofar as his work deals with inflexional homonymy, then, Plank is concerned with identifying conditions under which it is impermissible, or sufficient conditions for its avoidance.

Other linguists, by contrast, have concentrated on the other side of the coin, searching for conditions which favour inflexional homonymy or 'syncretism'. Hjelmslev (1935) asserted the need for a general theory of "les lois générales qui dirigent le phénomène du syncrétisme, et qui permettraient de prédire [his emphasis] les syncrétismes possibles et les syncrétismes nécessaires d'un système donné". Although in this remark 'syncrétisme' is implied to be a phenomenon of change, Hjelmslev sees clearly that uncovering the 'lois générales' will involve developing an 'explication synchronique [my emphasis]' for the phenomenon (page 60). His own general theory of syncretism was to have been one of the main topics of a promised sequel to La catégorie des cas (see (1935: iv)) which regrettably never appeared². At about the same time, Jakobson devoted section IX of his classic article 'Beitrag zur allgemeinen Kasuslehre' (1936) to a series of generalisations about syncretism within the Russian declension system, e.g.: "Unterscheiden sich der N[ominativ] und der A[kkusativ], so ist entweder der Unterschied A[kkusativ]-G[enitiv] oder der entsprechende Unterschied D[ativ]-I[okativ] aufgehoben". Although Jakobson's generalisations are language-particular, he clearly hoped that in the long run they would emerge as consequences of a general theory of 'Gesamtbedeutungen' in a Case system of the Russian type. It is therefore clear that, in spirit at least, Jakobson was working towards the same goal that Hjelmslev had set. It is also clear that the main focus of Jakobson's interest was the 'plane of content' rather than the link between 'content' and 'expression' -- that is, he was interested in relationships between morphosyntactic properties themselves rather

than between properties and their inflexional realisations. For Jakobson, in his discussion of syncretism, the only fact that is of interest about the shapes of the Russian inflexions is which pairs or groups of them are homonymous; he is not concerned with the type of deviation that they exhibit (in the sense of Chapter I)³. Our concern in this thesis, however, is precisely with the search for constraints on those types of deviation. It follows that, even if the sort of goal that Jakobson had in view is ultimately reached and a general theory of 'Gesamtbedeutungen' incorporating certain constraints on syncretism is achieved, the question will still remain whether there are any further constraints connected with the relationship between properties and their exponents. And it is this latter question which will concern us.

The linguist whose approach to syncretism seems closest to ours is Bazell, who, in his article 'A question of syncretism and analogy' (1960), alleged a tendency towards homonymy in instances where a minimal morphosyntactic contrast coincides with a minimal phonological contrast between the relevant inflexions. This claim certainly involves the relationship between morphosyntactic properties and their exponents, although in order to make it precise one would need a reasonably clear idea of what constitutes a 'minimal' contrast. But it is a claim quite independent of those which I shall be putting forward, as will shortly become clear.

8.2 The structure of the argument

This chapter, then, will be devoted to the search for evidence for general constraints on property-to-exponent relationships involving Deviation IV. I will, in fact, propose a generalisation to the effect that every instance of systematic inflexional homonymy must display one or other of two sets of clearly specifiable characteristics. The word 'systematic' here is important. I will suggest that inflexional homonymies fall into two classes, the first class consisting of sets of forms where the combined effect of morphological 'spell-out' rules and phonological rules just happens to be an identical phonetic representation,

and the second class consisting of sets of forms whose homonymy reflects some 'deeper' principle which must be explicitly incorporated in some way in any grammatical description which purports to be complete. Only homonymies of the second kind, I suggest, are subject to the generalisation I propose. This amounts to saying that I will specify necessary conditions for any homonymy to be considered systematic; it must comply with the proposed generalisation. But clearly, if such a proposal is to have any empirical content, an important prerequisite must be satisfied; it must be possible to determine readily in a reasonable number of instances whether a given homonymy is 'systematic' or 'accidental' independently of how it fits the generalisation. Unless we can determine this, the argument will be circular.

My argument will therefore take the following form. I will first argue that the distinction between systematic and accidental homonymies is well motivated, and that a reasonable number of clear cases can be identified on either side. I will then argue that there are at least some instances of systematic homonymy where the principle at work has to be regarded as morphosyntactic rather than purely syntactic, and where consequently the search for generalisations or constraints falls squarely within the scope of this thesis. At that point I will invite the reader to look at the list in Appendix E of more than forty inflexional homonymies, and point out a characteristic that many of them share. This will lead me to pose a question about the possible function of systematic homonymy, comparing and contrasting two versions each of two hypothetical Case-Number systems, one version illustrating homonymy and one not. The result of this discussion will be a link between homonymy and simplicity -- specifically, a contrast between conceivable inflexional patterns which, according to a straight-forward criterion of 'simplicity', are rendered simpler by the introduction of an element of Case homonymy, and conceivable inflexional patterns which such homonymy renders less simple. This argument is then applied to the homonymies in Appendix E in the following way: a great majority are found to belong to inflexional patterns of the kind in which homonymy contributes to sim-

plicity, not complexity, according to the criteria applied to our earlier hypothetical examples. I therefore introduce a new definition of 'syncretism', in accordance with which we can now say that the great majority of systematic homonymies in the data presented are syncretisms. Turning to the remainder of the apparently systematic homonymies --- those which are not 'syncretisms', according to the new definition ---, I will argue that they are nearly all examples of 'attraction' (a term I define in due course) as well as having further features in common. We thus arrive at an empirical generalisation: all systematic homonymies are either syncretisms (as defined) or attractions, with those further common features just referred to.

It will be seen that the claims I make in this chapter are related to the relevant linguistic data less directly than the claims I made in the previous chapters. The Peripherality Constraint, for example, is related to the relevant data in such a way that any instance of outward sensitivity to an individual morphosyntactic property rather than to a whole category will be a direct counter-example to the Constraint. On the other hand, an instance of inflexional homonymy which is neither a 'syncretism' nor an 'attraction' will not constitute a direct counter-example to my claims about necessary conditions for systematic homonymy, but will simply be classified as an accidental homonymy, not a systematic one; and, since I define 'accidental' and 'systematic' homonymy only implicitly by illustration rather than rigorously and explicitly, it may seem that the whole package of claims is intrinsically impossible to falsify and thus devoid of content. But this is not so; the difference is that the appropriate test for my claim about homonymy lies in how well it copes with a mass of data, not with individual facts. If nearly all the homonymies that most linguists would be inclined to regard as systematic emerge as syncretisms or attractions, and if nearly all the homonymies which are neither syncretisms nor attractions look as if they could reasonably be attributed to factors other than some specifically morphological principle of organisation (for example, independently motivated processes of phonological neutralisation),

then my empirical claim satisfies the test even if in a small minority of instances we are forced to classify as accidental a homonymy which we might otherwise be inclined to regard as systematic. The situation is somewhat similar to that which arises in syntactic research when a linguist 'allows the grammar to decide' whether certain rather doubtful sentences are grammatical or not. The fact that the syntactician ^{treats} some doubtful data in this way does not of itself vitiate his account of those data whose grammaticality is not in doubt.

I mentioned the need to test claims of the kind put forward here against a mass of data rather than against individual facts. But for such a test to be effective, we must obviously be confident that the mass of data which we use is not skewed or biased in some way which might affect the result. I will argue later that there is no reason to suppose that my actual data are skewed in this way, and that at the very least the onus of proof is on the objector who wants to claim that they are; but I will readily admit that my claim about homonymy, just like all the empirical claims in this thesis, still stands in need of test against a larger corpus of relevant linguistic facts.

8.3 Systematic versus accidental homonymy

The morphological data from Latin in (801) illustrate behaviour common to all Latin nouns, pronouns, adjectives and participles, namely the lack of any overt morphological distinction between the Dative and Ablative Cases in the Plural:

(801)	Singular		Plural
a. Dat	mensae 'table'	}	mensīs
Abl	mensā		
b. Dat	servō 'servant'	}	servīs
Abl	servō		
c. Dat	rēgī 'king'	}	rēgibus
Abl	rēge		

(801) (continued) Singular		Plural
d. Dat	fīnī 'end'	} fīnibus
Abl	fīne	
e. Dat	manūī 'hand'	} manibus
Abl	manū	
f. Dat	faciēī	} faciēbus
Abl	faciē	
g. Dat	mihi, mī 'me'	} nōbīs 'us'
Abl	mē	
h. Dat	illī 'that (one), him.'	} illīs 'them'
Abl	illō	

In traditional terms, this is described by saying that there is 'syncretism' of the Dative and Ablative Plural everywhere in Latin. This homonymy is stable over time; that is, at no period in the recorded history of Latin does any noun, adjective, participle or pronoun display distinct forms for these two Cases in the Plural. It is reasonable to conclude that we are dealing with something systematic and genuinely part of what the native Latin speaker 'knew', not a linguist's construct or a mere accidental homonymy between two Case-forms. (At the very least, the onus of proof is on the linguist who wants to contend otherwise.) (801) can be compared with the German verbal data in (802):

(802) . . .		Singular	Plural
a. 1st	bin 'am'	}	sind 'are'
3rd	ist 'is'		
b. 1st	habe 'have'	}	haben 'have'
3rd	hat 'has'		
c. 1st	liebe 'love'	}	lieben
3rd	liebt		
d. 1st	esse 'eat'	}	essen
3rd	isst		

(802) (continued)	Singular		Plural
e. 1st	wasche 'wash'	}	waschen
3rd	wäscht		

In both (801) and (802), we find a pair of properties which, however they may be realised in the Singular (and, in the Latin examples especially, a considerable diversity of realisations is evident, at least 'on the surface'), are always realised homonymously in the Plural.

It is clear that thorough-going inflexional homonymies of this kind, which apply to all members of a given part of speech, can be productive. Evidence of this can be found by comparing Serbo-Croat with the closely related South Slavic language Slovenian. In Slovenian, as in Eastern and Western Slavic languages generally, Dative, Instrumental and so-called 'Locative' Cases are morphologically distinct in the Plural of nouns. In Serbo-Croat, although Dative and Locative forms are no longer distinct anywhere, so that there is no longer any justification for recognising these two Cases as morphosyntactically distinct (whether or not they may be distinct at some 'deeper' level), Instrumental and Dative-Locative are still distinct in the Singular of most nouns. In the Plural, however, the Dative-Locative is always homonymous with the Instrumental. Yet this homonymy is not explicable historically as due to purely phonological developments. Compare the earlier and later versions of the paradigm of seljak 'peasant' (Thomason 1976: 373-378):

(803)	About 1250 AD	Today
Sg N	seljak	seljak
V	seljače	seljače
A	seljaka	seljaka
G	seljaka	seljaka
I	seljakom	seljakom
D	seljaku	seljaku
L	seljace	seljaku
Pl N	seljaci	seljaci
V	seljaci	seljaci

(803) (continued)

Pl A	seljaki		seljake
G	seljak		seljakā
I	seljaki	}	seljacima
D	seljakom		
L	seljacex		

On this, Thomason comments: "Like the replacement of + e by + u in the singular, the replacement of loc. pl. + ex by + ima was the result of a complex analogical process [my emphasis]". The term 'analogical' here is implicitly contrasted with 'phonological'; the acquisition by Dative-Locative and Instrumental Plural of a common ending -ima (probably 'borrowed' from the Dual) cannot be explained purely in terms of phonological processes affecting the earlier endings -om, -ex and -i. But this amounts to saying that at some point a specifically morphological relationship came to be established in Serbo-Croat between the way the two Cases Instrumental and Dative-Locative were realised in the Plural; diachronically, the homonymy is not simply a by-product of phonological change. We therefore have historical confirmation for what the synchronic generality of the homonymy throughout Serbo-Croat declension suggests, namely that the homonymy is genuinely part of the linguistic system and not merely the accidental result of, say, phonological neutralisation.

We have evidence, then, that at least some inflexional homonymies are systematic, in the sense of being more than mere accidental by-products of phonological processes or morphological 'spell-out' rules. But are all such homonymies systematic? On the basis of comparison with other areas of grammar, namely syntax and the lexicon, the expected answer is no. In lexicalised metaphors such as 'the foot of the mountain', 'the mouth of the river', 'the shallowness of his thinking', the use of the words foot, mouth, shallowness will be felt by all or nearly all speakers to have some connexion with their non-metaphorical or concrete uses, and will be treated accordingly both in dictionaries and in any linguistic theory of the lexicon. On the other hand, no speaker feels any connexion other than a purely phonetic one

beer 'alcoholic drink made from barley' and bier 'conveyance for coffins', or between the noun row meaning 'line, series' and the verb row meaning 'propel with oars'. There is no systematic relationship here for dictionaries or descriptions of the English lexicon to capture; and there is no reason to quarrel with the traditional analysis under which beer and bier (and row noun and row verb) are treated as distinct lexical items which merely happen to overlap phonologically. But if accidental homonymy is possible between distinct lexical items, why not between the realisations of distinct morphosyntactic properties too?

It is easy enough to find instances of inflexional homonymy which it is plausible to interpret as morphosyntactically accidental. Consider (804):

- | | | |
|--|---|--|
| (804) a. English: Present: strike | } | hit |
| Past: struck | | |
| b. English: Plural: women | } | ladies/ladies'
['leidiz] |
| Possessive: women's ⁴ | | |
| c. Turkish: Accusative: bahçe-yi
'garden (Acc)' | } | ev-i
'house (Acc)'
or 'his/her
house' |
| 3 Sg Poss: bahçe-si
his/her garden' | | |
| | | |
| d. Dyirbal: (Dixon 1972: 42): | | |
| Ergative: yara-ngu
'man (Erg)' | } | yamani-gu
'rainbow (Erg
or Dat)' |
| Dative: yara-gu
'man (Dat)' | | |
| e. German: Nom Pl: Gäste 'guests' | } | Gärten
'gardens' |
| Dat Pl: Gästen | | |

In all these instances, the homonymy in the right-hand column can be explained in terms of phonologically conditioned allomorphy, so that **it** can be regarded as morphosyntactically irrelevant. By this I mean that the inflected forms on the right have phonological characteristics which engender the homonymy as a by-product either of a general phonological process or of an alternation in which the choice between the alternants is determined by purely phonological characteristics of the environ-

ment (as happens with the Hungarian realisations -(a)sz and -ol for the property-combination 2nd Person Singular in the Present Indefinite Indicative of the 'normal' conjugation, often referred to before). Which of these two kinds of phonological explanation is appropriate in any given example in (804) does not matter for our present purposes; what does matter is that one or other of them can be invoked for each one. For example, in the German example at (804 e) we are entitled to say that the homonymy between Nominative and Dative in the Plural is phonologically explicable since (apart from those nouns with Plurals in -s) it is limited not to some lexically arbitrary or syntactically determined class of nouns but to precisely those nouns whose Nominative Plurals end in unstressed -en; and, for our purposes, it does not matter whether this is accounted for within the phonological component by a rule of 'degemination' affecting the sequence /n + n / (as Wurzel (1970) proposes) or by a 'spell-out' rule which assigns Dative Plural an overt realisation -(e)n only in certain phonological environments.

I will explain why the homonymies in the remaining examples (804 a-d) may also be regarded as morphosyntactically accidental. The verb hit which appears in the right-hand column of (804 a) belongs to the small class of verbs which, like cut, put, set, display no morphological distinction between Present and Past Tense forms. Now, membership of this class is, synchronically at least, arbitrary in the sense that there is no way of predicting that (for example) hit is a member of it but the verb fit is not, having a Past Tense form fitted rather than "fit". But, as is well known, the hit class verbs share an obvious phonological characteristic: they all end in -t. It is therefore natural to look for some phonological explanation for the Present-Past homonymy; and, since the Past Tense of so many English verbs involves an affixed [t] or [d], a plausible explanation on these lines is not hard to find. One might say, for example, that the hit class is simply a subclass of those semi-irregular verbs which, like smell, learn and build, either obligatorily or optionally take -t rather than -(e)d as their Past Tense affix,

whereas fit goes along with fell, earn and gild⁵ in requiring -(e)d; and that the only reason why the Past of hit is realised as [hit] rather than [hitt] is the phonotactic one that English does not allow long or geminated consonants in word-final position. The details of this explanation in terms of phonologically conditioned allomorphy do not matter; what matters is that an explanation on these lines is plausible, and that to call the Present-Past homonymy here accidental from the morphosyntactic point of view is therefore legitimate in a way in which it would not be legitimate if, say, the homonymy extended to verbs ending in bilabial consonants or vowels, or if English did not have an independently needed Past Tense suffix -t.

The phonological explanation for (804 b) is similar. It is significant, I suggest, that the homonymy between Plural and Plural Possessive forms is limited to just those nouns which have a Plural in -(e)s. There is, of course, no general phonological (or, more precisely, phonotactic) bar in English to the sequences [siz] or [ziz] in word-final position, since words like buses, roses, Katz's ['kætsiz] and Jones's ['dʒounziz] are perfectly well-formed phonologically; consequently, there is no general phonological bar to nonexistent Possessive Plural forms such as "diplomats's" or "ladies's". Nevertheless, the fact that the Possessive-Nonpossessive homonymy in the Plural is limited to just those nouns in which the exponent of Plural is a sibilant suffix (rather than being characteristic of a heterogeneous class containing some sibilant-Plural and some non-sibilant-Plural nouns) allows us to say that the alternation between 's and \emptyset as exponents of Possessive on Plural nouns is phonologically conditioned and therefore morphosyntactically uninteresting.

In Turkish (example (804 c)) the homonymy between the Accusative and 3 Sg Possessive suffixes observable with consonant-final stems is not found with vowel-final stems; and, whether we account for this by appeal to deletion, epenthesis or underlying allomorphy, the fact that the homonymy is restricted to a phonologically specifiable class of stems again suggests that from a

morph^osyntactic point of view it should be regarded as accidental. And the Dyirbal example at (804 d) is in relevant respects exactly parallel. The Dative suffix is -gu for all nouns; the Ergative suffix, on the other hand, has a range of exponents -ngu, -gu, -bu, -du, -du and -cu, the choice between which is determined phonologically. Clearly, those nouns whose phonological characteristics are such that -gu is the appropriate realisation of Ergative (of which yamani 'rainbow' is one) will display no overt morphological contrast between the Ergative and Dative Cases; but this is no more significant within Dyirbal, I suggest, than the facts in English that hit can be either Present or Past Tense, or that [biə] can 'realise' either beer or bier.

There are, then, a reasonable number of inflexional homonymies which can on more or less strong grounds be regarded as mere by-products of independent morphological or phonological rules or processes, and not as embedded systematically in the complex of rules which provide inflexional realisations for morpho-syntactic properties or combinations of them. This does not mean, however, that we will always without hesitation be able to assign inflexional homonymies to either the systematic class (along with examples (801) and (802)) or the accidental class (along with (804)). To illustrate the sort of doubts which may arise, I will turn again to examples from Latin.

Consider first example (801 b), repeated here for convenience:

(801)		Singular		Plural
	b. Dat	servō	'servant'	} servīs
	Abl	servō		

Here we see that in the Singular as well as in the Plural the Dative and Ablative Case-forms of servus are homonymous. This is true of all Latin nouns and adjectives belonging to the large and productive 'second declension', but it is not true of the other declension-types except, to some extent, that of (801 d). Have we a systematic homonymy here or not? At least we can say that this homonymy is quite general within a subset of Latin

nouns identified by clear independent criteria, namely their distinctive set of inflexional endings. What then of the homonymy illustrated in (805 a) below?

(805) Singular:		Singular:	
Nom	Gen	Nom	Gen
a.	<u>canis</u> 'dog'	b. dux	ducis
	ignis 'fire'	'general'	
	collis 'hill'	custōs	custōdis
	civis 'citizen'	'guardian'	
		cīvitās	cīvitātis
		'citizenship'	
		mens	mentis
		'mind'	
c.	consul	consulis	
	'consul'		
	pater	patris	
	'father'		
	caput	capitis	
	'head'		

The homonymy here is just as stable during the classical Latin period as those mentioned before. But no Latin scholar would regard it as being on a par with the homonymy between the Dative and the Ablative Plural. This is not pure prejudice. The nouns of (805 a) do not constitute a well-defined distinct grouping, like the second declension. Rather, they are all, along with those of (805 b) and (805 c), part of the superficially somewhat heterogeneous third declension, and share no distinctive morphological or other characteristics except the Nominative-Genitive homonymy. Moreover, I presented evidence in Chapter VI for saying that, in Golden Age Latin, the paradigm of (805 a) nouns is identical for purposes of paradigm economy with that of (805 b) nouns, which lack the homonymy in question; certainly, from the historical point of view, a large number of the nouns in type (805 b) formerly displayed this homonymy but lost it through the operation of a rule of syncope which was restricted to the Nominative. Finally, Nominative-Genitive homonymy does not occur in any other nominal or pronominal paradigm in Latin, whether Singular or Plural, nor in any adjectival paradigm except precisely that of e.g. brevis 'short', gravis 'heavy', which 'go like' the

noun ignis. It seems clear that some line needs to be drawn between the Plural homonymy in (801) and that in (805 a), and that the latter can reasonably be called accidental; but which side of the line the Singular Dative-Ablative homonymy of servus in (801 b) should fall is not obvious.

