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Morphological Productivity

LAURIE BAUER

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Why are there more English words ending in *-ness* than ending in *-ity*? What is it about some endings that makes them more widely usable than others? Can we measure the differences in the facility with which the various affixes are used? Does the difference in facility reflect a difference in the way we treat words containing these affixes in the brain? These are some of the questions examined in this book.

Morphological productivity is one of the most contested areas in the study of word-formation. This book takes an eclectic approach to the topic and concludes by applying the findings for morphology to syntax and phonology. Bringing together the results of twenty years' work in the field, it provides new insights and considers a wide range of linguistic and psycholinguistic evidence.

LAURIE BAUER is Professor of Linguistics at Victoria University of Wellington. He is the author of *English Word-Formation* (Cambridge University Press 1983) and *Introducing Linguistic Morphology* (1988).

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Preface

In 1995, when introducing a talk based on material later published as Bauer (1996) at the Free University of Amsterdam, I commented that I had been working on morphological productivity for over twenty years, but still did not feel that I had a coherent picture of the topic. In the intervening five years, I believe that I have achieved some degree of coherence in my view, and this book is an attempt to communicate that understanding. I still regard the picture presented here as provisional, and I expect it to change in the next five or ten years. Twenty-five years ago, morphological productivity was not at the forefront of linguistic theorising, but today, thanks to the dispute between connectionists and purveyors of alternative views of morphology, it has taken on a far greater importance. This not only makes it timely to provide a statement of a position, but guarantees that further research in the near future will outdate some of the comments that are made here. It is my hope that the work presented here will provide a stepping-stone in the development of a new deeper understanding – even if it is one which contradicts my own cherished positions.

I should like to thank all those who have helped in the evolution of this work over a number of years, but especially those connected with the production of this book: Mark Aronoff and Salvador Valera read and commented on a draft of the book, and made many useful suggestions, as did anonymous referees for Cambridge University Press; Andrew Carstairs-McCarthy provided examples and bibliographic help; colleagues and students from the School of Linguistics and Applied Language Studies at Victoria University commented on individual portions of the work; and colleagues from the School of Mathematical and Computing Sciences at Victoria University gave help and advice on statistical matters. And most of all, I should like to thank my wife, Winifred Bauer, for all her editorial and academic help.

1 *Introduction*

> 'Produktivität' zählt zu den unklarsten Begriffen der Linguistik.
> ['Productivity' is among the least clear concepts in linguistics.]
> Mayerthaler (1981: 124)

1.1 The issue

This book deals with productivity as it affects morphological systems. Unfortunately, in the present state of morphological studies, this opening statement may mean different things to different people, even a definition of 'productivity' being a matter of some dispute. In order to develop a position from which conclusions can be drawn, it is thus necessary to begin from the very beginning, and gradually to construct a secure foundation of notions to support the enterprise. We can begin this construction by considering one fundamental definition of 'productivity'.

Hockett (1958: 575) gives the label 'productivity' to that property of language which allows us to say things which have never been said before, the design feature that Chomsky (1965: 6) calls 'creativity'. We do not have to go any further to see that this is an area in which there is, at least, some terminological difference of opinion. To add to the confusion, Chomsky (1965: 5) also talks about syntactic processes being 'productive' without making it clear whether 'productive' and 'creative' are the same or different things, while others, such as Lyons (1977: 76–78), distinguish carefully between the two terms, though not all scholars draw the same distinction. The question of whether it is useful to distinguish between 'creative' and 'productive', and if so in what way, is one which will be taken up again later in this book (see section 3.10). First, though, it must be shown how productivity in Hockett's sense affects morphological structure.

The general assumption among linguists is that people can say things which they have neither said nor heard before because they know (albeit

not explicitly) the rules for the combination of appropriate elements into sentences and because they are able to assign meanings to those elements. In other words, starting from a store of elements each of which has a predetermined semantic and/or pragmatic value, people are able to create new combinations which allow the transfer of more complex meanings to their interlocutors. It is the creation of the new combinations from a set of stored elements which is crucial here. None of the word-forms in Oscar Wilde's

(1) I can resist everything except temptation.

is unfamiliar; it is the creation of the new combination of word-forms which produces the new and striking message. Similarly, in morphology, it is the creation of new lexemes and word-forms, never before heard or spoken, which provides the greatest proof that productivity is also a feature of that level of language. As Hockett (1958: 307) puts it 'The productivity of any pattern – derivational, inflectional or syntactical – is the relative freedom with which speakers coin new grammatical forms by it' or, as Di Sciullo and Williams (1987: 8) say, the fact that an affix can be used to make new words makes it 'productive in the most basic sense of the word'.

However, even these apparently simple statements give rise to new questions. Hockett talks of 'relative freedom' which raises the question of whether productivity is a yes/no question or a matter of gradient. Di Sciullo and Williams speak of 'the most basic sense' of 'productivity' implying that there are other less basic senses. How many senses of this term there may be is a question that will permeate this entire book. The question of gradient or scalar productivity will be dealt with specifically at a number of points (see especially chapter 5).

To put all this into some perspective, let us consider a few relatively simple examples, beginning with English plural marking. English has a number of different ways of marking plural on nouns, as is shown by plural nouns such as *cats*, *dogs*, *horses*, *oxen*, *deer*, *mice*, *hippopotami*, *cherubim* and so on. The productive aspect of this is shown by the fact that native English speakers have the ability to pluralise nouns they have encountered only in the singular. In a celebrated article, Berko Gleason (1958) illustrated that children aged under five can accurately produce the plural of *wug* as *wugs* (with the appropriate allomorph: /z/). Assume then that a speaker of English were to meet the following words, all new in the speaker's experience (since they are innovations designed to make the point):

(2)

argaz	'crate of specific style'
smick	'type of cracker biscuit'
brox	'piece of computer hardware'
ceratopus	'type of dinosaur'
cheppie	'type of antelope'

The question is: How will the speaker make these words plural? In English there is a high probability that a speaker will use the appropriate regular plural ending on any word. That is, we would expect to find *argazzes*, *smicks*, *broxes*, *ceratopuses* and *cheppies*. There is accordingly a high probability (though not complete certainty) that the many alternative plural markers will not be used. The examples above have been chosen to make this point, so I shall consider them in detail.

The noun *argaz* 'crate' is actually a Hebrew noun, and in Hebrew it takes an *-im* plural. It is therefore possible that the plural *argazim* will be found. There is not much precedent for using a Hebrew plural in English, but there is some (*cherubim*, *seraphim*, *kibbutzim*, *goyim* may be an exhaustive list). Use of the *-im* plural would require the particular type of crate to be perceived as being connected with Israel or Judaism, and demand knowledge of the correct Hebrew plural. The regular English plural is much more likely.

There is no particular reason to expect *smick* to have anything but a regular plural, and it would be extremely surprising to find anything other than *smicks*.

The word *brox* ends in the sequence *-ox*, and it is feasible that it might make its plural like *ox* and that we would find *broxen*. On the whole this seems very unlikely. The majority pattern with such nouns is illustrated by *box*, *cox*, *fox*, *pox* and not by *ox*. However, such an ending cannot be excluded, particularly with the meaning given above. Among certain computer mavens, the computer called a *Vax* has *Vaxen* as its plural. Here the final *-x* seems to have been sufficient to call forth the unusual plural marking. However, this plural is clearly an in-group form, and the normal plural is *Vaxes*. We would expect therefore to find *broxes*, too.

The word *ceratopus* is an interesting case. Words ending in *-us* in English are largely learned words, and a lot of them, though by no means all, have plurals in *-i*, following the Latin pattern. Thus we find *alumni*, *foci*, *fungi*, *gladioli*, *nuclei*, *stimuli* and so on, but also *campuses*, *choruses*, *geniuses* (*genii* is a joke), *isthmuses*, *polyanthuses*, *viruses*, *walruses* and so on. A plural in *-i* is thus a possibility for many learned words ending in *-us*. This possibility is strong enough to overcome etymology, so that we

hear, on occasions, *octopi* (*octopus* comes from Greek, not Latin), or *prospecti* (*prospectus* is a fourth declension Latin noun, not a second declension one, and its Latin plural is *prospectūs*). *Ceratopus* looks like a learned word, and thus appears to be a potential *-i* plural word. But a more learned analysis shows that it comes from Greek 'horny footed' and thus should not take such a plural from the etymological point of view. The classical plural *ceratopodes* seems unlikely (very few people use, or even know, *octopodes*), so that *ceratopuses* would, for anyone but the purest of purists, be the preferable solution.

The case of *cheppie* is even more complex. The word is taken from Tswana *tshêphê*, and its plural in Tswana is *ditshêphê* (Cole 1955: 88). English, which is notorious for its ability to borrow virtually any lexeme, seems in general reluctant to borrow plural forms from 'exotic' languages, so that the Tswana plural is probably unlikely in English. However, antelope form part of the class of huntable and edible animals that can take a zero plural, so that *cheppie* ought to be a possible plural form. This is counteracted by the fact that the final *-ie* would probably be associated with the English diminutive ending so that *cheppie* looks like a native word, and *cheppies* seems the most likely outcome.

The simplistic analysis of this state of affairs in English is that only the *-s* plural (with its three phonological variants) is normally used to make plurals of new words, and that all other plural forms can be lexically listed. While this type of analysis contains a considerable amount of truth, the discussion above should have indicated that it is overly simplified and that we are dealing with probabilities rather than certainties in this area. It is clearly the case that the *-s* plural is the most likely plural form (on these words or any others), but it is not the only possible plural form. We might, therefore, feel confident in saying that *-s* is the most productive plural ending in current English. But does saying that it is the 'most productive' imply that we can measure productivity on a scale, or is it simply a matter of *-s* being productive? It is less clear what we can say about the alternative endings. Is the Latin *-i* ending productive in English or not? Is there a difference in kind between the motivation for a putative *broxen* and the motivation for a putative *broxes*, or do they illustrate the same kind of phenomenon? These are questions to which we shall return (see e.g. sections 3.10, 3.12).

If we accept, for the sake of the argument, the simplistic view of English plural formation outlined above, we might wish to conclude that plural markers are divided into two classes in English: the productive and

the non-productive. We might consider that the productive marker was assigned by rule, while the unproductive markers were assigned by lexical list. This general picture is widely accepted in the literature, for example in much work on Lexical Phonology and Morphology. If this simplified picture were true, it might illustrate an ideal case. However, there are other patterns which depart considerably from this ideal. Consider next the case of plural in Dutch.

As in English, there are a number of plural markers in Dutch which are borrowed from Latin, Greek and Italian along with the lexemes they are used with. Also as in English, there are a small number of irregular native plurals: the plural of *kind* 'child' is *kinderen*, and there are no more than fifteen words which share this *-eren* ending (Geerts et al. 1984: 56–57). There are fewer such types in Dutch than in English. The default plural marker is *-en*, but there is another productive plural marker, *-s*. The ending *-s* is used if the noun is not already marked as irregular and

(a) if the noun stem ends in [əl], [ər], [əm] or [ən]: *lepel* 'spoon' > *lepels*; *bakker* 'baker' > *bakkers*; *bloesem* 'blossom' > *bloesems*; *keuken* 'kitchen' > *keukens*;

(b) if the noun stem ends in a vowel other than [ə]: *vla* 'custard' > *vla's*; there are several exceptions to this clause, but it can stand as a generalisation, albeit not as helpful a generalisation as it appears to be: Baayen (1989: 17) points out that there are only about half-a-dozen native words which fit the generalisation and all the examples cited by Geerts et al. (1984: 60) are borrowed words;

(c) if the noun stem ends in one of a number of specified derivational endings, including the diminutive *-(e)(t)je*, and *-eur*, *-oor*, *-aar* and *-erd*: *huisje* 'little house' > *huisjes*; *monteur* 'repair man' > *monteurs*; *majoor* 'major' > *majoors*; *schakelaar* 'switch' > *schakelaars*; *dikkerd* 'fatso' > *dikkerds*;

(d) if the base noun is a loan word and would take an *-s* plural in the lending language: *chef* 'chef' > *chefs*; *telefoon* 'telephone' > *telefoons*;

(e) if the base is a letter of the alphabet or an initialism: *b* > *b's*; *BV* 'plc' > *BV's*

(Geerts et al. 1984: 60–62; Van Marle 1985: 199–200; for a contrasting description of the distribution see Booij and Van Santen 1995: 65–70). In other cases, the ending *-en* is used for the plural.

The situation in Dutch, therefore, is importantly different from that in English. In English there is, in most cases, only one productive pattern. In Dutch, there are two competing patterns, both productive. However, as the situation has been described here, it is predictable which marker will be used. Under such circumstances the productivity of the different markers is entirely determined by the number of nouns with the appropriate base structure. (In fact, the situation described above is still a simplification, in that it ignores quite a large number of exceptional lexemes and in that nouns ending in [ə] appear to show unpredictable variation between *-en* and *-s* plurals: *klasse* 'class' and *hoeve* 'farmhouse' take *-en*, *tante* 'aunt' takes *-s* and *lade* 'drawer' can take either [Baayen 1989: 15]; nevertheless, the generalisation that has been drawn here remains largely true, and can be allowed to stand as an idealisation for expository purposes.)

Now let us turn to English adverbs ending in *-ly*, as discussed in Bauer (1992a). Speakers of current English appear reluctant to attach adverbial *-ly* to bases which already end in *-ly*. There are, for example, no instances of such words in the eighteen million words of the COBUILD corpus (see http://www.cobuild.collins.co.uk). Nonetheless, the first edition of *The Oxford English Dictionary* (1933; second edition 1989) lists *-ly* adverbs for all monomorphemic adjectives of English that end in *-ly*: *burlily*, *holily*, *jollily*, *melancholily*, *oilily*, *sillily*, *surlily*, *uglily* and *wilily*. At some point in the history of English, it appears, the reluctance to form such adverbs has, at least for certain classes of adjectives, not been as pervasive. However, although the same edition of *The Oxford English Dictionary* lists at least 473 words ending in adjectival *-ly* (e.g. *friendly*), it lists only ten *-ly* adverbials formed on the basis of such adjectives that are not obsolete or rare: *cleanlily*, *friendlily*, *ghastlily*, *homelily*, *kindlily*, *livelily*, *lonelily*, *lovelily*, *lowlily*, *manlily*. Here we have a large input class of potential bases, but an extremely small output class. In contrast to the situation we observed with Dutch plurals, the number of forms attested (or perhaps even possible) in the speech community does not appear to be fully determined by the size of the class of possible bases. It is cases of this type which appear to support the notion of productivity as a matter of degree, rather than as a simple yes/no choice.

Such observations also raise further questions. If someone were to use *-ly* adverbs such as those illustrated in examples (3)–(4) below, would they be producing ungrammatical sentences? Is the answer necessarily the same for both (3) and (4)? Does the answer depend on the period at which (3) and (4) were composed?

(3) Kim coughed poorlily.

(4) Pat was called upon to update the statistics monthlily.

In the next section I discuss the question of diachronic variation in productivity. Here I should like to note that this section raises two questions: whether the same notion of productivity (however defined) holds for the three cases that have been described above, and how variability in degree of productivity is to be accounted for.

1.2 Diachronic variation in productivity

Mayerthaler (1977) presents a discussion of French nouns and adjectives ending in *-al*. Some of these, like *cheval* 'horse', make their plural by replacing the *-al* with *-aux*. Others, like *bal* 'ball (at which you dance)', make their plural by adding *-s*. Historically, the *-aux* ending arose through the vocalisation of an *-l*, with the <x> a graphemic representation of an original final /s/. In a word like *chevaux*, the final consonant has disappeared and the /au/ diphthong has monophthongised to /o/, giving modern standard French /ʃəvo/. Words like *bals* did not undergo the process of l-vocalisation, but have lost the final /s/ in the pronunciation, so that the singular and plural are homophonous as /bal/. To summarise:

Table 1.1: *Historical development of* chevaux *and* bals

	ʃəval + s	bal + s
l-vocalisation	ʃəvau + s	–
monophthongisation	ʃəvo + s	–
loss of final /s/	ʃəvo	bal

According to Mayerthaler (1977: 109), the last noun to be taken into French and to make its plural like *chevaux* was *original*, in about 1600. Even *original* shows variation between *originaux* and *originals*. Nouns which have come into French since 1600 all have a regular *-s* plural. However, even today, there are more common nouns listed in dictionaries of standard French which make their plural like *chevaux* than like *bals* (Mayerthaler 1977: 105), and if adjectives are considered, too, there are considerably more that have *-aux* plurals than have *-als* plurals (1977: 106). We thus have a situation where in the fifteenth century there was only one way to make nouns ending in *-al* plural, and that was by changing

the *-al* to *-aux*. In the sixteenth century, starting about 1530 (Mayerthaler 1977: 109), the alternative of adding *-s* to new nouns ending in *-al* is introduced. In the seventeenth century, the only possibility with new nouns is to add *-s*. The result of this is that there are two classes of noun ending in *-al*, one that makes its plural in *-aux*, the other that makes its plural in *-als*. This is the current situation in standard French, though non-standard French frequently does away with the *-aux* class completely, and makes nouns like *cheval* take a regular *-s*-plural (Mayerthaler 1977: 110).

This process can be reformulated as follows: in the fifteenth century there was one productive method of making nouns in *-al* plural; in the sixteenth century a change took place so that this method was no longer productive, and a different method became productive. In other words, there was a diachronic change in what was productive with this phonologically-defined set of nouns. Moreover, this change cannot have been directly linked to the number of possible models for parallels, since even in the seventeenth century there were more common nouns which used the (then unproductive) *-aux* than used the (newly productive) *-als*. The change in productivity appears to be independent of the change in frequency.

Now consider the case of the suffix *-ment* in English. In Bauer (1983: 76) I commented that this suffix appears no longer to be productive. Certainly, if it is productive it is only marginally so. To trace the productivity of this affix, the following experiment was carried out. The electronic version of the second edition of *The Oxford English Dictionary* was searched for words which have the notation *-ment* in the etymology. When irrelevant words have been deleted this leaves 1,110 words containing the affix *-ment*. Unfortunately, this is not an exhaustive list of words in *-ment* in *The Oxford English Dictionary*, since words which follow on in another entry are not picked up in this way (for example, under ENTWINE there is a note 'Hence *entwinement*', and my search did not find *entwinement*). However, it does give a large sample, and a sample which we may assume to be fairly representative. *The Oxford English Dictionary* also provides approximate dates of first use by giving the date of the first attested use of each word. If we plot the number of first attestations from any period against the year, we get the result shown in Figure 1.1. The productivity of *-ment* peaks twice: first in the early seventeenth century and again in the early nineteenth century, but tails off rapidly in the twentieth century; from 1950 onwards the dictionary lists only one appropriate word: *underlayment* from 1956.

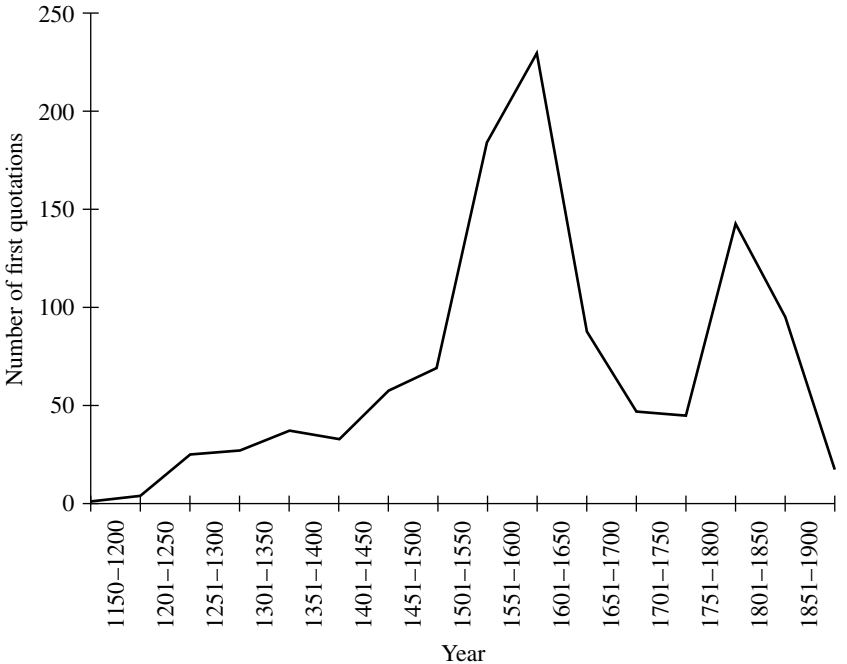

Figure 1.1: Productivity of *-ment* (based on *The Oxford English Dictionary*)

Figure 1.1 gives a picture of an affix which varies in productivity over time. The drop in productivity of this affix between approximately 1650 and 1800 cannot have been due to any loss of clarity of the meaning of the affix, or it would not have recovered again in the first half of the nineteenth century. Nor, on the face of it, can it be due to lack of suitable bases (although a separate study would be required to check that the number of new verbs did not also drop in the appropriate period). Indeed, there is no obvious reason for the observed fluctuations in productivity; we merely observe that productivity appears to change. That being the case, whether or not a particular morphological process is productive, or to what extent it is productive, is an important question which needs to be answered in giving a full synchronic description of the morphology of any language, and the variation in productivity should be considered in any diachronic description. The implication here seems to be that productivity is in itself an important part of a linguistic description, and that it cannot necessarily be reduced to other factors such as frequency,

input classes or clarity of meaning. This point, too, will be taken up in more detail later in chapter 3.

1.3 Summary

In this chapter it has been indicated that the potential for the creation of new words may be considered to fall within the purview of the design feature of language known as 'productivity' or 'creativity'. It has been shown that in some cases the potential for a new word derives automatically from the presence of an appropriate word in an input class (where input classes may or may not be in competition with each other). In other cases there appears to be variation in the degree to which the potential to make new words is exploited, such that the variation is not determined by the input class. In this latter case we have also seen that the degree to which the potential is exploited can vary diachronically, apparently without reference to such factors as the size of the input class or the semantic coherence of the output.

In the process of dealing with these matters I have raised a number of potential problems to which I will return in subsequent chapters. The questions, some raised explicitly, a few only implicitly, include:

(a) Is it useful to distinguish between 'productivity' and 'creativity' in morphology, and if so in what way?
(b) Are there several meanings for the term 'productivity', and if so do they conflict?
(c) Is productivity a yes/no matter or is it a matter of gradient?
(d) If it is a matter of gradient, does this imply that it is measurable on some scale?
(e) Is there a difference in kind or just a difference in degree between the new use of rare patterns to create words like *Vaxen* and the new use of normal patterns to create words like *laptops*?
(f) Does the use of unproductive processes lead to ungrammaticality?
(g) Is the answer to question (f) determined by specific constraints which may have been broken, and does it vary diachronically?
(h) What factors influence productivity if – as was suggested here – neither frequency nor semantic coherence do?

2 *A historiographical conspectus*

> [M]ere mention of the subject [sc. productivity] seems to be taken by many as an open invitation to anecdotalism.
>
> Aronoff (1976: 35)

> [P]roductivity and listedness are not grammatical concepts.
>
> Di Sciullo and Williams (1987: 2)

2.1 *Productivity* and its synonyms

Although the terms 'productive' and 'productivity' appear to be relatively new, the concepts in the form in which they have been outlined in chapter 1 are old. As Schultink (1992a: 188) points out, the whole notion of grammar implicit in the work of the Sanskrit grammarians assumes the idea of productivity. This idea has been available to English grammarians at least since Palsgrave's *Lesclaircissement de la langue francoyse* was published in 1530. As Stein (1999: 62) says more generally of this work, 'the concepts were there, but not yet the terms for them', so that Palsgrave discusses affixes such as English *-ness* as being 'certayne' in their application (cited in Stein 1999). The term 'productivity' is much more recent. As far as Schultink has been able to discover, the term is first used by Dietz (1838: 221), who says that

> Die meisten und wichtigsten Bildungsformen dagegen sind kraft ihrer wohl gefühlten Bedeutung lebendig und PRODUKTIV geblieben. [Most formative elements, and the most important of these, on the other hand, have remained living and PRODUCTIVE on account of their strongly-felt meaning. My translation, LB.]

Here we see the term 'productive' being used as synonymous with 'living', a metaphor which is common in the works of other scholars such as Darmesteter, Grimm, Karcevski, Lulofs, Nyrop and Paul, to name but

a few. The equation between these two terms is also made by Jespersen (1942: 4). Another metaphor is to refer to 'activity' rather than 'productivity' (Pike 1967: 169), although some scholars distinguish the two (Fleischer 1975: 71).

2.2 What is productive?

Superficially there is disagreement in the literature about what it is that is productive. For some scholars particular affixes are productive (Lulofs 1835: 157, cited in Schultink 1992a: 189; Fleischer 1975: 71); for others, it is morphological processes which are productive (Uhlenbeck 1978: 4; Anderson 1982: 585); for yet others, it is rules which are productive (Aronoff 1976: 36; Zwanenburg 1980: 248; Bakken 1998: 28); for a very few it is words which are productive (Saussure 1969: 228); for some it is groups of processes which are productive (Al and Booij 1981: 32; Anderson 1982: 585); in Bauer (1983: 65–74) the productivity of a complete module of the grammar is discussed; while for another group of scholars productivity is a feature of the language system as a whole.

By and large these various descriptions can be seen as differing statements of a single phenomenon, with a greater or lesser degree of specificity. In some places, however, there may be incompatible implications. Consider the difference between assigning the seat of productivity to affixes and assigning it to morphological processes. The problem with assigning productivity as a feature to affixes is that there are some word-formation processes which are non-affixal, but which may nevertheless be productive. Consider the examples of ablaut-motivated compounds in English listed in (1), from Matthewson (1991: 107). Here the segmental framework is reduplicated except for the stressed vowel, which is invariably /ɪ/ in the first occurrence of the framework, and either /æ/ or /ɒ/ in the second.

(1)

chitchat	shillyshally
dilly dally	snipsnap
dingle dangle	tittletattle
fiddle faddle	zigzag
flimflam	clipclop
jinglejangle	flipflop
knickknack	pingpong
mishmash	slipslop
pitterpatter	ticktock
riffraff	

This pattern appears to be 'productive' in the sense that new formations can be found using it – see Schiffman (1999) for further similar examples – yet it is not a matter of affixation. In order to avoid the unwanted implication that only affixation can be productive, we may thus prefer to assign productivity to morphological processes. A possible counter-argument to this position is that assigning productivity to processes might be read as implying that a type of process can be productive in itself (that one might say 'prefixation is productive in English', for example). This does not seem to me to be a major problem, however, since prefixation cannot be productive without at least some individual prefix being productive. Thus, if assigning productivity to morphological processes has a meaning above that of assigning it to individual ways of making new words, the one may be reduced to the other.

The difference between saying that productivity is a feature of morphological processes and saying that it is a feature of rules seems to me to be largely a matter of the author's conception of the grammar. The rule is a precise statement of how the morphological process operates. Saying that the process is productive presupposes the reality of the process; saying that the rule is productive presupposes the reality of the grammar. It does not seem to me that there is any empirical difference between these two versions.

Saying with Saussure that words are productive, on the other hand, clearly does involve a completely different model of the way in which language operates. Saussure's (1969: 228) statement that 'Il y a dans chaque langue des mots productifs et des mots stériles [There are in each language productive and sterile words]' comes in the middle of a discussion of analogy, after he has said 'On pourrait classer les mots d'après leur capacité relative d'en engendrer d'autres selon qu'ils sont eux-mêmes plus ou moins décomposables [It would be possible to classify words according to their relative capacity to generate others, depending on their own degree of analysability].' His idea is that a simplex word cannot in itself lead to an analogy; it is analysable words which allow further analogies to be built on their own pattern. The question of the distinction between rules and analogies is one to which we will have to return later (section 3.12); but even if one assumes a distinction, it is not clear that Saussure's position is justified here. As he himself says in the same passage: '*Magasinier* n'a pas été engendré par *magasin*; il a été formé sur le modèle de *prisonnier : prison*, etc. [*Magasinier* is not generated by *magasin*; it is formed on the model of *prisonnier : prison*, etc.].' That is, it is not the

complex word in itself which allows the analogy, but the analysis of the complex word which is permitted by the existence of both the simplex (or simpler) word and the complex word. It might be in order to conclude that a particular PATTERN is productive, but not, it seems to me, that a particular WORD is productive. It is perhaps worth noting parenthetically (as does Schultink 1992b: 207) that not even Saussure's own usage is consistent in this respect, and that in another passage (1969: 294) he speaks of affixes being productive. I would thus suggest that Saussure's terminology of words being productive should be dismissed as idiosyncratic (though, see also Guilbert 1975: 154), or possibly just imprecise, and certainly not particularly helpful.

Whether productivity should be attributed to processes or groups of processes seems to me to be a similar question to the one discussed above about affixes and morphological processes. If a group of processes is productive, it implies that at least one of the processes in the group is productive, and the productivity of the group can be reduced (perhaps with greater accuracy) to the productivity of the individual members of the group.

The same is true for discussions of the productivity of the word-formation component of a grammar or, indeed, for the productivity of a grammar as a whole (although syntactic as well as morphological productivity is involved there – assuming that the two can be distinguished; see section 7.2 for some discussion). Derivational morphology is productive because, and to the extent that, individual derivational processes are productive. The language system, or the grammar that describes that language system, is productive because, and to the extent that, the individual processes involved in the system and described in the grammar are themselves productive.

Whether we want to say that productivity is a design feature of language and is realised by the productivity of individual processes, or whether we want to say that it is because of the productivity of individual processes that language as a whole shows this design feature, we are left in a situation where the differing statements in the literature about what it is that is productive can be unified. We can now see that within morphology the important matter for discussions of productivity is individual ways of making words.

We must, however, also consider an alternative view here: one which can be related to the 'group of processes' view mentioned above. We can illustrate this view with reference to the past tense of English non-modal verbs. Given a small selection of English verb stems such as *catch*, *dodge*,

learn, *lie*, *ring*, *see*, *swing*, *walk*, *want*, it might not be predictable by any general rule what the past tense form of any particular verb will be. Nevertheless, it will be predictable that there is a past tense for each of the verbs. The views we have considered so far see productivity as a feature of individual morphological patterns (*-ed* suffixation for *dodge*, *walk*, *want*; ablaut patterns for *ring*, *swing* and so on). The alternative is that productivity should be viewed in terms of the slot in the paradigm rather than in terms of the individual patterns which can be used to fill that slot. This is, in effect, the view of separationist theories, like that of Beard (1995). According to this alternative view we would say that 'past-tense formation is productive in English' rather than '*-ed* suffixation is productive in English'. This argument appears to be used by Al and Booij (1981) and Anderson (1982) with reference to action nominalisations, whose irregularity of form, at least in English, is notorious (Chomsky 1970).

The same argument seems to me to apply here as was presented above. The productivity of the paradigm slot depends on the productivity of the individual processes which fill the paradigm slot. It is impossible to imagine filling a paradigm slot for any member of a word-class without using some morphological process to fill the slot in an individual case. Any productivity that might attach to paradigm slots must therefore be reducible to productivity of individual processes. The benefits that are gained from separationist theories are not in terms of our understanding of productivity, but in our capacity to deal with the fact that there is not a perfect correlation between form and meaning in morphology.

We should note, in closing this section, that there are scholars (like Di Sciullo and Williams, cited in the chapter epigraph, and Langacker 1987: 71–72) who deny that productivity is a matter of grammar at all, seeing it rather as the result of the way in which real speakers use a grammar. This is hard to square with the diachronic facts observed in section 1.2, but is a point of view which will have to be taken seriously (see sections 2.8 and 6.2).

2.3 Degrees of productivity

For some scholars, a morphological process is either productive or not (Booij 1977: 5; Zwanenburg 1983: 29). For others there are degrees of productivity. In this second group, there are scholars who take productivity to be infinitely variable along a scale (Bauer 1992a) and others who

take it that there are a small number of steps on the productivity scale, typically three: fully productive, unproductive and an intermediate step.

Pike (1967: 170) gives that intermediate step the label 'semi-active'. The circumstances under which morphological processes (Pike talks in terms of morphs, but we may interpret in the light of section 2.2) are semi-active are not made clear, and Pike later (1967: 191) suggests that 'There may, in fact, be a progressive gradation from highly active to completely inactive, with a number of stages in between.' He does not, however, elaborate on the 'number of stages in between'.

Substituting 'productive' for Pike's 'active', the term 'semi-productive' is also used by Dik (1967: 370). Dik, however, is explicit about the meaning of the term. For Dik a formation (again, we can substitute 'morphological process') is fully productive if it applies to an open class of bases and all possible outputs are acceptable to the native speaker, and semi-productive if it applies to an open class of bases and only some of the outputs are acceptable to the native speaker. If the class of bases is closed (that is if we can list the appropriate bases) the formation is non-productive. Since Dik's definition leaves open the grounds on which a speaker might object to a particular word formed by a particular morphological process, it is hard to avoid the conclusion that the class of fully productive formations must be vanishingly small in any language. For instance, the plural *-s* in English might not be acceptable because of blocking (? *oxes*), because it is hard for a speaker to think of a reason to use the word (? *musics*), for political reasons (? *Maoris*) and so on. (The normal English plural of the word *Maori* is rejected by some speakers of New Zealand English on the grounds that plural is not marked on nouns in the Maori language, and an English form should not be imposed upon a borrowed Maori word – although see section 1.1 for a comment on the acceptance of foreign plural marking in English. There is variation in the use of New Zealanders in the plural of this word. Hence political reasons could indeed be grounds for the rejection of a perfectly regular English form.)

The listedness of unproductive forms is made explicit in a famous passage from Karcevski (1932: 85), here cited from Schultink (1992a: 196):

> Il est très important de se pénétrer de cette idée que ce qui est «mort» peut être identifié par le *nombre*, et vice versa: ce qui est vivant ne comporte pas d'expression numérique. [It is very important to convince oneself of the idea that the 'dead' can be identified by number, and vice versa: that which is living admits of no numerical expression. My translation, LB.]

Various other scholars have made similar observations, but perhaps not so strikingly. Dik, to this extent, is citing received wisdom – though there are still problems with such a definition as will be shown later (section 6.3). He is possibly unusual in assigning so much to 'semi-productivity', though he may not have intended it to be such a catch-all category as it appears to be.

Matthews (1974: 52) also uses the term 'semi-productivity' and is quite clear that it does cover 'the majority of lexical formations'. In a passage which I find particularly clear in motivating this distinction between the productive and the semi-productive, he says

> if *the purpleness of the ceiling* is less secure than *the whiteness of the ceiling*, then why is *a purple ceiling* every bit as acceptable as *a white ceiling*? The answer is that Adjective + Noun is fully productive, whereas Adjective + *ness* is only semi-productive. But the essence of semi-productivity is that the rule itself allows borderline instances. (Matthews 1974: 222)

Other linguists also use the term 'semi-productivity'. Pinker and Prince (1991: 231) talk of semi-productivity where a process 'can to some degree be extended to new forms', but this leaves open the crucial question of what 'some degree' entails and where its borders lie. For Jackendoff (1997: 115) 'semiproductive regularity' arises when 'we don't know exactly what the output of a rule is in a particular case', and he specifically comments (1997: 121) that the productive/semi-productive distinction can be found in both inflectional and derivational morphology. This formulation is difficult to interpret, since a 'rule' which can give rise to an unknowable output must be a very different kind of rule from that normally denoted by the term. Nevertheless, the recognition that both inflectional and derivational processes may be less than fully productive is a welcome one. This returns us to the conclusion in section 2.2 that productivity is a feature of particular processes, not a feature of slots in a paradigm. Fox (1990: 124) uses the term 'semi-productive' more restrictively, saying that 'there are degrees of productivity of affixes, such that certain words could in principle be coined with them for special purposes (e.g. as technical or scientific terms) but not as a general rule.' I am not aware of any other efforts to equate 'semi-productivity' with productivity in particular registers, though the idea is an interesting one.

Fleischer (1975: 71) distinguishes between 'productive' and 'active' processes in terms of a criterion of frequency: productive processes create

large numbers of words, active ones create more moderate numbers of words. We shall see later that frequency is often a poor guide to productivity in the sense we have been using it so far (see section 3.4).

In a number of publications (Bauer 1983: 82–84; 1988: 71; 1994a) I have argued that the notion of semi-productivity is a pseudo-notion that should be given no theoretical status. This conclusion, however, depends upon a prior assumption that we can talk about potential words, and that a morphological theory should ideally be couched in terms of such potential. This question will be taken up later (section 3.2).

Botha (1968: 138–144), noting that many linguists (both within the transformational school and outside it) use labels such as 'immensely productive', 'very productive', 'marginally productive', etc., considers how this can be understood within a transformational grammar. He first considers a definition similar to that proposed by Dik (paraphrased above), and argues that it can only give rise to two classes: productive and unproductive. Summarising what he says, this is because the notion 'acceptable to the speaker' is not built in to the rules of a transformational grammar, and thus has no status. He also points out (1968: 139–140) that for a language to be a learnable system it is assumed within transformational grammar that the lexicon must be finite; that being the case, the distinction between open and closed classes vanishes, and the definition fails to draw any distinctions at all. I do not find these observations particularly helpful in the discussion of productivity. It can be argued, for example, that despite conflicting statements in the literature on the subject, transformational grammar is not an attempt to provide a model which will lead to learnable grammars of real languages. Transformational grammar of the type espoused by Botha is a model of the abilities of the *ideal* speaker-listener, who knows a language perfectly and makes no errors. No *real* speaker learns this system, since real speakers learn their language imperfectly and by gradual accretion. Any real speaker has, at any given time, a finite lexicon and a system which has been learned; any such speaker also has the ability to add to that lexicon within limits set by the human life-span and the power of memory (rather than by the kinds of constraints which affect an ideal speaker). Whether the ideal speaker is deemed to have a finite lexicon like a real speaker or an impossibly large and unlearnable lexicon (in human terms), the ideal speaker-listener must be able to understand new in-coming words, and must therefore have a distinction between open (productive) and closed (unproductive) classes. Similarly a transformational grammar provides the ability to deal with

productive and unproductive syntax by use of the grammar and the lexicon respectively (see section 7.2); there is no need to treat morphology any differently from the syntax.

Botha's comments also fail to allow for a common intuition and a common observation that word-formation processes are not all equally regularly, fruitfully and automatically employed. To the extent that they show us anything, it is perhaps that the model of transformational grammar within which Botha is operating is not suitable for discussing such matters. In particular, the quibble about 'open' classes is ruling out by definition the very phenomenon that discussions of productivity are trying to deal with. The number of words in the lexicon may or may not be finite (depending upon a number of matters of definition), but it is certainly the case that real speaker-listeners meet and produce new ones. Similarly, Botha's comment (1968: 140) that transformational grammar cannot deal with a category of 'unproductive' because 'A transformational grammar does not contain rules for generating ungrammatical compounds or sentences' appears deliberately to miss the point that language histories leave behind morphological traces which, while they may be analysable and describable in terms of some 'rule', are no longer productive. Consider, for example, the vowel shift rules of Chomsky and Halle (1968) which, when applied to words like *pronounce* and *pronunciation* produce 'grammatical' forms, but which would produce 'ungrammatical' forms (such as [əʊbesɪti] for *obesity*) if they were allowed to apply to all available bases. Most grammars of Botha's period actually do contain rules for generating ungrammatical strings, at least in their phonologies. The issues surrounding the distinctions synchronic/diachronic and competence/performance will be taken up again in sections 2.7 and 2.8 respectively, and the discussion there is relevant to Botha's position.

Botha goes on to suggest that the notion of restricted productivity might be given some reality within transformational grammar in terms of an index derived from the number of restrictions there are on the productivity of a given item. He illustrates the kind of restriction he means with reference to the Dutch de-adjectival adjectivalisation affix *-ig* ([əx]), which cannot be used if the base adjective ends in the sequence [əx]. Although he comes to no firm conclusion about how such an index should operate, the suggestion is one which must be taken seriously. Similar suggestions have been made by others – although the same conclusions have not always been drawn. Consider, for example, Di Sciullo and Williams's (1987: 8) query

> If productivity can be defined with respect to such features [or restrictions, LB], can any affix be less than 100 per cent productive? The answer is not obvious.

I agree that the answer is not obvious, and I am unsure what conclusion should follow if it turns out that a limit to productivity is set by restrictions on the base; accordingly I shall leave further discussion of this point until after the discussion of such restrictions (section 5.2). However, the discussion of the productivity of *-ment* in section 1.2 should be recalled: productivity (measured in terms of the frequency of new words) appears to vary diachronically for precisely the same affix.

2.4 Prerequisites for productivity

Three prerequisites for productivity are frequently mentioned in the literature, and for some authors these factors seem to be equated with productivity: frequency, semantic coherence and the ability to make new forms. In chapter 3 I return to discuss each of these in more detail when I consider the main concepts associated with notions of productivity. Accordingly, I do not wish to give a full discussion of these factors here, merely to point out some of the variant positions that have been taken with respect to them.

Many linguists argue that it is a mistake to equate frequency and productivity (see Botha 1968: 138 fn 19; Rainer 1987: 188), and such linguists frequently state that some scholars in the past have done this. However, clear statements of such an approach are hard to find. Rainer (1987) does not cite any; Botha cites several, but of those which are available to me, none gives an unambiguous statement of this position. The clearest example of such a position I have found comes from Fernandez (1968: 74):

> Most of the derivational affixes seem to be relatively unproductive, i.e. few roots seem to occur with any particular affix; in some cases the affix occurs only with one root.

Others are more moderate. For Fleischer (1975: 71) a single new word using a particular morphological process does not indicate that the process is productive, and some degree of frequency is necessary before productivity can be established. For Nyrop (1908: 73)

> La vitalité d'un suffixe dépend surtout de la fréquence de son emploi . . . Plus un suffixe est employé, plus il est capable d'extensions analogiques. [The vitality of a suffix depends above all on its frequency of use . . . The more a suffix is used, the more it can give rise to analogical extensions. My translation, LB.]

Here frequency is not the definition of productivity, but is one of the factors that is required for (high) productivity. Similarly, for Uhlenbeck (1978: 51) productivity is usually connected to high frequency, but not by necessity.

Similar comments apply to the notion of semantic coherence (I take the term from Aronoff 1976: 38). Although Aronoff himself (1976: 45) says that 'productivity goes hand in hand with semantic coherence. However, we have no real evidence as to which of these is primary, or even as to whether they are really distinct matters', for most scholars (e.g. Dietz 1838: 221 cited earlier; Nyrop 1908: 36; Karcevski 1932: 87) meaningfulness and semantic coherence are prerequisites for productivity.

Given that productivity has so far been defined as the potential for new words, it may seem strange to see this listed separately as a prerequisite to productivity. Nevertheless, there are some important points here which, eventually, will have to be taken into consideration in providing a definition of productivity. First, Darmesteter (1877: 70, cited from Schultink 1992a: 189) says that

> pour être vivant, le suffixe n'a pas besoin de produire des mots nouveaux. Son énergie reste latente et ne paraît au dehors que quand une circonstance extérieure, le hasard d'une nouvelle idée, d'un nouvel objet à exprimer, lui en offre l'occasion . . . S'il n'agit pas, il peut agir, et il donnera de nouveaux dérivés lorsque le besoin s'en fera sentir. [To be productive, it is not necessary for a suffix to produce new words. Its energy remains latent, and becomes perceptible only when some external circumstance (the chance of a new idea, of a new object to denote) provides the opportunity . . . If it does not act, it can act, and will produce new derivatives when the need is felt. My translation, LB.]

That is, the actual production of new words is not necessary to productivity, it is the potential which makes things productive; this is in direct contradiction of Fleischer's point noted above. The fact remains, however, that the production of new words may be the only evidence the observer has of this potential, and the lack of new words appears to deny the potential. Nevertheless, this is an important point, and we will discuss the place of the potential in dealing with productivity at a later stage (section 3.7).

On the other side of the coin, the production of a new word in itself is not always seen as evidence of productivity. Uhlenbeck's (1978: 51) definition of what a non-productive morphological process is contains the following phrase:

> non-productive procédés, which can be extended over new material only incidentally, or for special stylistic purposes.

In other words, it is possible to make new words using unproductive processes. Uhlenbeck (1978: 67 fn 39) gives the example of the use of the Sanskrit prefix *maha-* in Javanese, found only in a very small number of words but extended to the word for 'archbishop', *mahabiskop*, where it is prefixed to a Dutch borrowing. Fleischer (1975: 71) gives the example of the use of the German suffix *-nis* in the works of Heidegger, although it is not generally used to make new words. Dressler (1981: 428) lists a number of neologisms made on the basis of 'unproductive rules' as examples of poetic licence in the coining of words, including Gerard Manley Hopkins's *dare-gale* and *daredeath* and James Thurber's *kissgranny* on the analogy of words like *pickpocket*. This leads us on to the next topic.