A somewhat similar problem arises in Russian. I have already mentioned Jakobson's concern with homonymy in his 'Beitrag zur allgemeinen Kasuslehre'. Although he is concerned with generalisations about properties on the 'plane of content' rather than about the relationship between content and expression, it is just as important for him as it is for us (and perhaps even more important) to be able to distinguish systematic homonymies from accidental ones; and, in fact, he has no qualms about labelling certain Russian homonymies accidental. Thus (1936: 52): "jede Endung des Instrumentals Sing. masc. fällt bei den russischen Adjektiva mit der Endung des Dativs Plur. zusammen (zlym, božjim); jede Endung des Nominativs Sing. masc. fällt bei den qualitativen Adjektiven mit der Endung ihres Genitivs Sing. fem. zusammen (zloj - zloj, staryj - staroj, tichij - tichoj, sinij - sinej; die graphischen Unterscheidungen sind künstlich), und nichtsdestoweniger ist die Getrenntheit der grammatischen Kategorien in jedem dieser Fälle ausser Zweifel. Das sind bloss Paare homonymer Formen [my emphasis] ...". In Jakobson's terms, clearly, 'mere pairs of homonymous forms' correspond to our accidental homonymies, and are to be distinguished sharply from the examples of systematic 'Kasussynkretismus' which he discusses in section IX of his article. Yet, according to Jakobson himself, the putatively accidental adjectival homonymies that he mentions are more, not less, pervasive than the instances of 'syncretism' that he treats in section IX, since the latter only occur in certain nouns and adjectives while the former occur in all adjectives without exception. Nor is any obvious phonological explanation available for these 'accidental' homonymies, on the lines suggested for the examples in (804). The truth is that Jakobson's instances of 'mere homonymy' are not distinguished from his 'Case syncretisms' by any criterion apart from the rather vague one of 'Getrenntheit der grammatischen Kategorien'; so 'Synkretismus'

looks in some danger of becoming a mere label for those homonymies where some common element of meaning or function can be established within the framework of the 'allgemeine Kasuslehre'.

What these Latin and Russian examples show, then, is that inflexional homonymies exist whose status -- whether accidental or systematic -- is hard to determine on the basis of any clearly specifiable criterion. But this need not inhibit our search for generalisations about those whose status is clear; and any generalisations that we establish may themselves help to decide the status of the unclear instances. After all, unclear instances of homonymy in inflexion have parallels in the lexicon and in syntax. For example, no one would deny that the English nouns beer and bier, already mentioned, are distinct lexical items, although homonymous; it is not altogether obvious, however, whether ear 'organ of hearing' and ear 'seed-bearing part of cereal plant' should be treated as distinct, homonymous items or whether the latter should be treated as a specialised, quasi-metaphorical use of the former (cf. 'the eye of the storm', 'the mouth of the river'). (Historically, as it happens, the two uses of ear are definitely distinct, cognate with German Ohr and Ähre respectively. But this is, of course, irrelevant for determining how the native speaker perceives them in modern English. See Bloomfield (1935: 436) for discussion.) Similarly, few would deny that Chomsky's famous sentence Flying planes can be dangerous is ambiguous or, in other words, that associated with it are two distinct syntactic or syntactic-semantic representations which are realised homonymously. On the other hand, it is not obvious whether the 'specific' and 'nonspecific' readings of a sentence like I am looking for a woman with green eyes (in Spanish, Busco a una mujer ... versus Busco una mujer ...) should be treated as belonging to two grammatically distinct but homonymous sentences in English (in the widest sense of 'grammar') or whether they are grammatically identical in all respects.

We can sum up our discussion of accidental and systematic inflexional homonymies, then, by saying that both types clearly

exist, and the fact that we do not yet have explicit criteria for assigning certain problematic examples to one type or the other does not vitiate the distinction, any more than the existence of problematic examples in syntax, semantics and the lexicon vitiates the distinctions between pairs of sentences which share a semantic or syntactic structure and pairs which do not, and between pairs of word-forms which belong to the same lexical item and pairs which belong to different lexical items. We can now go on to explore further characteristics of the two sorts of inflexional homonymy we have identified, and particularly of the systematic sort. But before we do so, we need to answer a prior question: is systematic inflexional homonymy a morphosyntactic phenomenon at all, or is it rather a syntactic one? This question is important, because if the answer is that all systematic inflexional homonymies involve the complete disappearance of a morphosyntactic property distinction rather than the homonymous realisation of distinct properties, then it is at least questionable whether we ought to be discussing homonymy at all in the context of a search for constraints on the relationship between morphosyntactic properties and their realisations; for this answer to our question will imply that Deviation IV (at least as a systematic phenomenon) does not exist. The next section will therefore be devoted to showing that systematic inflexional homonymies can indeed be morphosyntactic rather than purely syntactic.

8.4 The morphosyntactic status of systematic homonymy

The two cardinal instances of systematic homonymy so far cited ((801) and (802)) share one important characteristic which distinguishes them both from the instances of allegedly accidental homonymy cited in (804) and from the more problematic Latin examples of Dative-Ablative homonymy in the Singular of the second-declension nouns and Nominative-Genitive homonymy in certain third-declension nouns ((801 b) and (805 a)). This shared characteristic is the generality of the homonymy within the parts of speech concerned: all Latin nouns, pronouns, adjectives and participles have homonymous Dative and Ablative Plural endings,

and all German verbs (even the highly irregular sein 'be') . . . have homonymous 1st and 3rd Plural forms in all Tenses and Moods. This characteristic is indeed the main reason for calling these homonymies systematic. Yet it also provokes the question whether in these instances it is really homonymy that we are dealing with. What is to stop us from saying that, in Latin words which inflect for Case, the Cases Dative and Ablative are just not applicable in the Plural, or, in other words, that the putative property combinations 'Dative Plural' and 'Ablative Plural' are realised homonymously in all paradigms because they do not exist as separate morphosyntactic 'slots' at all? And a parallel question can be asked about the German example.

We have broached here, in effect, the possibility of a new way of looking at the 'systematic-accidental' distinction. Those homonymies that we are least inclined to call systematic (e.g. those of (804) and (805 a)) are also, it seems, those where we are most confident about the genuineness of . . . the morphosyntactic distinction and therefore the existence of the homonymy. On the other hand, the homonymies that we are most strongly inclined to call systematic (such as that between Dative and Ablative Plural in Latin) seem also to be the instances where the genuineness of the morphosyntactic distinctions which underlie the alleged homonymies is most in doubt. Should we then simply treat 'systematic' as a not very appropriate label for homonymies which are apparent rather than genuine at the morphosyntactic level -- that is, instances where the purported morphosyntactic distinction does not exist?

Fortunately, there are empirical grounds for answering 'no'. First, consider Latin sentences such as the following:

(806) Mātrī et fīliae librum dedī
 mother-Dat and daughter-Dat book I-gave
 'I gave a book to the mother and daughter'

(807) Mātrī et fīliae in hortum ingressīs
 mother-Dat and daughter-Dat into garden entered-??

librum dedī
 book I-gave

'I gave a book to the mother and daughter when they had entered the garden'

(808) \bar{A} $\bar{m}\bar{a}t\bar{r}e$ et $\bar{f}\bar{i}l\bar{i}\bar{a}$ in hortum $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$
 by mother-Abl and daughter-Abl into garden entered-??
 liber acceptus est
 book received was

'The book was received by the mother and daughter
 when they had entered the garden'

(806) contains a straightforward conjoined noun phrase, unmodified, whose two conjuncts are both in the Dative Singular. In (807), however, the conjoined noun phrase is modified by a participial phrase containing one element ($\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$) which would traditionally be described as agreeing with it in Number and Case. But the traditional account is hard to square with the new approach to systematic homonymy that we are now considering, according to which 'systematic homonymy' does not involve homonymy at all at the morphosyntactic level. The conjoined noun phrase in (807) is syntactically Plural, as is usual in Latin, and $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ is, not surprisingly, Plural too. But, although each conjunct of the noun phrase is unequivocally Dative, we are not permitted under this approach to describe the participle $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ which agrees with it as Dative too, because ex hypothesi the Dative Case is incompatible with the Plural Number, and the Case to which $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ belongs is one peculiar to the Plural which combines the functions of Dative and Ablative. This Plural-only Case also crops up in (808), where $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ modifies a conjoined noun phrase each of whose conjuncts is this time Ablative. We must therefore posit some sort of Case-change rule which will ensure that when either Dative or Ablative Singular nouns are conjoined the resulting noun phrase belongs to this peculiar Plural-only Case. But no such Case-change rule needs to be invoked for conjoined noun phrases whose conjuncts belong to Cases other than the Dative or Ablative. The only reason why we need such a rule is that our new approach to systematic homonymy commits us to claiming that $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ in (807) is indistinguishable from $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ in (808) not only 'after' its Case-ending has been spelt out but also 'before' -- that is, not only in morphology but also in surface syntax. No such contortions are imposed on us if we stay with our earlier assumption, under which the $\bar{i}n\bar{g}r\bar{e}ss\bar{i}s$ of (807) and that of (808) are allowed to be morphosyntactically distinct

word-forms whose homonymy is purely a matter of inflexional realisation.

At first sight the choice between the two approaches may seem to depend purely on considerations of 'simplicity' in description, with no obvious empirical consequences. But this is not quite so. As I said earlier, the kind of feature-changing rule which the second approach necessitates is not independently required in Latin. But we can easily envisage a hypothetical 'pseudo-Latin' extremely similar to Latin in which it would be required -- a pseudo-Latin, for example, in which a participle modifying a conjoined noun phrase with Nominative Singular conjuncts was inflected differently from a participle modifying a simple Nominative Plural noun, in that the modifier of the conjoined noun phrase was inflected like a modifier of a simple Plural noun phrase whose Case was not Nominative but Accusative. In this hypothetical Latin-like language, we would find contrasts as follows:

(809) *Fīlliae* . . . in hortum *ingressae* sunt
 daughters-Nom into garden entered-Nom are
 '(Her) daughters entered the garden'

(810) *Māter* et *fīlia* . . . in hortum *ingressās*
 mother-Nom and daughter-Nom into garden entered-"Acc"
 sunt
 are

'The mother and daughter entered the garden'

A special rule would be needed in this language to ensure that the conjoined noun phrase differed syntactically from the simple noun phrase in such a way as to trigger the difference in concord behaviour, and differed in a way which could be most naturally accounted for by changing the Case of the conjoined noun phrase from Nominative to Accusative. 'Feature-changing' rules of broadly this kind are by no means unknown; for example, in Icelandic such a rule is needed to ensure that, when a conjoined noun phrase some of whose conjuncts are Masculine and some Feminine has a predicate adjective agreeing with it, the Gender of the adjective will be Neuter (Einarsson 1945: 133)⁶. But the fact that actual Latin does not exhibit the sort of behaviour

illustrated in (809) and (810) militates against any invocation of feature-changing rules of this kind to cope with the Dative-Ablative Plural, since in actual Latin (as opposed to pseudo-Latin and Icelandic) such rules would have no independent motivation.

Despite the consistency of the pattern illustrated in the right-hand column of (801), then, there is good evidence for saying that the properties Dative and Ablative are indeed applicable in the Plural in Latin, just as much as in the Singular, and that the identity of form there is indeed due to a real systematic morphological homonymy. But it is still logically possible that there may be homonymies that are not real, in this sense -- that is, instances where the morphosyntactic property distinction presupposed by the use of the term 'homonymy' does not exist. The arguments one can adduce for the existence of homonymy in word-forms such as ignis (Nominative-Genitive) and servō (Dative-Ablative) rely on the fact that these word-forms are used in sets of syntactic environments clearly subdivisible on the basis of overt morphological distinctions exhibited by other members of the same part of speech. But the expression 'other members of the same part of speech' is vague. How many other members are needed? Is just one enough? For an example of a practical difficulty caused by this vagueness we can look again at Latin.

Morphologically and syntactically, a Genitive Case is extremely well motivated in Latin for nouns, adjectives and participles, and the Genitive Plural is in fact formally distinct from all other Case-forms even more generally than the Genitive Singular is (which, as we have seen, is homonymous with the Nominative in nouns like ignis, and is also homonymous with the Dative in nouns such as mensa). Within the Genitive, there is no motivation for any more delicate distinction at the morphological level in these parts of speech. But with personal pronouns things are somewhat different. The Plural pronouns of the 1st and 2nd Persons (nōs and vōs) distinguish two Genitives: nostrum versus nostrī and vestrum versus vestrī. The syntactic functions

of these two forms are more or less clearly distinct. Ernout (1953: 103) says: "L'usage a distingué les deux formes: nostrum, vestrum s'emploient comme génitif partitif; pars nostrum, vestrum; nostrī, vestrī comme génitif objectif; miserēm nostrī 'aie pitié de nous'." Should we then say that Partitive and Objective Genitives should count everywhere as two distinct Cases, which happen to be realised homonymously with all nouns, adjectives, participles and pronouns except the two we have mentioned? The problem is in fact very similar to the one posed by the modern English pronominal forms I, he, she, they versus me, him, her, them; do we conclude from them that an underlying contrast between Nominative and Accusative Cases should be recognised for nouns too? Another problem of this kind is posed by the contrast exhibited only in the verb be between a Past form ('John was in in London yesterday') and an Irrealis form ('If John were in London today, ...'), which has been discussed by Huddleston (1975); on the strength of this one overt contrast, should we regard the morphosyntactic distinction as applicable to all English verbs?

To all these questions I suspect that (pace Huddleston) most linguists would answer no; the overt morphological distinctions are restricted to too few forms to justify positing a corresponding morphosyntactic distinction everywhere else.⁷ But most linguists would probably also be hard put to it to decide precisely where the line should be drawn -- that is, precisely how many overt morphological contrasts, and of what kinds, would justify generalising the morphosyntactic property distinction which these contrasts express. For our present purposes, the uncertainty does not matter. Just as with those homonymies which are not clearly either accidental or systematic, some of the problematic instances may ultimately be resolvable by reference to generalisations based upon the clear instances. For the time being, however, what matters is that clear instances do exist where a systematic homonymy must be recognised as a genuinely morphological phenomenon.

8.5

The data of Appendix E: a first observation

43 homonymies are listed in Appendix E. This list has no pretension to be a statistically respectable sample of inflexional homonymies drawn from all potential sources -- that is, from all inflected languages. It is, rather, a fairly random collection drawn from languages which I happen to know something about, predominantly Indo-European ones. Initially, therefore, there is bound to be doubt about the legitimacy of basing any general conclusions on it. But I will assume for the time being that these data are indeed an adequate starting-point for discussion; and I will argue in section 8.10 that the nature of the generalisations that seem to emerge from our discussion is such that they are unlikely to be spurious ones due to bias in the selection of the data. Superficially, this may seem a circular line of argument. But there is no logical guarantee that, having made our initial assumption about the adequacy of Appendix E, any general conclusion should emerge at all, let alone one which is intrinsically unlikely to be spurious. So the fact that general conclusions of that kind do emerge genuinely confirms the legitimacy of the initial assumption.

Columns 5 and 6 in Appendix E are particularly important. In these columns the morphosyntactic context for the homonymy is indicated; and in all instances of systematic morphosyntactic homonymy (as opposed to accidental homonymies and 'homonymies' which are syntactic or semantic rather than morphosyntactic in nature), some entry must occur in one or both of these two columns. The distinction between the two columns is that column 5 contains contexts consisting of morphosyntactic properties which are realised simultaneously with the homonymous properties (including, for example, Plural as the context for Dative-Ablative homonymy in Latin), while column 6 contains contexts which are wholly or partly non-simultaneous. To put it another way, in homonymies with an entry in column 5 Deviation III is always involved as well as Deviation IV.

Let us consider now the pattern of entries in columns 5

and 6. One conceivable pattern, and the pattern we would probably expect to find on the assumption that systematic homonymy is subject to no general constraints specifically to do with the property-exponent relationship, is that of a roughly even distribution of entries, with about the same totals in column 5 and column 6. Yet this is not what we observe in Appendix E. For the great majority of entries (33 out of 43), the conditioning context includes properties which are simultaneous, and hence specified in column 5. The majority of the examples therefore illustrate the cooccurrence of Deviation IV with Deviation III. Is there any logical necessity for this? the answer is no. It is easy to construct examples which display homonymy without a morphosyntactic context which is realised simultaneously.

Consider a Turkish nominal paradigm:

(811)	Singular	Plural
Nom	ev 'house'	ev-ler
Acc	ev-i	ev-ler-i
Gen	ev-in	ev-ler-in
Dat	ev-e	ev-ler-e
Loc	ev-de	ev-ler-de
Abl	ev-den	ev-ler-den

This illustrates the sort of behaviour I characterised in Chapter I as particularly apt for an Item-and-Arrangement framework of description; there is no sensitivity, no simultaneity and no homonymy. Now consider a nominal paradigm for a hypothetical language closely resembling Turkish:

(812)	Singular	Plural
Nom	ev	ev-ler
Acc	ev-i	ev-ler-i
Gen	ev-in	ev-ler-in
Dat	ev-e	ev-ler-e
Loc	ev-de	} ev-ler-de
Abl	ev-den	

In this hypothetical paradigm, there is sensitivity, in that Ablative is realised as -den in the context Singular but -de in the context Plural; furthermore, this sensitivity results in homo-

nymy in the Plural, in that both Locative and Ablative are realised alike. We can also easily envisage a hypothetical paradigm in which the Case-ending for the Locative and Ablative in the Plural differs from that of both Cases in the Singular -- being, say, -be; in such a paradigm, clearly, the Locative as well as the Ablative is realised sensitively.

The hypothetical paradigm (812) and the variant just mentioned both illustrate the logical possibility of homonymy with a morphosyntactic context which is not realised simultaneously (except insofar as the 'unexpected' Plural Case-forms are regarded as sharing the realisation of Plural). But, on the basis of the evidence presented in Appendix E, this sort of homonymy is uncommon in comparison with homonymies in which the same morphological material realises both the properties whose distinction is 'neutralised' and the property or properties which furnish the context for that neutralisation. There are three possible reasons for this: it may be due to bias in the sample of languages cited (admittedly heavily weighted towards Indo-European); it may be a mere accident, not rooted in any general linguistic constraint on the realisation of morphosyntactic properties; or it may indeed have a general linguistic explanation. Of these three, the last is at least worth considering. I will suggest in the next section what this explanation might be -- why, in other words, non-accidental homonymy should be largely restricted to cumulated or simultaneous inflexional realisations.

8.6

A possible function for systematic homonymy

Let us assume, as argued in section 8.3, that there is a genuine linguistic phenomenon of systematic homonymy distinct from mere accidental homonymy of inflexional realisations. One way of approaching a general linguistic explanation for it is to consider what characteristics it must have in order to contribute to morphological simplicity (in a naive, pre-theoretical sense of the term) or in order to help native speakers to learn and remember some aspect of the grammar of their native language more easily. Starting from an a priori notion like this of

the function of systematic homonymy, we will arrive at certain conclusions about the circumstances which will favour it and the manner in which it should be represented in grammatical descriptions. These conclusions can then be tested against linguistic evidence such as ^{that} presented in Appendix E. I will argue that the conclusions which flow from the most plausible a priori view of the function of systematic homonymy do in fact square very well with the linguistic evidence so far presented. In particular, they include a prediction that homonymy should be commonest among forms which realise more than one morphosyntactic property simultaneously. This suggests the desirability of a new technical term or terms for systematic homonymies of kinds which promote morphological simplicity; we will turn to that question in section 8.7.

Let us consider first a hypothetical paradigm involving more than one morphosyntactic category but displaying neither morphological homonymy nor any kind of simultaneous realisation (including cumulation). Let us suppose that it is a nominal paradigm, and that the categories concerned are Number and Case. We are not concerned with the phonological shape of the inflexions except insofar as they realise more than one property simultaneously or homonymously; we can therefore represent the inflexions (which we can assume to be suffixes) by arbitrary letters, given the conventions that (a) a sequence of two letters indicates two separable morphs and (b) distinct letters indicate phonologically distinct morphs. Here is the hypothetical paradigm:

(813)	Singular	Plural
Nom	a	p a
Acc	b	p b
Gen	c	p c
Dat	d	p d

Such a pattern (what Pike (1963; 1965) calls a 'simple matrix') is quite plausible for natural languages, since it is very similar to what we observe in Turkish (see (811)) or Hungarian nouns. I want to concentrate here on one aspect of it: memorability. There are five inflexional realisations for the morphosyntactic

properties and their combinations to be learnt and remembered:

(814) p	Plural
a	Nominative
b	Accusative
c	Genitive
d	Dative

Compare this now with an inflexional paradigm which still lacks cumulation but in which there is homonymy:

(815)	Singular	Plural
Nom	a	p a
Acc	b	p b
Gen	c	p c
Dat	d	p c

In this paradigm there are still only five inflexional realisations to be memorised, but there is an added complication in the way two of them (c and d) are distributed. This complication can be expressed on the following lines:

(816) In Plural contexts, Dative is realised by the morph which is generally associated with Genitive; or:
Dat → Gen / Pl + _____

How this fact might be represented in a grammatical description is not important for the moment; what is important is that it is an 'extra' fact which would seem likely, a priori, to make the task of remembering paradigm (815) more burdensome than that of remembering (813).

In (815) we posited homonymy without any increase over (813) in the number of suffixes involved. But one can well envisage a paradigm with homonymy in which there is such an increase:

(817)	Singular	Plural
Nom	a	p a
Acc	b	p b
Gen	c	p e
Dat	d	p e

This paradigm seems, on the face of it, to present an even greater

memory burden than (815), since the speaker must remember six, not five, suffixes (see (818)) as well as a rule for the occurrence of the 'extra' one (see (819)):

(818) p	Plural
a	Nominative
b	Accusative
c	Genitive
d	Dative
e	Genitive, Dative

(819) In Plural contexts, Gen and Dat are both realised by the same morph (or: Dat = Gen / Pl + _____), namely e.