2.5 The domain of productivity

The examples cited above of the coining of new words despite the fact that the processes by which they are formed are not productive are all examples of individual words or series of words produced by specific individuals. Most linguists also wish to exclude some general patterns of word-making from the domain of productivity. For Aronoff (1976: 21) clippings (such as *deli* from *delicatessen*), blends (such as *mimsy* from *miserable* and *flimsy*) and acronyms (such as *AIDS* from *acquired immune deficiency syndrome*) are not regularly derived, and thus cannot be productive, even if new words are created using these mechanisms. For Bauer (1988: 33) it is not even clear that these things are morphological.

Schultink (1961) – and following him a number of linguists, especially from the Netherlands, such as Booij and Van Marle – wish to exclude words which are intentionally coined, leaving only such words as are automatically coined without speakers or hearers necessarily being aware of them. This would presumably exclude the clippings, blends and acronyms mentioned above, as well as the poetic coinages discussed at the end of the last section. It covers much more as well. It might, for instance, cover the terms in *-nis* coined by Heidegger, referred to above, and it covers

many journalistic formations which are coined to attract attention. These include what I have earlier (Bauer 1983: 264) called 'playful formations', such as those seen in the headlines 'Every dognik has its daynik', 'Sputnik may be deadnik' and 'Beatnik goes neatnik', but also include far less obvious instances. Van Marle (1985: 59) extends the list explicitly to exclude from notions of productivity any formation on a non-native base. Personally, I find this position to be rather exaggerated, but the idea is that the use of a non-native word must involve intention. Where restrictions on productivity of this kind are made, the wider aspect of coining new forms is frequently given another label, usually 'creativity'. This particular way of distinguishing between productivity and creativity will be considered in greater depth in section 3.10.5.

The generalisation here is that, for many scholars, productivity is defined as being a rule-governed matter (see e.g. Botha 1968: 135–136; Uhlenbeck 1978: 5). This formulation was, of course, not available to linguists before the generative era, but generally seems to be compatible with the views expressed by earlier scholars.

The discussion above about productivity appearing only in certain kinds of word-formation can be seen as restricting the notion of productivity to certain domains. There is another sense in which productivity is sometimes seen as linked to specific domains. Consider a suffix such as *-able* in English, and some words which appear to contain it:

(2)

acceptable	comfortable
agreeable	honourable
believable	knowledgeable
commendable	marriageable
desirable	objectionable
eatable	
findable	
guessable	
kissable	
etc.	

The words in the first column are all formed from verbs, and there are many hundreds of parallel examples; the words in the second column are (probably) formed from nouns, and there are not many such examples in standard English dictionaries. It has been realised for over a hundred years that affixes are productive on the basis of particular categories (Kluge 1886: 2 cited in Schultink 1992a: 191). This notion has been developed more recently into the notion of positive conditions on

the base (Aronoff 1976: 63). For Aronoff the productivity of a morphological process may vary for each morphologically specifiable type of base that can be analysed. Although lip-service is now paid to this notion, it has not yet, to my knowledge, led to detailed descriptions of the productivity of individual word-formation processes incorporating such information.

Another related suggestion is made by Kastovsky (1986), although again it has not led to specific analyses that I am aware of. Kastovsky points out that word-formation serves two functions, which he calls labelling and syntactic recategorisation, but which I prefer to call LEXICAL INNOVATION and TRANSPOSITION. Transposition deals with changes which are simply changes of word-class, while lexical innovation deals with the creation of new lexemes to denote newly perceived entities. The two are not mutually incompatible, and many instances of word-formation do both, and the same process of word-formation may have one of these functions as its focus on one occasion, and the other on another. Kastovsky (1986: 596) cites the examples in (3) as showing the priority of transposition and lexical innovation respectively:

(3) a. And he knew no one was going to take him offstage and *beat* him; the *beating* of prisoners was not authorized.
b. . . . a larger male not only stole his peanut but gave him a *beating*.

Kastovsky suggests that not only should productivity be discussed for individual functions of any given morphological process (for example, the productivity of agentive *-er* should not be confused with the productivity of instrumental *-er*, etc.), but that productivity for the different functions of lexical innovation and transposition should also not be confused with each other.

Finally, for this section, there is the question of whether a productive morphological process must be said to be productive in all the words in which it appears. There is general agreement that it need not. Nyrop (1908: 37) using examples such as *corbeau* versus *renardeau*, Pike (1967: 170) using examples such as *hamlet* versus *brooklet* and Aronoff (1976: 22) with the whole notion of 'once-only' rules all agree that individual words coined by productive processes may nevertheless become lexicalised (in the sense of section 3.3, though they do not use that terminology). This contrasts with the general approach in the phonological literature where, for instance, a particular affix is usually associated with a particular boundary (or put at a particular level) in all its occurrences.

2.6 How to define productivity

Rainer (1987) claims that there are six types of definition of productivity current in the literature:

(a) a definition in terms of the frequency of the output words (discussed already in section 2.4);
(b) a definition in terms of the number of available bases, that is in terms of the frequency of the input category (Lieber 1981: 114–115);
(c) a definition in terms of the proportion of words actually used to the number of words potentially created by a particular process (Aronoff 1976; Al and Booij 1981);
(d) a definition in terms of the possibility of forming new words (the definition which has been provisionally adopted here, though we have already seen that it requires some changes);
(e) a definition in terms of the probability of new forms occurring (Harris 1951: 374–375; Aronoff 1983: 163);
(f) a definition in terms of the number of new forms occurring in a specified period of time – Rainer comments that this is a practical way of considering (d), as was implicitly done with the discussion of *-ment* in section 1.2.

Rainer notes that some of these definitions are qualitative, others quantitative; some are synchronic, others diachronic; some build on 'existing' words, others on 'potential' words (see section 3.2). With this amount of variation, it must be admitted that studies of productivity are in a rather poor state, and at the very least some terminological distinctions are necessary. I shall attempt to provide some of these in chapter 3. Before that, however, we need to consider the notion of productivity within two major linguistic dichotomies: synchrony versus diachrony and competence versus performance.

2.7 Synchrony and diachrony

It is often stated that back-formation (e.g. the derivation of *edit* from an earlier *editor*) is a diachronic phenomenon (see e.g. Allen 1978: 214). The major source of this observation is Marchand (1969), where it is stated, variously, that

> The process called backderivation (backformation) has *often* diachronic relevance only. (Marchand 1969: 3; my italics, LB)
>
> The term backformation, backderivation therefore has diachronic relevance only. (p. 391)
>
> The statement . . . that the term backderivation has diachronic relevance only, does not therefore apply to *burgle/burglar* or to similarly structured pairs . . . (p. 393)

As is pointed out in Bauer (1983: 65), there must nevertheless be some synchronic potential which allows back-formation to take place. In this case, the process of back-formation is always diachronic, and the fact that it existed as a process may not be visible to future users of the language, but the potential to use it must be a synchronic one.

Although the same point is not usually made in respect of other kinds of word-formation, it would seem to remain true. The link between *long* and *length* is something that can be determined in terms of the meanings of the two words (but the same would be true of *winter* and *hibernate*, and of *lung* and *pneumonia*), and in terms of some diachronic perspective. This is dealt with under the heading of lexicalisation in section 3.3. The general point is that diachronic changes leave synchronic traces in the morphology. For example, the irregular plural *oxen* is the trace of a regular Middle English plural formation which has persisted in that one word but not, for example in *eyes* (Middle English *eyen* or one of a number of related forms; compare Scottish *een*). An isolated form like *oxen* is usually treated by morphologists as an unpredictable lexical entry which prevents the application of the general rule (e.g. with Lexical Morphology, *oxen* would be listed in the lexicon and would block the application of the Level II *-s* affixation rule). Where the remnants are in the form of a whole series, however, treatment is not so consistent.

Consider English nouns formed with *-ment* again. On the whole, this affix has little morphophonemic effect on the base to which it is added, it has a regular meaning and, even if it is no longer productive in the sense that it is not being used any more to make new words, there might appear to be no great harm in treating its appearance as being rule governed. This creates problems, though. First, there are words like *augment*, *sacrament*, *tegument* where the *-ment* appears to have been attached to obligatorily bound bases ('stems' in the jargon of level ordering) in defiance of its usual behaviour. This might be dealt with by establishing a separate suffix *-ment*$_2$ in such words, or by treating them as unanalysable.

Second, we find the form *governmental*, where the *-ment* and the *-al* have been added in the wrong order for standard level-ordering theories (Aronoff 1976: 53–55; Bauer 1992b). Precisely how this is to be dealt with is a genuine problem (for some discussion see Bauer 1992b), but let us make the charitable assumption that it can be done, whether by recognising yet another *-ment* affix or by some kind of deletion of the morphological boundary. Third, there are words with *-ment* that appear to be formed on the base of words of the wrong category, such as *basement*, *bushment* 'cluster of bushes', *funniment*, *knowledgement*, *merriment*, *oddment*, *rabblement* and *scarcement* (see *The Oxford English Dictionary*). These might be dealt with by multiplying the number of homophonous *-ment* suffixes. Fourth, there are words where the meaning is not what one would predict from a verb + *ment* construction: *commitment*, *department*, *detachment* and, to a lesser degree, perhaps, *embankment* and *endearment*. If all *-ment* words are treated as lexically listed, it is objected that a generalisation is being lost. If they are all treated as synchronically formable words, numerous problems like those outlined here emerge. The truth is that changes can affect words after they have been formed, with the result that they become isolated from the majority pattern. When *knowledgement* was formed, *knowledge* was a verb. The meaning found in *an endearment* is a specialisation of a more general meaning. To use a fairly widespread terminology, words become LEXICALISED (see Bauer 1983: 48 fn 4; see also section 3.3 below). Linguists differ in how they treat such cases, but what we see is diachronic processes affecting some words in a series and not others, with the result that the generality of the pattern is masked. In other words, a synchronic pattern is disrupted because of events which took place at an earlier time. Drawing a hard and fast line between synchronic and diachronic does not necessarily allow this type of situation to be dealt with optimally.

To develop this point slightly, we can note that while lexicalisation as discussed just above is a process which affects individual words diachronically, the result is that at any synchronic moment different words will be at different stages of lexicalisation, the diachronic process being reflected in the synchronic status of individual words (Bakken 1998: 63–65).

A second problem for word-formation with the distinction between synchrony and diachrony is that it is frequently the case that a diachronic event is the evidence for a synchronic state. Consider the definition of productivity that we have been working with so far: a morphological process is productive if it can be used to coin new words. The productivity

may lie in the potential, but the only evidence of this potential is the actual coining of new words. The coining of new words changes the language norm (in Coseriu's sense; see Coseriu 1975) if not the language system.

In Coseriu's view, there is a language system (Saussure's *langue*) which determines what is possible in any given language. This system is, however, not fully exploited: in terms more usually used in phonology than in morphology, there are accidental gaps in the way in which the system happens to be used. That part of the set of possibilities provided by the system which is actually exploited in the language of a particular community is the norm for that community. Dictionaries list words which are part of the norm, not part of the system (Coseriu 1975: 71); the distinction between American English *windshield* and British English *windscreen* is a matter of norm, although, in this particular case, both may be exploiting the same systemic factors. In other places the norm may not even reflect the system: *oxen* is part of the norm; the system would demand *oxes* (Coseriu 1975: 69). The speech (or *parole*) of an individual can be seen as a more or less direct realisation of the norm, possibly with a distinction being drawn between individual and societal norms (Coseriu 1975: 87).

The coining of new words does not change the system, in this terminology, but does (or may) change the norm. By changing the norm the coinage has changed the way in which the language is used; the language after the event is subtly different from the way it was before. In this sense, coining a new word is a diachronic event and, given that, we can conclude that diachronic events provide evidence for the postulation of synchronic states. It must be added here that there is some dispute as to whether a change in the norm counts as a diachronic event or not. To cite just one proponent of the view that it does not, consider the statement in Koefoed (1992: 16), formulated in terms of analogy rather than in terms of rules, but still making the point clearly:

> Analogy is not linguistic change: in the changes that we name analogical changes, the 'analogical' innovation is nothing other than the product of the 'mécanisme de la *langue*', the play of the syntagmatic and associative relations between speech signs, obviously in so far as they are internalised by speakers of the language. [My translation, LB.]

It does not seem to me that the problem is as straightforward as this suggests. The potential for language change is not in itself linguistic change, but the use of that potential, in so far as it affects the norm of the language, could be seen as language change. Perhaps we should say that

even if the act of innovation is not language change, it may have the effect of changing the language (e.g. by extending the set of bases to which subsequent affixation can apply). In any case, the line between synchrony and diachrony is not as clear-cut as many would have us believe.

Part of the difficulty here may be with the way in which the terms 'synchrony' and 'diachrony' are generally applied. 'Diachrony' is applied to a situation such as tracing the development of English from 1400–1900 (Crystal 1980: 109), i.e. it is typically used when discussing relatively long time-spans. Even though variationist sociolinguistics has reduced the relevant time-span, it is rarely less than about a generation (say twenty-five years). Although we pay lip-service to the idea that 'synchronic' means 'at a single moment in time' (Matthews 1997: 367), in practice the 'single moment' stretches to cover notions such as 'modern-day English' (Crystal 1980: 344). Since there is much variation and no little change in 'modern-day English' (see e.g. Bauer 1994b), we tend to view synchronic studies as discounting minor differences, and idealising the data. Once we start looking at individual acts of coining words, however, we are focusing precisely on these minor differences, and the rather loose use of the term 'synchronic' becomes a liability. If we really freeze 'synchronic' as a point in the mathematical sense (i.e. one that has no dimensions), then coining is not a problem, since any coining must either have occurred before the 'single moment in time', or be yet to occur (in which case the word to be coined is clearly irrelevant to the synchronic description). The diachronic evidence for a synchronic potential can in fact be found only after the event. Coinage can be diachronic without affecting the synchronic picture.

Although the synchrony–diachrony distinction may still be usable for some aspects of word-formation, any theory which insists on the clear separation of synchrony and diachrony is going to have difficulty in dealing with the facts. It becomes clear why some people define productivity in terms of the number of new formations occurring within a particular, specified, period of time (section 2.6 (f)): it is one way of overcoming the conflicting demands of synchrony and diachrony.

2.8 Competence and performance

If the concepts of synchrony and diachrony cause problems for the morphologist, those of competence and performance are simply impossible to deal with. Consider Chomsky's (1965: 3–4) introduction of the distinction, in a widely-cited passage:

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance . . .
>
> We thus make a fundamental distinction between *competence* (the speaker-hearer's knowledge of his language) and *performance* (the actual use of language in concrete situations).

As is pointed out by Botha (1968: 133, 135) there is a conflict between such a definition of competence (which is linked to the ideal speaker-hearer) and productivity. Botha argues that a notion of productivity implies the creation of new words. Yet new words would have to be created by the ideal speaker-listener, and 'it would have been scarcely possible for him to be unfamiliar with them' (Botha 1968: 135). Second, he argues that an expansion of the lexicon changes the language, because '[t]he domain of the separate syntactic rules generating sentences in terms of lexical items is . . . changed' (Botha 1968: 135). This means, he says, that such creativity is rule-changing creativity and outside the scope of competence. In either case, he sees incompatibility with the notion of competence.

Halle (1973) tries to solve essentially the same problem by the use of a feature [± lexical insertion], but this is not obviously an improvement. By Botha's argument, a change from [– lexical insertion] to [+ lexical insertion] would be a case of rule-changing creativity and nothing new has been achieved. The feature [± lexical insertion] is nothing more or less than a formal-looking means of capturing Meys's (1975) notion of ITEM-FAMILIARITY, and familiarity with a particular word (or lack of it) is not something which is easily predicated of an ideal speaker-listener who knows a language perfectly.

It is not clear that Botha's problems here are genuine problems. First, he is constrained by having no notion of norm (see section 2.7) in his grammar. The notion of norm would allow us to deny that in looking at coinage we are dealing with any change in the system; rather, we could then argue, we are dealing with change in the way the system has been exploited. Botha (1968: 133) has to conclude that familiarity (and thus productivity) is a feature of performance, because having excluded competence he has nowhere else to locate it. The notion of norm can be seen to have some value here.

But the question of what 'synchrony' means (see section 2.7) also arises here. A theory of transformational grammar of the type that Botha

espouses is a theory about the synchronic structure of a language. On a fine enough definition of synchrony, there is no innovation within a synchronic grammar, and so the question does not arise. Botha (1968: 135) appears to dismiss this argument because he sees it as indicating 'that an ideal speaker-listener knowing his language perfectly at moment M^1 does not know it perfectly any more at a later moment M^{1+n}', but this is not a necessary consequence. The ideal speaker-listener would know the language perfectly at M^{1+n} if the synchronic moment were taken as M^{1+n}, but the grammar would be minutely different from that found at moment M^1.

If this is the case, productivity can be something to do with competence, although this implies a certain picture of how morphological rules work. This will be taken up in detail, and comparisons drawn with phonology and syntax, in section 7.2. In the meantime, it can be noted that such a view of competence would include morphological productivity on a par with syntactic productivity: it would be a potential enshrined in the rules. Just as the potential to create this sentence is one which lies in the syntactic rules of a synchronic grammar at the moment before this sentence was actually produced, so the potential to use an unfamiliar word lies in the grammatical rules themselves.

Some authors see the question of whether a particular rule is part of the grammar or not as part of competence, but the question of how far a particular rule is used as part of performance. These include Kastovsky (1986) and possibly Corbin (1987), and may include all of those who deny productivity any role in grammar (e.g. Harris 1951: 255; Langacker 1987: 71–72). This goes beyond the question of whether there is potential for a new form, and looks instead at how many new forms are created (cf. section 2.6 (e, f)). The implication here is that there is no direct link between the possibility of creating words of a certain form and the number of such words created. We have already seen that this may be contentious (section 2.3), and the matter will be discussed in more detail in chapter 5. Here we should note that for the moment the question of whether there are constraints limiting the occurrence of potential words without being part of the rules of grammar is an open one. Botha (1968: 134 fn 14) claims that there are such factors (and he includes familiarity or lack of it in here). An example might be the political correctness argument that prevents some speakers of New Zealand English adding a plural *-s* to the word *Maori* (discussed in section 2.3). It may thus be that a word like *Maoris* is grammatical but not acceptable for such speakers. The danger

with such a distinction, as I pointed out in Bauer (1983: 84), is that 'performance' can become 'a rag-bag category', used to dismiss all the data which does not fit neatly with the analyst's theoretical preconceptions. Factors such as word-length or the requirement that words should be coined only for nameable entities are the kind that might be included as competence factors by one theorist and performance factors by another.

There are certainly sufficient problems involved in the interface between competence and productivity for us to be able to agree with Schultink (1992b: 211) when he says that if productivity is a systematically meaningful concept, the strict distinction between competence and performance needs to be taken up for reconsideration.

2.9 Summary

It has been argued that, although scholars seem to disagree about what it is in morphology that is productive, the disagreement is more apparent than real. Productivity can, at all times, be considered to be a feature of individual morphological processes.

On the other hand there does seem to be genuine disagreement about degrees of productivity, with some scholars accepting two, some three, some an infinite number of degrees of productivity.

It has been shown that frequency, semantic coherence and the production of a new word, while equated with productivity by some scholars, rather seem to be prerequisites for productivity than productivity itself. And it has been pointed out that there is disagreement in the literature about how much word-making can or should be encompassed under the heading of 'productivity'.

It has been shown that the term 'productivity' is apparently used in very different ways in the literature, at least in so far as the different kinds of definition that are given are not compatible with one another.

Finally, it has been shown that the standard dichotomies of synchrony versus diachrony and competence versus performance need to be considered with great care if they are to be compatible with the basic facts about morphological productivity.

3 *Fundamental notions*

> We must take most of the old terms as they are, and make the best use of them that we can, supplementing them where it is necessary, and limiting the meanings of all terms, old and new, as precisely and unambiguously as possible. But this is no easy task, and I have the greatest sympathy with Sweet, who wrote to me at the time when he brought out his *New English Grammar*: 'I have had most difficulty with the terminology.' Jespersen (1924: 343)

> 'When *I* use a word,' Humpty Dumpty said in a rather scornful tone, 'it means just what I choose it to mean, – neither more nor less.'
> Carroll, Lewis, *Through the Looking Glass* (1871: ch. 6)

3.1 Introduction

The aim of this chapter is to introduce, discuss and define a number of notions that appear to be necessary in the study of morphological productivity. To a certain extent, this chapter continues the work of the last chapter, in that it considers distinctions and the terms for them that have been introduced in the literature on morphological productivity. But it goes beyond the last chapter in considering matters in much more detail and suggesting terminological distinctions where none have previously been made or where those that are made appear unsuitable. While no further attempt will be made to define productivity as such until the end of this chapter, much of this chapter is concerned with setting up the categories that might be in the same paradigm as productivity, and hence implicitly with delimiting productivity. Other fundamental ideas to be considered in this chapter are those underlying a coherent discussion of morphological productivity. It is with one such set of ideas that we shall begin.

3.2 Words: existing, new, potential and probable

Because morphological productivity is a phenomenon that relates to inflectional as well as derivational morphology, it is my intention here to use the term WORD in a way which is systematically vague as to the distinction between lexeme and word-form (Bauer 1988: 9). Accordingly, I treat the words I discuss in a notationally uniform manner, writing them in italics whether they are to be understood as word-forms or lexemes, unless the distinction becomes vital. This practice has already been covertly used in the first two chapters.

3.2.1 Existing words

Van Santen (1992: 63–74) stresses the importance of the notion of 'actual' (Aronoff 1976; 1983), 'occurring' (Allen 1978: 25) or 'existing' (Bauer 1988: 62–64) words for the concept of morphological productivity. If productivity is concerned with the potentiality of new formations, then it must be possible to discover whether or not something is new, and this implies that it can be compared with a list of formations which are not new but 'established' (Bauer 1983: 50). Van Santen's point here seems to me to be unassailable. If, as she so nicely phrases it, 'Productivity is manifested in the space between the existing and the impossible' (Van Santen 1992: 72; My translation, LB), then we must be able to recognise the existing. In brief, if we do not accept a category of existing words, we can have no notion of productivity.

Unfortunately, while we must accept Van Santen's point here, we must also accept that the notion of existing words raises several theoretical problems. The first of these is: Existing for whom or what? One possible answer is 'for the individual speaker'. This answer is implicit in psycholinguistic studies of morphological productivity, which attempt to determine whether and how individuals react to forms they are unlikely to have met before. But for most of linguistic theory, the knowledge of the individual is an irrelevancy, since the individual speaker's knowledge of things such as whether a word exists or not can be influenced by factors such as memory lapses, education and personal experience of the world. This leads to two other possible answers. The first is that we should consider it to be existence in the mind of the ideal speaker-listener who, as we have seen (section 2.8), is not influenced by such factors. As has already been shown, however, the notion of the ideal speaker-listener is not easily compatible with notions of productivity precisely because it

is not clear how such a construct could be unfamiliar with a possible word of the language. This leaves the answer that 'existing' words exist in the speech community.