Where there is no cumulation, then, it seems a priori that paradigms with homonymy should always impose a greater burden on speakers' memories than paradigms without any homonymy. What if we turn to paradigms involving cumulation? Let us suppose that in our hypothetical language Case and Number are cumulated. Its nominal paradigm could then be represented as follows (in what Pike calls an 'optimal' or 'ideal' matrix):

(820)	Singular	Plural
Nom	a	e
Acc	b	f
Gen	c	g
Dat	d	h

Here, in contrast to (813), there are not five but eight suffixes to remember:

(821) a	Nom Sg	e	Nom Pl
b	Acc Sg	f	Acc Pl
c	Gen Sg	g	Gen Pl
d	Dat Sg	h	Dat Pl

Let us compare this now with a paradigm in which, as in (815) and (817), there is homonymy between the Genitive and the Dative in the Plural:

(822)	Singular	Plural
Nom	a	e
Acc	b	f

(822) (continued)	Singular	Plural
	Gen	c g
	Dat	d g

In paradigms without cumulation, as we have seen, homonymy seems a priori to increase the memory burden. With cumulation, on the other hand, we find the opposite. In contrast to the eight realisations listed in (821) for the paradigm (820), we find for paradigm (822) a need to memorise only seven realisations, as in (823) (or, at most, seven realisations plus one 'rule' on the lines of (824)):

(823) a	Nom Sg	e	Nom Pl
b	Acc Sg	f	Acc Pl
c	Gen Sg	g	{Gen} Pl
d	Dat Sg		{Dat} Pl

(824) Genitive and Dative have the same realisation in the Plural, or: Gen = Dat / Pl

The upshot of our discussion is that homonymy has contrasting consequences for the memorability of inflexional paradigms, a priori, according to whether or not they also exhibit cumulation. When we examined the hypothetical paradigms (813), (815), (817), (820) and (822), the following emerged:

(825)	Facts to be remembered:	Example:
a. Without cumulation:		
i. no homonymy	5 suffixes	(813)
ii. homonymy between two Cases without new distinct suffix	5 suffixes plus rule (816)	(815)
iii. homonymy between two Cases with new distinct suffix	6 suffixes plus rule (819)	(817)
b. With cumulation:		
i. no homonymy	8 suffixes	(820)
ii. homonymy between two Cases	7 suffixes plus rule (824)	(822)

What (825) shows is that, without cumulation, homonymy makes paradigms a priori more difficult; with cumulation, it makes them easier, if we assume that a rule such as (824) is easier to re-

member than a distinct inflexion such as h for Dat Pl in (821). Moreover, without cumulation, one can envisage two sorts of homonymy, the first involving no 'new' inflexions (as in (815)) and the second involving at least one such 'new' inflexion (as in (817)); and the second of these introduces more difficulty than the first.

8.7 Syncretism, attraction and the Systematic Homonymy Claim

We began the previous section by assuming that the function of systematic homonymy might be to contribute to memorability and learnability. Among the three conceivable kinds of homonymy discussed, only one was found to do so: that of (825 b. ii). Moreover, of the other two kinds, one (that of (825 a. ii)) was found to introduce less complication than the second (that of (825 a. iii)). It is time now to confront these conclusions, based entirely on assumptions and a priori reasoning, with some empirical evidence. Having done so, we can decide whether there is any ground for removing the recurrent phrase 'a priori' from our reasoning in section 8.6 and whether the distinction drawn between the three types of homonymy does indeed seem likely to reflect some linguistic reality.

The empirical predictions which flow from our conclusions are, in general terms, fairly plain:

(826) The commonest and most stable instances of systematic inflexional homonymy will be of the (825 b. ii) type (that is, the morphosyntactic properties which constitute the context for the homonymy will be realised simultaneously with the properties neutralised).

(827) Other instances of systematic inflexional homonymy will generally turn out to be of type (825 a. ii) rather than type (825 a. iii).

We will deal with (826) first.

In terms of Appendix E, (826) predicts that entries in

column 5 (for simultaneous contexts) will be commoner than entries in column 6 (for non-simultaneous contexts). This prediction is confirmed. Appendix F tabulates the 43 homonymies of Appendix E according to whether their conditioning factors involve non-simultaneous morphosyntactic properties (i.e. entries in column 6) or not. 30 of these do not, as against 13 which do; and, out of the 30, there are 23 which both have simultaneously-realised conditioning factors (i.e. an entry in column 5) and cannot be dismissed as accidental on any obvious independent grounds. Let us therefore find a use for the traditional term 'syncretism' by defining it in such a way that it can replace the rather unmemorable term 'of the (825 b. ii) type' which appears in our prediction at (826). We will need a definition on the following lines:

- (828) A systematic inflexional homonymy is a syncretism if (a) the homonymous forms are simultaneous exponents of more than one morphosyntactic property, and (b) the conditions under which the homonymy occurs (or: the context for the homonymy) can be stated entirely in terms of properties thus realised.

We can now restate our prediction at (826) as follows:

- (829) The commonest and most stable instances of systematic inflexional homonymy will be syncretisms.

Using the notation introduced in (824), a syncretism is a homonymy expressible in the following fashion:

$$(830) A = B / \frac{\quad}{C}$$

where A, B and C are morphosyntactic properties or combinations of them and '=' means 'has the same realisation as'.

I do not attach great importance to this notation, however, except as a device for illustrating graphically the contrast between syncretisms and other actual or conceivable types of homonymy. For example, in the version of pseudo-Turkish discussed earlier in which both Locative and Ablative were realised in the Plural by a suffix -be distinct from the realisation of either

Case in the Singular, that homonymy would be expressed as follows:

(831) Loc = Abl / Plural + _____

The context for the homonymy here is clearly not of the kind characteristic of syncretisms, as I have just defined them, because it involves sequentially ordered, not simultaneous, properties.

There is, of course, no point in defining terms for their own sake; a definition is useful or sensible only to the extent that it facilitates the formulation of strong, interesting generalisations. Now, our definition of syncretism at (828) certainly facilitates the formulation of the empirical claim at (829). But this claim is itself not particularly strong; it asserts only a statistical tendency, placing syncretism towards one end of a scale of types of homonymy ranging from commonest to least common. The claim it makes about the sort of homonymy illustrated by pseudo-Turkish at (812) is correspondingly weak; it says only that this will be relatively rare or perhaps 'highly marked'. Can we, then, claim anything more precise than (829), and thereby establish the usefulness of the term 'syncretism' more positively? The answer, I believe, is yes. To show this, I will need to turn to the second of our two predictions, that at (827).

Prediction (827), like prediction (829), is supported by evidence from Appendix E. Of the 13 homonymies in Appendix E for which there is an entry in column 6 (that is, the 13 which are definitely not syncretisms, under our present definition), 12 can plausibly be classified as falling under (825 a. ii). These are the 12 referred to in Appendix F as 'attractions'; for, as with (826) and (829), I propose to define a new term which will facilitate the reformulation of (827) as (833):

(832) A systematic inflexional homonymy is an attraction if (a) it is not a syncretism and (b) it involves the realisation of two or more properties (A and B) in some context by an inflexion which elsewhere realises only one of these properties (B). In such circumstances we can say that A is attracted to B, or there is attraction from A to B.

- (833) Instances of systematic inflexional homonymy which are not syncretisms will generally turn out to be attractions.

Appropriating the notation introduced in (816), we can interpret 'A → B' as meaning 'A is attracted to B', and say that an attraction is a homonymy expressible in the following fashion:⁸

$$(834) A \rightarrow B / C \dots + \frac{\quad}{D} + \dots E$$

- where
- A, B, C, D and E represent morphosyntactic properties or combinations of them;
 - D and either C or E (but not both) may be null.

There is, however, more to be said about the twelve 'attractions' in Appendix E. They are subclassified in Appendix F on a basis involving the notions of 'peripherality', 'centrality' and 'principal exponent' introduced in Chapter II. What emerges is that, if we disregard those three out of the twelve which are probably to be regarded as accidental,⁹ none of them involves a morphosyntactic context which is realised exclusively by a principal exponent or exponents more central than the neutralised properties. I will cite some examples to make this point clearer.

First, consider the version of pseudo-Turkish at (812) in which ev-ler-de is homonymously both Locative and Ablative Plural. In this form, the property which constitutes the context for the homonymy (namely Plural) has a principal exponent (namely -ler) which is more central than that of the neutralised properties Locative and Ablative (namely -de, in this version of pseudo-Turkish). So, although the pseudo-Turkish homonymy at (812) is indeed an attraction from Ablative to Locative, according to the definition at (832), it is not an attraction of a kind exemplified in the actual linguistic data of Appendix E.

One of the attractions which do comply with the generalisation just made is that involving the properties Definite and

Indefinite in the Hungarian Past Tense (37 in Appendix E). Part of the paradigm for the Hungarian verb ír 'write' is as follows:

(835)		Indicative:	
		Indefinite	Definite ¹⁰
Present:	Sg 1	ír-ok	ír-om
	2	ír-sz [i:rs]	ír-od
	3	ír	ír-ja
	Pl 1	ír-unk	ír-juk
	2	ír-tok	ír-játok
	3	ír-nak	ír-ják
Past:	Sg 1	ír-t-am	ír-t-am
	2	ír-t-ál	ír-t-ad
	3	ír-t	ír-t-a
	Pl 1	ír-t-unk	ír-t-uk
	2	ír-t-atok	ír-t-átok
	3	ír-t-ak	ír-t-ák

The aspect of this paradigm that interests us is the homonymy between the two 1st Singular Past forms. The morphosyntactic context for the homonymy is two-fold: Past Tense and 1st Singular. The property Past has a clearly identifiable principal exponent in the suffix -t-, which is found in all Past forms and no others; moreover, this -t- is clearly more central than the realisation of Definite and Indefinite, which must be located in the morphological material that follows it. But, whether or not this remaining material can ever be segmented further into an exponent of Person-Number and an exponent of Definiteness or Indefiniteness, it is clear that in no form does Person-Number have a principal exponent which is more central than the exponent of Definiteness; either the realisations of the two are simultaneous or else the realisation of Person-Number is more peripheral (as seems plausible in the 2nd and 3rd Plural). In the 1st Singular form in particular, the suffix -am must be regarded as a simultaneous exponent of both 1st Person (which is part of the context for the homonymy) and Definite-Indefinite (which are the properties neutralised). Moreover, we are entitled to call this homonymy an attraction because -am 'looks' Definite rather than Indefinite. The usual final consonants of 1st Singular forms in Hungarian, not only in the Present

Tense illustrated here but also in the Subjunctive and Conditional Moods, are -k for the Indefinite and -m for the Definite respectively; and the 'expected' 1st Singular Past Indefinite form would therefore be "ir-t-ak" rather than the actual ir-t-am. So, in terms of our notation, the homonymy can be expressed as follows:¹¹

$$(836) \text{ Indef} \rightarrow \text{Def} / \text{Past} + \frac{\quad}{1 \text{ Sg}}$$

The crucial difference between irtam and our pseudo-Turkish İoc-Abl Plural evlerde in (812) is that the morphosyntactic context for the homonymy in irtam, though in part realised more centrally than the neutralised properties, is not wholly so. Of course, a 'not wholly more central' context might be an altogether more peripheral one; and an example of attraction of this kind is furnished by the Finnish Comitative and Instructive Cases (39 and 40 in Appendix E). The Comitative suffix for both Singular and Plural, as illustrated in (256), is -ine¹², which 'looks' Plural inasmuch as -i- (or an allomorphic variant -j-) occurs in all Finnish Plural Cases except the Nominative and is in many of them the sole characteristic distinguishing them from the corresponding Singular Cases. This justifies us in segmenting a form such as pöytineen 'with his table(s)' as pöyt-i-nee, and expressing the attraction as follows:

$$(837) \text{ Sg} \rightarrow \text{Pl} / \frac{\quad}{\quad} + \text{Comitative}$$

The same applies to the Singular-Plural Instructive form pöytin:

$$(838) \text{ Sg} \rightarrow \text{Pl} / \frac{\quad}{\quad} + \text{Instructive}$$

We have found evidence, then, not only for the correctness of prediction (833) (that is, that systematic non-syncretisms will generally be attractions), but also for a further empirical claim involving the location of morphosyntactic contexts for attractions. Moreover, syncretisms and attractions between them exhaust (or nearly exhaust)¹³ those homonymies listed in Appendix E which cannot plausibly be dismissed as accidental. It is convenient, therefore, to consolidate the empirical claims advanced so far into a single generalisation -- highly vulnerable in principle, and therefore all the more interesting if correct:

(839) Systematic Homonymy Claim

All systematic homonymies involving the identical realisation of distinct morphosyntactic properties are either: (a) syncretisms; or (b) attractions in which not all properties belonging to the morphosyntactic context have principal exponents more central than the neutralised properties (i.e. at least some properties belonging to the context are realised simultaneously with or more peripherally than the neutralised properties).

One purpose of attempting to refine the rather vague predictions (829) and (833) emerging from our a priori discussion of the function of systematic homonymy in section 8.6 was to establish whether the terms 'syncretism' and 'attraction', as defined here, were genuinely useful. This purpose is now fulfilled; if the Systematic Homonymy Claim turns out to be sustainable when confronted with further evidence (bearing in mind that, as I said in section 8.2, it is its performance in coping with a large body of facts rather than individual examples of homonymy that matters), then the usefulness of the two definitions is amply demonstrated. Two fairly obvious questions about 'attraction' do remain, however: one about its possible function (in the sense of section 8.6) and one about the complexity of part (b) of the Systematic Homonymy Claim, in which the term 'attraction' appears. I will deal with these in turn in the next section.

8.8 Two questions about attraction

I suggested in section 8.6 that systematic homonymies of the kind we have since christened 'syncretisms' served a function in that they reduced what had to be learnt or memorised in a paradigm. Systematic homonymies of other conceivable kinds, however, all tended apparently to increase the memory-load by comparison with otherwise similar homonymy-free paradigms. This applied even to attractions, which differed from 'non-attractions' only in that the extra load they imposed was less. Why, then, should attractions happen at all? And if attractions occur despite the fact that (at least on a priori grounds) they must always be

regarded as dysfunctional, to what extent can the functional argument be said to 'explain' syncretism as a phenomenon in actual languages?

To answer these questions fully would require a complete account of when and where various types of morphological homonymy may or must occur. I do not pretend to give a full account of this here; but it is clear that we will need to acknowledge factors militating both in favour of and against inflexional homonymy which are independent of the Systematic Homonymy Claim, even if the Claim is correct. For a factor militating against homonymy, one need look no further than the obvious sense in which even syncretism will always be dysfunctional; it will always either create ambiguity or else throw speakers back on non-morphological means for realising syntactic distinctions which in some contexts are realised at least partly by morphological means. For factors operating in the opposite direction -- that is, factors favouring even those homonymies which are not syncretisms -- we can look again at the Hungarian and Finnish examples used to illustrate attraction in the previous section. In the Hungarian example, we noted that the homonymous form írtam 'I wrote (Def/-Indef)' 'looked' Definite rather than Indefinite, and that the 'expected' 1st Sg Indef form would be "írtak". But the form írtak does occur in the Indefinite Past, namely as the 3rd Plural form; so the Definite-Indefinite homonymy in the 1st Singular has a clearly identifiable consequence within the Indefinite Past, namely the avoidance of a homonymy between 1st Singular and 3rd Plural forms. Moreover, this avoidance of homonymy within a set of forms sharing the same Tense-Mood-Definiteness properties is a general characteristic of Hungarian, with which the existence of a 1st Sg Past form "írtak" would conflict. It seems legitimate, then, to say that the attraction in the 1st Singular forms serves a function within Hungarian, even though it is for the time being unclear how the 'general characteristic' just mentioned is to be accounted for in terms of either Hungarian grammar in particular or linguistic theory in general. There seems to be a similar dislike in Finnish of homonymy between Case-forms sharing the

same Number; and it is probably significant, therefore, that the earlier Singular-only Instructive form which has been ousted by the Singular use of the 'Plural-looking' form in -in happened to be homonymous with the Genitive (Hakulinen 1957: 68; Aaltio 1964: 256)¹⁴.

There are, then, conflicting principles, both language-particular and almost certainly also language-universal, which may favour the existence of a homonymy even if, on the basis of the argument in section 8.6, it is a dysfunctional one. Given this, the most that we can reasonably expect of a partial explanation for homonymy on the lines of section 8.6 is that it should be correct in predicting that, among those systematic homonymies which are dysfunctional by the memorability criterion, less dysfunctional types should be preferred over more dysfunctional ones, and homonymies with a positive effect on memorability (i.e. syncretisms) should be favoured overall. This expectation is fulfilled, insofar as the Systematic Homonymy Claim holds good; and, in turn, the Systematic Homonymy Claim is reinforced by the fact that it is consistent with the way in which one might expect systematic homonymy to operate, given our present (admittedly meagre) knowledge of the conflicting influences affecting it.

A second question connected with attraction involves also the part of the Systematic Homonymy Claim in which it appears. By comparison with the part concerning syncretism this part appears rather complicated, even messy; and, even granted what I have just said about the limits to functional explanation, many linguists are likely to be suspicious about an alleged constraint so complex and at the same time so apparently pointless. I will attempt here to enhance the plausibility of this part of the Claim by suggesting reasons for thinking that a more general constraint underlies it, even though I will not attempt here to formulate that more general constraint.

Hjelmslev (1935: 107-108) speaks of the 'domination' of one grammatical category by another. He remarks: "L'interdépen-

dance entre les catégories [grammaticales] est un fait de domination. Dans un système grammatical, certaines catégories sont dominantes et certaines autres catégories sont dominées ... La domination consiste en ceci que la catégorie dominée engage de syncrétismes sous la pression de la catégorie dominante [his emphasis]". As an example, he cites in Latin the syncretisms between Cases sharing the same Number and the absence of syncretisms between Singular and Plural in any Case as evidence for the domination of Number over Case in Latin. The domination of one category over another is, however, language-particular: "On verra qu'il y a des langues où les faits de domination sont toutes autres qu'en Latin, où par exemple les cas dominent les nombres et non inversement".

Hjelmslev does not develop or exemplify his theory of domination further, and I have little systematic to add here. Let us experiment, however, with nearness to the root as a criterion for domination; category A dominates category B if, when A and B are not realised cumulatively, A is realised more closely to the root than B. Such a criterion is bound to need amendment, since the order in which properties are realised in relation to the root is far from constant, even within one language (as is shown, for example, by the Huave facts discussed in Chapter II). But, if we confine ourselves to the languages represented in Appendix E (both Indo-European and others), the criterion does yield a reasonably consistent classification of nominal and verbal categories into broad bands, as follows, where higher dominates lower:

(840) Verbs:

Tense

Mood

Definiteness

(Hungarian only)

Person, Number¹⁵

Nouns and adjectives:

Number

Case

We can now define a homonymy as being 'consistent with domination' if the governing context involves categories which dominate the category or categories to which the homonymously-realised properties

belong; conversely, a homonymy 'violates domination' if the opposite relation holds. Among all the homonymies of Appendix E, only nine violate domination. But it is striking that seven of these (examples 8, 9, 36, 37, 38, 39 and 40) are also instances of attraction, according to our definition at (832), and the remaining two (examples 12 and 13) can be argued on independent grounds to be accidental homonymies; moreover, of the eight reasonably clear instances of attraction (examples 8, 9, 21, 22, 36, 37, 39 and 40), all but two (21 and 22) are also instances where domination is violated.

This degree of overlap between two independently-defined characteristics (attraction and domination-violation), even on the basis of so small a sample, seems too great to be accidental, and may point towards a more satisfactory functional explanation for attraction. But exploring this matter further is beyond the scope of this thesis. In particular, it will involve determining to what extent there is a 'natural' or 'least marked' order of realisation of properties which our ^{hierarchy}embryo_h can be taken to reflect, and whether anything systematic can be said about violations of the 'natural' order. This topic is in principle independent of the question of how sensitivity is affected by order, which we dealt with in Chapter II, but it would not be surprising to me if the 'domination hierarchy' turned out to have implications for sensitivity too.

8.9 Consequences of the Systematic Homonymy Claim for language change

I have mentioned the tug-of-war between memorability (to which syncretism contributes) and the avoidance of ambiguity (with which syncretism conflicts). This tug-of-war can be expected to have consequences for language change. A priori, we would expect that languages with persistent inflexional homonymies, unless they do away with these homonymies analogically, should develop non-morphological means of expressing those morphosyntactic properties most affected, and in particular should do so more readily than languages without inflexional homonymies. Insofar as this rather

vague expectation is testable, the evidence seems to bear it out. A number of linguists have attributed the 'analytic' tendencies of west European Indo-European languages and the phenomenon of linguistic 'drift' in general largely to the ambiguities engendered by the phonological attrition of inflexional endings (see e.g. Vennemann (1975) and the references he cites); other linguists have vigorously rejected this explanation (e.g. R. Lakoff 1972; M. B. Harris 1978). But the Systematic Homonymy Claim has as one corollary a rather more novel and more precise expectation about morphological change, relevant to the long-standing controversy about drift and particularly to the question whether the relationship between the phonological attrition just mentioned and the loss of inflexional endings is one of cause and effect, or whether they should both be seen rather as somehow aspects of a single process, influencing one another mutually.

Let us assume first that the Systematic Homonymy Claim is incorrect, so that 'syncretisms' (in my sense) are not as such any more likely to be systematic than homonymies which are not syncretisms. The origin of homonymies within inflexional paradigms cannot then be attributed even in part to any specifically morphological principle involving simultaneous exponence, and, if we compare the relative weight of phonological and specifically morphological factors in the west European analytic 'drift', the balance is likely to come down squarely on the side of the phonological factors. But then, if we look at the distribution of phonological innovations of the kind that are said to have contributed to the loss of inflexion in Europe, we find (at least at first sight) a rather mysterious correlation: these innovations seem to be especially characteristic of 'fusional' rather than agglutinating languages. That is, ^{it is} in languages where cumulation of morphosyntactic properties is, or has been, typical, such as the Germanic, Romance and Slavic languages, that phenomena such as the neutralisation of vowels in final or unstressed syllables and the loss of final consonants have putatively caused pernicious large-scale homonymy and consequent loss of inflexions, rather than in languages where cumulation is less typical, such as Hun-

garian and Turkish. We are faced with choosing between two equally unattractive conclusions: either the 'agglutinating' languages just happen to be phonologically more conservative than many of the 'fusional' ones, or else such phonological innovations as have occurred in the agglutinating languages just happen not to have resulted in inflexional homonymy of the kind found in the fusional ones.