In practical terms, however, this answer is not very satisfactory unless the speech community can be defined extremely narrowly. Even then, it is not clear how it would be possible to check whether a particular word existed in the speech community without asking all members of the speech community whether they knew the word. If we take the speech community in which this book is being written and read to be the community of English-speaking linguists or, more generally still, the community of people for whom English is a native language or another language in which they are proficient, it is by no means clear how it would be possible to identify either the common core of vocabulary that is shared by the community, or every word which exists within the community. Accordingly, we are in the awkward theoretical position of accepting in principle that there is a set of existing words, and of being able to identify some of those words, but not being able to say with confidence for a huge number of attested words whether they were or were not in existence before that attestation of them.

This problem is usually solved, in a practical way, by making the assumption that the word-list of some large reference work (or set of reference works) is equivalent to the set of existing words. This methodological assumption is clearly false. As a first objection, it can be noted that no such reference work can be printed without having become outdated, since the rate of formation of new words is so high that some new words must be coined in the time it takes to publish any large work. This first objection can be easily met by saying that such a reference work can only give a list of words existing at some particular date, a date which will inevitably be in advance of publication. However, a little thought will make it clear that this is not necessarily a great improvement: since new words can always be added to a language, but not all new words become established in the language (see section 3.2.2), there must be a time when a new word has been introduced but when it is not yet clear whether or not it will become established, and thus worth listing in a reference work. Furthermore, it may not be self-evident at which stage a word can be said to 'exist'. Does existence date from the first coining or from the establishment of the word? If the former, there are many words which 'exist' but are probably never captured in reference works. If the latter, the reference works will provide a better guide to existence, but there may be

words which are familiar to a number of members of the community which nevertheless do not 'exist'.

To avoid such paradoxes, it is necessary to define the terms we have rather more carefully. Let us say that a word is an EXISTING WORD from the moment it is first coined. The word may be ITEM-FAMILIAR to individual speakers, without having become part of the norm of the language. A word is ESTABLISHED once it becomes part of the norm, that is, once it is item-familiar to a large enough sub-set of the speech community to make it worth listing in reference works.

Now it is clear that actual reference works list established words rather than existing words (there are some exceptions in *The Oxford English Dictionary*, noted as nonce words, but in general the statement holds). Only in the kind of ideal world that contains ideal speaker-listeners can we hope to find a list of existing words. It follows that the methodologically practical assumption of the equivalence of the word-list of any reference work or set of reference works and the set of existing words is inevitably flawed. This is not to say that there is any superior method which should be substituted for it, but it does indicate that results based on such methodology may need careful interpretation. This leaves us with the theoretical awkwardness of being unable to state whether a particular word for which we have an attestation 'existed' or not prior to the attested utterance. It seems to me that this is an inevitable result of talking in terms of existing words and the language of a speech community rather than an individual, but that it does not in itself invalidate either of these other assumptions.

To give some idea of the size of problem here, a five-page extract chosen at random from *Time* magazine (*Time* 19 February 1996, pp. 21–25) was studied, and all the compounds and clear derivatives in those pages were marked. The compounds were then checked against the material in the quotations section of the electronically searchable *Oxford English Dictionary* (2nd edition). Of the 148 compounds identified in the text (including compounds within compounds, e.g. *tightrope act*), 67 could not be traced in *The Oxford English Dictionary*. Of those 67, just 12 were judged to be item-familiar (they are listed in (1), so that readers can decide for themselves how fair this is). The criterion used for a compound here was not orthographic (for arguments, see Bauer 1998), and an orthographic criterion would reduce the size of the problem here, but not eliminate it. In other languages, such as German, where no such orthographic distinction is found, the problem seems (but may not actually be) comparatively larger.

(1) Items from a small corpus judged item-familiar but not listed in *The Oxford English Dictionary*

Dayton peace plan	tour-group operator
Dayton accord	U.S. envoy
x million total	Lufthansa flight
crash site	hostage taking
airline official	U.S. ambassador
accident site	opposition figure
island republic	

This result can be compared with the one obtained for German with a much larger sample (1,331 tokens) by Thiel (1973: 379): she found that 37.9 per cent of attested compounds were listed in dictionaries while 62.1 per cent were not. The difference might be a difference between the two languages, or a difference in methodology (I searched the quotation material, not the headwords); nevertheless, it can be seen that the number of compounds attested in either language is considerably larger than the number which is established. Where the derivatives are concerned, the problem is not nearly so great. Of the almost 350 derivatives noted from the same passage in *Time* magazine, only three cannot be found in *The Oxford English Dictionary* – and one of those is a matter of spelling rather than anything else. The three words are *Islamicization*, *czarist* (*tsarist* is in *The Oxford English Dictionary*) and *pan-Turkism*. Neither of the two relevant words is item-familiar to me.

In conclusion we can say that, given the definitions that have been proposed here, the number of non-established derivatives met in normal text is likely to be extremely small, at well under one per cent of attested derivatives. The number of non-established compounds, at least in English, is much higher. We might expect no more than one fifth of these non-established forms to be item-familiar. This means that, while there is a gap between the words we can attest and those which we might want to think of as 'existing' prior to that attestation, we can give some kind of estimate of how large the gap is, and thus have some idea of how far out we are likely to be if we use a dictionary like *The Oxford English Dictionary* as a guide to which words exist.

Kiparsky (1982: 26) claims that 'no rule of grammar ever depends upon whether a word is "actual" or not', and thus implies that the notion of an existing word is theoretically uninteresting as well as practically difficult. Various pieces of evidence have been adduced against this claim (for references, see Plag 1999: 9), in particular pointing out that some words which are formed by affix-substitution can only be formed from an

existing word, and also that words with idiosyncratic meanings (which arise through the process of lexicalisation – see section 3.3 – and are thus the result of the word being an existing word) carry these idiosyncrasies forward when other words are derived from them. For example, *Methodist* and *Methodism* share a meaning which cannot be recovered from *method*, and this meaning is carried forwards into *Methodistical*. This theoretical dismissal of the notion of existing word is not justified and, whatever the associated problems, we need to retain the notion.

3.2.2 New words

To the extent that we are unable to tell which words exist, we are equally unable to tell which words are new. To the extent that we can make estimates of the numbers of one, we can also make estimates about the numbers of the other. There is a well-established vocabulary for discussing new words, although the implications of the terminology are not always as clear as could be wished.

To avoid labels like 'create' or 'produce' which (as will be shown later) may have undesired implications, it is normal to say that a new word is COINED. *The Oxford English Dictionary* defines *coin* in this sense as follows:

> **coin** *v.[1] c spec.* To frame or invent (a new word or phrase); usually implying deliberate purpose; and occasionally used depreciatively, as if the process were analogous to that of the counterfeiter.

However, as it is used here it will not necessarily imply deliberate purpose, and it will not be intended to have any depreciative connotations.

In the diachronically-based vocabulary of the lexicographer, a newly-coined word may be either a 'nonce word' or a 'neologism'. The distinction is basically one in terms of the degree to which the word becomes part of the norm of the speech community involved: a NONCE WORD fails to become part of the norm, and is not generally seen as part of the lexicographer's brief. Again the definition from *The Oxford English Dictionary* may be instructive:

> **nonce-word**, the term used in this Dictionary to describe a word which is apparently used only for the nonce . . . ; similarly ***nonce-use***, etc.; similarly ***nonce-borrowing***, ***-combination***, ***-form***, ***-formation***, ***-meaning***.

The phrase *for the nonce* is defined variously as

> For the particular purpose; on purpose; expressly. [. . .] For the occasion; hence (in modern use), for the time being; temporarily.

There are apparently two sides to this definition (distinguished as different meanings by the editors of the dictionary): an 'on purpose' meaning and 'for the particular occasion' meaning. Again, I would wish to avoid the 'on purpose' meaning, since it is tied up with other controversies about productivity, and follow the meaning indicated in Zandvoort (1972: 43–44), 'Some of them are nonce-words, i.e. spontaneous creations by a speaker or writer, coined for the occasion'.

By contrast with a nonce word, a NEOLOGISM is a word which becomes part of the norm of the language, and thus is part of the brief of a lexicographer (as witness the title of Green [1991] and the description in the first sentence of its introduction, where it is stated that the words covered have 'entered the language'). There are two problems with this particular piece of terminology. The first is that it also has another meaning, namely (again to take the definition from *The Oxford English Dictionary*):

> An invented or concocted word or word-sound without recognizable meaning, freq. interpolated in otherwise correct sentences, and used by persons in a variety of neuropsychiatric disorders.

This meaning conflicts with the less technical meaning in that neologisms always have a meaning and may be argued to have a regular form as well. Such a conflict in meaning is unfortunate when the two meanings are used in the same discipline. The second problem is more fundamental. It is probably not possible to tell at the point when a word is coined whether it will turn out to be a nonce word or a neologism in this sense. Indeed, if the notion of potential word is accepted (see section 3.2.3), it would not be expected that there should be any difference in the linguistic structure of the two (any differences would be in the pragmatic uses to which the word can be put). Accordingly, either should be equally good evidence for the productivity of a morphological process. This being the case, it is likely to be true that the investigator neither can nor should make a distinction between nonce word and neologism in this way. Accordingly, a term is required which is neutral with regard to the diachronic implications that these terms have. Rather than redefine either one (as in Marchand 1969: 9; Bauer 1983: 45), it may be preferable to have a new word to act as the superordinate term. The obvious, ordinary language term is 'new word', but that is ambiguous between new to the individual and new to the society. An alternative found in the literature is COINAGE (Marchand 1969: 9; Strang 1970: 27), which is rather more suitable as a technical term.

To sum up, at the moment when a word is coined, it may not be possible to tell what its eventual status will be in a language: it may become part of the norm of the language and turn out to have been a neologism, or it may not, and remain a nonce word. If there is any distinction in the linguistic structures of these two sets of words, it remains to be demonstrated.

3.2.3 Potential words

The notion of a possible (Allen 1978: 25; Van Santen 1992) or a POTENTIAL WORD (Halle 1973: 6; Aronoff 1983) derives directly from a generative view of grammar (although it is not dependent upon such a view). If it is, as is suggested by Aronoff (1976: 19), 'the task of a morphology to tell us what sort of new words a speaker can form', there is an implication that there are words which might exist, even though they do not. Such a view is congruent with a commonsense approach to vocabulary, reflected in ordinary language use. Consider the following examples:

> I've always had this feeling that it was all eminently postponeable, if that's a word. (Erdman, Paul, *The Panic of '89*. London: Sphere, 1986: 141)
>
> It needs such people, what with increased mechanisation and electronification – if there is such a word. (Lyall, Gavin, *Uncle Target*. Sevenoaks: Coronet, 1988: 47)
>
> 'Do we call her a hit person,' I said.
> 'A gunette,' Quirk said. (Parker, Robert B., *Valediction*. New York: Dell, 1984: 239)
>
> The very unattainableness . . . is there such a word? Anyway, you can see what I mean. (Ormerod, Roger, *The Second Jeopardy*. London: Constable, 1987: 154)
>
> . . . it was the temporary abode of the char-à-bancers, as Teesdale called them, the package holidaymakers of Thomson, Horizon and Yugotours. (Barnett, James, *Diminished Responsibility*. London: Secker and Warburg, 1984: 2)
>
> If a guy who plays around is called a womanizer, what do you call a woman who does the same thing – a manizer? (Sanders, Lawrence, *Timothy's Game*. Sevenoaks: New English Library, 1988: 147)
>
> He had a Tibetan prayer wheel . . . He was kind of Zenny, I guess you'd call it. (Beck, K.K., *Without a Trace*, New York: Jove Books, 1988: 31)
>
> Is there also an ethicisation of the poor if I can coin the word? (Radio New Zealand, Morning Report, 14 December 1993)

> 'Yes. As for attention to detail – that matters, that is – Reed would swear you were the greatest attentionist in the business.'
> 'There's no such word, is there, son? No such word as attentionist.'
> 'Then there ought to be,' said Wanda. 'There's attender . . .'
> 'Which among other things means one who pays heed or attention.'
> 'I meant it differently. Anyhow, I've coined the word.'
> 'And a very good word, too.' (Clark, Douglas, *Storm Centre*. London: Gollancz, 1986: 20)

All these quotations illustrate the filling of what is perceived as a LEXICAL GAP and a consciousness about the coinage. For a coinage to occur, there has to be a need (Bauer 1999b), a real or perceived gap in the speaker's lexicon. (I say 'perceived gap' here, because the need may arise from temporary memory failure or from ignorance of an established word, as well as from the absence of any appropriate lexeme in the norm of the language being used.) I take it that this is what Bolozky (1999: 7) means when he says that 'lexical formation is first and foremost semantically based and concept driven'.

When there is a perceived gap, the language system provides methods for filling it, by exploiting the potential which the system provides. This is amply illustrated in the examples above. There are, however, two points which need to be made here.

The first is that there is a potential to coin words which do not fill lexical gaps. Frequently such potential words will be blocked (see sections 3.2.4 and 5.2.8) from becoming part of the norm by the existence of a synonymous word; occasionally the existence of a synonym derived from the same base will not prevent the introduction of a new word, as witness *normalcy* in relation to *normality* or *orientate* in relation to *orient* (verb).

The second point is that it is arguable that words can be coined which are not licensed by the system. This problem, already discussed in section 2.4, will be taken up again in section 3.7.

Productivity is all about potential. A process is productive if it has the potential to lead to new coinages, or to the extent to which it does lead to new coinages. We are aware of productivity only through the new coinages and the patterns of familiar and unfamiliar words coined by the relevant process (for further discussion, see section 5.3).

3.2.4 Probable words

Because speakers do not necessarily exploit all potentials equally, it is the case that not all potential words are equally probable. Some scholars thus want to distinguish between those words which are potential words

and likely to occur and those which are not likely to occur (presumably on a gradient from 0 to 1). For others, this probability in itself is to be equated with productivity. For instance, Aronoff (1983: 163 fn 1) says that 'In word formation, the code word for probability is *productivity*' which appears to make no distinction between the two. The same is true of Motsch (1977: 191).

However, some scholars seem to draw a distinction. In particular, Van Santen (1992: 87) takes this point of view, claiming that

> Not only the potential and actual members of a productive category must be accounted for, but it must be indicated which possible words qualify for realisation/actualisation. [My translation, LB]

It may not be entirely clear precisely what distinction is being drawn here, but I take it that a 'possible' word is defined in terms of the linguistic system while a probable word is determined by extra-systemic factors. That there are extra-systemic factors influencing the coining of new words is probably not particularly controversial: what is controversial is determining which factors are systemic and which are extra-systemic. Some of the factors that have been considered extra-systemic are listed below.

(a) Blocking (see section 5.2.8): Aronoff (1976: 43) defines blocking as 'the nonoccurrence of one form due to the simple existence of another'. Aronoff (1994: 373) makes the point explicitly that 'Blocking is a comparison between actual and potential words', thus implying that words which are blocked are indeed potential words, but ones with a low probability of occurrence. Van Marle (1985: 205) makes the point overtly: 'blocking is a phenomenon which we consider to relate above all things to "language use" (and not to "language structure").'

(b) Meaning: Motsch (1977: 186) appears to imply that because of the denotation and connotation of some word-formation patterns, they are not in common use. Consider the affix *-some* occurring in words like *twosome*. In principle, a *twenty-five-some* may be possible, but it is not usual because we do not usually operate (in our society) with groups of twenty-five people. Baayen (1989: 25) points out that the Dutch suffix *-erd* as in *bangerd* 'scaredy-cat', which creates pejorative sounding nouns from adjectives, is rarely used in written styles; its connotations are inappropriate for the style.

(c) Aesthetics: This is something of a catch-all category, but includes factors such as word-length which are sometimes said (e.g.

Guilbert 1975: 191) to provide some level of discouragement for word-formation processes (see also Botha 1968: 144 fn 27).

(d) 'Accidents of cultural history' (Lees 1968: 121): A person whose job is to sell things happens not to be called a *seller* in English (although presumably they could have been and the word is found with slightly different meanings), since *salesman*/*saleswoman* is the established form. Although we have a noun *neglect* corresponding to the verb *neglect*, there is no noun in common usage which corresponds to the verb *ignore* in English. (*The Oxford English Dictionary* lists *ignoration*, with citations from the late nineteenth century, but this does not seem to have survived.) In these cases there is no apparent linguistic reason for the current usage, it just so happens that a particular possible form has not become part of the norm in the appropriate sense. Botha (1968: 134 fn 14) says that 'A competence grammar, by definition, does not aim at accounting for the unacceptability of linguistic forms determined by sociological factors', by which he seems to mean many of the same factors.

(e) Failure of hypostatisation: Coining a new word presupposes that there is an entity to be denoted by the new word. If there is no such entity, there is no need for a word. Bolinger (1975: 109) comments with respect to a potential *loather* that 'It is not that the language cannot *form* the noun ****loather***, but simply that we have no use for it. What retinue of people would it designate?'

To the extent that factors like these are indeed extra-systemic, we might wish to see them as factors which reduce the probability of a particular potential word ever coming into existence without reducing the productivity of the morphological process which gives rise to that word. In such cases, it seems to me, we are more likely to want to speak of a particular word being improbable (for particular non-systemic reasons) than to talk of a class of probable words. Accordingly, I acknowledge a possible distinction between PROBABILITY and productivity, without finding it desirable (or even particularly meaningful) to establish a class of probable words.

3.3 Lexicalisation

Most potential coinages have a range of potential senses: they are potentially ambiguous. When a word is coined, however, it is coined in a particular sense, and the likelihood of the coinage being used in other

potential senses is reduced. Thus it might be argued that the act of coinage is sufficient to limit the potential inherent in the language system. For instance, the compound *run time* (dated in Green [1991] as originating in 1965) could in principle mean 'the time at which a person or animal starts a race', 'the proportion of the time during which a person is running as opposed to walking', 'the time for which a machine is functioning', and various other things besides the established 'time at or during which a computing task is executed'. The word was – as words typically are – coined with just one of these meanings, and as that meaning becomes an accepted part of the norm, other meanings become less probable for this particular item. Not all words become established as part of the norm, but those that do, by that very fact, become free to diverge from their original form or meaning in ways that are unrelated to the forms and meanings of their original elements. *Waistcoat* can become /weskɪt/; *prepossessing* can lose all connection with *possess* meaning 'own'. For Bakken (1998: 93) the meaning specialisation is a necessary prerequisite for formal divergence, though the case is perhaps less clear with reference to derivatives than to compounds (see also just below).

Eventually, the meaning is not only specialised in this way, but it may also – for a number of reasons – drift away from any apparent connection with one or all of its elements. When this happens, it appears that the formation could no longer be coined synchronically by the appropriate morphological process. German *Schreibfeder* 'write·feather = fountain pen' could no longer be coined with the same meaning because we no longer use quills for writing, and *Feder* 'feather' has not been extended to mean 'writing implement'. *Learnèd* could no longer be derived from *learn* with the same meaning; it has changed its meaning since it was formed.

Similarly, it is frequently the case that – for any one of a number of reasons – the form of a derivative fails synchronically to reflect the process by which it was coined. *Length* could not be formed from *long* any more. *Semstress* could, today, not be formed by a process of *-ess* suffixation, because there is synchronically no base *semster*. *Hussy* has moved a long way in its phonology from current *housewife*, although they were originally a single polysemous lexical item.

The literature provides a large and not entirely systematic vocabulary for dealing with all of this. Some scholars (e.g. Lipka 1977) distinguish between the meaning and formal sides of the changes, others (e.g. Bauer 1983) do not. Ignoring a varied and picturesque series of metaphors in

the literature – including 'petrification' (Leech 1974: 226), 'ossification' (Guilbert 1975: 154), 'frozen'-ness (Allen 1978: 90), 'fossilisation' (Allen 1994: 4466) – I shall retain a well-established technical term here, and shall use the word LEXICALISATION for the whole process whereby an established word comes to diverge from the synchronically productive methods of word-formation (for earlier uses of the term, see Bauer 1983: 48 fn 4; for contrasting uses of the term, Bakken 1998: 58–60).

There is a certain amount of disagreement in the literature as to whether lexicalisation is a matter of yes or no, or whether words may be lexicalised to some extent (in particular, whether they may be phonologically lexicalised but semantically regular or vice versa). Bakken (1998: 69–70) for example argues that phonological demotivation is always a result of prior semantic demotivation and correspondingly sees semantic specialisation as the basis of lexicalisation (see further in section 3.5). In Bauer (1992a) it is demonstrated that some facets of a particular existing word can be quite regular, while others are irregular. This implies that different types of lexicalisation can be distinguished and that words can be partly lexicalised. That being the case, it is useful to be able to keep the various types apart terminologically.

My personal preferred solution here is simply to talk about SEMANTIC LEXICALISATION or PHONOLOGICAL LEXICALISATION, etc. as is required. It must be admitted, however, that this is not a very fine-grained analysis, and that more elaborate terminologies are available. For instance, Lipka (e.g. 1994) uses the term 'idiomatisation' for semantic lexicalisation. This term nicely captures the idea that semantic lexicalisation involves the loss of compositionality of meaning, just as the formation of idioms does, and my main reason for avoiding it is not its suitability as a term, but the failure to indicate commonalities between demotivations (again Lipka's term) arising for different reasons, phonological or semantic. The potential disadvantage of my terminology here is that it may suggest that it is not the case that lexicalisation can be something that applies only partly, as I have specifically claimed just above.

Equally, it is possible to distinguish between various grounds for lexicalisation: change in the society in which the word is used (cf. *Schreibfeder*, cited earlier) versus change in the language (e.g. *mincemeat* in the sweet sense derives from an earlier use of *meat* to mean 'food') (see Lipka 1977). It is also possible to distinguish between places where semantic lexicalisation arises through the word acquiring extra semantic specification (contrast *wheel chair* and *push chair*, both of which have

wheels and can be pushed) or losing some of its semantic specification (*understand* no longer being connected with *stand*, for instance). While I do not wish to deny such differences (discussed by Lipka 1977), I know of no terminology to cover them, and such distinctions are not likely to be necessary in the course of further discussion of productivity in general.