Let us suppose, on the other hand, that the Systematic Homonymy Claim is broadly correct. If so, then in 'fusional' languages there is a specifically morphological factor independent of any phonological ones contributing to the incidence of homonymy, namely the fact that only in fusional languages can systematic homonymies of the kind I have called 'syncretisms' occur. Assuming that widespread inflexional homonymy, however caused, will hasten the drift towards analyticity, the Systematic Homonymy Claim will thus imply that, other things being equal, fusional languages will become analytic faster than agglutinating ones do. The Systematic Homonymy Claim thus points to an explanation for the correlation which in the last paragraph I called 'mysterious'. If some sound change introduces a homonymy in an agglutinating paradigm, that homonymy has to be regarded as accidental, and is consequently relatively vulnerable to removal by 'analogical' processes; on the other hand, a homonymy introduced in a fusional paradigm can be treated as systematic, and will therefore be less vulnerable to removal by morphological means (as opposed to syntactic ones, involving the replacement of 'synthetic' by 'analytic' or periphrastic modes of expression).

The Systematic Homonymy Claim thus implies a prediction about the relative speed of the drift to analyticity which an admittedly quite cursory look at the European evidence seems to confirm. It tends, too, to confirm the view of Martin Harris and others that, even if phonological change may contribute to inflexional attrition, it is not its sole cause. One is tempted to speculate further about the relationship between morphological cumulation and phonological change. If 'agglutinating' languages

are indeed phonologically more conservative than 'fusional' ones -- or, at any rate, display fewer phonological innovations with 'neutralising' effects likely to engender morphological homonymy -- can we say that a general principle of morphological organisation embodied provisionally in the Systematic Homonymy Claim may have an inhibitory effect on sound changes of certain types in languages with certain morphological characteristics? We are used to thinking of phonological innovation (or the lack of it) as proceeding quite independently of morphology and syntax, even though it may have quite radical morphological and syntactic consequences; but if my speculation is correct, then an influence may operate in the opposite direction too, inasmuch as certain phonological innovations tending to create morphological homonymies may be inhibited in 'agglutinating' languages (as opposed to 'fusional' ones) in virtue of a general propensity to avoid morphological homonymies which are unsystematic, at least in circumstances where an ambiguity is likely to arise. This speculation squares well with Lehmann's (1973) suggestion that certain phonological characteristics are typical of 'agglutinating' languages. On the other hand, it runs counter to Lightfoot's contention (1979: 123-124, 149) that, in linguistic change, "grammars practice therapy rather than prophylaxis", since what I am positing for agglutinating languages is essentially a prophylactic avoidance of certain kinds of sound change. On balance, the speculation certainly seems to me worth testing; but to do so would take us well beyond the bounds of this thesis.

8.10 The reliability of Appendix E

I promised in section 8.5 to revert later to the question whether the generalisations emerging from our discussion risked turning out to be spurious because of bias in the selection of the data in Appendix E on which they are based. As I have said, Appendix E contains a relatively high representation from Indo-European; and I must admit that this bias has less to do with statistics than with my relative ignorance of non-Indo-European languages. But the bias would seriously endanger our conclusions (in particular, the Systematic Homonymy Claim) only if it could

be shown that there were, or might be, 'family-specific' reasons why simultaneous exponence of contextual and neutralised properties should favour homonymy, or, conversely, why sequential exponence should inhibit it.

To demonstrate such reasons, it would not be sufficient merely to show that many Indo-European homonymies (such as the Neuter Nominative-Accusative one) are old and well-established; for this very persistence could equally be attributed to the fact that these homonymies are syncretisms (in my sense) and therefore conform to the Systematic Homonymy Claim. Rather, one would need to show that Indo-European languages have an inherited penchant for homonymy quite independent of factors such as simultaneity in inflexional realisation. But the only way to show this would be to show that systematic homonymies crop up in Indo-European 'agglutinated' structures (by contrast with, say, Turkish and Hungarian ones) just as freely as in 'fused' structures. Yet this does not seem to happen; and the absence of such 'agglutinated' homonymies cannot simply be put down to an Indo-European distaste for agglutination altogether, since 'agglutinating' as well as 'fused' morphological structures are common in attested Indo-European languages of all times and places (cf. Latin amā-t-ur (LOVE + 3 Sg + Passive) 'he is loved' alongside amā-minī (LOVE + {2 Pl Passive}) 'you (Pl) are loved').

We can be reasonably confident, then, that the Indo-European bias in Appendix E is not seriously distorting. Quite apart from this, it is logically possible that even without any family-specific tendency towards homonymy as such, systematic homonymy should be relatively more frequent in Indo-European than in other language families. This will be not merely possible but probable if it is the case that the factors which (I have argued) facilitate systematic homonymy generally, such as simultaneous exponence, are relatively more frequent in Indo-European -- something which has indeed been suggested by some linguists; thus, Hjelmslev (1935: 83) claimed that the Case-systems of Indo-European languages were quite unusual in their lack of 'regularity' (i.e., in effect,

their high incidence of sensitivity, homonymy and simultaneous exponence). There is clearly matter here for much further investigation.

My argument is not intended to suggest that we should rest content with Appendix E and not bother to seek further relevant evidence. I freely admit that forty-odd more or less random examples are a slender basis for a generalisation of the nature of, and as ambitious as, the Systematic Homonymy Claim. I believe I have shown, however, that if someone wants to say that the Claim is so inadequately supported as not to constitute even a sensible starting-point for further investigation, the onus is on him to prove this rather than on me to disprove it.

Footnotes to Chapter VIII

1. The rapporteur for the exercise, Geneviève Corréard, comments on "l'extrême diversité des réponses", and Bazell's comment (page 25) is apt: "If the opposition between linguists is to be neutralised, the "archiopinion" will turn out to be zero".
2. The section on syncretism in Hjelmslev (1961) (pages 87-93) does not go beyond definitions and a few remarks about logical entailment and set theory.
3. A generation later, Bierwisch (1967) is likewise interested in syncretisms within German declension primarily as evidence for the assignment of Case-Number combinations .. to 'natural classes' on the basis of their analysis in terms of 'features' such as 'Oblique', 'Governed'.
4. It has been argued (most recently by Janda (1980)) that the English possessive -s is a clitic rather than an affix. But even if this is right, it merely reinforces the point that the homonymy of two items does not guarantee any systematic connexion between them.
5. A word-form gilt exists, of course, but not as the Past Tense of gild in modern English.
6. Icelandic thus contrasts with better-known languages such as Italian which, in male chauvinist fashion, require Masculine concord in such circumstances.
7. It is not clear to me whether the Case system that Chomsky (1980) posits for English noun phrases is morphosyntactic (in my sense) or purely syntactic. If the former, then we have here an instance where the syntactic evidence in favour of an alleged general morphosyntactic distinction is allowed to outweigh the morphological evidence against it.
8. (834) is not, in fact, a perfect schematic representation of attraction, as we will discover when discussing some Russian examples at section 9.2c. But (834) is still useful for comparison with (830): the two illustrate visually the main differences between attraction and syncretism.
9. One of these three (the medieval Italian homonymy

between the Singular Persons in the Imperfect Tense) will be discussed in the next chapter.

10. The Definite forms are used with a 'definite' 3rd Person direct object (i.e. one accompanied by a demonstrative or possessive adjective or the definite article).

11. Vago (1980: 53) arrives independently at what is in effect a view of the 1st Sg Past Indef ending as derived by attraction from the 1st Sg Past Def. He posits a 'morphological rule' as follows:

$$\begin{array}{l} \text{m-Suppletion} \\ \left[\begin{array}{l} 1 \text{ Sg} \\ + \text{ PAST} \end{array} \right] \rightarrow [+ \text{ DEF}] \end{array}$$

12. Followed obligatorily, in nouns, by a Personal Possessive affix.

13. The sole exception is example 23 (the Dative-Locative homonymy in the Russian a-declension), which is discussed in Chapter IX.

14. The English translation of Hakulinen's work (1961: 74) oddly contradicts both Aaltio and Hakulinen himself (in the German version) on the subject of the Singular use of the Plural Instructive form; but this seems to be a translator's or proof-reading error.

15. Note that, as already mentioned in section 5.3, verbal stem alternations seem to realise Tense or Aspect contrasts far more commonly than Person-Number contrasts (Hooper 1979).

CHAPTER IX
SOME INDIVIDUAL HOMONYMIES

9.1 Introduction

Column 7 in Appendix E contains comments on some of the homonymies listed. A few of them, however, offer points of interest which merit a somewhat longer discussion than there is room for there. Section 9.2 in this chapter is devoted to these longer discussions. Section 9.3 is devoted to two sets of data, in Italian and the American Indian language Yurok, which were omitted from Appendix E because proper discussion of them requires the presentation of much more evidence than is feasible in the Appendix. Nevertheless, as I hope to show, their behaviour is compatible with the conclusions we drew from Appendix E in Chapter VIII, and in particular the Systematic Homonymy Claim.

9.2 Homonymies from Appendix E

9.2 a Nominative-Accusative Neuter homonymy in Latin, Sanskrit and Russian (examples 4, 14, 20)

In Chapters V and VI we discussed at some length the relevance to paradigm economy of lexically determined categories such as Gender in nouns and Transitivity in verbs. Our conclusion, briefly, was that identical inflexional patterns should not be considered distinct paradigms solely on the basis of difference in properties of this kind (so that the Latin mensa Fem 'table' and nauta Masc 'sailor' could be said to belong to the same paradigm in spite of their Gender difference), but that distinct inflexional patterns could be assigned to the same paradigm (or, rather, 'macroparadigm') if all their inflexional differences corresponded consistently to a difference in some lexically determined property (so that Latin dominus 'lord' Masc and bellum 'war' Neut could be assigned to the same paradigm despite their inflexional differences in the Nominative and Accusative). One could paraphrase this conclusion by saying that, for the purposes of paradigm economy, a lexically determined property A counts as having an inflexional realisation in some

word-form if and only if there is some other property B within the same category which triggers a different realisation from A for the same 'slot' in the paradigm. On this basis, Neuter is realised inflexionally in most Latin, Sanskrit and Russian noun paradigms in the Nominative and Accusative and nowhere else, since in most paradigms it is only in the Nominative and Accusative that Neuter nouns differ inflexionally from non-Neuters.

This conclusion, reached without reference to any considerations about homonymy within paradigms, turns out to be very convenient for our present purposes too. It allows us to say that the -a of Latin bella 'wars' realises not only the properties Plural and Nominative or Accusative but also Neuter, simultaneously; so, since Neuter is the morphosyntactic context for the Nom-Acc homonymy, it allows us to say that this homonymy is a syncretism. Suppose, on the other hand, that we had concluded that lexically determined morphosyntactic properties ought never to count as being inflexionally realised at all for the purpose of grouping inflexional patterns into paradigms. If we apply this conclusion to the Neuter Nom-Acc homonymy, we find that it destroys the characteristic which entitles us to call the homonymy a syncretism, because the contextual property Neuter is no longer realised simultaneously with the neutralised properties; and in the Plural at least we cannot call this homonymy an attraction either, since the ending -a does not 'look' either specifically Nominative or specifically Accusative. In order to go on calling this homonymy 'systematic', then (as we would clearly wish to do, in view of its remarkable persistence and regularity throughout Indo-European), we would have to either invent some special definition of 'syncretism' applicable only to lexically determined morphosyntactic contexts or weaken the Systematic Homonymy Claim or both.

The upshot is, then, that the Paradigm Economy Hypothesis and the Systematic Homonymy Claim are not altogether independent, as one might expect, but provide a certain measure of support for one another in that one and the same way of handling lexically

determined properties contributes to the most economical and 'natural' formulation of both.

9.2 b Singular and Plural as part of the context for Russian noun and adjective homonymies (examples 21-25)

Several of the Russian examples of Case homonymy cited in Appendix E differ in one important respect from the Latin examples cited earlier in the Appendix: Number (Singular or Plural), as part of the context for the homonymy, is given in column 6 rather than column 5. This is because in at least some Russian nouns of the relevant kinds there is a consistent stress difference between Singular and Plural; so the realisation of Number in these nouns, being partly suprasegmental, cannot apparently be regarded as wholly simultaneous with the purely affixal realisation of Case, even though there are many other nouns in which the whole job of realising Number is done by the Case-Number affix. As a result, several of the Russian homonymies can apparently not be regarded as syncretisms, according to my definition.

For the homonymies numbered 21 and 22 this does not matter, since there are independent reasons, presented in section 9.2c below, for regarding them not as syncretisms but as attractions; and, since the suprasegmental elements in the realisation of Singular and Plural cannot be regarded as more central principal exponents of those properties (which, in any case, do not constitute the whole morphosyntactic or morphosemantic context for the homonymies, in view of the entries in column 5), the entries in column 6 are no obstacle to calling these attractions systematic. But homonymy number 23 presents more of a problem. There is no strong independent evidence for calling it an attraction; yet the entry in column 6 precludes our calling it a syncretism. Must we then simply classify this homonymy as accidental, if we are to maintain the Systematic Homonymy Claim?

Such a conclusion would not, so far as I can see, represent a very serious blow to the Claim. The independent synchronic grounds for regarding this Dative-Locative homonymy as systematic

are not overwhelming, seeing that, as stated, it is restricted to one declension-type. Yet multiple homonymies involving the Dative and Locative as well as other Cases are found elsewhere in Russian, as 24 and 25 in Appendix E indicate; and it would be useful if, while maintaining the Systematic Homonymy Claim, we did at least have the option of regarding homonymy 23 as systematic.

Fortunately, independent evidence which would reconcile the Systematic Homonymy Claim with this option does exist. This evidence has to do with the inflexional status of stress in Russian nominal paradigms. During the discussion of 'slabs' and paradigm economy in section 5.3, I mentioned the apparent problem posed by Russian nouns such as stol, vxod and zub, which display distinct stress patterns in the Plural and therefore seem at first sight to conflict with the expectation that, if the Paradigm Economy Hypothesis is correct, only one 'principal part' need be cited in each Number in order to identify the inflexional behaviour of any Russian noun unambiguously. In my answer to this problem, I referred ahead to Chapter VII and the evidence presented there to the effect that stem allomorphy and other non-affixal types of inflexion did not 'count' for the purpose of paradigm economy. Returning to the problem in section 7.4, I argued that, although forms such as vxódov 'entrances (Gen Pl)' and zubóv 'teeth (Gen Pl)' contrasted in stress, it was only the affixal part of the inflexion which was relevant to determining whether they were inflexionally distinct; and, since they shared the suffix -ov in the Genitive Plural, they were in fact not distinct in this Case. We therefore already have reasons involving paradigm economy for putting stress on a different footing from affixation as an inflexional procedure in Russian nouns. What if we do so in connexion with homonymy too? Let us suppose that the suprasegmental realisation of any property is ignored for the purpose of determining whether, as part of the morphosyntactic or -semantic context for a homonymy, it belongs in column 5 or column 6. At once, the homonymy at 23 becomes classifiable as a syncretism, because the purely suffixal realisation of Singular in Russian nouns is

simultaneous with that of Case, and therefore belongs in column 5, not column 6.

I have said enough, I hope, to show that classifying the homonymy at 23 as systematic may after all be compatible with the Systematic Homonymy Claim. But there is an intriguing fact about the further Russian examples at 24 and 25 which tempts one to speculate further. This is the fact that, whereas in 23 we find only two Cases syncretised, in examples 24 and 25, where stress has clearly no role in realising Number, the syncretism extends to three or four. So far we have regarded systematicity in black and white terms: either a homonymy is systematic (and thus to be indicated explicitly in an accurate grammatical description) or it is not. What 24 and 25 suggest, however, is that there may be degrees of systematicity. Ignoring stress in 23 allows us to call the homonymy there systematic; but in 24 and 25 there is no inflexional use of stress to ignore in the first place. Could it be that this difference renders renders the homonymies in 24 and 25 more systematic than that of 23? If so, we may have the beginnings of an explanation for why the homonymies of 24 and 25 involve more Cases -- Genitive and even Instrumental as well as Dative and Locative. Expressed very vaguely, my speculation is that the more systematic a homonymy is, the more 'slots' it will tend to embrace in the relevant inflexional paradigm. But to test this speculation would require some independent measure of 'degrees of systematicity'; and I do not at present see what sort of measure one could use.

9.2 c Accusative-Nominative and Accusative-Genitive
homonymy in Russian (examples 21, 22)

In section 5.4, when discussing the 'macroparadigm' concept in connexion with paradigm economy, I referred to a set of Russian nominal paradigms which, for convenience, I will repeat here:

(515) a.

Sg Nom	student	professor	ženščina
Acc	studenta	professora	ženščinu

(515) a. (continued)

Sg Gen	studenta	professora	ženšćiny
Dat	studentu	professoru	ženšćine
Ins	studentom	professorom	ženšćinoj
Loc	studente	professore	ženšćine
Pl Nom	studenty	professora	ženšćiny
Acc	studentov	professorov	ženšćin
Gen	studentov	professorov	ženšćin
Dat	studentam	professoram	ženšćinam
Ins	studentami	professorami	ženšćinami
Loc	studentax	professorax	ženšćinax

b.

Sg Nom	akt	dom	kvartira
Acc	akt	dom	kvartiru
Gen	akta	doma	kvartiry
Dat	aktu	domu	kvartire
Ins	aktom	domom	kvartiroj
Loc	akte	dome	kvartire
Pl Nom	akty	doma	kvartiry
Acc	akty	doma	kvartiry
Gen	aktov	domov	kvartir
Dat	aktam	domam	kvartiram
Ins	aktami	domami	kvartirami
Loc	aktax	domax	kvartirax

The point of interest in these paradigms, here as in Chapter V, is the Accusative forms. Except for the Accusative Singular of ženšćina and kvartira, all the Accusative forms are homonymous with either the Nominative form or the Genitive form of the same Number. But there is a straightforward criterion for determining which homonymy will occur in a given noun, namely whether it is Animate or Inanimate. In section 5.4 I used this as justification for assigning each of the three nouns in (515 a) to the same macroparadigm as the noun immediately below it in (515 b): all three nouns of (515 a), where Accusative and Genitive are homonymous everywhere except in the Singular of ženšćina, are Animate, whereas all three nouns of (515 b), where Accusative and Nominative are homonymous everywhere except in the Singular of kvartira, are

Inanimate. Our concern now, of course, is not with how these paradigms fit the Paradigm Economy Hypothesis but with how the homonymies fit the Systematic Homonymy Claim. In view of the straightforwardness and generality of the Animacy criterion just mentioned, we would clearly like these homonymies to emerge as systematic according to the Claim. And bearing in mind what was said in section 9.2a about lexically determined properties, I suggest that they do.

In section 9.2a I argued that the lexically determined property Neuter, because it assumes macroparadigmatic relevance in the Nominative and Accusative of nouns in several Indo-European languages, can be said to have an inflexional exponent in those Cases, so that the Nom-Acc homonymy can legitimately be classed as a syncretism. For precisely the same kind of reason, one can say that, in Russian, the properties Animate and Inanimate have an inflexional exponent in the Accusative forms which they affect. Clearly, too, Animate or Inanimate, as appropriate, is realised simultaneously with Accusative, since what we are dealing with is a single suffix, not a sequence of suffixes. Can we then call the Gen-Acc and Nom-Acc homonymies syncretisms?

There is a difference between this situation and that of the Neuters which should give us pause. In the Neuters, there was macroparadigmatic evidence for the realisation of the property Neuter in both the Cases Nominative and Accusative. On the other hand, in the nouns of (515), there is no macroparadigmatic evidence for the realisation of either Animate or Inanimate in either the Genitive or the Nominative; for the exponents of Nom and Gen show no alternation correlated with Animacy. To put it another way, we are going beyond the evidence if we represent (say) the Acc-Gen homonymy of professor by means of the notation for syncretisms introduced at (830), thus:

$$(901) \text{ Acc} = \text{Gen} / \frac{\quad}{\text{Animate}}$$

This is because in the Genitive forms professora (Sg) and professorov (Pl), as opposed to the homonymous Accusative forms, we have

no independent reason for saying that the property Animate is inflexionally realised at all.

Let us take it, then, that these homonymies are not syncretisms. It does not follow that the Systematic Homonymy Claim requires us to classify them as unsystematic. In fact, they fit the definition of 'attraction' given at (832); furthermore, they belong to the class of attractions which are consistent with the Systematic Homonymy Claim. This is easy to show. Firstly, the Accusative-Genitive forms in an Animate noun like professor or (in the Plural) ženščina are not, as it were, neutral in shape between the two Cases (as the Nom-Acc forms in Neuters are neutral), but are rather Genitive forms which also have an Accusative function, on the strength of the fact that Inanimate nouns belonging to the same macroparadigms as professor and ženščina (such as dom and kvartira) have the same inflexions realising Genitive only; and, in just the same way, the Inanimate Nominative-Accusative homonymies of dom and (in the Plural) kvartira can be shown to be fundamentally Nominative forms which also function as Accusatives. Secondly, neither Animate nor Inanimate has a principal exponent more central than the exponent of Case. For these two reasons, they do not fall into the class of 'attractions' which the Systematic Homonymy Claim rejects as unsystematic.

The main respect in which these attractions differ from those that we have considered so far is that the morphosyntactic context for them is realised simultaneously with the neutralised properties, just as in a syncretism. They look, in fact, like something intermediate between a syncretism and an attraction of the kind representable as in (834) (where it was assumed that, in any attraction, at least part of the context would not be realised simultaneously in this way). It may well be that we will need in due course to redraw the boundary between the two sorts of homonymy which the Systematic Homonymy Claim allows as systematic. What is important for the present, however, is that no special pleading is needed for the Claim to classify these Russian homonymies as systematic in one way or the other.

9.2 d Medieval Italian parlava 'I/you/he/she spoke'
(example 29)

This example illustrates how the fact that a homonymy violates the Systematic Homonymy Claim and must therefore be classified as 'accidental', though at first sight an awkward and unwelcome consequence of the Claim, may turn out on historical grounds to be a welcome consequence after all.