So far a label has been given to those established words which could not be contemporary coinages, namely that such words are lexicalised. It might in addition be useful to have a label for those established words which, despite their being established, do still form part of a synchronically productive series, differing only from potential words in that, by being used, they have come to have a specific reference (see the discussion of *run time* above). Following Bauer (1983) I shall term such words INSTITUTIONALISED. 'Institutionalised' and 'lexicalised' are thus, by definition, complementary terms, co-hyponyms of 'established'. Bakken (1998: 72) prefers the term 'conventionalisation' here, defining a conventionalised word as one which can be recognised and understood out of context, while a coinage requires its context to be comprehensible. She also notes that conventionalisation is a scale, sliding into lexicalisation. I find this discussion valuable in that it stresses the immediacy with which coinages become institutionalised and the gradualness of the diachronic shift from coinage to lexicalised word. I shall, however, retain my own terminology here.

It is perhaps tempting to equate lexicalisation with STORAGE of ready-made lexical items in the brain, but this temptation must be resisted. While it must, by definition, be the case that lexicalised forms are stored (or have some information about them stored), it is also the case that some institutionalised forms are stored. What fraction of the institutionalised words are stored is a question which demands an experimental answer. It might be the case that all institutionalised words are stored, it might be the case that only some are stored, the others re-generated 'on line'. It must be the case that some institutionalised words are stored, since if they were not stored it would seem unlikely that they could become lexicalised by diverging semantically from the meaning of their elements (as with the case of *mincemeat* cited above). In some psycholinguistic work there seems to be an assumption that storage is used for derivation and irregular inflection while generation is used for regular inflection. Personally, I doubt that any such neat distinction applies (if only because it is so hard to define inflection!), though this is the kind of question that deserves some study. It should not simply be

assumed that the linguistic and the psycholinguistic categories match. This question is taken up again in chapter 4.

3.4 Frequency

It is here necessary to distinguish various notions of frequency, and to consider how frequency interacts with productivity.

3.4.1 Type frequency

TYPE FREQUENCY (sometimes called 'lexical frequency') is concerned with the number of items in the language that contain the item or process under consideration. For instance, the type frequency of *-ric* is one because there is only one word of English that contains the suffix *-ric* (namely *bishopric*). Similarly, the type frequency of *-ter* is two, since *-ter* occurs only in *laughter* and *slaughter*. In the words of Marcus et al. (1995: 212) 'Type-frequency refers to the number of different words in a class, each counted once.' In most cases type frequency is not determinable for the entire language, as it is in the examples given just above, but a frequency can be determined in relation to some well-defined corpus, such as the words listed in *The Oxford English Dictionary* or in Lehnert (1971). In principle, however, it is an absolute, determinable for the language as a whole.

The alternative label 'lexical frequency' is unfortunate in respect of inflectional affixes (at least for people who do not believe in the full entry theory of the lexicon), so that 'type frequency' is a better label. The type frequency of plural *-s*, for instance is the number of regular countable nouns in the language (which is not in fact a knowable total, but clearly a large one).

3.4.2 Token frequency

TOKEN FREQUENCY, sometimes called 'text frequency' is concerned with the number of times a particular item occurs in a given text. '"Token frequency" refers to the number of occurrences of a word' (Marcus et al. 1995: 212); that is to say, repetitions of the same word count as separate items in the token frequency total. So the token frequency of *-ric* in a text about bishops might be 37, if the word *bishopric* occurs 37 times in the text. The token frequency of plural *-s* in this paragraph up to this point is six, even though the tokens of *-s* occur on different words. Since absolute numbers are of little value here, these are more usefully expressed as percentages of the occurrence of some other element (or process) in the

same text. If real numbers are used, you need to know how big the text is before the information has any meaning.

3.4.3 The relationship between frequency and productivity

As has already been mentioned (section 2.4), there are some scholars who appear to equate productivity and frequency. The equation can be shown to be false in two complementary ways. On the one hand, there are morphological processes which appear to be productive but which do not give rise to many new words. An example is *a-* prefixation in English, as in the words *ablaze*, *aflutter*. Marchand (1969: 139–140) lists about 90 such words (some now obsolete). Barnhart et al. (1990: 1) show that these words continue to be productive by listing four new ones: *aclutter*, *aglaze*, *asquish*, *awhir*. Yet in the scheme of English vocabulary, and given the number of potential verbal bases for such formations, this is not a large number. Another way of viewing this is to consider those cases where productivity might be considered a function of the input class (see section 1.1). If there is a small input class of bases, there can never be many new words using that procedure; if there is a large input class of bases there can be many such words. For instance, the prefix *step-* as in *step-father* appears to be largely limited to the class of nouns denoting members of the immediate family and superordinate terms for them (*step-mother*, *step-father*, *step-sister*, *step-brother*; *step-child*; for some people *step-grandparents* seems to be acceptable; *step-aunts*, *step-uncles* and *step-cousins* are marginal, even though they are comprehensible). Whether or not this prefix is still productive is a problem to which we will return (section 7.1.5). However, the numbers of forms to be found which include this prefix are limited by the size of the set of bases to which it can attach. It is to this facet of productivity that Aronoff (1976: 36) refers when he says that equating productivity and frequency 'isn't fair'. On the other side of the coin, there are instances where a particular morphological process has a high type-frequency, but appears not to be productive. The suffix *-ment* discussed in section 1.2 provides an example here. Marchand (1969: 331–332) makes no effort to list all *-ment* derivatives; Lehnert (1971) lists over 700. However, when we consider recent formations, we find that Barnhart et al. (1973) list only one new form in *-ment* coined in English, namely *Englishment* (a word not listed in the second edition of *The Oxford English Dictionary*).

The relationship between frequency and productivity is, then, at best an indirect one. Type frequency is the result of past productivity rather

than an indication of present productivity. According to Corbin (1987: 177) a morphological process is *rentable* (a French term which I will translate as PROFITABLE, following Carstairs-McCarthy 1992: 37) to the extent that it may be used or has been used to produce large numbers of new words. From what Corbin says it appears that she sees profitability as both a potential and a past achievement. The two should probably be distinguished. For the former, we could talk about POTENTIAL PROFITABILITY. For the latter we already have the term GENERALISATION suggested by Bauer (1988: 61). ('Profitable' is an accurate, though to my mind not particularly elegant, translation of *rentable*; alternatives might be *productive* – were this not already overused – or *fecund*, but since Carstairs-McCarthy's label has become accepted in the morphological community, I have chosen to retain it.)

Corbin contrasts profitability and availability. A morphological process is AVAILABLE (Carstairs-McCarthy's translation of Corbin's *disponible*) if it can be used in the production of new words. This is the sense in which we have been using the term 'productive' up until now. In making this terminological distinction, parallel to the distinction drawn by Kastovsky (1986: 586) between 'the scope of the rule [availability] and its actual utilization in performance [profitability]', Corbin assigns frequency to a different area of description from the question of whether or not a new word may be formed, and thus underlines the distinction between the two.

3.4.4 The relationship between frequency and markedness

According to Mayerthaler (1981: 136–140), frequency (of either kind) is an epiphenomenon of markedness, less marked constructions tending to be (but not always being, because of markedness reversal, etc.) more frequent. It is not clear to me how far this is true in derivational morphology. For instance, we can justify regarding the *-th* suffix in *width* as more marked than the *-ness* suffix in *wideness* because the vowel change is less constructionally iconic than the simple addition of forms (Mayerthaler 1981: 24), since it is less transparent (see section 3.5). But it is not clear whether there is any justification for seeing *-ric* in *bishopric* as more marked than *-dom* in *kingdom*, despite the fact that *-dom* in this meaning has a higher type frequency than *-ric* (consider *dukedom*, *earldom*, *officialdom*, *princedom*, *sheikdom*). For this particular pair it might be argued that the frequency difference is too slight to register on any scale of naturalness. The same argument could not hold, however, for nominalisations in *-age* (*breakage*) and *-ment* (*placement*). These differ greatly in type frequency

but, except circularly, it is not clear that there is any evidence for difference in markedness. In other words, it is not obvious that Mayerthaler's claims here can really be maintained in the face of the evidence.

If, for the sake of argument, we do provisionally accept Mayerthaler's point of view, certain conclusions follow. Since more marked constructions are expected to be replaced by less marked ones, this implies that there is likely to be a diachronic tendency (over long time periods, it should be added) for the frequent to become more frequent and the infrequent to become less frequent. Examples such as the replacement of *recur* by *reoccur* and *atypical* by *untypical* would illustrate such a trend, where the failure of blocking increases the number of forms which fit with productive patterns. Since Mayerthaler (1981: 135) also links productivity to naturalness (= unmarkedness), there is a further implication of an indirect link (albeit of a rather different sort) between frequency and productivity here. For Mayerthaler, however, naturalness is the fundamental notion, and the others derive from it, at least to a certain extent. It is no doubt interesting to speculate about whether the frequency of a particular construction is a cause of language change or a result of language structure, but there may be a risk of circularity here.

3.4.5 The relationship between frequency and lexicalisation

Aronoff (1983: 168) makes the claim that 'semantic complexity and [token] frequency go hand in hand', and argues that lexicalised words have a higher token frequency than do non-lexicalised ones. He arrives at this conclusion having compared the token frequency of words ending in *-ivity* and *-iveness* in the Brown Corpus of written American English. While speakers seem to form words in *-iveness* rather than in *-ivity* (so that only *-iveness* is assumed to be productive), words in *-ivity* have a much higher token frequency. The figures are shown in table 3.1.

Table 3.1: *Token frequency of nouns based on adjectives in* -ive *(from Aronoff 1983: 168)*

	Mean token frequency	SD	N
X*ivity*	9.56	23.89	23
X*iveness*	0.64	3.21	103

The implication seems to be that there is a causal link between lexicalisation and token frequency, since lexicalisation leads to greater polysemy (Aronoff 1983: 168). However, since the bases of X*ivity* words are on average also significantly more frequent (at 27.26, SD 29.21) than those of X*iveness* words (at 13.13, SD 20.45) (Aronoff 1983: 168), alternative explanations are also possible: perhaps by the time that *-ness* became the productive nominalisation ending for these words, all the most useful bases had already been taken by *-ity*, for example. The correlation remains, however. There may also be an implication here that productivity is likely to correlate with higher type frequency but lower token frequency than non-productivity (see further below, section 5.3.4), but such an implication can hold only with closely comparable pairs (such as *-ivity* and *-iveness*); it is not clear that knowing that the type frequency of the suffix *-ment* in the same corpus is 162 with a mean token frequency of 21.90, while the type frequency of the prefix *step-* is 4, with a mean token frequency of 2.50 (figures derived from Francis and Kučera 1982) necessarily gives us a great deal of information about the relative productivity of the two, or their productivity relative to that of *-ness* as shown in table 3.1.

3.5 Transparency and opacity

3.5.1 Refining the notion of transparency

Cutler (1980) takes transparency to be a basic unanalysable notion. She defines something as being transparent if (1980: 46) 'the phonology of the base word [including stress] is preserved'. TRANSPARENCY (and its converse, OPACITY), then, deals with the phonological realisation of morphological categories. Cutler's definition implies that transparency deals only with the phonological realisation of bases, but there is no a priori reason to believe that affixes cannot also differ in terms of their transparency. A more sophisticated definition, therefore, would not necessarily assume that it applied only to one kind of morph. Such a definition is implied in Dressler (1985). He treats transparency as biuniqueness between form and meaning. He also points out that a lack of biuniqueness can be more or less serious, so that transparency for him is a cline. He provides (1985: 330–331) a hierarchy of transparency, which is reproduced in table 3.2.

Table 3.2: *A hierarchy of transparency (from Dressler 1985)* ('$' = syllable boundary)

I	Only allophonic rules interfere between form and meaning	excite$·ment
II	Phonological rules such as resyllabification interfere between form and meaning	exis$t·ence
III	Neutralising phonological rules such as intervocalic flapping interfere between form and meaning	[raɪɾ·ə˞] for both *writer* and *rider* (American English)
IV	Morphophonemic rules which cause no fusion between morphemes (for example, velar softening) interfere between form and meaning	electri[s]·ity
V	Morphophonemic rules involving fusion interfere between form and meaning	conclu[ʒə]n
VI	Morphological rules such as those reflecting the English Great Vowel Shift interfere between form and meaning	dec[ɪ]sion
VII	Weak suppletion creates opacity (no generalisations available)	child·ren
VIII	Strong suppletion creates opacity (stems with forms unpredictable by general rule alternate)	be → am

Both these definitions of transparency deal with only one of the possible deviations from what is sometimes known as Humboldt's universal, namely that one function corresponds to one form. Mayerthaler (1981: 34–35) distinguishes between uniformity and transparency, but the precise relationship between the two is unclear to me from his work. One reading goes as follows. There are two deviations from Humboldt's universal: one form to many meanings or one meaning to many forms (the two can combine to give many-to-many form–function relationships). If a single meaning has more than one form (i.e. if there are several allomorphs for a particular morpheme) then we are dealing with non-uniformity. If there are many meanings for a single form (i.e. cases of homonymy or polysemy) we are dealing with lack of transparency. Since this use of 'transparency' is precisely the reverse of that implied earlier, I would prefer to retain the term 'transparency' for a general, superordinate term, and suggest that we talk about UNIFORMITY and MONOFUNCTIONALITY as factors giving rise to TRANSPARENCY. Under this terminology, table 3.2

deals, strictly, with types of uniformity. Allen (1978: 27) talks in what are perhaps more perspicuous terms of 'phonological transparency' and 'semantic transparency', and points out that lack of semantic transparency equates with loss of compositionality or lack (1978: 132) of what Aronoff terms SEMANTIC COHERENCE. There is a link back to lexicalisation here, though it must be noted that while lack of semantic compositionality implies lexicalisation, lexicalisation does not necessarily imply lack of semantic compositionality. Since this viewpoint is directly opposed to that of Bakken (1998) (reported in section 3.3), it may require some justification. That justification is implicit in the examples given in table 3.4 in section 3.12.3, where there are phonological changes apparently due to lexicalisation without any semantic demotivation being obvious.

It should be noted that each of uniformity and monofunctionality has two possible fields of application: bases and affixes. This means that there are various sub-types of transparency, as illustrated in table 3.3. In table 3.3, the focus is on the semantic side of transparency. It must be recalled that the phonological side of transparency is also important, and that the types in table 3.2 can be superimposed on the types in table 3.3. With this kind of situation, the number of degrees of transparency becomes

Table 3.3: *Sub-types of transparency*

		bases	affixes
uniformity	+	English inflectional *-ing* causes no allomorphy in the base.	English inflectional *-ing* has no allomorphs.
	–	English plural *-s* causes base allomorphy in a few words like *knife*.	English 3rd person singular *-s* has three phonological forms
monofunctionality	+	Monomorphemic bases are seldom non-polysemous, but some like *googol*, *helium* or *goo* seem to be.	The affix *-scape* can only mean 'type of scene'
	–	Since the word-form *banks* can realise at least four lexemes (two nouns, two verbs), it is multifunctional.	The suffix *-s* can be plural, possessive, 3rd person singular present, or a diminutive.

significant. We might feel that we get less transparency if both base and affix are either non-uniform or multifunctional, we might feel that uniformity is more important for transparency than monofunctionality or vice versa, we might feel that allomorphy in bases reduces transparency faster than allomorphy in affixes or vice versa. We might feel that phonological factors outweigh semantic factors only if they pass a certain threshold, and so on. If motivated weightings can be established for such factors, it may in principle be possible to set up a hierarchy of transparency, elaborating on Dressler's given in table 3.2. In practice this may turn out to be neither possible nor particularly valuable.

3.5.2 The relationship of transparency to productivity

Cutler (1980) implies that morphological processes which retain transparency are more productive than those which do not because of their transparency. That is, for her transparency is, if not a prerequisite for productivity, at least a major encouragement to productivity. Since transparency is also a matter of naturalness for natural morphology, it would be expected that the more transparent something is, the more productive it is likely to be, other things being equal, since productivity is said to derive from naturalness (see section 3.8). However, the two are not synonymous. With the suffixation of *-ment* we have a morphological process which appears to be transparent but not productive, and Aronoff (1983) cites the addition of *-ity* to adjectives in *-able* (to give nouns in *-ability*) as a productive process, even though it is not totally transparent (for instance, it does not maintain the stress of the base to which it is added).

3.6 Regularity

There are several meanings of the word 'regular' as applied to morphological processes, and these need to be distinguished.

The first is that for some scholars 'regular' seems to mean transparent, that is 'without any morphophonemic irregularities of form'. We already have a terminology to allow us to cover this sense of regularity.

The second meaning of 'regular' is that a process is regular if and only if it is the process used to create the majority of appropriate forms in the language. For instance, *-s* plural in English in this sense is 'regular', but no other plural form is. This might not seem unreasonable, since this type of regularity overlaps with other types, but it would also be the case that the *-en* plural in Dutch (see section 1.1) would be 'regular' and no other

type, which seems less justifiable. The intention here seems to be to reflect the distinction between 'major' and 'minor' rules (Lakoff 1970), which is supposed to equate with productivity. However, examples such as the plural of French nouns in *-al* (see section 1.2) indicate that the majority rule is not necessarily the productive one (Mayerthaler 1981: 135–136) or not necessarily the only productive one (as the Dutch plurals show). It seems preferable to use the term MAJORITY PROCESS / PATTERN / RULE / FORM for processes / patterns / rules / forms which are 'regular' in this sense. This is the terminology used by Marcus et al. (1995: 216) who object to 'regular' being employed in this way.

A third meaning of 'regular' is the etymological one of working by rule. According to this meaning, both the Dutch *-en* plural and the Dutch *-s* plural would be regular, since rules can be formulated to describe both. The English *-en* plural in *oxen*, on the other hand, is a matter of lexicon not a matter of a rule at all, and so would be irregular. The more usual terminology for this sense of regular is RULE-GOVERNED, and that will be retained here (although see the discussions in section 3.12 on problems with such a notion). This is essentially the definition Bloomfield (1935: 213) gives when he says

> We can make our general statement cover one group, but will then have to furnish a *list* of the cases that do not fall under the general statement. A set of forms that is not covered by a general statement, but has to be presented in the form of a list, is said to be *irregular*. We try, of course, to arrange our description so that as many forms as possible will be included in general statements.

(See also Bloomfield 1935: 274.) It appears from this passage that Bloomfield envisages only the possibility where there is a single general statement, and that everything else is listed. We have already seen with the example of Dutch plurals (section 1.1) that this is not necessarily correct. It is certainly true within generative phonology, for example, that rules are stated for more than the default case, so that there is a certain amount of equivocation possible even with the notion of rule. Bloomfield, however, also raises two other issues in this passage. He mentions the notion of frequency, which we have already considered in the notion of a majority form; he also mentions the idea of a list. It has already been pointed out that listedness correlates with lack of productivity (see section 2.3), so that it appears that rule-governedness equates with productivity. We will return to this equation in section 3.10.4, although it must be

noted that this equivalence depends upon the notion of rule, which will also be taken up in section 3.10.

A fourth meaning of 'regular' is found in the psycholinguistic literature. Here 'regular' seems to mean 'freely generalisable' (thus explicitly in Marcus et al. 1995: 216). Pinker and Prince (1988: 135) talk about distinguishing 'real from apparent regularities' where the 'real' ones are those which speakers can generalise over and 'apparent regularities' appear to refer to unproductive minor patterns. This meaning seems to be perfectly synonymous with what I have been terming 'productive'. I take it to be an open question whether this meaning of 'regular' is or is not synonymous with 'rule-governed': there is considerable debate in the psycholinguistic literature as to whether that equation can be made, and that debate will be considered in section 4.2.5.

3.7 Attestation

Reviewing work by Motsch, Hale (1966: 301) pertinently raises the question 'On what basis are attested forms to be accepted?' That is, how is the linguist to tell whether an attestation of a new word at a particular period indicates productivity of the relevant morphological process at that time? It has already been shown (section 2.4) that new words can be the result of the use of non-productive patterns and, indeed, that special effects may be obtained by using such patterns for coinages. It is thus clear that attestation is, in itself, not necessarily an indication of productivity.

Part of the difficulty here is that it is apparently necessary to draw a distinction between what the individual does and what the speech community does. It is the individual who seeks particular effects through the use of non-productive patterns. Likewise, the individual may, consciously or unconsciously, use patterns which are no longer productive in the speech community at large. The example cited by Fleischer (1975: 71) of Heidegger using the otherwise unproductive suffix *-nis* to produce constructions like *Wahrnis des Wesens der Wahrheit* 'true-ness of the nature of truth' is such a case, presumably (though not necessarily) consciously done. In the case of someone of the stature of Heidegger, some of these phrases may go on to become part of the normal (philosophical) language of the speech community. In general, however, it would not be expected that such words would be adopted by the speech community, since they would run counter to the accepted rules of the speech community. Thus, however much Heidegger used *-nis*, it does not for that reason

become 'productive in German'. Following Bauer (1988: 65), we might talk of INDIVIDUAL PRODUCTIVITY here, or we could rephrase this as competence-productivity. 'Productivity' *tout court* is usually taken to refer not to individual productivity, but to SOCIETAL PRODUCTIVITY, which we might rephrase as langue-productivity. There are practical problems involved in drawing the distinction.

Individual productivity is relatively easy to test: we can overtly test the reactions of individuals to suggested new words, or put people in a situation where they are forced to coin new words; we can observe the output of an individual, looking for patterns which that individual uses. In principle, this tells us only about individual productivity. It is usually assumed that if a large enough number of individuals is tested, and as a group they show a strong enough trend in a certain direction, this is an indication of what the societal pattern of productivity is. Since the group of people tested frequently have things in common (e.g. being members of the same psychology class), we could be finding out about the behaviour of a sub-set of the speech community rather than the speech community as a whole, but in principle the experimental method could be changed to avoid this issue. Where dictionaries of neologisms and the like are used as evidence it ought to be the case that the editors of the dictionaries have already made the decision that the words in the entries have become part of the norm of the speech community. The best of these dictionaries give citations to support the entry, and it may be clear from these how widely the formation is being used. Where individual citations are used as evidence, it should ideally be the case that the pattern is not restricted to one particular word or one particular speaker/writer – even though it will often be the case that individual speakers/writers contribute larger numbers to such a corpus than others.

Perhaps the best that can be done is to give a list of factors which would give rise to suspicion that the morphological process was not societally productive, and where extra evidence should then be sought.

(a) Words which occur only in poetry or poetic and/or highly literary prose are suspect.
(b) Words which occur only in headlines are suspect.
(c) Words which appear to be PLAYFUL FORMATIONS (Bauer 1983: 264) are suspect. Playful formations are words in which a particular morphological process is used without any obvious regard for its semantic import. Rather, playful formations are used for their

phonetic qualities (echoing another word in the same utterance) or simply as means of gaining attention (see section 2.5).

(d) Words or morphological processes which occur only in the speech/writing of a single individual are suspect.

(e) Morphological processes which are used only in the coining of technical terms may be suspect.