The medieval Italian Imperfect Tense of parlare 'speak' is as follows:

(902) Sg 1	parl-av-a
2	parl-av-a
3	parl-av-a
Pl 1	parl-av-amo
2	parl-av-ate
3	parl-av-ano

The hyphens are inserted to draw attention to the element av-. This appears throughout the Imperfect and nowhere else in the paradigm, so it is unequivocally a principal exponent of Imperfect.¹ On the other hand, the homonymy of the three Singular Persons is peculiar to the Imperfect, so it is the property Imperfect which constitutes the morphosyntactic context for it. But, if so, the homonymy cannot be considered systematic, because it conforms to neither part (a) nor part (b) of the Systematic Homonymy Claim: it violates part (a) because Imperfect is not realised simultaneously with the Singular Persons, and it violates part (b) because, although it might be considered an attraction on the ground that -a is a specifically 3 Sg ending in another Tense (namely the Present), the contextual property Imperfect has a principal exponent more central than the neutralised properties. The homonymy must therefore be considered accidental -- and, moreover, an accidental homonymy that cannot be attributed to phonological conditioning, in the way that (for example) the homonymy between -i 'his/her/its' and -i 'Accusative' in Turkish evi 'his house/house (Acc)' can be (see (804 c)).

Why might this conclusion be after all a welcome one?

The answer has to do with our expectation about what is likely

to happen, over time, to a homonymy which lacks synchronically any phonological or systematically morphological underpinning. A natural expectation, as implied in section 8.9, is that, unless the neutralised properties cease to be morphosyntactically relevant (that is, in this instance, unless Imperfect Tense verb-forms cease to be inflected for Person-Number in Italian), some new morphological means will be found to realise the distinctions overtly and so destroy the homonymy. And this expectation is fulfilled. The three Singular Persons are realised differently in the Present Tense of all Italian verbs, as exemplified in (903 a); and in modern Italian the Present Tense endings for the 1st and 2nd Persons Singular have been taken over in the Imperfect too (see (903 b)):

(903) a.	a. Present	b.	b. Imperfect
Sg 1	parl-o		parl-av-o
2	parl-i		parl-av-i
3	parl-a		parl-av-a
Pl 1	parl-iamo		parl-av-amo
2	parl-ate		parl-av-ate
3	parl-ano		parl-av-ano

The situation, then, is that purely phonological changes between Latin and Italian, namely the loss of final consonants, created a new homonymy in the Imperfect in medieval Italian of a kind which, according to the Systematic Homonymy Claim, must be classified as accidental: Latin -ābam, -ābas, -ābat > -ava, -ava, -ava. Moreover, this new homonymy lacked any synchronic phonological underpinning, in that the homonymous -a suffixes were not phonologically conditioned allomorphs of suffixes elsewhere distinct, as in the examples at (804). But the fact that in due course the homonymy was removed tends to confirm that to classify the homonymy as accidental is indeed correct.

The confirmation is far from absolute, of course. I have not claimed that accidental homonymies as such are intolerable and must immediately be removed as soon as they arise; and to make any such claim would clearly be wrong, since in another

Romance language (Spanish), for similar historical reasons, a similar homonymy arose in the Imperfect which is likewise 'accidental' in my framework but which has not been removed by any later morphological innovation:

(904	Sg 1	habl-ab-a
	2	habl-ab-as
	3	habl-ab-a
	Pl 1	habl-áb-amos
	2	habl-ab-ais
	3	habl-ab-an

What I believe I have shown, though, is that to rush to chalk up as counter-evidence to the Systematic Homonymy Claim all 'accidental' homonymies which are phonologically unaccountable in purely synchronic terms is over-hasty; it is at least worth investigating whether later historical changes, if known, do not actually convert the apparent counterevidence into confirming evidence.

9.2 e Georgian kal-ta 'to/by/of daughters' (example 41)

As presented in Appendix E, the ancient Georgian homonymy between the Dative, Ergative and Ablative Cases in the Plural is a straightforward syncretism with no particularly remarkable features. But consider the following partial nominal paradigms of the noun kal 'daughter', illustrating a contrast between ancient and modern Georgian (Vogt 1971):

(905)		Singular (ancient and modern)	Plural (ancient)	Plural (modern)
	Nom	kal-i	kal-ni	kal-eb-i
	Dat	kal-s	kal-ta	kal-eb-s
	Erg	kal-ma	kal-ta	kal-eb-ma
	Gen	kal-is	kal-ta	kal-eb-is

Clearly, a morphological innovation has taken place (for whatever reason) whereby a Plural paradigm in which Number and Case were realised simultaneously, at least in some Case-forms, has been replaced by one where they are realised sequentially, with a separate identifiable Plural marker -eb-.

Logically, when this Plural marker was introduced, the pattern of homonymy observable in the ancient Georgian Plural could have been retained; modern Georgian might, in other words, have ended up with a form such as "kal-eb-ta" realising homonymously Dat, Erg and Gen Pl. What we actually observe in modern Georgian, however, is an inflexional pattern displaying no homonymy and no sensitivity either: a straight-forward identity of Case-endings between Singular and Plural.

It would be unrealistic, especially without a thorough study of Georgian historical morphology, to point to any one factor as causing the obliteration of the homonymy. But we can at least say that, if the Systematic Homonymy Claim is correct, the observed development is quite natural, whereas the development of a new homonymous form such as our hypothetical "kal-eb-ta" would not be expected. The reason is that "kal-eb-ta" would have been no longer a syncretism nor even an attraction, but would have had to be analysed as accidental, and would thus have represented an increase in complexity over the form kal-ta which it replaced. What actually happened, on the other hand, represents a decrease in complexity inasmuch as it introduces the maximally simple one-to-one pattern of exponence.

9.2 f Latin regam: Present Subjunctive and Future Indicative
(example 9)

As long ago as section 2.4, I mentioned that the 1st Singular Future Indicative Imperfective form regam from the Latin verb regō 'rule' (and corresponding forms of all Latin third and fourth conjugation verbs) constituted prima facie counter-evidence to the Peripherality Constraint. I promised to deal with this apparently uncomfortable fact later. The time has now come to fulfil this promise. The discussion of regam will turn out to be of wider interest, in that it confirms that what on other grounds we would probably consider the most natural way within grammatical theory of relating the systematic homonymy 'rules' of a language to its inflexional 'spelling rules' is also, in all probability, the correct way.

The reason why regam constitutes a problem for the Peripherality Constraint becomes clear when we examine the complete Person-Number paradigm of which regam is a part, alongside the Imperfective Present Subjunctive:

	(906) Future Indicative:		Present Subjunctive:	
	Active	Passive	Active	Passive
Sg 1	regam	regar	regam	regar
2	regēs	regēris	regās	regāris
3	reget	regētur	regat	regātur
Pl 1	regēmus	regēmur	regāmus	regāmur
2	regētis	regēminī	regātis	regāminī
3	regent	regentur	regant	regantur

In these paradigms, it is easy to identify -ē- as a principal exponent of Future Indicative (in my terminology) and -ā- of Present Subjunctive. But the realisation of Future Indicative is sensitive, seemingly, in that there is a second exponent, -ā- (shortened to -a- by a regular Latin phonological process), used in the 1st Sg both Active and Passive. But, since 1st Sg is realised more peripherally (by -m in the Active and -r in the Passive, both realisations being found in other Tense-Mood combinations), this sensitivity is 'outwards'; and, since the same realisation for Future Indicative is not used with all Person-Number combinations (which are all, like 1st Sg, realised more peripherally), this instance of outward sensitivity contravenes the Peripherality Constraint.

What is also relevant, however, is the consistency of the homonymy between these 1st Person Singular forms. As (906) shows, it applies in both the Active and the Passive. Moreover, it is a feature of Latin attested at all periods until periphrastic Future formations like regere habeo take over, whereas 'analogical' Future forms such as regem and reger, distinct from the Present Subjunctive forms, are exceedingly rare. But at the same time it cannot be called a syncretism, since syncretism is limited by definition to instances where the homonymously-realised properties and the properties which constitute the conditioning environment for the homonymy are realised by the same morphological material. It is, rather, a case of attraction, consistent with the Systematic

Homonymy Claim inasmuch as part of the morphosyntactic context (1st Singular) is realised more peripherally than the neutralised properties, thus:²

$$(907) \left[\begin{array}{c} \text{Future} \\ \text{Indicative} \end{array} \right] \rightarrow \left[\begin{array}{c} \text{Present} \\ \text{Subjunctive} \end{array} \right] / \text{-----} + \left[\begin{array}{c} \text{1st} \\ \text{Sg} \end{array} \right]$$

Restriction: Third and fourth conjugations only

As I have said, I do not intend too much weight to be placed on the details of the notation of attraction-statements such as (907) and syncretism-statements in which the arrow is replaced by an equals-sign. The essential point for our present purposes is that (907) is a statement about how the realisations of different property combinations are ordered in certain Latin word-forms, and not a statement about what these realisations actually are. (907) is therefore in one sense more abstract, or further removed from the actual surface shapes, than statements specifying the actual phonological representation of 'Present Subjunctive' or '1st Singular Active' in different contexts. This point is crucial, because it is only in connexion with statements of this second kind that questions of sensitivity can arise, and thus only to statements of this second kind that the Peripherality Constraint applies. In other words, in any word-form, the arrangement of morphosyntactic properties whose realisation must comply with the Peripherality Constraint is not what one might call the 'underlying' arrangement, but rather the arrangement which emerges from the enforcement of any prescriptions (especially language-particular ones) on syncretism or attraction.

How does this bear on the realisation of the property-combination 1st Sg Impf Fut Indic Act in association with the Latin third-conjugation verb-root reg-? The grammar of Latin will specify that this word-form is subject to the attraction stated at (907), rendering it homonymous with the Present Subjunctive. This has the effect of ensuring that this form is never spelt out, or related to its realisation, directly, but always indirectly, by way of the rules or statements which realise the 1st Sg Impf Pres Subjunc Act. So it is only the latter combination of properties which (in association with third and

fourth conjugation verbs) ever has to run the gauntlet of the Peripherality Constraint; and it does so successfully, since no outward sensitivity is involved in the analysis of regam when it is treated as a Present Subjunctive form. No special provision has to be made, therefore, to exempt the Future Indicative regam from the Peripherality Constraint; the fact that it is thus exempt follows directly, I suggest, from the way in which systematic homonymy within inflexional paradigms is to be accounted for. And the fact that, within my provisional framework of constraints, no special provision has to be made to account for the interaction of a constraint on Deviation II (the Peripherality Constraint) and a constraint on Deviation IV (the Systematic Homonymy Claim) provides a further element of confirmation for the framework as a whole.

9.2 g Accusative-Genitive homonymy in the Plural of nouns in classical Arabic (example 35)

It will be recalled that some classical Arabic data appeared in the course of our discussion of stem allomorphy in relation to the Peripherality Constraint in section 7.2. In some of the nominal inflexion patterns there -- specifically, those of muʕallimun 'teacher' and ḥayawānun 'animal' in example (717) -- the Genitive and Dative Cases are realised homonymously in the Plural. I will repeat these paradigms here, for convenience:

	(717) Singular	Plural	Singular	Plural
Indef Nom	muʕallim-un	muʕallim-ūna	ḥayawān-un	ḥayawān-ātun
Gen	muʕallim-in	muʕallim-īna	ḥayawān-in	ḥayawān-ātin
Acc	muʕallim-an	"	ḥayawān-an	"
Def Nom	muʕallim-u	(as above)	ḥayawān-u	ḥayawān-ātu
Gen	muʕallim-i		ḥayawān-i	ḥayawān-āti
Acc	muʕallim-a		ḥayawān-a	"

In this respect, these Plurals differ from the 'broken' Plural pattern of rajulun 'man' ((716) in section 7.2):

(716)	Singular	Plural
Indef Nom	rajul-un	rijāl-un
Gen	rājul-in	rijāl-in
Acc	rajul-an	rijāl-an

(716) (continued)	Singular	Plural
Def Nom	rajul-u	rijāl-u
Gen	rajul-i	rijāl-i
Acc	rajul-a	rijāl-a

The question we must now consider is: are the homonymies exhibited in the declension of ḥayawānūn and muḥallimūn systematic or accidental, according to the Systematic Homonymy Claim, and is the classification imposed by the Claim plausible on other grounds? To put it another way, do these Arabic facts tend to support the Claim or not?

We can quickly deal with the homonymy between the Indefinite and Definite forms of the Plural of muḥallimūn. Everywhere else in (717) and (716), each Indefinite form is distinguished from the corresponding Definite form by a final -n. The Plural of muḥallimūn, however, where the homonymy occurs, is just where this -n is lacking. We can legitimately say, then, that in these Plural forms Definite and Indefinite have no realisation independent of that of Plural itself. The conditions are thus fulfilled for recognising a syncretism:

(908) Def = Indef / Plural

Of more interest is the homonymy between Genitive and Dative in the Plural of both muḥallimūn and ḥayawānūn. In shape, the Acc-Gen form ḥayawānātin 'looks' Genitive rather than Accusative, because -i(n) is, in the Singular of all three nouns as well as in the Plural too of rajulūn, a marker of Genitive solely. The homonymy is therefore, seemingly, an attraction. This is supported by the fact that the morphosyntactic context Plural has a pretty clear principal exponent in the paradigm of ḥayawānūn, namely the suffix -āt-. Yet this -āt- is more central than the -in which realises the neutralised properties. The attraction is therefore not of the kind which is classified as systematic under part (b) of the Systematic Homonymy Claim. It must therefore be accidental, apparently.

The behaviour of muḥallimūn suggests that this rather un-

welcome conclusion about ḥayawānūn may be wrong, however. In the paradigm of muḥallimūn, the shape of the homonymous forms again suggests an attraction from Acc to Gen. This time, however, if we look for an exponent of Plural, the most obvious candidate seems to be the element -na, which is more peripheral than the Gen-Acc -ī. This time, therefore, if we treat the homonymy as an attraction, it does comply with the requirement of part (b) of the Systematic Homonymy Claim, and can be classified as systematic:

(909) Acc → Gen / _____ + Plural

What makes one homonymy seem accidental and one systematic, then, is a difference in order of realisation of contextual and neutralised properties. Is there any plausible way of exploiting this difference so as to render both homonymies systematic after all? I will suggest tentatively that there is, given quite natural assumptions about the interaction of 'homonymy rules' and spell-out rules -- assumptions that are consistent with our treatment of the Latin verb-form regam in section 9.2f.

I will assume that in syntactic surface structure the property-combination Accusative Plural on all Arabic nouns is an unordered bundle, so that the Accusative Plurals of raḥulūn, muḥallimūn and ḥayawānūn could be represented something like

[raḥul], [muḥallim] and [ḥayawān]. But we found in section 9.2f
 [Acc.], [Acc] and [Acc] evidence
 [Pl] [Pl] [Pl]

for a stage intermediate between the syntactic surface structure representation of inflected word-forms and the phonological representation which follows morphological 'spell-out', namely a stage at which systematic homonymy 'rules' operate -- rules which specify, in the case of syncretisms, that two or more properties or combinations of properties are realised alike, and, in the case of attractions, that one property 'becomes' another for the purpose of inflexional realisation. Moreover, by virtue of the importance that the Systematic Homonymy Claim lays on the order in which properties are realised, such homonymy rules clearly presuppose at least a partial 'unpacking' of the property bundles which are associated with filled lexical nodes at the output of the

syntax. Thus, for the attraction rule (907) to apply so as to produce a Future Indicative form such as regam or regar,

the property-bundle $\left[\begin{array}{l} \text{1st Person} \\ \text{Singular} \\ \text{Future} \\ \text{Indicative} \end{array} \right]$ must already have been 'unpacked'

to yield $\left[\begin{array}{l} \text{Future} \\ \text{Indicative} \end{array} \right] + \left[\begin{array}{l} \text{1st Person} \\ \text{Singular} \end{array} \right]$. So, to handle regam (quite

apart from any other considerations) we need to posit not only 'homonymy rules' and realisation or 'spell-out' rules but also 'unpacking' or sequencing rules distinct from both the other types; and, taking morphosyntactic property bundles as our starting-point and their phonological realisations as our goal, rules of these three types must at least sometimes apply in the following order:³

- (910) 1. Sequencing rules
2. Homonymy rules
3. Spell-out rules

I say 'at least sometimes' rather than 'always' because there is evidence that in some situations sequencing is partially dependent on phonological factors; for example, in the Zulu Immediate Past Continuous Tense, where the Tense-marker be- "precedes the Participial Subject Concord except where the latter has no consonant" (Rycroft & Ngcobo 1979: 100). Situations like this raise a whole host of questions about the detailed operation of realisation rules which I will not attempt to tackle in this thesis. But, if we take it that the distinction between the three types of rule is adequately justified and that the interaction between them may (as in the Zulu example) be quite complex, we can construct at least a tentative account of the behaviour of the Arabic nouns which will reconcile the Plural of ḥayawānun with the Systematic Homonymy Claim.

As (717) and (716) illustrate, the stem, the Plural inflexion and the Case inflexion may be linearly ordered in Classical Arabic nouns in three ways:

- (911) a. 'Broken' Plurals (e.g. rajulun): $\left[\begin{array}{l} \text{Stem} \\ \text{Pl} \end{array} \right] + \text{Case}$
- b. 'Sound' Plurals in -ūna (e.g. muḥallimun):
Stem + Case + Plural

(911) (continued)

c. 'Sound' Plurals in -ātun (e.g. hayawānun):

Stem + Plural + Case

Now, describers of Arabic seem traditionally to regard the main morphological distinction in declension as that between 'broken' Plurals and the rest; the distinction between different types of 'sound' Plural is less fundamental. Taking our cue from this, we can easily enough account for the various linear orders illustrated in (911) by means of two stages of sequencing:

(912) Sequencing rules for Arabic Plurals:

Stage 1 (distinguishing 'broken' and 'sound' Plurals):

$$\left[\begin{array}{c} \text{Stem} \\ \text{Pl} \\ \text{Case} \end{array} \right] \rightarrow \left\{ \begin{array}{l} \left[\begin{array}{c} \text{Stem} \\ \text{Pl} \end{array} \right] + \text{Case} / \text{broken-Plural nouns} \\ \text{Stem} + \left[\begin{array}{c} \text{Pl} \\ \text{Case} \end{array} \right] / \text{sound-Plural nouns} \end{array} \right.$$

Stage 2 (distinguishing two types of 'sound' Plural):

$$\left[\begin{array}{c} \text{Pl} \\ \text{Case} \end{array} \right] \rightarrow \left\{ \begin{array}{l} \text{Case} + \text{Plural} / \text{-ūna class} \\ \text{Plural} + \text{Case} / \text{-ātun class} \end{array} \right.$$

The next step is to relate to these sequencing rules the homonymy rule (or rules) needed to account for the Acc-Gen homonymies.

In my account of regam, I assumed that the relevant homonymy rule applied after sequencing but before spell-out; indeed, this was the only ordering relationship available, since we had no ground for splitting the sequencing process into stages as we have for Arabic. But, given that the sequencing process in Arabic is thus split, there are fairly obvious alternative positions for the Acc-Gen homonymy rule: before and after Stage 2 of sequencing. Let us consider the two alternatives in turn, beginning with the second. As we have already seen, this works satisfactorily for -ūna-type sound Plurals in the sense that we can frame an attraction rule for it which is systematic in terms of the Systematic Homonymy Claim:

(913) Acc → Gen / _____ + Pl

But it does not work for the -ātun-type sound Plurals, since they seem to require a distinct rule such as the following, which cannot be classed as a systematic attraction consistently with the Systematic Homonymy Claim:

(914) Acc → Gen / Pl + _____

What is more, from the analysis involving the pair of rules (913) and (914) there flows no natural explanation for why the same Acc-Gen homonymy does not apply in broken Plurals such as that of rajulun in (716); the restriction of rule (914) to sound Plurals (such as that of ḥayawānun), which is necessary to prevent it from applying when the realisation of Plural is simultaneous with the stem, is an arbitrary 'extra' fact. On the other hand, if we adopt the first of the two alternative positions, these difficulties disappear. If the homonymy rule applies before Stage 2 of sequencing, it applies before Case and Plural have been 'unpacked' in sound Plurals; consequently, it can perfectly well count as systematic, and indeed as a syncretism:

(915) Acc = Gen / Plural

This rule, in virtue of its position before Stage 2, takes care of both the muḥallimūn homonymy and the ḥayawānun one; moreover, it takes care of the fact that there is no Acc-Gen homonymy in the broken Plural of rajulun, since in this noun Case will have become detached from Plural at Stage 1 of sequencing. On the basis of this account, then, the homonymies in both kinds of sound Plural are syncretisms, not attractions, and are thus both classifiable as systematic. At (916) I illustrate how the various types of rule apply in the 'derivation' of the inflexional realisations for the Accusative Plural of all three of our exemplary nouns:

(916)	Broken Pl	Sound Pl in <u>-una</u>	Sound Pl in <u>-atun</u>
Morphosyntactic representation	[RAJUL Plural Acc]	[MUḤALLIM Plural Acc]	[HAYAWĀN Plural Acc]
Sequencing: Stage 1	[RAJUL] Pl + Acc	M. + [Pl Acc]	H. + [Pl Acc]
Homonymy rule (915)	Cannot apply	M.+ [Pl Acc/Gen]	H.+ [Pl Acc/Gen]
Sequencing: Stage 2	Does not apply	M.+ Acc/Gen + Pl	H.+ Pl + Acc/Gen
Spell-out	rijalan	muḥallimīna	ḥayawānātin

There are two problems with this account, however, one of them merely apparent and the other genuine. I will deal with the apparent one first. It has to do with the similarity of the Acc-Gen endings (-ī- or -in) above to the specifically Genitive ending observable in the Singular (-in in all the Indefinite paradigms of (716) and (717)). The analysis that I first put forward for the Acc Pl forms mufallimīna and ḥayawānātin involved attraction from the Accusative to the Genitive in the Plural. This obviously accounts for the similarity. But I have now rejected that attraction analysis in favour of one involving syncretism. Yet, seemingly, a syncretism analysis has nothing to say about the similarity, since the ultimate realisation of the property Accusative in the form ḥayawānātin in the last line of (916) will be taken care of by a special rule (or rules) for spelling out Acc/Gen, and not by the rule that spells out Genitive as -in in the Singular form ḥayawānin. The similarity of the word-final affixes in ḥayawānātin and ḥayawānin therefore emerges as accidental. Is this not a drawback?

The answer is no, for a reason connected with the poverty of the Arabic vowel system. In all or nearly all Arabic declensional patterns, Case-endings differ from each other only in the quality of their vowels. Given that the Nominative (Indefinite) ending generally is -un, this means that we can expect the Acc/Gen ending to be of the shape -Vn, where V is some vowel. Yet Arabic has only three vowels (at least underlyingly), namely a, i and u. It follows that the Acc/Gen ending, if it is to be distinct from the Nom -un, can only be -in or -an; in other words, it is bound to be homonymous with either the 'normal' Genitive ending -in or the 'normal' Accusative -an. The fact that our analysis requires us to treat the homonymy between the Genitive -in and the Acc/Gen -in as accidental from a morphological point of view is therefore no embarrassment, because homonymy between the Acc/Gen ending and one of the other Case endings is inevitable.

The genuine problem with my account has to do with the power of the machinery I have invoked to deal with these Arabic facts. Each part of the machinery -- the sequencing rules, the

homonymy rules and the spell-out rules -- is independently motivated; but because of the complexity of the relationship between a set of morphosyntactic properties and their exponents which the machinery allows, we must be on our guard against the possibility of invoking the machinery to deal with not only actual *prima facie* counter-examples to claims such as the Systematic Homonymy Claim but any conceivable counter-examples. For example, if we permit without restriction homonymy rules to precede sequencing rules, we will be able to 'legitimise', or reconcile with the Systematic Homonymy Claim, even the sort of behaviour which we posited for pseudo-Turkish at (812) in section 8.5 and whose absence from the data of Appendix E was fundamental to the Claim. Before we can rest content with the sort of account presented at (916), then, we must establish precise conditions under which homonymy rules can be interspersed among sequencing rules. But bearing in mind that sequencing rules can apparently under some circumstances be interspersed among rules of the third type illustrated in in (916), namely spell-out rules, this will be a difficult task; and I will not attempt it here.

9.2 h 3rd Singular Feminine and 2nd Singular Masculine of prefix conjugation in Semitic (example 33)

As example 33 in Appendix E I cited the homonymy between 3 Sg Fem and 2 Sg Masc in the Hebrew 'prefix conjugation', which is used alongside a morphologically quite distinct 'suffix conjugation' to express a syntactic contrast which for present purposes we can simply label Aspectual: Imperfective versus Perfective. The same homonymy is found in the 'prefix conjugation' of all ancient and modern Semitic languages, whatever its syntactic function. On grounds of stability, then, it seems reasonable to regard the homonymy as systematic. Moreover, since in the Hebrew forms the contextual properties Imperfective and Singular have no exponents (or, at any rate, no affixal exponents) which are obviously distinct from the exponents of the neutralised property-pairs 3 Fem and 2 Masc, the homonymy seems to fall neatly into place as a syncretism, expressible thus:

$$(917) \begin{bmatrix} 3 \\ \text{Fem} \end{bmatrix} = \begin{bmatrix} 2 \\ \text{Masc} \end{bmatrix} / \begin{bmatrix} \text{Imperfective} \\ \text{Sg} \end{bmatrix}$$

If we look slightly further afield, however -- first at the Plural of the prefix conjugation in Hebrew and then at the prefix conjugation as a whole in classical Arabic --, we will observe that this analysis in terms of syncretism seems to miss something, in that relationships in these paradigms between morpho-syntactic properties and their exponents seem to be rather more complex than that analysis implies. I will therefore offer an alternative analysis which makes more sense of this complexity. I do not claim that the evidence in favour of this alternative analysis is by any means conclusive; but it is worth discussing, partly because it illustrates the same sort of interaction between sequencing, homonymy and spell-out that I posited for the Latin and Arabic facts discussed in the previous two sections, and partly because the account of the homonymies which emerges still fits the Systematic Homonymy Claim.

The complete Hebrew prefix conjugation of the simple tri-consonantal stem q-b-r 'bury' is as follows:⁴

(918) :	Singular	Plural
3 Masc	yiqbor	yiqbārū
3 Fem	tiqbor	tiqbōrnā
2 Masc	tiqbor	tiqbārū
2 Fem	tiqbārī	tiqbōrnā
1	'eqbor	niqbor

The homonymy listed as 33 of Appendix E is indicated by the box. But I would like to draw attention to two further features of this paradigm. Firstly, if we ignore the Feminine forms, we find a neat correlation between prefix and Person: 3rd yi-, 2nd ti- and 1st 'e- or ni- according to Number. This suggests that the boxed homonymy might more aptly be regarded as an attraction than a syncretism. Secondly, not only the 3 Sg Fem but also the 3 Pl Fem form has a 2nd Person homonym, but this homonym is the 2 Pl Fem rather than the 2 Pl Masc. In the Plural (as opposed to the Singular) there is a clear exponent of Feminine Gender distinct from those of 2nd or 3rd Person, namely the suffix -nā, so this homonymy can almost certainly not be regarded as a syncretism; yet it would clearly be an advantage (other things being equal) if our description could relate the Sg and Pl homonymies to each

other in some way.

Let us turn now to the second promised piece of new data, namely the corresponding paradigm for q-b-r 'bury' in classical Arabic (where we find a Dual Number as well as a Singular and Plural):

(919)	Singular	Dual	Plural
3 Masc	yaqburu	yaqburāni	yaqburūna
3 Fem	taqburu	taqburāni	yaqburna
2 Masc	taqburu	taqburāni	taqburūna
2 Fem	taqburīna	taqburāni	taqburna
1	'aqburu	-	naqburu

Here we find in the Singular the same 3 Fem/2 Masc homonymy as in Hebrew, extending to the Dual too; moreover, if we ignore the Dual and (for the moment) the 3 Pl Fem, we find a perfect correspondence between the Hebrew and Arabic paradigms, in that the Hebrew affixes yi-, ti-, 'e, - \emptyset , -i, -ū and -nā are distributionally identical to the Arabic ya- ta-, 'a-, -u, -īna, -ūna and -na respectively. On the other hand, there are no homonymies in the Plural of the Arabic paradigm at all. What we find in Arabic, in fact, is an exact correlation in the Plural of the prefixes ya- and ta- with the properties 3rd Person and 2nd Person respectively.

Clearly, the degree of isomorphy between the Hebrew and Arabic paradigms requires to be reflected in our descriptions of the property-exponent relationships in the two languages, unless we find strong evidence to the contrary. At the same time, our descriptions must accommodate the differences between Hebrew and Arabic with regard to homonymy: the fact that they share a homonymy between certain 3rd and 2nd Person forms in the Singular but that only Hebrew displays any such homonymy in the Plural. A further relevant fact is that the Arabic state of affairs is apparently older than the Hebrew one. Ugaritic and the Palmyrene dialect of Aramaic are reported to resemble Hebrew in this respect, but all other attested Semitic languages seem to resemble Arabic, including the other dialects of Aramaic (which belong

to the same North West subfamily of Semitic as Hebrew does); and it is usual (though not totally uncontroversial) to postulate for proto-Semitic a 3 Pl Fem form with the prefix ya-, not ta- (Moscatti 1964: 144). Assuming that the Hebrew state of affairs is an innovation, therefore, we require (other things being equal) an analysis according to which the Hebrew Plural paradigm, being less conservative, is in some sense simpler than the more conservative Arabic one; and we require this despite the fact that it is Arabic and not Hebrew that displays the neat one-to-one relationship between Persons and prefixes in the Plural.

I will propose now an analysis of the property-exponent relationships in the Arabic and Hebrew prefix conjugations which fulfils all these requirements. First the Arabic paradigm:

- (920) Sequencing: a. 2 Sg Fem is suffixed.
 b. Person is prefixed.
 c. Number is prefixed in 1st Person, suffixed in 2nd and 3rd Persons.
 d. Gender is suffixed.

Homonymy
 (attraction): e. $3 \rightarrow 2 / \text{---} + \text{Stem} + \left[\begin{array}{l} \text{Fem} \\ \text{Sg or Du} \end{array} \right]$

- Spell-out
 - prefixal: f. $1 \rightarrow \left\{ \begin{array}{l} 'a- / \text{Sg} \\ na- / \text{Pl} \end{array} \right.$
 g. $2 \rightarrow ta-$
 h. $3 \rightarrow ya-$
 - suffixal: j. $2 \text{ Sg (Fem)} \rightarrow -\bar{i}na$
 k. $Du \rightarrow -\bar{a}ni$
 m. $Pl \rightarrow \left\{ \begin{array}{l} -\bar{u}na / \text{Masc} \\ -na / \text{Fem} \end{array} \right.$
 n. $Sg \rightarrow -u$
 - stem: p. $CCC \rightarrow -CCuG-$

Note particularly that the attraction rule at (920 e) fulfils the requirements imposed by the Systematic Homonymy Claim for classification as systematic, since the contextual properties Sg (or Du) and Fem do not have a principal exponent more central than the neutralised properties. I will illustrate the operation

of these rules in accounting for the realisation of the Arabic forms taqburu (3 Sg Fem) 'she buries' and yaqburna (3 Pl Fem) 'they (Fem) bury':

(921)

Morphosyntactic representation:	$\begin{bmatrix} Q & B & R \\ 3 \\ \text{Sg} \\ \text{Fem} \end{bmatrix}$	$\begin{bmatrix} Q & B & R \\ 3 \\ \text{Pl} \\ \text{Fem} \end{bmatrix}$	
Sequencing:	$3 + \text{QBR} + \begin{bmatrix} \text{Sg} \\ \text{Fem} \end{bmatrix}$	$3 + \text{QBR} + \begin{bmatrix} \text{Pl} \\ \text{Fem} \end{bmatrix}$	by (920 a)
Homonymy:	$2 + \text{QBR} + \begin{bmatrix} \text{Sg} \\ \text{Fem} \end{bmatrix}$	-	by (920 e)
Spell-out:	ta-qbur-u	ya-qbur-na	by (920 g, h, m, n, p)

The analysis that I propose for the Hebrew paradigm differs significantly only in the homonymy rule, (922 e):

(922) Sequencing: a.
 b.
 c.
 d. } As for Arabic ((920) above)

Homonymy (attraction) : e. $3 \rightarrow 2 / \text{---} + \text{Stem} + \text{Fem}$

Spell-out

-prefixal: f. $1 \rightarrow \begin{cases} \text{'e-} / \text{Sg} \\ \text{ni-} / \text{Pl} \end{cases}$

g. $2 \rightarrow \text{ti-}$

h. $3 \rightarrow \text{yi-}$

- suffixal: j. $2 \text{ Sg (Fem)} \rightarrow \text{-ī}$

[k. Not applicable, as there is no Dual]

m. $\text{Pl} \rightarrow \begin{cases} \text{-ū} / \text{Masc} \\ \text{-nā} / \text{Fem} \end{cases}$

n. $\text{Sg} \rightarrow \emptyset$

- stem: p. $\text{CCC} \rightarrow \text{-CCoC}^{-5}$

As will be seen, the homonymy rule posited for Hebrew at (922 e) is simpler than that posited for Arabic at (920 e), in that it applies to all (or rather both) Numbers, and not merely to some. How this slight simplification of the homonymy rule accounts for the difference between Arabic and Hebrew in the 3 Pl Fem

is illustrated in the following 'derivations' of Hebrew verb-forms corresponding to the Arabic ones of (921):

(923)

Morphosyntactic representation:	$\begin{bmatrix} Q & B & R \\ 3 \\ Sg \\ Fem \end{bmatrix}$	$\begin{bmatrix} Q & B & R \\ 3 \\ Pl \\ Fem \end{bmatrix}$	
Sequencing:	$3 + QBR + \begin{bmatrix} Sg \\ Fem \end{bmatrix}$	$3 + QBR + \begin{bmatrix} Pl \\ Fem \end{bmatrix}$	by (922 a-d)
Homonymy:	$2 + QBR + \begin{bmatrix} Sg \\ Fem \end{bmatrix}$	$2 + QBR + \begin{bmatrix} Pl \\ Fem \end{bmatrix}$	by (922 e)
Spell-out:	ti-qbor	ti-qbor-nā	by (922 g, m, n, p)

The two derivations set out at (921) and (923) illustrate two points worth emphasising. First, because the Hebrew homonymy rule has a wider scope than the Arabic one, the 3rd Person prefixal spell-out rule (922 h) plays no part in the Hebrew realisation of either of these Feminine 3rd Person forms. This is just like what happens with the Latin forms such as the 1st Person Singular Imperfective Future Indicative regam, according to my analysis: the property Indicative has been replaced by Subjunctive by the time that spell-out takes place, so the spell-out rule for Indicative is never involved. A second and more important point concerns the effect of the attraction rules at (920 e) and (922 e). The property which undergoes attraction is a property (3rd Person) to which a sequencing rule has already applied, namely rule (920 b) or rule (922 b); consequently, this property does not acquire the same exponents as the attracting property everywhere in the word, but only those exponents which will subsequently be spelled out at a particular position in the word. So, since it is only prefixal, not suffixal, occurrences of the property 3rd Person that rules (920 e) and (922 e) convert to 2nd Person, it is no surprise that the 3rd Person Singular Feminine form does not acquire the suffixal exponent of the 2nd Singular Feminine, namely -īna in Arabic and -ī in Hebrew. The fact that in Hebrew the operation of rule (922 e) yields 3 Fem forms which are homonymous with 2 Masc in the Sg but 2 Fem in the Pl can thus be accounted for without any special complication in

the homonymy rule itself; the contrast is a natural consequence of the fact (as expressed in rules (922 m) and (922 n)) that the suffixal realisation of Plural is sensitive to Gender but that of Singular is not.

As I have said, I believe that my synchronic account of the morphology of both the Hebrew and Arabic prefix conjugations derives some support from its implication that, from the diachronic point of view, the change from the older proto-Semitic system essentially preserved in Arabic to the newer Hebrew system involves a simplification in the attraction rule. But, one might say, this diachronic support is weak unless I can offer some suggestion about why Hebrew did not adopt the more obvious simplification, namely that of doing ^{away} with the attraction rule altogether. The first point I would make in reply to this is that, as I said in Chapter VIII, the Systematic Homonymy Claim does not purport to provide a complete account of inflexional homonymy. It says nothing about sufficient conditions either for the occurrence of inflexional homonymy or for its non-occurrence, but deals only with conditions which must necessarily be fulfilled in order for an inflexional homonymy to be morphologically systematic. So the fact that the Claim has nothing to say about what promotes the maintenance of the attraction rule is no serious drawback. Having made that point, however, I think one can find a plausible reason, independent of the Systematic Homonymy Claim, for the maintenance of the rule.

If the attraction rule were completely removed from the set of rules for Arabic at (920), it is the realisation of the 3 Sg and Du Fem forms which would be affected; specifically, they would emerge with an initial prefix ya- rather than ta-. But this would result in another homonymy, namely one between the two Genders in the 3 Sg and the 3 Du. In other words, in place of the actual Arabic paradigm at (919), we would find the following:

(924)	Singular	Dual	Plural
3 Masc	yaqburu	yaqburāni	yaqburūna
3 Fem	yaqburu	yaqburāni	yaqburna

(924) (continued)	Singular	Dual	Plural
2 Masc	taqburu	taqburāni	taqburūna
2 Fem	taqburīna	taqburāni	taqburna
1	'aqburu	-	naqburu

But there is something odd about this outcome, from a general linguistic point of view. It has often been suggested that the 3rd Person Singular is the least 'marked' member of the Person-Number category, and that it is because of this unmarked status that in several languages it has formed the starting-point for analogical re-formation of other Person-Number forms (Bynon 1977: 101 and references cited there). Another justification for its unmarked status is the fact that, if any morphosyntactic category such as Gender is applicable to other Person-Number slots, it will generally be applicable to 3rd Singular too; thus, for example, in Zulu the multi-term noun Class system, which affects verbal inflexion in the 3rd Person Plural, does indeed operate in the 3rd Singular too, although not in either the 1st or 2nd Persons Singular.⁶ In (924), on the other hand, we observe a Gender distinction between Masc and Fem morphologically expressed in three relatively 'marked' Person-Number slots (2 Sg, 2 Pl and 3 Pl) but unexpressed in the putatively least 'marked' slot, the 3 Sg. This immediately suggests a motivation for the maintenance of some form of attraction along the lines of (920 e) and (922 e) in all the Semitic languages: to have abandoned it would have resulted in a typologically unusual and for that reason probably 'difficult' and unstable inflexional pattern. We have here, then, at least a partial explanation for why in Hebrew the proto-Semitic attraction rule was simplified rather than abandoned entirely. Of course, one's attitude to this partial explanation will depend upon one's attitude to typological generalisations of the Greenbergian kind generally; but that is a large issue which it would be impossible to discuss adequately here.

My whole analysis of the Hebrew and Arabic prefix conjugations must, of course, remain tentative until we know much more about the nature and interaction of the three types of rule (se-

quencing, homonymy and spell-out) that I have posited.⁷ But one can at least say, I think, that the analysis presented captures what seems intuitively to be systematic about the homonymies in the Hebrew and Arabic prefix conjugations in a fashion consistent with the Systematic Homonymy Claim, and also relates in a diachronically plausible fashion the Hebrew distribution of inflections to what was probably the common Semitic distribution, preserved in Arabic and most other Semitic languages.

9.3 Further homonymies in Italian and Yurok

9.3 a Italian *parlassi* 'I/you spoke (Imperfect Subjunctive)'

In section 9.2d I argued that, although the Singular homonymy in the medieval Italian Imperfect Indicative paradigm had to be classed as accidental on the basis of the Systematic Homonymy Claim, this was no drawback because subsequent changes suggested that that classification was correct. But there is another apparently accidental homonymy in the paradigms of all Italian verbs which has not been removed by any morphological innovation. Consider the Imperfect (or Past) Subjunctive forms of parlare 'to speak'.

(925)	Sg 1	parl- <u>áss</u> -i
	2	parl- <u>áss</u> -i
	3	parl- <u>áss</u> -e
	Pl 1	parl- <u>áss</u> -imo
	2	parl- <u>ás</u> -te
	3	parl- <u>áss</u> -ero

The 1st and 2nd Singular forms are homonymous. Furthermore, if we take it that the context for the homonymy is the property-combination Imperfect Subjunctive, the homonymy seems to be unequivocally accidental (according to the Systematic Homonymy Claim), because the contextual properties have a clear principal exponent -ass- [as] (spelled -as- before a consonant) which is more central than the neutralised properties.

As in section 9.2d, I will argue that this classification as accidental is correct, but for a different kind of reason. Consider the paradigm of a 'strong' Preterite -- that is, a Preterite exhibiting the sort of stem allomorphy discussed in

section 7.2:

(926) Sg 1	féc-i	'I made'
2	fac-ésti	
3	féc-e	
Pl 1	fac-émmo	
2	fac-éste	
3	féc-ero	

Let us now set the endings of (926) and (925) alongside:

	(927) a. 'Strong' Preterite Indicative	b. Imperfect Subjunctive
Sg 1	-i	-i
2	-esti	-i
3	-e	-e
Pl 1	-emmo	-imo
2	-este	-te (or /ste/)
3	-ero	-ero

Two points emerge straight away. Firstly, in three of the six slots these two sets of endings are the same, namely the 1st and 3rd Singular and the 3rd Plural; secondly, these three slots are just those in which the strong Preterite ending is unstressed. When we recall that the -ass- element marking Imperfect Subjunctive in (925), and the corresponding element in all Italian verbs, is always stressed, a phonological basis for the choice between the (927 a) endings and the (927 b) endings suggests itself: the (927 b) set follows stems (in the 'stem₃' sense) that are intrinsically stressed, while the (927 a) set follows other stems. The spell-out rules for (927 a) and (927 b) jointly will look something like this:

(928) 1 Sg	-i
2 Sg	-i / [+ stress] + _____ -ésti elsewhere
3 Sg	-e
1 Pl	-imo / [+ stress] + _____ -émmo elsewhere
,2 Pl	-ste / [+ stress] + _____ -éste elsewhere
3 Pl	-ero

Bringing the Preterite Indicative and Imperfect Subjunctive endings together in this way is supported diachronically by the fact that, in the Singular of the Imperfect Subjunctive, the -i -i -e pattern represent an innovation, replacing an accidentally homonymous inherited ⁸ -e -e -e pattern (< Latin -em -ēs -et), and the source of the innovation is thought by at least some scholars to be the Preterite Indicative. Thus Grandgent (1927: 162): "Inasmuch as -em, -ēs, -et all gave the same result, the three persons of the singular originally ended alike in -e. Early, however, the example of the perfect [i.e. Preterite] led to the introduction of -i in the 1st and 2d: facessi facessi facesse, after feci facesti fece". This diachronic support would be reinforced if there were found to be morphological innovations in the Plural of the Imperfect Subjunctive which could plausibly be attributed to the same process of paradigmatic amalgamation. And this is in fact the case; in parlare, for instance, where purely phonological development would yield endings such as "-assémo -asséte -ássen(o)", we in fact find -ássimo -áste -ássero (as indicated at (925)), where the 3rd Plural -ero is clearly 'borrowed' from the Preterite.