(f) Following Schultink (1961), many Dutch morphologists would generalise over all of these (and many other types besides) and say that any word which is consciously formed is suspect or, indeed, cannot be a case of productivity. This matter will come up again in section 3.10.5.

(g) Morphological processes which are instantiated in a single word cannot be assumed to be productive.

Within the limitations that this suggests, however, attestation of a new word must be taken as prima facie evidence that the morphological process giving rise to the coinage is productive. In my experience, linguists rarely spell out their presuppositions about attestation in this way. Nevertheless, I believe that for many some such set of guidelines would be implicit in their thinking, and that they would not readily accept as evidence an attestation which fell into one of the categories listed above.

3.8 Markedness and naturalness

According to Mayerthaler (1981: 124)

> Ein morphol. Prozeß ist maximal natürlich, wenn er innerhalb seines ik. F-Potentials appliziert und wenn er kategorie-spezifisch optimal kodiert und wenn sein Output einer Fregeschen Interpretation zugänglich ist. [A morphological process is maximally natural if it applies within its iconic function-potential and if it is optimally coded in a category-specific way and if its output has a Fregean interpretation available. My translation, LB.]

Unfortunately, this definition, precise as it may be, is not maximally transparent itself, and requires some interpretation. First, then, in order for a morphological process to be maximally natural it must 'apply within its iconic function-potential', that is it must give the possibility of a constructionally iconic output. In general, this means that it must be an additive process, rather than a case of subtraction or conversion.

Constructional iconicity arises when an increase in form reflects an increase in semantic complexity. Second, in order to be maximally natural, a morphological process must be 'coded optimally in a category-specific way'. The presupposition here is that there are universal patterns of naturalness for specific categories (e.g. that the third person is likely to be the unmarked person in verbs, that the singular is likely to be unmarked with relation to the plural and the dual in nouns, and so on). A maximally natural morphological process must not conflict with these principles of universal markedness, and so must not add affixes for the singular of nouns, for instance. Third, the output of a morphological process, if it is to be maximally natural, must have 'a Fregean interpretation available', that is its meaning must be a function of the meaning of its parts or, in Aronoff's terms, it must be semantically coherent. The more morphological processes diverge from these ideals, the less 'natural' they are.

For Mayerthaler, productivity is a function of a number of factors of which one is naturalness. Others are the type frequency of appropriate bases, and the degree to which a particular morphological process is consistent with the general pattern of the language within which it occurs. Naturalness itself is a function of a large number of factors, including transparency (see section 3.5.2), and frequency is a result of naturalness (see section 3.4.4). This captures well the fact that there are extremely complex chains of causation involved here, and also the fact that although many of the factors discussed in this chapter are interrelated, they cannot be equated. The sense in which productivity is derived from naturalness still remains to be elucidated, however.

It seems that naturalness increases productivity because only if a morphological process is maximally natural is it maximally analysable and maximally computable. That is, the more natural a morphological process is, the more likely it is that forms using it will be readily understood and will be produced with ease by speakers. Note that this implies a gradient view of productivity, a question to which we will return (section 5.3).

In Figure 3.1 an attempt is made to synthesise all of this in diagrammatic form. An arrow in this diagram indicates that the factor at the tail of the arrow provides a reason for the factor at the head of the arrow (possibly only one reason of many). Some of this is simply a summary of what has been said in the last few sections, some of it is a slight interpretation of what has been said. Labels have been kept short to fit on the diagram, but all refer to material in sections of this chapter.

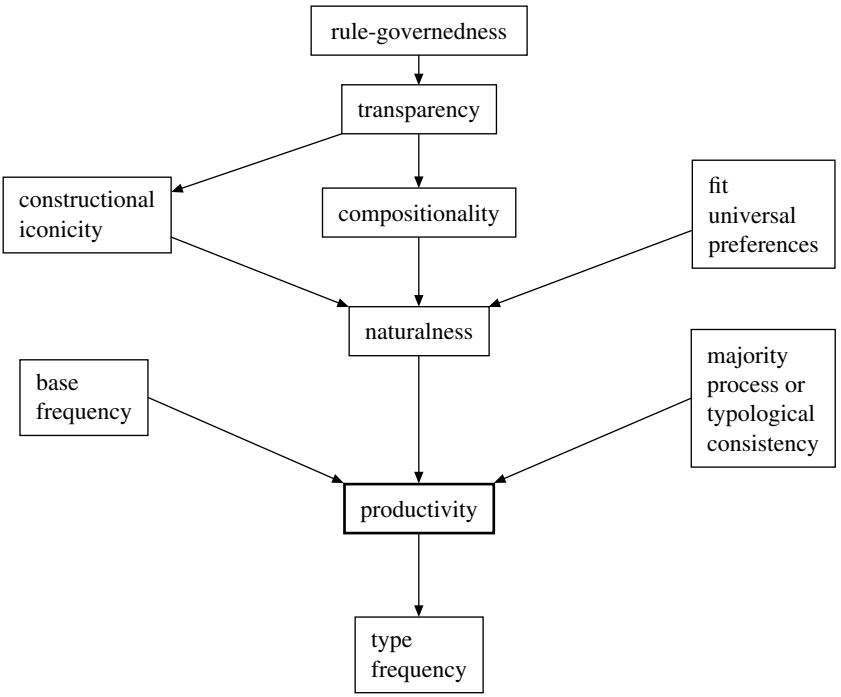

Figure 3.1: Relationship between various factors connected with productivity

3.9 Default

In the default conceptualisation of rule format, according to Zwicky (1986: 306), 'one case is viewed as the otherwise, or elsewhere contingency; . . . it is overridden by the rule for another case.' Marcus et al. (1995: 192) provide a definition:

> *default*: an operation that applies not to particular sets of stored items or to their frequent patterns, but to any item whatsoever, as long as it does not already have a precomputed output listed for it.

In other words, defaults provide a generalisation of principles which have acquired a plethora of titles in the literature: the Elsewhere Condition (Kiparsky 1973), Proper Inclusion Precedence (Koutsoudas et al. 1974), the Panini Principle (Zwicky 1989: 304), all of which I take to be different names for the same basic notion, and Blocking (Aronoff 1976, 1994; Rainer 1988), which is usually seen as entirely separate. What these principles share is the idea that a particular case may overrule a wider generalisation.

For example, in general the past tense of English verbs is created by adding *-ed* to the verb stem, but in the case of the lexeme GO, this is overruled by a particular statement that the past tense of this verb is *went*. The widest possible generalisation is the DEFAULT; it is what is used if all more specific cases fail to apply.

The notion of default has been used mainly in the description of inflectional systems, not only by Zwicky (1985, 1986, 1989) but also by scholars implementing computational models of inflectional systems, such as the Network Morphology illustrated in Corbett and Fraser (1993), and other implementations using the DATR language, such as Cahill and Gazdar (1999).

The relevance of this for productivity is that any default is of necessity productive. Zwicky (1989: 303) goes so far as to gloss default as 'general, predominant, and productive', and Pinker and Prince (1988: 134) refer to defaults as 'regularities [which] can be enshrined as permanent productive rules'. That being the case, it needs to be asked precisely what the relationship between a default and productivity is. Are both notions needed, or does one necessarily imply the other?

To show how this works, a fragment of German nominal inflection will be considered, specifically plural marking. Durrell (1991: 9–10) lists seven ways of marking plurals in German nouns (other lists vary slightly depending on the way matters are classified), as indicated in example (2).

(2)

	Singular	**Gloss**	**Plural**
a.	das Fenster	'window'	die Fenster
b.	der Vater	'father'	die Väter
c.	der Wald	'wood, forest'	die Wälder
d.	das Schaf	'sheep'	die Schafe
e.	der Stuhl	'chair'	die Stühle
f.	die Schule	'school'	die Schulen
g.	das Baby	'baby'	die Babys

These different forms are not all equally available, and especially not equally available for nouns of all genders. German nouns can be one of three genders: masculine (shown by a singular article *der* in (2)), feminine (with article *die*) and neuter (with article *das*). No feminine nouns use process (a) or process (c) and only two feminine nouns use process (b). Only two neuter nouns use process (b), only one uses process (e) and only about a dozen use process (f). The masculine nouns illustrating processes (b) and (c) above are rare, in that there are only about thirty nouns in these two groups combined (Durrell 1991: 11).

In German, the general pattern is that the singular and the plural of nouns are morphologically distinct, so that pattern (a) is recessive (Wurzel 1989: 88, 153–154), though still productive. The pattern in (g), conversely, is becoming more widely used: it is used for words ending in an unstressed vowel other than /e/, for proper names, for phrases which are rank-shifted to be used as nouns, for many loan words, for nouns created by onomatopoeia, for clippings and acronyms, and for minor word-classes used as nouns (cf. English *ifs and buts*), and is replacing older irregular forms in a number of words (Wurzel 1989: 134, 146–147, 150, 154; Clahsen et al. 1992: 228–229; Marcus et al. 1995: 230). Clahsen et al. (1992: 229), Marcus et al. (1995), Wiese (1996: 138) and Cahill and Gazdar (1999: 18) accept this pattern as the default for nominal plurals in German (and Marcus et al. [1995: 229] trace this idea back to 1940), although this view is not universally shared (Wurzel 1990: fn 6). It should be noted, though, that *-s* is not the only productive plural marker in German, nor is it the most common. One count puts *-s* plurals as constituting only 0.02 per cent of plural tokens in an average text (Mugdan 1977: 97 cited in Glück and Sauer 1990: 63), although the number of types is probably higher. Marcus et al. (1995: 227–228) report on a number of measures that put the number of types between 1 per cent and 4 per cent of nouns. Pattern (e) is productive for masculine nouns, pattern (f) for feminine nouns, and patterns (a), (c) and (d) for neuter nouns of appropriate phonological structures (Keller 1978: 560). Some of these classes are very numerous, and some are fed by productive derivational morphology.

We have already seen examples of plurals in Germanic languages (section 1.1) which show that while in some cases the default marker may be the only productive one (English), it need not be (Dutch). The German example confirms this, but shows that the default may even be a minority form. Thus Zwicky's equation of default with 'predominant and productive' (cited above) need not necessarily be the case. While all defaults must be productive, they need not exhaust the set of productive processes; and they need not be predominant in any numerical sense (though if predominance is measured in psychological terms, we might find different answers; see Clahsen et al. 1992). This being the case, the notion of a default must be distinguished from the notion of productivity.

3.10 Creativity

Deciding whether there is a distinction between productivity and creativity and if so what it should be is a major problem for studies of

morphological productivity. There are almost as many different answers as there are scholars attempting to discuss the topic, and there is not necessarily a 'right' answer, since the terms have a definition within a particular framework which is not necessarily transferable to other frameworks. Here I shall attempt to show some of the strands that have been singled out by various linguists, and suggest a neutral or (ideally) less confusing terminology to distinguish the various factors.

3.10.1 Simplex words

Baayen and Lieber (1991: 815) suggest that new simplexes must be formed by something other than productivity, perhaps by creativity. This seems to be in line with comments made by other scholars. Productivity is seen as being a phenomenon which deals with constructions, and the creation or manufacture of simplexes is not a matter of morphology at all. This is the logic by which Aronoff (1976: 20) excludes what he calls 'oddities' such as blends and acronyms from the study of word-formation, and Mayerthaler (1981: 128–129) classes as 'secondary simplexes' not only blends and acronyms, but also shortenings, back-formations and 'half-compounds' (apparent compounds which contain a unique morph such as the celebrated *cranberry* or the German *Himbeere* 'raspberry' which works in the same way). Of these he says that they have no morphological structure which allows morphosemantic interpretation.

We can thus restrict the application of the label 'productivity' to the description of constructions (which, in morphology, means complex words of various kinds), and note that it is possible to coin new words by means other than productivity. We can provisionally use the term 'creativity' for this, but let us mark it as just one possible use for this term, and call what happens in the coining of simplexes CREATIVITY[1].

3.10.2 Figurative extension

One of the standard ways in which the vocabulary of languages changes is by the extension of existing forms to new meanings. Sometimes these are simply more specific meanings of existing words, such as the use of *buddy* to refer to a person caring for a person with AIDS (Engineer n.d.: 12), some are more obviously figurative in origin, such as the extension of *bypass* from a road to a blood vessel in *bypass operation* (Tulloch 1991). Since such processes can occur with simplexes or with complex words, it would seem that they are not morphological as such. (This might be controversial: it might be argued that the two meanings are related by conversion. If conversion is to be viewed as a morphological process – as

I believe it should be – it should be expected to have a relatively constant meaning or set of meanings. Including figurative extensions under this label would vastly increase the number of meanings which would have to be recognised. The conservative option, preferred here, is thus to distinguish between conversion and figurative extension.) If figurative extension is not morphology, we do not want to include it under a morphological heading, and it would seem that nothing is lost by excluding it from productivity. Again, provisionally, we can use the label 'creativity' for the process that allows such cases of extension, but let us call it, for the moment, CREATIVITY[2].

3.10.3 Non-productive creativity

It has already been argued in section 3.7 that there are cases of coinings which are not cases of productivity – the extension of non-productive patterns, patterns used only by isolated individuals, etc. If such coining is not productive, it seems possible to use the label 'creative' to describe it, though again we should provisionally use the label CREATIVITY[3].

3.10.4 Generalising over productivity and creativity

The question now arises as to whether creativity[1], creativity[2] and creativity[3] have anything in common apart from the labels we have given them, and whether there is a generalisation to be made about what is productive and what is creative. If such a distinction is possible, we need to ask what other types of formation might be covered by the generalisation.

One plausible suggestion is that what the three types of creativity that have been isolated above have in common – and what distinguishes them from the cases of productivity – is the fact that they are not rule-governed. This is not an original suggestion, and many have suggested that rule-governedness should be seen as central to the notion of productivity (Botha 1968: 135; Booij 1977: 4; Lyons 1977: 549; Bauer 1983: 63; Botha 1984: 18; Van Marle 1985: 43 [with reference to Aronoff 1976]; De Haas and Trommelen 1993: 17; Katamba 1993: 73; etc.). For some of these authors (e.g. Lyons 1977), creativity and productivity are complementary terms, indicating distinct ways of coining new forms or uses for them; for others (e.g. Van Marle 1985), 'creativity' is a superordinate term with 'productivity' as its hyponym. Clearly, either usage is justified, since these terms are defined within a particular theory, but I propose that CREATIVITY and PRODUCTIVITY should be taken as hyponyms of INNOVATION, and distinguished according to whether or not rule-governedness is envisaged.

A definition of this type, however, brings with it its own problems. In this case the problems centre on the way in which 'rule-governed' is used in the generative literature. In particular, the problem arises with the distinction between major and minor rules. To take a phonological example, alternations which arose historically through the English Great Vowel Shift are treated by Chomsky and Halle (1968) as being rule-governed, even though there is a large amount of evidence that these alternations are not productive (Ohala 1974; Steinberg and Krohn 1975; McCawley 1979; Jaeger 1984; Wang and Derwing 1986). On a morphological level, if it is possible to have a rule which says that *bishop* + *-ric* → N, is there anything which is not rule-governed? What is required is a definition of 'rule-governed' which makes it clear that 'rules' like this one are not relevant. Such a definition, however, cannot be set up in terms of frequency (recall the example of French plurals in *-als* and *-aux* in section 1.2). A textbook discussion of the notion 'rule-governed' (Radford 1988: 19–20) uses the fact of novel utterances to indicate that linguistic behaviour is rule-governed. That is, rule-governed is defined in terms of productivity, which is where the discussion above seems to be leading. That being the case, we cannot define productivity in terms of rule-governedness without vicious circularity.

However, given this problem, and the terminology introduced above, we are still free to define creativity as non-productive innovation. This will capture the same intention, and without overtly calling upon the circularity that discussion of 'rule-governedness' seems to require. However, it implies a rather more detailed definition of productivity than has yet been given here, one which takes into account the problems raised in section 3.7, for instance. Foreseeing such a definition of productivity, I shall allow this definition of creativity to stand. I should note, however, that such a definition does not allow easy settlement of disputes as to what is creative and what is productive, since the distinction becomes ultimately a matter of definition.

Is there, then, an alternative generalisation to be captured here, one which will invest the distinction between creativity and productivity with some testable content? The distinction is related to a perceived lack of generality and predictability in creativity which is supposed to be present in productivity; these, too, are weasel words, though, since they appear to indicate differences of degree rather than differences of kind. Just as it may be predictable that metaphorical extension can take place, but the precise likeness that will be lexicalised may not be predictable, so it may

be predictable that a compound will be formed, but not predictable what the precise relationship between the elements of that compound will be. Given the fact that *virus* no longer means only 'submicroscopic entity dependent upon living cells' we cannot predict what particular figurative extension will affect it. Given a new compound like *designer drug*, we cannot necessarily predict what the relationship between the designer and the drug will be (examples from Tulloch 1991). Neither is totally predictable, but with *designer drug* it is predictable that the compound will denote a kind of drug that has some sort of relationship to designers. Going back to a point made in section 3.10.1, we can say that the extension to *virus* has no predictability which is dependent upon its structure, while *designer drug* has a certain amount of such predictability, and a derivative like *unban* has even more such predictability. The more such predictability there is, the more general the specific statements that can be made about the pattern of innovation as a whole, and the closer the innovation is to being productivity rather than creativity. Botha (1968: 135) captures this basic idea under the label of 'rule-changing creativity', which is contrasted with 'rule-governed' productivity. I find the notion useful for some instances which are on the borderline between productivity and creativity, although it does not seem to be helpful in regard to creativity[1] and creativity[2] above.

3.10.5 How inclusive is creativity?

As has just been stated, given the problems of delimiting productivity and creativity in relation to each other, any borderline between them becomes a matter of definition. Correspondingly, any borderline can be justified – even an arbitrary one – as long as it is consistently drawn and can be consistently maintained. Imagine a cline between creativity and productivity. The closer the dividing line is drawn to the productivity end, the more conservative the analysis is, since it attributes less to rule-governed behaviour. Various suggestions have been made in the literature on how conservatively the line should be drawn.

The most conservative suggestion is one that has already been discussed in section 3.7, from Schultink (1961). According to this proposal words which are consciously created are excluded from consideration as instances of productive use of morphological processes. In this connection, consider the following discussion, reconstructed by Churma (1987: 44) from a Philosophy of Linguistics class. The Professor is looking for a word which will mean 'the quality of being a chair':

> [Professor]: . . . chairness. Chairness? That's not quite right.
> [General agreement that it's not]
> [Student]: Chairity?
> [General unhappiness about the suggestion]
> [Student]: Chairosity?
> [More unhappiness]
> [Student]: Chairhood.
> [General agreement that this is the right word]

(Speakers who are not familiar with the American English variety in which the proposed *chairity* is homophonous with *charity* may need this extra piece of information drawn to their attention to make sense of that particular suggestion, but the scenario should be familiar to everyone, even if not all speakers of English will necessarily agree with the outcome in this particular case.) If we believe that words which are consciously coined are not productively coined, then we must accept that *chairhood* in this discussion is not productively coined. Yet, according to Marchand (1969: 293)

> 'Nonce-words' with meaning 'status of –' are fairly frequent. Examples are *bearhood*, *cathood*, *cubhood*, *doghood*, *duckhood*, *I-hood*, *selfhood*, *tailhood*.

And to such examples we can add the following:

> 'I'M A BUM . . .'
> '. . . Is that the reason for your bumhood?' (Marshall, William, *Whisper*. London: Mysterious Press, 1988: 17)
>
> The sound of the monsters of the river beginning the long journey to handbaghood broke out behind Teppic. (Pratchett, Terry, *Pyramids*. London: Corgi, 1989: 249)
>
> I was still mulling the pros and cons of hubbyhood. (Sanders, Lawrence, *McNally's Caper*. New York: Berkeley, 1994: 284)

Now the question arises as to whether *-hood*, which (*ex hypotheosi*) is not being used productively in *chairhood*, is being used productively in any of these other words, and if so in which. There are, in effect, two distinct problems here:

(a) Can a morphological process be productive in one coinage and unproductive in another, if both are coined in the same state of the language system?
(b) How is 'conscious' coining to be determined from available data?

With regard to (a), it seems to me to be unfortunate if a single process is used both productively and creatively and a distinction has to be drawn between the two uses. As can be seen already, it is difficult enough to draw a distinction between the two when it is possible to decide for a process whether or not it is productive; if the decision has to be made for each individual coinage, then drawing a distinction between productively and creatively formed words becomes many times more difficult. However, if this is a theoretical requirement, it is perhaps acceptable. The problem then comes down to question (b) above. As far as I can see, there is no way in which it can be determined, as a general rule, which words are coined consciously and which words are coined unconsciously. There is no necessary phonetic difference (although there can be an intonational difference), there is no necessary syntactic difference, no necessary semantic or stylistic difference (although such factors might be deemed to be relevant on occasions). Moreover, if a coinage is specifically acknowledged as such in its original context (as in some of the examples cited in section 3.2.3), there is no guarantee that the word was not coined unconsciously, but then picked up in a mental scan of the speaker's own words. In short, however desirable it might be to distinguish between conscious and unconscious coinages, it does not seem to me that it is a practical possibility. As Plag (1999: 14) puts it, it is impossible to operationalise unintentionality, and all rules can be applied intentionally. Accordingly, I do not believe that this criterion for distinction is a usable one.

Next, consider Van Marle's (1985: 59) suggested restriction that 'All morphological processes that demand their base to be nonnative must be precluded from the productive coining of words'. Van Marle justifies this hypothesis on the basis that if a particular process demands a non-native base, there is an implication that coiners of new words have the ability to recognise words from the appropriate language as forming a potential base class. This is not knowledge about how to use language, but knowledge about language, and thus implies a certain amount of conscious consideration of the processes involved in the creation of new words. Superficially, this suggestion is very convincing, but I have two problems with it.

My first problem concerns Van Marle's own discussion of his proposed restriction. Van Marle introduces the proposed restriction cited above in a discussion of the productivity or otherwise of modern Dutch *-iteit*, which in general represents a borrowed French *-ité* (e.g. *absurditeit*, corresponding to French *absurdité*). However, there is a small number of

words where the suffix is added to a native Germanic base, as in *flauwiteit* 'stupid remark' (< *flauw* 'silly'), *leukiteit* 'niceness', *stommiteit* 'stupidity'. For Van Marle, these are not cases of productivity, because of the suggested constraint. But it would seem that examples such as these indicate that the suggested restriction does not apply to this particular case, since it is not true that *-iteit* suffixation demands that its base should be non-native. In my view, this brings us back to the question that was raised earlier: is it possible to have an *-iteit* affix which is used productively in words like *stommiteit*, but non-productively in cases like *absurditeit*? Van Marle answers this question in the negative (implicitly) but assumes that it must indicate that all use of the suffix is non-productive. To my mind, this set of data could be used to argue for a position whereby all use of the suffix is in fact productive, a conclusion that might be strengthened by the 'pseudo-French' examples mentioned by Van Marle (1985: 59) such as *bizarriteit* 'strangeness' corresponding to French *bizarrerie* and *loyaliteit* 'loyalty' corresponding to French *loyauté*, which indicate that *-iteit* has a certain use in Dutch which is not determined by French parallels.