Synchronically, one must admit, this account cannot be regarded as complete until it has been extended to cover two further sets of Preterite endings partially similar to those in (927): the 'weak' second conjugation set in which the exponents of 1 Sg, 3 Sg and 3 Pl (-éi, -é and -érono) have stress, and the set in which they are preceded by an 'empty' stress-bearing element -étt- (-étt-i, -étt-e, -étt-ero). But the important point for our purposes, if the analysis in (928) is provisionally accepted, is that the apparently unappealing analysis of the homonymy between 1 Sg and 2 Sg in (925) as accidental is confirmed to be correct. This is because the spell-out rules for the two slots are clearly distinct: only the former yields -i as the sole exponent, while the latter yields -i as a phonologically conditioned allomorph of -ésti. The situation is thus exactly parallel to that of the spell-out rules for the Dative and Ergative Cases in Dyirbal discussed in connexion with example (804 d) in section 8.3: the former rule will yield -gu in all phonological environ-

ments while the latter rule will do so only in some. We have another example here, then, of a homonymy which looks at first as if it ought to be considered systematic but whose classification as accidental by the Systematic Homonymy Claim is found on closer examination to be motivated independently.

9.3 b The bipersonal conjugation in Yurok

Yurok is (or was) an Algonkin language of northern California, described by Robins (1958). The homonymies in its bipersonal conjugation are of interest because they involve questions about sequencing. As usual, I do not pretend here to offer a definitive analysis; but, once again, I suggest that a plausible analysis is available which is consistent with the Systematic Homonymy Claim.

In the bipersonal conjugation of Yurok verbs, some but not all logically possible combinations of pronominal subject and object are indicated by a suffix usually analysable, *prima facie*, into an object element and a subject element in that order. For the o-class, one of the two main conjugation-classes of Yurok verbs, the bipersonal suffixes are as follows:

	(929) Subject:					
	Singular			Plural		
	1	2	3	1	2	3
Object Sg 1		-opa?	-ope?n		-opa?	-opa:±
2	-ocek'			-ocoh		
3	-osek'	-ose?m		-osoh	-ose?m	
Pl 1						
2	-oc'o?			-oc'o?		
3	-os?o?					

In the larger e-class the forms are as follows:

	(930) Subject:					
	Singular			Plural		
	1	2	3	1	2	3
Object: Sg 1		-a?	-(ep)e?n		-a?	-epa:±
2	-icek'			-icoh		
3	-isek'	-ese?m		-esoh	-ese?m	
Pl 1						
2	-ic'o?			-ic'o?		
3	-es?o?					

As will be seen, the e-class forms are mostly the same as the o-class ones except that they show an -e- or -i- in place of the suffix-initial -o-; but there are greater differences in the 1st Singular Object series. There are three homonymies in each of the two conjugation-classes, indicated in the diagrams by arrows. How do these fit the Systematic Homonymy Claim?

First let us consider the -ose?m pair in (929); my remarks will also apply to the -ese?m pair in (930). Since -os- appears as the first element in all the suffixes of the third line of (929), it seems reasonable to regard it as the exponent of 3rd Singular Object. The homonymy is thus between Singular and Plural as regards 2nd Person Subject in 3rd Singular Object contexts. One way of expressing this might be as follows:

$$(931) \text{ Sg} = \text{Pl} / \begin{array}{l} \text{3rd} + \text{2nd} \\ \text{Sg} \\ \text{Obj} \quad \underline{\text{Subj}} \end{array}$$

We are forbidden by our definitions to regard this as a syncretism, since the context for the homonymy is only partially simultaneous with the neutralised properties Singular and Plural (Subject); 2nd Subj is realised simultaneously with these properties but 3rd Sg Obj is not. If, however, we look at the unipersonal conjugation in Yurok, evidence for an attraction analysis appears. In the unipersonal conjugation, only the pronominal subject, not the object, is realised by a suffix, thus (ignoring certain details irrelevant here):

	Singular			Plural		
	1	2	3	1	2	3
<u>e</u> -class	-ek'	-e?m	-?	-oh	-u?	-eł
<u>o</u> -class	-ok'	-o?m	-?	-oh	-o?w	-oł

From this we can see that the element -e?m in -ose?m resembles the specifically Singular 2nd Person suffix in the e-class unipersonal conjugation, and not either of the Plural 2nd Person suffixes -u? and -o?w. We therefore have grounds for regarding the -ose?m homonymy as an attraction from Plural to Singular:

$$(933) \text{ Pl} \rightarrow \text{Sg} / \begin{array}{l} \text{3rd} + \text{2nd} \\ \text{Sg} \\ \text{Obj} \quad \underline{\text{Subj}} \end{array}$$

And, since part of the conditioning context for the homonymy is

simultaneous with the neutralised properties (namely the properties 2nd Person and Subject), it meets the Systematic Homonymy Claim's requirements for being classed as a systematic attraction. One can, of course, legitimately ask why the attraction should occur -- why, in other words, we do not find a 2nd Plural Subject form such as "-osu?" contrasting with a form -ose?m restricted to 2nd Singular Subject function. But the Systematic Homonymy Claim states only necessary, not sufficient, conditions for systematic homonymies, so it is not incumbent on me now to propose any answer to that question.

I turn now to the -oc'o? homonymy of (929) (corresponding to the -ic'o? homonymy of (930)). An attraction analysis, like that offered for -ose?m, is possible here too, but is not compelling; and there is little to choose between it and a syncretism analysis involving a different view of the sequence in which the various relevant properties are realised. Note first the contrast between the 2nd Plural Object form -oc'o? and the 2nd Singular Object form -ocek'. Assuming that what we are dealing with in both these forms is an object element and a subject element in that order, it is at least possible to analyse the 2nd Person Object elements as displaying a Singular-Plural contrast involving glottalisation: Singular -oc- versus Plural -oc'-. This is supported by the 3rd Person Object forms, which contain consistently an element -os- in the Singular contrasting with -os? in -os?o?, the only bipersonal suffix incorporating a 3rd Plural Object function. On this basis, the homonymy involves essentially only the final element -o?. Can this be described as either a syncretism or a systematic attraction? It clearly cannot be a syncretism, since part of its morphosyntactic context is not simultaneous, thus:

(934) Sg = Pl / 2nd + 1st
 Pl
 Obj Subj

Yet, if we are to call it an attraction instead, we must be able to say in which direction the attraction takes place: whether Singular to Plural or vice versa. This amounts to asking whether -o? functions anywhere else as either a specifically Singular

or a specifically Plural 1st Person suffix. When considering the corresponding question about -ose?m, we were able to find an answer in the unipersonal conjugation: -e?m was specifically Singular there. But -o? appears as neither a Singular nor a Plural form for the 1st Person in the unipersonal conjugation, as (932) shows. There is a relevant occurrence of -o? elsewhere, however, namely in the bipersonal suffix -os?o? already mentioned. As (929) indicates, this suffix has only one morpho-syntactic function, since it lacks the 1st Plural Subject function that -oc'o? has. On the basis of this perhaps rather slender evidence, we can decide the direction of attraction, thus:

$$(935) \text{ Pl} \rightarrow \text{Sg} / \begin{array}{l} \text{2nd + 1st} \\ \text{Pl} \\ \text{Obj} \quad \underline{\text{Subj}} \end{array}$$

And this attraction can be classified as systematic in accordance with the Systematic Homonymy Claim, for just the same reason as (933).

An alternative analysis, involving syncretism, derives its plausibility from the limited distribution of the putative object element -oc'- and subject element -o? into which -oc'o? has been segmented. The suffix -o?, as we have seen, bears no similarity to either the 'usual' 1st Sg Subject suffixes -ek' and -ok' or the 'usual' 1st Pl Subject suffix -oh; and the putative object element -oc'- is peculiar to the suffix -oc'o?. Should we then treat -oc'o? and -os?o? as unsegmented wholes? To do so will involve positing bipersonal forms in which Object and Subject properties are realised simultaneously; but that is no obstacle, because we will need to recognise such simultaneity anyway, in the 1st Sg Object form -a? of the e-conjugation. What sort of systematic homonymy can -oc'o? be said to display, then, on this assumption? The answer is syncretism, fairly obviously, because the morphosyntactic context will be wholly simultaneous with the neutralised properties:⁹

$$(936) \text{ Sg} = \text{Pl} / \left[\begin{array}{c} \left[\begin{array}{c} \text{2nd} \\ \text{Pl} \\ \text{Obj} \end{array} \right] \\ \left[\begin{array}{c} \text{1st} \\ \underline{\text{Subj}} \end{array} \right] \end{array} \right]$$

And, if the homonymy is a syncretism, the Systematic Homonymy Claim again permits us to regard it as systematic.

The main drawback with this syncretism analysis is that it requires us to treat the parallel between non-glottalised Singular forms -oc- and -os- and glottalised Plural forms -oc' and -osʔ- as an accident, at least synchronically. This need not be a serious drawback; there are, after all, plenty of instances where phonological or morphological processes which were once productive in some language leave traces of themselves at later stages of the language in alternations which can no longer be plausibly related by any general rule. But it is pointless to speculate about this Yurok example further without any knowledge of the history of the language. The main point that I want to make here is that there is no great difficulty in finding some analysis of -oc'oʔ under which it can be regarded as both systematic and consistent with the Systematic Homonymy Claim.

The third Yurok bipersonal homonymy -- that of -opaʔ (or, in the e-conjugation, -aʔ) -- is more interesting. Just as -os- can be identified as an exponent of 3 Sg Obj in the third line of table (929), so -op- can be identified in the o-conjugation as an exponent of 1 Sg Obj. So, inasmuch as 1 Sg Obj constitutes part of the context for the -opaʔ homonymy, we cannot apparently call it a syncretism. In order to be able to call the homonymy systematic consistently with the Systematic Homonymy Claim, therefore, we must apparently produce grounds for calling it an attraction of the kind specified in the Claim. This means that we must find some context in which -aʔ functions either as a specifically Singular or a specifically Plural exponent of 2nd Person Subject. The problem before us is, so far, just like that of finding an -eʔm to 'go with' -oseʔm, or an -oʔ to 'go with' -oc'oʔ. But this time we get no help either from the unipersonal conjugation or from elsewhere in the bipersonal conjugation of o-class verbs. The exponents of 2nd Person Subject illustrated in (932) do not include -aʔ, and -aʔ occurs in no other 'slots' in the bipersonal conjugation either. So our attempt to analyse the -opaʔ

homonymy as an attraction founders too; and we are seemingly left with the conclusion that, if we are to maintain the Systematic Homonymy Claim, we must treat this homonymy as accidental -- not necessarily a wrong conclusion ultimately but definitely an unwelcome one at present, given the apparent propensity for homonymy in the Yurok bipersonal conjugation.

At this point, however, the principal difference between the e-conjugation and the o-conjugation comes into play. We find in the first line of table (930) that the element -ep-, corresponding to the o-class's -op-, is consistently present only in the 3 Pl Subj form; with the 2 Subj forms it is consistently absent. The suffix -a? must, therefore, be treated as realising simultaneously the properties belonging to both subject and object, and the schematic statement corresponding to (933), (935) or (936) might be:

$$(937) \text{ Sg} = \text{Pl} / \begin{bmatrix} \text{1st} \\ \text{Sg} \\ \text{Obj} \\ \text{2nd} \\ \text{Subj} \end{bmatrix}$$

The homonymy of the -a? pair in (930) can therefore legitimately be called a syncretism, and therefore systematic. But the analysis at (937) also suggests a new way of looking at the -opa? pair in the o-conjugation. We now have independent grounds, furnished by the e-conjugation, for regarding the property-combination 1st Sg Obj as realised not merely by the element -op- but also by the element -a? which we previously treated as realising 2nd Subj only. This has implications for the way in which sequencing operates in 1st Sg Object forms of the o-conjugation. On the strength of the forms of that conjugation alone, we would probably be inclined to posit a sequencing rule such as (938):

$$(938) \begin{bmatrix} \text{Stem} \\ \vdots \\ \text{Object} \\ \vdots \\ \text{Subject} \end{bmatrix} \rightarrow \text{Stem} + \begin{bmatrix} \vdots \\ \text{Object} \end{bmatrix} + \begin{bmatrix} \vdots \\ \text{Subject} \end{bmatrix}$$

-- that is, essentially, the Object properties and the Subject

properties are 'unpacked' suffixally in that order. But, on the strength of how -a? functions in the e-conjugation, we have reason for believing that, at least in 2nd Person Subject forms, the 1st Sg Object enjoys 'extended exponence', being assigned to not one but two suffixal positions:

$$(939) \begin{bmatrix} \text{Stem} \\ \text{[1st Sg Obj]} \\ \text{[2nd \dots Subj]} \end{bmatrix} \rightarrow \text{Stem} + \begin{bmatrix} \text{1st Sg Obj} \end{bmatrix} + \begin{bmatrix} \text{[1st Sg Obj]} \\ \text{[2nd \dots Subj]} \end{bmatrix}$$

The significance of (939) for the -opa? homonymy is that it, too, can now be regarded as a syncretism, in virtue of the same syncretism rule (937) that we invented to describe the -a? homonymy in the e-conjugation.

It is true, of course, that in -opa? the property combination 1st Sg Object does have an exponent -- in fact, a principal exponent -- which is not simultaneous with -a?, namely -op-; but the important point so far as the analysis of the homonymy is concerned is that we have independent evidence for saying that -a? too realises 1st Sg Object directly, and not merely indirectly as a conditioned allomorph of 2nd Subject. So, even though we cannot call the -opa? homonymy an attraction and there are at first sight solid objections to calling it a syncretism, we can nevertheless reconcile it with the Systematic Homonymy Claim because a closer look at the bipersonal forms of the e-conjugation as well as the o-conjugation provides evidence for regarding it as as a syncretism after all.

Footnotes to Chapter IX

1. One might prefer to segment the forms as parla-v-a etc., in which case it will be -v- that is the principal exponent of Imperfect; but this does not affect our present argument.
2. In my formulation at (907) I disregard as being unimportant at present the question of where Imperfective is realised in the forms regam and regar. In fact, it seems most plausible to regard it as having a principal exponent in the stem reg-, which consistently contrasts with a Perfective stem rex- [reks].
3. Warburton (1973), arguing for a sort of compromise between WP and IA approaches to modern Greek verb morphology, draws a rather similar distinction between 'segment transformations', which 'unpack' certain complexes of morphosyntactic properties into strings, and the realisation rules which subsequently assign phonological shape to the properties.
4. This and the subsequent Arabic examples are taken from Moscati (1964: 142).
5. I assume that the stem alternation -qbor- ~ -qber- is to be accounted for by a phonological rule rather than a phonologically sensitive spell-out rule, but the issue is not crucial here.
6. Greenberg's Universals 44 and 45 (1963: 96) are relevant here too, even though the hypothetical paradigm at (924) violates neither of them directly.
7. A more specific problem has to do with what this analysis implies for the pre-history of Semitic going back to proto-Hamito-Semitic. In Hamitic languages such as Berber, there is good synchronic evidence for analysing a verbal prefix t-, presumably cognate with the ta- and ti- of the Arabic and Hebrew prefix conjugations, as a specifically Feminine marker rather than a 2nd Person one (Theodora Bynon, personal communication). If the analyses presented in (920) and (922) are on the right lines, therefore, some reinterpretation of verbal morphology must have taken place at some point in the history of either the Semitic branch or the Hamitic branch of the Hamito-Semitic family. It

remains to be seen whether this is consistent with what is known about the historical facts.

8. Or ^x-e -i -e, if one assumes that Latin -ēs regularly yielded Italian -i (cf. Lausberg 1967: 82).

9. The combination of simultaneous properties in (936) is divided into two 'sub-combinations' because the Person and Number relevant to the Subject must obviously not be jumbled up with those relevant to the Object. This ad hoc notation does, of course, raise questions both about the organisation of morpho-syntactic properties on the 'plane of content' and about how sequencing works. I will not attempt to go into these questions here.

CHAPTER X

CONCLUSIONS AND NEXT STEPS

The aim of this thesis was to propose and defend certain generalisations about the relationship between morphosyntactic properties and their inflexional exponents. The search for such generalisations was expressed as a search for constraints on deviation from the 'simplest' one-to-one relationship observable in certain 'agglutinating' morphological patterns. Four principal generalisations have in fact been put forward: the Peripherality Constraint, the Paradigm Economy Hypothesis, the Macro-paradigm Uniqueness Claim and the Systematic Homonymy Claim. I have also discussed two other recent proposals which seem to bear on this topic: the Adjacency Condition and the Atom Condition.

In this concluding chapter I want to suggest briefly where one might go from here. A whole host of questions are still to be answered, some of which I have mentioned in the preceding chapters, and different linguists will differ on which are the most important; what I offer here is simply my own opinion on what the most pressing questions at this stage are (or, perhaps, which are the most glaring omissions from what I have said), classified under five headings:

- (a) testing the old generalisations;
- (b) establishing new generalisations;
- (c) relating the generalisations to each other within a 'theory₂' of inflexion;
- (d) relating the generalisations and the theory to other areas of grammar;
- (e) relating the generalisations and the theory to 'external' evidence.

The evidence on which each of my four generalisations is based has been drawn from several languages and language families, but I have clearly examined only a fraction of the actual inflexional behaviour which might conceivably be relevant. An ob-

vious task for the future, then, is to broaden the inquiry by testing the old generalisations against more data. At various points I have mentioned aspects of particular languages which ought to be looked at for this purpose. For example, by looking in Fulfulde and Cheremiss, we may hope to glean hints on how, if at all, the Peripherality Constraint can be extended to cover situations where categories are not realised in a constant order; and by looking at the history of the older Germanic languages we may hope to test my suggestions about how the Constraint relates to stem allomorphy. On the Systematic Homonymy Claim there is much to be done in determining whether the fundamental distinction between 'systematic' and 'accidental' homonymies is plausible on other grounds -- whether, for example, the homonymies we classify as accidental on grounds to do with morphological 'expression' correlate closely with those that would be so classified by someone studying homonymy (or syncretism) primarily from the point of view of morphological 'content' -- that is, someone who, like Jakobson or Bierwisch, is interested in those relationships between the properties within a category which are independent of the shapes of their realisations. There is also much to be done in justifying the purported interaction of 'sequencing' and homonymy which I invoked when discussing certain Latin, Arabic and Hebrew facts in Chapter IX. Finally, in connexion with paradigm economy, there will almost certainly turn out to be other instances of 'illegal' paradigm mixture which will need close examination on the lines of Chapter VI in order to determine whether they can be reconciled with the Paradigm Economy Hypothesis. A probable instance of this kind, not previously mentioned, is the capiō sub-type of the Latin third conjugation, which 'goes like' the ordinary third-conjugation type of regō in some forms and like the fourth conjugation (audiō) in others; the fact that this sub-type does not survive as such in any Romance language is encouraging from the point of view of defending the Paradigm Economy Hypothesis, but its history needs to be compared in detail with what the Hypothesis requires or permits at each stage.

In terms of the four deviations mentioned in Chapter I,

we have so far proposed constraints on only two: Deviations II and IV. So in any attempt to establish new generalisations there are obvious areas to explore. I have already suggested in passing that paradigms which are partly 'fusional' and partly 'agglutinating', in traditional terms, are relatively rare -- in other words, paradigms where some combinations of properties are realised simultaneously (exhibiting Deviation III) while other combinations involving the same categories are not. If this rarity is genuine, it may point to a constraint on Deviation III. And Deviation II may well be subject to further constraints not directly involving peripherality, paradigm economy or 'adjacency'.

In keeping with my announced intention to seek descriptive generalisations rather than (at this stage) an explanatory 'theory₂' I have not made any systematic attempt to relate the generalisations to each other. Even so, certain interconnexions have come to light. In the Latin 1st Person Singular Imperfective Future Indicative regam 'I will rule', systematic homonymy (in the form of an attraction to the Subjunctive) comes to the rescue of the Peripherality Constraint in a reasonably uncontrived fashion, assuming only a quite natural relationship between 'homonymy rules' and morphological spell-out. Again, assigning macroparadigmatic relevance to the lexically determined property Neuter in the Nominative and Accusative of nouns in Latin and other Indo-European languages has a convenient consequence in that it allows us to call the Nom-Acc homonymy in Neuter nouns systematic (more specifically, a 'syncretism'). And, if it turns out that all inflexional counterexamples to the Adjacency or Atom Condition involve roots (or 'stems₃', in the sense of Chapter VII), then an explanation for both this and the Inflexional Distinctness Criterion (whereby stem allomorphy does not 'count' for paradigm economy purposes) may perhaps be sought in terms of a fundamental distinction between stem allomorphy and other kinds of inflexion, particularly affixation. This, in turn, might help to explain the paradox that in a language such as English relatively 'useful' morphological distinctions of Case and Gender have been lost while a 'useless' distinction between regular s-Plural and irregular ablaut-Plural nouns has been stubbornly retained; that is, it might not

be accidental that the irregular Plurals all involve stem alternation rather than a range of 'rival' Plural affixes (except for the very marginal -en of oxen and children).

Another set of questions about which I have had little to say in this thesis concerns the internal structure of morphosyntactic categories -- the relationships between the properties which they contain, involving the distinction between more and less 'marked' properties within a given category and possibly involving the decomposition of properties into constituent 'features' or components. This neglect is defensible in the context of an inquiry into exponence relationships specifically; but the fact that so important an aspect of morphological organisation is left untouched by such an inquiry shows clearly that a theory₂ of property-exponent relationships will not constitute a full theory₂ of inflexion. And it is fairly easy, even at this stage, to guess at ways in which a theory₂ of the internal structure of categories might impinge on a theory₂ about constraints on the four deviations of Chapter I. For example, we might explore whether sensitivity, even of the so far unconstrained 'inward' kind, is equally common among less 'marked' and more 'marked' properties; for example, is the realisation of Instrumental Plural in Russian nouns just as likely to be sensitive, on general grounds, as that of Accusative Singular? On the face of it, the answer seems likely to be no: Instrumental Plural in Russian in fact has only one realisation (-ami) whereas Accusative Singular has several, and the same sort of asymmetry is observable in all those languages whose inflexional paradigms display more allomorphy in (to put it crudely) the top left-hand corner than in the bottom right-hand corner. Ideas of this kind about the relevance of markedness to morphological exponence have, of course, been put forward before, most famously perhaps by Kuryłowicz (1949) as part of an account of 'analogical processes' in linguistic change; the value of Kuryłowicz's proposals is weakened, however, by the lack of any coherent synchronic theory of morphology embracing, in my terms, an account of how property-exponent relationships are constrained.¹ Clearly, developing a complete theory₂ of morphology will require us to explore property-exponent re-

relationships and what one might call 'property-property relationships' side by side.

The task of relating generalisations about inflexion to other areas of grammar falls neatly into two, involving phonology on the one hand and syntax and semantics on the other. I have said something about the former in sections 1.8 and 4.2, and about the latter in sections 1.2, 2.6 and 8.4. I have nothing further to say about the relationship with phonology, except to emphasise that a reasonably firmly established account of what is possible and what is not in inflexional exponence will add a useful new ingredient to the debate about phonological representation, and that there are enough instances of inflexion that are phonologically uncontroversial in relevant respects to enable us to contribute to that debate without arguing in a circle. On the syntactic side, one important topic about which I have so far said nothing is the distinction between inflexional affixes and 'clitics', in the sense of Zwicky (1977). As is well known, there are no generally agreed criteria for distinguishing affixes from clitics synchronically, and affixes are often derivable etymologically from independent words which have undergone cliticisation (for example, the Person-Number endings of the Future and Conditional Tenses in French, or the Passive suffix -g in Swedish). Yet, on the face of it, if clitics and affixes get where they are by processes of radically different kinds -- for example, by syntactic movement rules and by morphological realisation rules respectively --, then we would expect 'core' instances of clitics to have at least some general characteristics that 'core' affixes do not share, and vice versa. Moreover, if we can find any characteristics which clearly belong to one class rather than the other, we will have criteria which may be useful for helping to determine in unclear cases whether something is an affix or a clitic; we can also hope to investigate more fruitfully the diachronic processes whereby clitics change into affixes and, perhaps, affixes into clitics. Is there any sign yet of any such clear characteristics? I suggest that one inflexional characteristic worth examining with this in mind is sensitivity. For an inflexional affix to be sensitive to some lexically or syntactically

determined property of its 'host' (or stem) is commonplace; on the other hand, it seems at least unusual for clitics to be sensitive to their hosts as such, other than phonologically. This is not logically necessary; for example, one can perfectly well envisage a language in which object pronouns which are cliticised to verbs vary in shape according to verbal Tense or conjugation-type. If this sort of behaviour is never found, it is at least worth asking whether this indicates a fundamental contrast between inflexional and syntactic organisation.

The final set of pressing questions, I suggest, relates to 'external' evidence for our generalisations, where by 'external evidence' I mean what Kenstowicz & Kisseberth (1977: Chapter I) and Dressler (1977: 7) mean: evidence from psycholinguistics (including aphasia and language acquisition), speech errors, linguistic change and linguistic variation (dialectology and sociolinguistics). I have already used arguments from linguistic change quite extensively, especially in Chapter VI. This implies that I share the view, now widely held, that de Saussure's rigid separation of synchrony and diachrony was mistaken, and that comparing the output of the grammars of two historically successive 'états de langue' can yield clues about the framework in which these grammars ought to be written and hence about the synchronic organisation of these grammars themselves -- a view whose revival owes much to Kiparsky (1968 b) and which has recently been argued forcefully by Lightfoot (1979). How much attention one pays to psycholinguistic arguments will depend on how one sees the relationship between the linguist's description of a language system and the psychologist's description of how users of that language acquire, speak and understand it. Debate about this has been long and inconclusive. My own view is that it is at least worth exploring how close the correlation is between (for example) inflexional behaviour that seems complex on 'internal' grounds and behaviour that is acquired late in childhood or that is easily disrupted by speech disorders. Little work has been done on this in the West, although Hooper (1979) has recently turned to the acquisition of morphology as a source of evidence for morphological generalisations, drawing on some of the work

reported by Ferguson and Slobin (1972). But a considerable amount of the work on the acquisition of highly inflected languages that has been published in the Soviet Union and other east European countries will almost certainly turn out to be relevant to the sort of inquiry I am advocating here. This gives the student of inflexional morphology a further reason for learning Russian, beyond the fact that it is itself delightfully complex morphologically.

Footnote to Chapter X

1. For a recent comment on the historical morphologist's continued need for a synchronic theory of morphology, see Andersen (1980: 2).

Appendix A: Chronology of main sources for Latin morphological changes (cf. Chapter VI)

Inscriptions:

Literary
authors:

Grammarians:

Ennius (born 239 BC)

Plautus (fl. c. 200 BC)

Terence (c. 190-159 BC)

Milestone of
Popilius (132 BC)

Varro (116-27 BC)

Cicero (106-43 BC)

Lucretius (c. 94-c. 55 BC)

Virgil (70-19 BC)

Horace (65-8 BC)

Monumentum Ancyranum
(12 AD)

Priscian (early
6th cent. AD)
Appendix Probi
(c. 7th cent.)

Appendix B: The manuscript tradition of Lucretius (cf. Chapter VI)

Lucretius was not a popular author in later antiquity, and, unlike Cicero and Virgil, did not become a 'school author' to be imitated in matters of style. He is not among the authors frequently quoted as linguistic models by the sixth century grammarian Priscian, whose work was influential as a school grammar throughout the Middle Ages. There would thus have been no particular incentive for copyists to tidy up the morphology in the texts of Lucretius before them in order to comply with a more recently crystallised school norm. There is, moreover, positive evidence that this did not happen. Only three complete manuscripts and a few fragments of Lucretius survived to the Renaissance, all of which are regarded by classical scholars as descended from a relatively early common source or archetype written in France in the third or fourth century AD (Bailey 1947). This source is of course too early to be influenced by Priscian, who did not write until two centuries later. Conscious normalising influences could in principle have crept in between the date of the archetype and that of our oldest surviving manuscripts, which belong to the ninth century; but if they had done so we would expect a much more consistent preference than Ernout's (1918) study reveals for one or other of the options permitted by the school grammars. On the other hand, the manuscripts do exhibit a usage consistent with the evidence of inscriptions contemporary with and earlier than Lucretius and on one point (the Accusative Plurals of mens and gens) they conform strikingly with a dictum of Lucretius's near-contemporary, the grammarian Varro.

Appendix C: Chronology and causation of morphological changes in the Latin third declension (cf. Chapter VI)

I
Predictions flowing
from the Paradigm
Economy Hypothesis

II
Implementation of these predictions

III
Developments and con-
straints not predicted
by the Hypothesis

IV
Timing

I Syncope, producing
mixed declension,
hence ...

Prehistoric (although a
few unsynocopated forms
are preserved in early
Latin).

2 Declensional merger
must take place to
yield not more than
two paradigms.

3 In Singular: replacement of -ī
by -e in Ablative of syncopated

Mainly prehistoric.
Varro implies "fontī",
"montī" to be wrong,
but preserves dentum.

A-type nouns (partī remains as a
vacillating exception). In Plural:
replacement of Genitive -um by -ium
in e.g. dens to achieve phonologi-
cally predictable distribution
within a new merged AB type (pa-
rentum a vacillating exception),
but ...

4 Failure of -īs/-ēs
in Accusative Plural
to follow distribution
of -ium/-um renders two-
paradigm solution in
Plural impossible, hence ...

Appendix C (continued)

I

Predictions flowing from the Paradigm Economy Hypothesis

5 Single-paradigm solution required in Plural.

II

Implementation of these predictions

Developments and constraints not predicted by the Hypothesis

III

IV

Timing

6 Phonologically determined distribution of -ium/-um in Genitive

throughout (exceptions vacillate or are partly refunctionalised on basis of Gender distinction). Accusative

-is retained as exception; much vacillation, defection and some attempt at refunctionalisation (viz. -is as specifically adjectival ending in Monumentum Ancyranum).

Defection to -ēs in

Accusative not yet evident in Milestone of

Popilius, but clearly under way in Lucretius,

who illustrates it in both B-type nouns (mentēs etc.) and A-type ones (ignēs etc.).

7 Non-phonologically-predictable encroachment of -e on -ī in Ablative Singular of remaining A-type nouns, hence ...

Begun by Varro's time

(ove, ave beside ovī, avī), but not yet established in Plautus

or Terence nor even in Lucretius.

'Old' distribution of -ī and -e still substantially observed by Lucretius in

8 Single-paradigm solution required in Singular too.

9 In Ablative, -ī/-e thoroughly refunctionalised on adjective/noun basis (exceptional -ī remains in

Appendix C (continued)

I
Predictions flowing
from the Paradigm
Economy Hypothesis

II
Implementation of these predictions
III
Developments and constraints not predicted
by the Hypothesis

IV
Timing

some nouns, which vacillate). In
Nominative, -is/-g distribution
analysable as phonologically de-
termined, with a few exceptions
(orbis, apis).

A-type stems, and -i
retained in a few nouns
consistently by e.g.
Virgil (a literary af-
fectation?). But re-
functionalisation, begun
in Lucretius, is complete
by Monumentum Ancyranum.

Appendix D: Inflexional vacillation among German nouns of declension-type X (Name) (cf. Chapter VII)

	Sprach- Brockhaus 1935	Cassels Revised 1978	Langensceid ^h t New Muret-Sanders Encyclopaedic 1974
Buchstabe	VIII	X	X; - <u>n</u> I 'rare'
Friede	X; - <u>n</u> I	X; - <u>n</u> I	X; - <u>n</u> I
Funke	VIII; - <u>n</u> I	VIII; - <u>n</u> I	X; - <u>n</u> I
Gedanke	X	X	X
Glaube	X	X	X; - <u>n</u> I 'rare'; VIII 'rare'
Name	X; - <u>n</u> I 'weniger gut'	X; - <u>n</u> I 'Austrian'	X; - <u>n</u> I 'rare'
Same	VIII; - <u>n</u> I	X; - <u>n</u> I	X 'literary'; - <u>n</u> I
Schade	X; 'heute meist' - <u>n</u> :II	X with Um- laut; - <u>n</u> :II	- <u>n</u> II; <u>schade</u> without - <u>n</u> as adjective only
Wille	X; 'sel- tener' - <u>n</u> I	X; - <u>n</u> I 'Austrian'	X; - <u>n</u> I 'rare'; VIII 'rare'

Roman numerals refer to declension-types exemplified in (505).

Appendix E: A selection of inflexional homonymies (cf. Chapter VIII)

1	2	3	4	5	6	7
Language	Homonymy between:	Part(s) of speech	Morphological or phonological class restriction	Morpho-syntactic context: simultaneous	Morpho-syntactic context: not simultaneous (+ = with more central exponent)	Comments
1 Latin	Dat, Abl	Noun, Adj, Part, Pron i.e. all to which Case applies)		Plural		Stable from earliest to latest Latin.
2 Latin	Dat, Abl	Noun, Adj, Part	2nd decl.	Singular		An early Latin development.
3 Latin	Gen, Dat	Noun, Adj, Part	1st decl.	Singular		An early Latin development.
4 Latin	Nom, Acc	Noun, Adj, Part, Pron		Neuter		An inherited Indo-European trait. See Chapter IX.
5 Latin	Nom, Acc	Noun, Adj, Part	3rd, 4th, 5th decl.	Plural.		Spread to old <i>i</i> -stem nouns by about 1st century AD, with loss of distinct Acc Pl ending <i>-is</i> . (See Chapter VI.)
6 Latin	Dat, Abl	Adj (and Part functioning as Adj)	3rd decl.	Singular		Developed by about 1st century AD. Some exceptions, e.g. <i>vetus</i> 'old', comparatives in <i>-ior</i> .
7 Latin	Nom, Gen	Noun, Adj	3rd decl. with Nom Sg <i>-is</i>	Singular		Best regarded as accidental (see Chapter VIII, section 8.3).

Appendix E (continued)

	1	2	3	4	5	6	7
8 Latin	Fut Indic, Subjunc	Fut Indic, Subjunc	Verb			+Perfective Pl or 2 or 3 Sg	Gradual merger, still incomplete at Plautus's time. Attraction from Indic to Subjunc in 3 Pl, Subjunc to Indic elsewhere.
9 Latin	Fut Indic, Pres Subjunc	Fut Indic, Pres Subjunc	Verb	3rd, 4th conjug.		+Imperf 1 Sg	Attraction from Fut Indic to Pres Subjunc. See Chapter IX.
10 Greek (anct.)	Nom, Acc	Nom, Acc	Noun, Adj, Part, Pron		Neuter		See on 4.
11 Greek (anct.)	2nd, 3rd Persons	2nd, 3rd Persons	Verb		Dual Indic or Subjunc (but see col. 7)		The homonymy does not occur in those Indic forms which carry a prefixed <i>e-</i> (the 'augment') peculiar to the Indicative -- i.e. in those forms where Indicative Mood is not realised wholly simultaneously with Number, Person and Voice. Nor does it ever occur in the Optative, which always has a clearly identifiable and more central principal exponent <i>-oi-</i> , <i>-ai-</i> or <i>-ei-</i> . Hence the homonymy is restricted to forms where it can be classed as a syncretism.
12 Greek (anct.)	Indic, Subjunc	Indic, Subjunc	Verb	Verbs in <i>-ō</i> ('thematic')	1 Sg Present Active		Present Indicative and Subjunctive endings typically differ in having short and long vowels (or diphthongs) respectively. But the 1 Sg <i>-ō</i> has no short-vowel alternant. The homonymy therefore results naturally from spell-out and is best seen as accidental.

Appendix E (continued)

	1	2	3	4	5	6	7
13 Greek (anct.)	Fut Indic, Aorist Subjunc	Verb	Verbs with identical stems for 'sigmatic' Aorist and Future	1 Sg Active			The Person-Number endings of the Future Indicative are like those of the Present Indicative in most - <u>ō</u> verbs, and those of the Aorist Sub- junctive are like those of the Pre- sent Subjunctive. Hence, see note on 12 above.
14 Sanskrit	Nom, Acc	Noun, Adj, Part, Pron		Neuter			See on 4.
15 Sanskrit	Instr, Dat, Abl	Noun, Adj, Part, Pron		Dual			The clitic forms of the 1st and 2nd Person pronouns have a Dat-Acc-Gen homonymy instead, and there are no clitic forms for Instr or Abl. This supports regarding these homonymies as morphological rather than syn- tactic.
16 Sanskrit	Gen, Loc	As for 15		Dual			Distinct in 1st and 2nd Person pro- nouns, which have a clitic Dat form homonymous with those for Acc and Gen (cf. on 15, 16 above).
17 Sanskrit	Dat, Abl	As for 15 (but see col. 7)		Plural.			The distinction in the -as declen- sion is traditionally seen as im- ported from the pronominal declen- sion. Does this indicate that the homonymy in all other nouns must be analysed as accidental?
18 Sanskrit	Gen, Abl	Noun, Adj, Part	Except in -as declen- sion	Singular			

Appendix E (continued)

	1	2	3	4	5	6	7
19 Vedic	2nd, 3rd Persons	Verb	'Athematic' Aorist stems	Singular Indicative	Aorist		
20 Russian	Nom, Acc	Noun, Adj, Part, Pron.		Neuter			
21 Russian	Nom, Acc	As for 20		Inanimate (any Gender)	Plural		
				Inanimate (Masc)	Singular		
22 Russian	Acc, Gen	As for 20		Animate (any Gender)	Plural		
				Animate (Masc)	Singular		
23 Russian	Dat, Loc	Noun	-a declension			Singular	
24 Russian	Gen, Dat, Loc, Ins	Adj		Singular Feminine			

The homonymy is clearly due to fairly recent phonological developments and is removed later by substitution of other types of Aorist stem (Kiparsky 1972: 203-205). Accidental, therefore.

See on 4.

Number is in col. 6 rather than col. 5 because there are some nouns where stress is an exponent of Number (i.e. where Plural forms are all stressed differently from Singular forms). These are argued in Chapter IX to be attractions from Accusative to Nominative or Genitive, as the case may be.

Singular is in col. 6 rather than col. 5 for the reason given above. But, again, see Chapter IX.

There is no stress shift in attributive adjective paradigms, so Number is here clearly cumulated with Case.

Appendix E (continued)

	1	2	3	4	5	6	7
25 Russian	Gen, Dat, Loc	Noun	Feminine 'soft' stems (ending in palatalised consonant)	Singular			In none of these nouns is Number unambiguously indicated by stress, so for this group it is cumulated with Case. See, however, Chapter IX.
26 Russian	1st, 2nd, 3rd Person	Verb		Past			One might argue that the category Person is simply not relevant to verbs in the Past Tense, just as Gender is not relevant in the Present. If so, this is not a morphological homonymy.
27 Lithuanian	Sg, Pl	Verb		3rd Person			
28 German	1st, 3rd Person	Verb		Plural			
29 Italian (medieval)	1st, 2nd, 3rd Person Singular	Verb			+Imperfect		Superficially, an attraction to 3rd Person. But the homonymy was later removed by the introduction of distinct endings borrowed from the Present. This confirms that to treat the homonymy as accidental (as the nature of the entry in col. 6 requires, assuming the Systematic Homonymy Claim) is correct. See Chapter IX.
30 Italian (modern)	1st, 2nd, 3rd Person Singular	Verb		Present Subjunctive			No tendency to remove the homonymy. Note that there is no entry in col. 6.

Appendix E (continued)

	1	2	3	4	5	6	7
31 French	Sg, Pl	Noun	Except most of those in <u>-al</u> and a few others; and in 'liaison' in formal style				
32 English	Possessive, Plural	Noun	Except for those with irregular plurals (<u>man</u> , <u>wife</u> etc.)				
33 Hebrew	3rd Person Fem, 2nd Person Masc	Verb		Singular Imperfective			
34 Classical Arabic	Definite, Indefinite	Noun	'Sound' Plurals in <u>-ūna</u>	Plural			
35 Classical Arabic	Genitive, Accusative	Noun	All 'sound' Plurals	Plural			

Phonologically accountable, probably accidental. Alternatively, perhaps, an indication that there is no genuine morphosyntactic Number contrast in most modern French nouns (as opposed to determiners); if so, not a morphological homonymy.

The fact that nouns irregular in Pl are not similarly irregular in Poss strongly suggests that this homonymy is accidental (see section 8.3).

There may, however, be some independent tendency at work in the modern English predilection for -s endings.

An old, stable, homonymy, found in the 'prefix conjugation' of all Semitic languages. The only example in this appendix of a homonymy apparently involving pairs of morpho-syntactic properties. Has this made it more durable? But see Chapter IX.

See Chapter IX, section 9.2g.

See Chapter IX (where it is argued that this is a syncretism, and therefore systematic, despite the presence of Plural in col. 6 rather than col. 5).

Appendix E (continued)

	1	2	3	4	5	6	7
36 Hungarian	Definite, Indefinite	Verb			1st or 2nd Plural	+Conditional	The homonymy in the 1st Person is relatively new (late 19th century). In shape, the morphs look Inde ^f , indicating attraction. (Vago (1980: 57) similarly talks of the Def suffix being "replaced by" the Inde ^f one.)
37 Hungarian	Definite, Indefinite	Verb			1st Sg	+Past	Morph shape suggest attraction from Inde ^f to Def. See section 8.8.
38 Hungarian	Indicative, Subjunctive	Verb		Except those 1st Pl with stems in <u>-t</u>	1st Pl	Definite	The nature of the restriction in col. 4 suggests that this homonymy is accidental. If not, then the shape of the relevant forms suggests attraction from Indic to Subjunc.
39 Finnish	Sg, Pl	Noun				Comitative (Case)	Morph shape suggests attraction from Sg to Pl. See sections 8.7 and 8.8.
40 Finnish	Sg, Pl	Noun				Instructive (Case)	
41 Georgian (antc.)	Dat, Erg, Gen	Noun			Plural		See Chapter IX.
42 Dyr̄ibal	Instr, Erg	Noun, Adj, Marker					The Instr and Erg are probably best regarded as a single Case morpho-syntactically, although (as Dixon (1972) argues) fulfilling distinct syntactic functions. See section 1.2 for relevant comments.

<u>Appendix E</u> (continued)	
1	2
43 Dyirbal	Dative, Allative
3	4
	Noun, Adj (<u>not</u> Marker)
5	6
7	

It seems best to treat Dative and Allative on the same lines as Nominative and Accusative in English; as the latter contrast is restricted to personal pronouns, so the former is restricted to 'markers'. Since 42 and 43 are the only clear prima facie examples of morphological homonymy in Dyirbal, it may be possible to say that Dyirbal eschews this kind of homonymy altogether -- not surprisingly (if the Systematic Homonymy Claim is correct), since it has few if any instances of simultaneous exponence (Deviation III).

Appendix F: Classification of examples in Appendix E (cf. Chapter VIII)

	Totals
No entry in column 6:	
- no entry in column 5 either (so accidental or non-morphological)	31, 32, 42, 43 4
- probably accidental (despite entry in column 5)	7, 12, 13 3
- remainder (potential syncretisms)	1, 2, 3, 4, 5, 6, 10, 11, 14, 15, 16, 17, 18 (note 1), 20, 24, 25, 26 (note 1), 27, 28, 30, 33, 34, 41 23
	Subtotal: 30
Entry in column 6:	
- not attractions:	23 (note 2) 1
- attractions where properties in column 6 (a) constitute the sole morphosyntactic context for the homonymy (i.e. there is no entry in column 5) and (b) all have principal expo- nents more central than the neutralised (col. 2) properties	29 (note 1) 1
- other attractions (i.e. where there is an entry in column 5, or the co- lumn 6 properties do not have principal expo- nents as above)	8, 9, 19 (note 1), 21, 22, 35 (note 2), 36, 37, 38 (note 1), 39, 40 11
	Subtotal: 13
	<u>43</u>

- Notes: 1. Possibly or probably accidental (see column 7).
2. Argued in Chapter IX to be a syncretism, despite appearances.

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