My second problem concerns an extension of Van Marle's proposed restriction. Would it be possible for a morphological process to be productive if it demanded a native base? Such cases are not uncommon, in a number of languages. For instance, the Dutch suffix *-aar* as added to verbs (*wandelaar* 'hiker' < *wandelen* 'to hike') is used only with Germanic verbs of a certain phonological and morphological structure (De Haas and Trommelen 1993: 170). In German the adjectivalisation suffixes *-isch* and *-lich* are largely in complementary distribution determined by whether the base is foreign or native (Fleischer 1975: 269), *-lich* being used on native bases. Even in English, which does not generally make as firm a distinction between native and non-native in word-formation, suffixes such as *-en* (as in *wooden*), *-ster* (as in *youngster*) and *-th* (as in *warmth*) have overwhelmingly been used on native bases. Now, although these cases do not require any bilingualism, they do require the same ability to recognise foreign words as such, otherwise no one could exclude them from the class of potential bases. Unless the real distinctions can be shown to be phonological rather than etymological, it would seem that the argument based on knowledge about the language (and thus consciousness in the coining) would apply here, too. This would have the effect of excluding large parts of the morphology of many languages from consideration as instances of productivity, and in terms of other behaviour of the relevant morphological processes seems too restrictive.

Accordingly, if Van Marle's suggested restriction on the notion of productivity is to stand at all – and I have some reservations about that – I believe that it needs to be made even more restrictive than Van Marle makes it: it must apply strictly to processes which demand (Van Marle's word) non-native bases and where no formation is possible without some degree of bilingualism. Such a case may be the coining of new neo-classical compounds in (for example) English. The coining of words such as *fetology*, *holograph*, *teleonomy* (all from Barnhart et al. 1990) demands a certain knowledge of Greek and Latin. Although it may not always be clear whether a particular word has been coined as a neo-classical compound or as a blend, if neither is productive this may not have practical consequences in the study of morphological productivity. The reason I am hedging my words here is that there is a rather fuzzy borderline between neo-classical compounding and affixation in English: *immunoelectrophoresis* might be argued to be either, *immunodeficiency* may be prefixation, *immunocyte* is presumably neo-classical compounding (examples also from Barnhart et al. 1990). I do not think there is any reason to suppose that instances where an affix is of foreign origin must also be classed under Van Marle's generalisation (after all, most English affixes are of foreign origin), and so I see a distinction between neo-classical compounding and affixation which may, however, be difficult to maintain in practice.

Finally, in this section, consider the suggestion in Bauer (1983: 96) that there is a distinction between productivity and the situation where a new word is formed on the basis of a single lexeme, without 'giving rise to a productive series'. The example given there is of German *zweisam* formed on the basis of *einsam* 'one·some = lonely' (Motsch 1977: 187). The distinction between a formation of this kind, and a formation like *cuddlesome* (Marchand 1969: 347) is largely a matter of how large the series is: with *zweisam* the series consists, apparently, of just two words. (In December 1998 I saw in Freiburg-im-Breisgau a bill advertising a concert by a classical trio called *Dreisam*. However, this apparent continuation of the series is probably motivated by the fact that the river in Freiburg is the Dreisam so that we have a pun based on morphological reanalysis rather than a case of productive word-formation with the suffix *-sam*.) For *cuddlesome*, Marchand lists about twenty nonce words from the nineteenth and twentieth centuries. The crucial question here is whether this represents a difference in frequency or a difference in type. Given that it has been argued that frequency should not be equated with productivity

(section 3.4.3) this is a vital question. However, it has also been suggested here (section 3.7) that productivity can only be tested with several new words, and instances like *zweisam* exclude this possibility. A clue for a distinction here may be gained from Botha's (1968: 135) term 'rule-changing creativity' (see section 3.10.4). At the point at which coining a term changes the rules, it is still a matter of creativity; when coining a word exploits the rules it is a matter of rule-governed productivity. The creation of *zweisam* might conceivably have started a new trend, which would have been the proof that a new rule had been created. It did not, so we have no evidence of a changed rule, and so no evidence of a movement to productivity. By this analysis a minimum of three forms – the original base, the form which reanalyses it to create a new rule, and a subsequent use of the new rule – are required to prove productivity, and larger numbers provide more convincing proof. *Zweisam* remains a matter of creativity. For further discussion see section 3.12.5.

Three suggestions from the literature for drawing a distinction between creativity and productivity have been canvassed in this section, and all have been found to be problematic. The first, it seemed, was not even testable to ensure consistency. The last two, while possibly testable, raised matters of principle. The conclusion for the moment must be that we have as yet no valid way of drawing a clear distinction between what is creative and what is productive, although we may feel we recognise clear cases at either end. One possible hypothesis to flow from this is that creativity and productivity are not distinct categories, but prototypes. This would explain the findings recorded here, without necessarily allowing us to say which we were dealing with on any given occasion. While this is a reasonable hypothesis in the present state of the discussion, it is no more than that.

3.11 Paradigm pressure

In recent years, a number of linguists have drawn attention to the paradigmatic forces which affect the coining of new words (Van Marle 1985; Becker 1993; Bauer 1997; Booij 1997). We can start with the relatively naive observation that new words often arise in the presence of another word with a similar base which may have the function of making the new word easier to process. This is a special case of what Baayen and Neijt (1997) call 'contextual anchoring'. Some examples using the suffix *-ee* are given below.

> Positively and negatively charmed segments are potential governors. Charmless segments are potential governees. (Charette, Monik, The minimality condition in phonology. *Journal of Linguistics* 25 (1989): 164)

> 'Maybe,' he offered, 'they didn't want the follower and the followee to meet.' (Littell, Robert, *The Sisters*. London: Jonathan Cape, 1986: 172)

> If there was any conning to be done, Jack was supposed to be one of the conners, not one of the connees. (Westlake, Donald E., *Trust Me on This*. London: Allison and Busby, 1988: 109)

> 'Now, do we climb into bed together for a week?'
> Jack Corrigan came up with a rare smile.
> 'Just tell me one thing, Frank. Am I the fucker or the fuckee?' (Martins, Richard, *The Cinch*. New York: Villard Books, 1986: 172)

> The inquisitor becomes the inquisitee. (Thomas, Ross, *Briar Patch*. London: Hamish Hamilton, 1984: 114)

In some cases, the paradigmatic force seems to be a major factor allowing the coining of a new word, whether or not another member of the appropriate paradigm is textually adjacent. Consider the following:

> Like NP1 it is not agentive, and it is typically affected by the verb, generally as a benefactee or malefactee. (Gil, David, Case marking, phonological size and linear order. *Syntax and Semantics* 15 (1982): 122)

> There is too much of the biographer in it, and not enough of the biographee. (Cited in *The Oxford English Dictionary* at **biographee**)

Such instances may be indicative, but they are certainly not conclusive. Much stronger evidence is provided by those cases where the form of a derivative can only be predicted from the form of another member of the paradigm, especially a semantically unrelated member of the paradigm. Simple instances of this arise in some cases of back-formation which show paradigmatic forces at work. These include forms like *self-destruct* < *self-destruction*, and the following examples, where the back-formation is either particularly striking or etymologically 'incorrect':

> If you have to, seduct him. (Pratchett, Terry, *Wyrd Sisters*. London: Corgi, 1988: 205). [Back-formation from *seduction*]

> Piotr Borisovich learned . . . how to surveille others, how to detect and evade the surveillance of others. (Littell, Robert, *The Sisters*. London: Jonathan Cape, 1986: 129)

> The crowd . . . thought Carella looked like a baseball player. They deducted this because of his athletic stance and his long slender body. (McBain, Ed, *Eight Black Horses*. London: Hamish Hamilton, 1985: 3)

However, even classic examples like to *burgle* from *burglar* indicate that back-formation makes assumptions about the particular paradigm within which an agentive word ending in /ə/ must be assumed to exist by the speaker. More elaborate examples are also available, however.

One such is presented by the following data from Dutch (Van Marle 1985: 264–266). In Dutch there are deverbal agentive nouns unspecified for gender in *-er*, *-der* and *-aar*. As a generalisation, these are all in complementary distribution, conditioned by the phonology of the base to which they are added. To each of these corresponds a female deverbal personal name. However, there is a single female marker *-ster* corresponding to all of these neutral markers. Where the neutral form ends in *-er* or *-der*, the *-ster* replaces that affix; where the neutral forms ends in *-aar*, the *-ster* is affixed to the neutral form; for examples, see (3).

(3) **Examples of Dutch neutral and female deverbal agentive nouns**

Verb base	**Gloss**	**Neutral**	**Female**
reken·en	'calculate'	reken·aar	reken·aar·ster
trommel·en	'drum'	trommel·aar	trommel·aar·ster
hur·en	'rent'	huur·der	huur·ster
spaar·en	'save'	spaar·der	spaar·ster
gokk·en	'gamble'	gokk·er	gokk·ster
strijd·en	'fight'	strijd·er	strijd·ster

Crucially, where the base of the neutral form is irregular, the base of the female form is equally irregular, so that we cannot really consider the verb to be the base of this paradigm. We find both *betwet·er* and *betweet·ster* 'know-it-all' despite the fact that there is no base verb *betwet·en* (Van Marle 1994: 2930), and many other irregularities are shared between the two sets of formation, e.g. the irregular neutral agentive *reiz·ig·er* 'traveller' from *reiz·en* 'to travel' has as its corresponding feminine form *reiz·ig·ster* with the same irregular empty morph (Van Marle 1985: 269–274). We have to conclude that the *-ster* derivative is formed directly from the gender-neutral agentive, and not from the verb.

Booij (1995) presents a case from Dutch where, he argues, the need for paradigmatic information is paramount. This concerns the form of adjectives corresponding to toponyms, such as *Zweeds* 'Swedish' corresponding to *Zweden* 'Sweden'. Booij argues, on the basis of data like

that given in (4), that the toponymic adjective is derived not from the toponym itself, but from the name of the inhabitant of the place.

(4) **Inhabitant names and corresponding adjectives in Dutch (from Booij 1995)**

Toponym	**Inhabitant**	**Adjective**	**Female inhabitant**
België	Belg	Belg·isch	Belg·isch·e
Drente	Drent	Drent·s	Drent·s·e
Finland	Fin	Fin·s	Fin·s·e
Hongarije	Hongaar	Hongaar·s	Hongar·s·e
Noorwegen	Noor	Noor·s	Noor·s·e
Rusland	Rus	Russ·isch	Russ·isch·e
Zeeland	Zeeuw	Zeeuw·s	Zeeuw·s·e
Zweden	Zweed	Zweed·s	Zweed·s·e

He gives the following generalisation:

> [A]djectives corresponding to toponyms which have a corresponding inhabitant-name without any of the suffixes *-er*, *-(en)aar*, *-aard* or *-ier* are derived by the affixation of *-s* or *-isch* to a stem which is identical in form to the inhabitant-name. [My translation, LB]

He suggests that this can be extended to cases like *Chomskyaans* from the noun *Chomskyaan* 'supporter of Chomsky('s methods, ideology, etc.)' and *Marxistisch* from *Marxist* (noun). He also suggests that the names of female inhabitants of places, which semantically appear to be derived from the toponym or from the name of the unmarked inhabitant, are formally derived from the adjective given above, so that a female Belgian is a *Belgische*, a female Norwegian is a *Noorse*, as shown in (4).

The importance of such cases for the argument here is that we have instances where a particular form can be derived only from another form in the same paradigm, even though it is not semantically the most obvious word to base the derivative on. A *Chomskyaanse analyse* does not have anything to do with supporters of Chomsky's methods – it could be presented by someone who wanted to show Chomsky's method to be faulty – but formally requires reference to the form which, in isolation, would mean that. This implies that cross-referencing within the paradigm is the only way to find a regular way to generate the appropriate forms.

All this indicates the importance of paradigmatic structure to the coining of new words, but doesn't say anything about the productivity of the patterns involved. The importance of paradigmatic pressure for studies of productivity will become clear in the next section, when we take up the notion of analogy.

3.12 Analogy

3.12.1 Is there any difference between analogy and productivity? First approach

How is the plural of a word like *arb* 'a dealer in stocks of companies facing take-over bids' (Tulloch 1991) to be generated? Should it be generated by comparison with the plural of an already known word, like *barb*, or should it be generated by the application of a suitable sequence of rules which adds an appropriate element and determines its precise form on the basis of phonological compatibility with the root? In a case like *arb* there is no observable difference in the output: it will be *arbs* in either case, and this will be true in the vast majority of instances. However, in the case of a word like *lox*, while it is clear that the sequence of rules will provide *loxes*, it is not clear what the method based on comparison will yield: if a comparison is made with *box* it will give *loxes*, but if a comparison is made with *ox* the plural will be produced as *loxen*. There is thus the possibility of observable empirical differences between the two methods of dealing with the creation of new morphological forms, and thus at least a prima facie case for there being a genuine question for linguistic theory as to whether morphological innovation is rule-governed or driven by analogy.

The choice is, of course, not quite as simple as that might seem to imply. There are innumerable compromise solutions, as is shown with reference to one set of regularities by Derwing (1980). There could be distinctions to be drawn between derivational and inflectional morphology on this point, between native and non-native vocabulary, and so on. Nor is it necessarily the case that the same answer will always be true. Attested new plurals such as *Vaxen* as one possible plural of *Vax* (the computer) alongside *Vaxes* indicate that analogy is sometimes chosen above rule, but not necessarily always. This does not, however, invalidate the basic question, which we could perhaps reformulate as 'To what extent is morphological innovation rule-governed, to what extent is it driven by analogy?'. In such terms, it seems to me, the question is fundamentally a psycholinguistic one: How do real speakers actually form new words? Just occasionally, this psycholinguistic question will have systemic results, however, and it would be interesting to know just how widespread and identifiable such systemic results are.

There are a number of reasons for believing that morphology is basically a matter of rule-governed behaviour, and a number of reasons for believing that it works basically by analogy. There are also counter-arguments

to many of these arguments. In what follows, I shall try to provide the arguments and their counter-arguments, many of which have been discussed by Becker (1990). The result will be that any conclusions depend on the weight that is given to various arguments and counter-arguments.

3.12.2 Reasons for taking a rule-governed approach to morphology

One standard objection to the notion that word-formation is determined by analogy rather than rule-governed is that the notion of analogy fails to make suitable predictions. There are two ways in which this might be true. Either analogy fails to make appropriate predictions about what potential words are (Becker 1990: 16), or it fails to make predictions about the possible direction for analogical levelling (Becker 1990: 23).

The entire generative approach to word-formation in which word-formation is seen to be governed by rules is built on the assumption that there are potential words, and that there are some strings of formatives which do not form potential words. The distinction between these is defined by the rules. Thus *beigeness*, whatever its status as an existing word, is clearly a potential word by virtue of the rule that *-ness* can be added to any adjective to make a noun (with suitable overriding principles of blocking taken into account). *Beigity*, on the other hand, is not a potential word, even though *-ity* can be added to adjectives to form nouns (*liberality*, *lucidity*), because *beige* does not belong to one of the classes of possible base for the rule of *-ity* suffixation.

The fundamental principle underlying the notion of analogy is that any new form can be created as long as there is a suitable pattern for it to be formed on. To a large extent the suitability of the base must lie in the eye of the coiner, so that it is not clear that it is possible to define classes of bases to which an analogy can apply. While many monosyllabic adjectives undergo morphophonemic change with the addition of *-ity* (words like *clarity*, *sanity*, *vanity*), the existence of *oddity* shows that they need not, and *beigity* could be formed on the pattern of *oddity*. While principles of analogy would allow us to say that a string such as *odd·ity·al* is not a potential word, because there is no model for it, analogy would not allow us to distinguish between a potential word like *beigeness* and a string like *beigity* which is not a potential word. Thus the approach which views such innovation as being rule-governed is seen to account for the relevant data more adequately.

The counter-argument to this point is that forms such as *beigity* are indeed potential words, just rather improbable potential words, while forms like *beigeness* are rather more likely because they are supported by

more patterns which are more similar. Becker (1990: 23) goes so far as to state 'es gibt offenbar keine unmöglichen Analogiebildungen! [there are obviously no impossible analogical formations. My translation, LB]' He claims (1990: 17–18), for instance, that *heye* as a parallel to *hear* is not impossible, just extremely unlikely because there is only one pair which can act as a pattern, and thus that the word is not easily segmentable.

In a discussion of the outcome of analogical levelling, Becker (1990: 23) provides the following example from the history of English:

(5) **Analogical levelling in the history of English (from Becker 1990: 23)**

	Infinitive	**Past singular**	**Past plural**
Old English	biitan	baat	biton
Modern English	bite	bit	bit
Old English	riidan	raad	ridon
Modern English	ride	rode	rode

For the verb *bite* the plural stem has been generalised, for *ride* it is the singular stem that has been generalised in the past. Since analogy is at work in either case, analogy cannot predict which of these will occur, even if there is a general principle involved. Rules can, of course, invoke general principles.

Becker's (1990: 24) response to this is to say that predictions about the direction of levelling are nothing to do with a theory of analogy, they are to do with a theory of markedness or naturalness, and thus there is no argument to answer here.

The next argument in favour of a rule-governed approach to morphology is that analogy would allow too much. As a general rule, principles of analogy are more permissive than are rules (or conversely, rules are more constrained than analogy). Three particular examples will be discussed below.

The first example of analogy allowing too much comes from non-productive patterns. It is generally accepted by students of productivity in word-formation that some processes which were once productive are no longer productive. Probably the clearest example of this from English is the use of *-th* as in *dearth*, *girth*, *truth*, etc. (see, e.g. Bauer 1988: 59–60). However, these words persist in the vocabulary of English, and thus are available as potential patterns for analogy. If it is the case that *nearth*, *herth* and *newth* (from *near*, *herd* (verb) and *new*, on the patterns of the words listed above) are impossible formations, then any theory of analogy has to explain why such analogies should be blocked. Such 'facts' are no problem for a theory in which the formation of such words is rule-governed: the rule has simply disappeared.

The second example of analogy being too permissive concerns restrictions on the type of base used in a process. Earlier the example of *beigeness*

as opposed to *beigity* was cited. There is no doubt that *-ity* suffixation is productive, for example on the end of words ending in *-able*/*-ible*, but it is not generally productive for all adjectives, including the adjective *beige*. Where *-th* is concerned, there is simply no productivity for that affix; where *-ity* is concerned the affix is productive, but only with certain classes of base. Analogy can cope with cases like *beigity* only if it is the case that there are no possible patterns to copy. Rule-governed approaches can cope perfectly well with restrictions on bases.

The third example of analogy being too permissive concerns the creation of new paradigms. While new paradigms are occasionally created, such as the series in *-burger* or *-teria* or *-nomics*, new paradigms are rare. Yet if analogy is the ruling principle in word-creation there is no reason why new paradigms should not be created all the time. Why not, based on *automobile*, a whole series of *petrolmobile*, *gasmobile*, *electromobile* (note that this would be a different series from the *Batmobile*, *bloodmobile* series which does exist)? Why not, based on *satellite* either *satedark* or *sateheavy*? If word-formation is generally rule-governed, the absence of a plethora of such patterns is expected; if it is generally analogically-based, the absence of a plethora of such patterns has to be explained.

The counter-argument here is also based on a denial of the so-called observed facts. It is not true that analogy would allow too much. All the things which are claimed to be impossible or not to occur are not only possible, they do occur.

There are many known instances of apparently non-productive morphology being used again. Some of these are cases of 'exaptation' (Lass 1990) or the use of morphological material whose function has vanished for some new purpose. Lass (1990: 88–95) cites the example of the historical weak adjectival inflection in Netherlandic becoming instead a marker of attributive usage on certain classes of adjective in Afrikaans. Matthews (1974: 54–55), in a different context, discusses the way Indo-European inchoative **-ēsk* moves through Latin to an inflectional formative in Italian forms such as *apparisco* 'I appear'. In these cases forms survive but not morphemes (if we may use such a term, these days) since the meaning side of the morpheme has been lost. In other cases the morphemes survive. In English we have the much maligned suffix *-dom*, which was regularly discussed as dead morphology until Wentworth (1941) showed that it was still in common productive use. This suffix, however, was probably mis-analysed rather than resurrected. The case of the Slavic first person singular verbal *-m* discussed by Janda (1996) is a clearer example. This was found on only five verbs in Common Slavic, but spread

to all verbs in Slovak and Macedonian (Haspelmath 1997), as a result of phonological changes in the other verbs. This particular example is a striking one. For a form which occurs on only five lexemes to become general is unexpected since analogical change usually makes changes towards a majority form (as has happened with the gradual loss of strong verbs in Germanic, most of which have moved into the regular weak paradigm). Accordingly, we might not expect the English diminutive suffix *-erel* to be resurrected, since there is probably only one lexeme in which it is analysable (and then only in some varieties of English), namely *cock-erel*. Nevertheless, we cannot exclude such a possibility entirely.

Where the creation of new paradigms is concerned, this is in fact common. A few random examples from Barnhart et al. (1990) are *acrolect*, *acupressure*, *spendaholic*, *alphametic*, *archosaur*, *bigemony*, *decidophobia*, *multiversity*. The lack of certain specific examples does not show that the pattern of analogy is not used widely.

3.12.3 Reasons for taking an analogical approach to morphology

The first reason for assuming that morphological processes involve analogies rather than rules comes from the irregular semantic relationships found in word-formation. It was well-known even prior to Chomsky (1970) that the semantic relationships pertaining in complex words were not regular. Chomsky used this fact to argue that complex words could not be constructed by syntactic transformational rules, which were assumed to have a constant semantic effect. But the argument can be taken further to indicate the likelihood of a role for analogy.

Consider the following sentences and the accompanying glosses for the nominalisations (taken from *The Oxford English Dictionary*):

(a) It is a *simplification* to express it in that way. ['The result of the action or process of VERBing']
(b) The *purification* of the water is not yet complete. ['Action or process of VERBing']
(c) These days everyone looks for instant *gratification*. ['State of being VERBed']
(d) Geography in any extended *signification* of the word. ['That which is VERBed']

Here we see that the same nominalisation ending can have at least four possible glosses. Now imagine someone who needs or wants to interpret a new nominalisation for a verb ending in *-ify*. Consider the following genuine example:

> It needs such people, what with increased mechanisation and electronification – if there is such a word. (Lyall, Gavin, *Uncle Target.* Sevenoaks: Coronet, 1988: 47)

or, from a spoken source

> the commodification of culture.

Rather than give a rule for the interpretation of these forms (or for their formation), which implies a spurious unity to the procedures, the argument goes, it is more satisfactory to see them as being formed/interpreted by analogy with cases such as (a) to (d) (specifically, by analogy with (c) above).

Parallel with this argument, we can consider an argument based on the irregularity of formal relationships in morphology. Again, Chomsky (1970) brought the well-known formal irregularity of nominalisation processes in English into focus. One of the results of this irregularity is that it is possible for the same verb to have several non-homophonous nominalisations. Consider, for example, (6).

(6)			
	commission	remission	transmission
	committal	remittal	transmittal
	commitment	remitment	
		remittance	transmittance

This, too, can be seen as an argument in favour of an analogical approach to word-formation. The argument goes as follows. *Remittal* is found in a religious (remittal of sins) and quasi-legal (remittal of costs) sense from the sixteenth century (if we take the dates in *The Oxford English Dictionary* seriously). When a nominalisation of *transmit* is required in a legal context, analogy with *remittal* allows the formation of *transmittal*. Later, a similar analogy allows the formation of *committal* in a number of religious or quasi-legal contexts (committal to the grave, to writing, to Parliament). This cannot be a rule, for there is no requirement on *-al* nominalisations that they should have a religious or quasi-legal tone (consider *arrival*, *disposal*, *refusal*), nor do verbs ending in *-it* usually take *-al* nominalisations (consider *elicit*, *inhabit*, *orbit*). Specific analogies seem much more explanatory than rules restricted to such small sets of verbs.

Similar questions are raised by the well-known problem of English plurals with more than one form, sometimes two synonymous forms (*spectra*, *spectrums*), sometimes non-synonymous forms (*indexes*, *indices*,

for some speakers). This irregularity can give rise to insecurity on the part of the individual speaker or on the part of a large group of speakers as to what form to use, and thus give rise to variation. Forms such as *prospecti* and *octopi* (see section 1.1) are not easily explained in terms of a rule, but are easily explained in terms of analogy with other words ending in *-us*. Derwing and Skousen (1994: 194) summarise this by saying that while rules make a sharp distinction between regularities and exceptions, real usage does not. They also note (1994: 195) that rules cannot account for variation (which the *octopi* example illustrates).

The counter-arguments to these arguments from regularity deal with the interpretation of the observed irregularity. The argument from the meanings of nominalisations presupposes that very precise meanings are generated as part of the nominalisation process. It is equally possible that very broad meanings are generated by the nominalisation process, and precise meanings are imposed by pragmatic inference – inferences which in their turn become institutionalised. Under this alternative assumption, the interpretation of *the commodification of culture* would work as follows: *Commodification* means 'nominalisation of *commodify*' (which in turn means 'verbalisation of *commodity*'); in context, the nominalisation must be interpreted as 'state of being commodified', but in a different context, such as 'Her commodification of the culture horrified me', 'act of commodifying' would make more sense.

Where irregularity of formal relationships is concerned, there is no doubt that the formal realisation of nominalisations is synchronically irregular. But at any given period, only a small number of nominalisation processes are likely to be productive, and those that are productive are likely to be in complementary distribution (see the discussion in section 6.4). Once a particular nominalisation is accepted in the community as having a particular meaning, it is always possible to use another productive nominalisation ending to denote something different. But it is still rule-governed. The results of the process, however, look as if they are not rule-governed. The error is in trying to deal with existing words by generative rules, instead of trying to deal with potential words.

As far as the foreign plurals are concerned, these are not productive and not rule-governed. Only the *-s* plural is rule-governed, which is why it tends to take over from other plurals. The use of foreign plurals is a matter of what is in one's personal lexicon and conscious decision-making. It is for this reason that people can get the plurals of *prospectus* and *octopus* wrong – because it is not rule-governed, but simply something

they try to work out by logic. Such errors do not prove that analogy is the general way of performing morphological processes.

The next argument against a rule-governed approach is based on the observed irregularity of morphophonemic processes. In table 3.4 I present examples of English adjectives with final-syllable stress which end in /l/. The question is, what happens when the adverbial *-ly* is added to such adjectives. Do we find double or single /l/? The answers provided by Wells (1990) and Jones (1988) (*EPD* 14) are listed in table 3.4.

Table 3.4: *Single or double /l/ in adverbial forms of certain adjectives according to pronunciation dictionaries*

	Wells 1990	*EPD* 14
banal	ll	–
droll	ll	ll
dull	ll ~ l	l ~ ll
foul	ll	ll ~ l
frail	ll	ll
full	l	l
ideal	ll	l
pale	ll	ll
real	l	l
shrill	ll	l ~ ll
small	–	–
sole	ll	ll
swell	–	–
tall	–	–
vile	ll	ll
whole	ll ~ l	ll ~ l

Where *-ly* is added to an adjective without final stress, there is never more than one /l/. So what is the rule? Clearly, it is not easy to state a rule, since there is so much variation (apparently lexically conditioned). Such a distribution is easily explained in a model based on analogy: since both types are found in the language, both are available as bases for analogy. It might not be possible to predict which analogy will be followed in any particular instance, but this does not interfere with the basic principle.

The counter-argument from the position of someone who believes in rule-governed behaviour is as follows. Of course it is true that

morphophonemic variation is not totally regular. Any assumption that it will be ignores the well-known phenomenon of lexicalisation. Once words are established, they are allowed to change, phonologically as well as semantically. The error is in assuming that all listed words are produced by a single set of rules. That is not necessarily the case.

Becker (1990: 55–57) points out that there are inflectional forms which have clearly been created on the basis of other inflected forms in the same paradigm. He cites, for instance, Louisiana French *ils sontaient* for standard *ils étaient*, and the loss of earlier French *amons* for *aimons*. Such forms cannot be accounted for by rules deriving the inflected form from a stem, but have to be accounted for by analogy. We have already seen (section 3.11) that there are examples of words formed on the basis of other members of the paradigm rather than from the ostensible base of the derivative. In English derivation, *-ism* and *-ist* appear to be linked to each other without any necessary reference back to a recognisable base. Thus a *socialist* is a practitioner of or believer in *socialism*, where the precise relation between *social* and the affix is the same in each case, though not easily specified in either. Similarly, a *Methodist* practises *Methodism*, although synchronically any link with *method* has been lost. And in the pair *baptism* and *baptist* there is no free base from which to form either word. Again, analogy can account for such cases easily, but there is a problem in rule-governed approaches.

However, if we are committed to a rule-governed approach to morphology, even such examples are not necessarily insurmountable. It would be foolish to deny the phenomenon of analogical levelling, but that in itself does not disprove the notion that morphological behaviour is rule-governed. It is clear that analogy plays a part in word-formation, but that does not mean that analogy alone underlies it. Analogical levelling simplifies the rule system, thus making it easier for subsequent generations to generate forms by rule. In particular, the pairs in *-ism* and *-ist* do not show that such words must be created by analogy. Although it is the unmarked case for words to be derived from other words (Aronoff 1976; Dressler 1988), rules can also delete or truncate material before adding another affix. Words without apparent bases can still be derived by rules.

Irregular back-formation also provides an argument for analogy. The nominalisation of *destroy* is *destruction*, from which is formed *self-destruction*. But the verb from *self-destruction* is not *self-destroy*, it is *self-destruct*. Back-formation in such cases clearly works by the deletion of material rather than by the undoing of any rule of formation. That is,

back-formation is not a process in which rules are undone. Forms like *burgle* from *burglar* show that there need not even be an appropriate affix present. All there has to be is a parallel pattern in the minds of speakers. In other words, there must be an analogy. There are at least two possible answers to this. First, it has already been argued that irregular back-formation shows paradigm pressure (section 3.11), and it may not be considered to be in the rule-governed part of morphology. Alternatively, it can be argued that if back-formation is seen as a rule deleting affixes or forms homophonous with affixes under appropriate semantic circumstances rather than as a rule undoing a formation rule, there is no paradox involved in talking about a rule of back-formation.

Finally, another argument for analogy and against rule-governed morphology can be based on phonaesthemes. PHONAESTHEMES are recurrent sounds or sound sequences which appear to indicate a semantic field of reference. For example, the /gl/ in *gleam*, *glimmer*, *glare*, *glint*, *glitter*, *glow*, *gloss* seems to refer to light. Morphologists have traditionally been extremely chary of classing such things as morphemes for at least two reasons. First, words containing phonaesthemes cannot be exhaustively segmented and, second, phonaesthemes do not work everywhere: consider *glum*, *gloom*, *gloaming*, *glower*, *glib*, *gloat* where the /gl/ appears to have connotations of darkness and unpleasantness and *glass*, *glamour*, *glove*, *glottis*, *glucose*, *glory* where it has neither kind of association. If such words are deliberately formed because of this common element, or gain general acceptance because of it, then analogy rather than rules appears to be at work. The answer to this is that phonaesthemes are not morphological, and thus not subject to the same rule-governed behaviour that morphology is. Even if phonaesthemes do arise through analogy (and the possibility that they are a post-hoc rationalisation of random behaviour should not be ruled out) this would not prove that morphology must arise in the same way.

3.12.4 Parallel distributed processing

In discussing analogical models of morphology, there is one which deserves special mention since it is perhaps the most extreme analogical model to be proposed and at the same time the most widely discussed. That is the connectionist model using parallel distributed processing (or PDP) presented in Rumelhart and McClelland (1986). Rumelhart and McClelland's model may be taken to incorporate Bybee's model with schemas (see Bybee 1985, 1988), since it presents an explicit model which appears to build in

precisely the kinds of mechanisms to which Bybee makes less explicit reference (though Derwing and Skousen [1989: 65] see Rumelhart and McClelland's model as rather more restrictive). Rumelhart and McClelland (1986: 218) specifically set up their PDP model as an alternative to rule-based models, saying 'we would only suggest that parallel distributed processing models may provide a mechanism sufficient to capture lawful behavior, without requiring the postulation of explicit but inaccessible rules.' Their argument is thus essentially an argument based on Ockham's razor.

In their article, Rumelhart and McClelland report on an attempt to teach a computer to produce the past tense forms of English verbs. The computer was presented with a number of pairs of forms, a stem or present tense verb and the corresponding past tense verb. The verbs with which it was presented were divided into three groups. First, during the training phase, a group of 10 very common verbs (8 of them irregular) were presented 10 times each. This was seen to correspond to the high frequency of irregular verbs in basic spoken English, and thus to the earliest input with which children are faced when they acquire English. Next a group of 410 medium-frequency verbs (334 of them regular) were added and the full set was presented 190 times. The model was then 'frozen' so that no more learning took place. Finally, in the test phase, a set of 86 rare verbs (72 of them regular) was presented to the system with the stem form only, and the system's output was recorded. The computer simply noted the phonological form of the input and the phonological form of the output in the first 200 trials and, in training, connection strengths between various input and output forms were adjusted to minimise the difference between the computer's output and the natural language form. The 86 test forms provide a check on the way in which the system has learned to produce past tenses on the basis of previous throughput. Importantly, there is no notion of a rule here, simply a mapping of one set of forms onto another set of forms.

Rumelhart and McClelland (1986: 261) report that 91 per cent of the test verbs had the correct set of output features generated by the computer. The proportion was slightly lower for irregular verbs than for regular verbs. But even this impressive achievement was not the most impressive feature of the model. The model is said to replicate, in various ways, the stages that children acquiring English also go through: irregular verbs are first learnt with their irregular form, and then later regularised (e.g. early *came* is replaced by *comed*), before returning to the form which is 'correct' by adult norms; forms ending in /t/ and /d/ are left unchanged in the

past tense more often than other verbs, and more often than adult norms suggest; where verbs are regularised, stem + *ed* (e.g. *comed*) is the initial regularisation, but past + *ed* (e.g. *camed*) becomes a stronger reaction in older children. The computer mimics all of these patterns as it learns the past tense forms of verbs.

In a very positive review of the PDP programme, Sampson (1987) argues that Rumelhart and McClelland's model is superior to a model which uses rules to generate the various forms: rules, says Sampson (1987: 876), 'may more or less accurately *describe* the output behavior of this model; but nothing corresponding to such rules exists within the model: the rules in no sense *explain* its behavior.' The explanation which Sampson sees in the PDP model comes in the fact that it sees the past tense forms as a function of the present tense forms and the frequency of use of various patterns, which is information which is available to the child in its linguistic environment. A rule, as a simple statement of regularity, has to be deduced over a large number of exposures, and is not based, in the same immediate way, upon the directly observable data.

If it is the case that such a model can deal both efficiently and in a life-like manner with a problem area such as the past tense of English verbs with a high rate of accuracy, it might seem that there is an open-and-shut case made for some kind of analogical or connectionist approach to the study of inflection. However, the backlash from linguists who have a vested interest in the primacy of the rule has been so great, and so overwhelming, that the PDP model of morphology appears to have suffered a severe setback. While the ferocity of the attack might give the impression that the PDP model is viewed as a dangerous competitor, which strikes fear into the opposing camp, the arguments presented against Rumelhart and McClelland's picture of morphological competence are extremely convincing. I shall not attempt to review all the counter-arguments here, since many of them focus on practical details of Rumelhart and McClelland's model rather than the general picture. Rather I shall pick out some of the points which I see as being major principled objections to what Rumelhart and McClelland claim to have achieved.

First of all, it is claimed that the apparent parallels with real language acquisition are illusory, and the result of the way the PDP model is constructed and/or an insufficient appreciation of the way first language acquisition really works (Marcus et al. 1992). Even supporters of the model accept this criticism (MacWhinney and Leinbach 1991), so we can treat it as justified without going into all the details which prove the

point. It seems unlikely that this point is crucial to the acceptance of the connectionist view of morphology, in any case: the apparent parallels emerged as a by-product of the original implementation, and were cited because they appeared to support the idea that the PDP model was in some sense a 'natural' one; but the parallels were a by-product, not a central reason for using the model in the first place, and so are not central to the acceptance of the model. Furthermore, providing a parallel with real-time language acquisition is not a determining factor dividing the PDP model and the rule-governed model that its detractors support: the rule-governed model does not provide an accurate reflection of natural language acquisition, either. This objection (or set of objections) to the PDP model can thus be discounted at this stage.

Second, it is objected that Rumelhart and McClelland's model is not accurate enough (Pinker and Prince 1988: 125). Changing Rumelhart and McClelland's criteria for accuracy so as to include all forms which pass the threshold for being acceptable, Pinker and Prince (1988: 125) calculate that there is a 28 per cent failure-rate over the 72 regular verbs in the test set. The Rumelhart and McClelland model fails to provide a past tense for some verbs such as *jump* and *pump* (Pinker and Prince 1991: 236–237), and it is said to operate like a child who has not yet worked out what the rules are, not like a proficient adult user (Pinker and Prince 1988: 136, 166). Four of the outputs for regular verbs (though not necessarily the preferred options) are what Pinker and Prince (1988: 124) term 'grossly bizarre': *squawkt* as the past tense of *squat*, *membled* as the past tense of *mail*, *toureder* as the past of *tour* and *maded* as the past of *mate* (Rumelhart and McClelland 1986: 264). However, MacWhinney and Leinbach (1991: 136), while agreeing that the Rumelhart and McClelland model is deficient in this way, say that the problem can be solved with more training of the system, and that what is at fault here is not the basic principle of connectionism, but merely the particular model that Rumelhart and McClelland used. Again, even if we did not have MacWhinney and Leinbach's assurance that an implementation with more examples and more training succeeded in avoiding such problems, this would seem entirely plausible given the limited amount of data that Rumelhart and McClelland used, so that this objection, too, can be discounted.

Third, it is objected that Rumelhart and McClelland's model fails to capture a relevant generalisation about the phonology of affixation, namely that a vowel is inserted to split up a sequence of two identical consonants (or two consonants viewed as too similar) in forms like *mended* and

buzzes (Pinker and Prince 1988: 105–106). In fact, it is very difficult to know what the appropriate phonological generalisation is in such cases. Within a morpheme, English does not allow any immediate repetition of the same phoneme, but vowels separating identical phonemes in words like *tattoo* and *secede* are presumably part of the underlying specification of those morphemes, since they never alternate. Thus, the only place where the generalisation can have any observable effect is over morpheme boundaries. Between words there is no such restriction, so that *what time* and *this second* may both contain superficial geminates, as may compounds such as *gas station* and *cat train* (both US English). In suffixation, such sequences are not blocked if the sounds repeated are sonorants: *evenness* and *wholly* both contain geminates for many speakers. So any generalisation which is left to make would seem to concern the various affixes of form /z/ (Pinker and Prince [1988: 102] suggest there are nine of these) or /d/. MacWhinney and Leinbach (1991: 135, 149–150) claim, but do not demonstrate, that a fuller implementation overcomes the problem that Pinker and Prince see here. It is not clear to me that the model does in fact make any real generalisation here, but since it is not clear precisely what generalisation we might want any model to make (or what generalisation we might expect any real speaker to use or be aware of), this does not seem to be a particularly strong criticism.

Fourth, and much more seriously, it is claimed that the Rumelhart and McClelland model is incapable of dealing with homophonous verbs (Pinker and Prince 1988: 110–114). Consider (7) (Pinker and Prince 1988: 113; Marcus et al. 1995: 202 fn 6):

(7) a. Tomorrow she will /rɪŋ/ the bell. She /ræŋ/ the bell.
b. Tomorrow she will /rɪŋ/ the washing out. She /rʌŋ/ the washing out.
c. Tomorrow she will /rɪŋ/ the pigeon. She /rɪŋd/ the pigeon.

Since Rumelhart and McClelland's model works exclusively by mapping one phonological form onto another phonological form, it has no way of distinguishing between the homophonous lexemes in (7), still less of explaining why there should be two different irregular patterns and one regular pattern. It certainly cannot predict the regular pattern in (7c), even though it is predictable to the linguist, given that the verb *ring* in (7c) is derived by conversion from a noun, and that such verbs are typically regular, even when homophonous with irregulars: compare *braked*, *grandstanded*, *sleighed* (Kiparsky 1982: 12; Pinker and Prince 1988: 111). MacWhinney and Leinbach (1991: 136) claim that this problem can be

solved within a connectionist model. All that is required is that semantic features be added to the network as well as phonological features. It is certainly the case that if a connectionist model is to find links between related forms, it is going to require some semantic features, even if no more than the feature [PAST] used by Bybee (1985: 130). Without such features the necessary conditions for discovering a parallel between the pair *string*/*strung* and the dialectal *bring*/*brung* are simply not present. We must then assume that semantic features are present, and that they too can be used in creating or destroying parallels. In the case of (7a) and (7b), the semantics is supposed to be sufficient to break the parallel, so that the system learns the two as separate items. Yet in the case of *brung*, the system is supposed to ignore the semantics of the lexical base in favour of the inflectional semantics (the marking for past tense), and allow an innovative extension. It is not clear to me that this is a coherent set of demands on the system, or how any system of this nature can be expected to cope with such disparate facts. Furthermore, to capture the (7c) generalisation properly, the system would have to carry as a feature the derivational history of the verb *ring* or some reference to the existence of the noun *ring*. I assume that this would either have to be reduced to some aspect of the semantic structure of the verb, or simply not be captured. Here, it seems to me, the PDP model still does have some questions to answer. Daugherty and Seidenberg (1994: 383) specifically query the need in a connectionist model for traditional lexical entries as used by linguists. Linguists such as Derwing and Skousen (1994: 197) see lexical entries as one of the benefits of their model over connectionist alternatives. This is an area where the two approaches are clearly differentiated.

Finally, and most tellingly, Pinker and Prince (1988: 121–122) and Marcus et al. (1995: 197) argue that the Rumelhart and McClelland model fails to capture the nature of defaults in natural language (see section 3.9). It was noted above that the Rumelhart and McClelland implementation failed to find a past tense form for verbs such as *jump*. This, it is claimed, would be impossible for a real speaker because there is a default in place which directs that in the absence of any specific information to the contrary, the past tense will be made by the suffixation of *-ed*. Marcus et al. (1995: 197) list 21 sets of circumstances where this default value is required. Crucially, the default overrules generalisations based on phonological structure alone.

An example from derivational morphology may make this a little clearer, and provide a Gedankenexperiment which shows something of

the problem. Imagine a situation where, because of some crisis in East Asia, the United Nations set up a special volunteer force to cope with the conditions there. This would probably immediately become known as the United Nations Special East Asian Volunteers and, given the precedent of other UN bodies, become an acronym UNSEAV. Now assume further that deploying a force of this type became seen as a typical UN way of dealing with problems in many regions in East Asia or beyond, we might then convert the acronym to a verb, and say that *It has been decided to Unseav the border between North and South Korea* (this would be a typical route for such a word to take: it appears that verbs based on acronyms derive from nouns; Cannon 1987: 109). Assume further that someone wished to protest against this action: what nominal form of the verb *Unseav* would they use to fit the slot in *We wish to protest in the strongest terms against the __ of the border between North and South Korea*? The question is an interesting one, because all English verbs that have a stem ending in the sequence /siːv/ have a nominalisation ending in /sepʃən/. A priori, this seems a very unlikely form for a speaker to choose, since the alternation between *-ceive* and *-ception* is not a productive one in English. There are other verbs which end in the sequence /iːv/, and they show a variety of nominalisation patterns, including *believe/belief*, *bereave/bereavement* and *retrieve/retrieval*. These, however, do not form a regular pattern in the way that verbs ending in /siːv/ do. Given that, it is hard to see how a connectionist model could provide a nominalisation other than /ʌnsepʃən/ – even though a native speaker would not even be sure how to spell such a word. In the particular case, the notion of a default is itself perhaps slightly difficult (see further section 6.4), though we might nominate *-ing* as the relevant affix, and certainly *We wish to protest in the strongest terms against the Unseaving of the border between North and South Korea* would be perfectly acceptable. This is then a case where a connectionist approach, because of the way it works, seems destined to do something that a real speaker would not do, and where a default setting is required which the connectionist model does not obviously provide.

MacWhinney and Leinbach (1991: 138) suggest that there is no real problem here, and that the connectionist model is simply a way of approaching the data that a rule-based model also deals with, but without the theoretical impedimenta that the rule-based model demands. I do not find this answer at all convincing, although I agree that MacWhinney and Leinbach are stressing one of the very attractive features of the connectionist programme. Examples like the *Unseav* one suggest to me

that Pinker and Prince are correct in seeing a crucial problem for the PDP model here. Specifically, this seems to me to set up the necessity that Ockham's razor requires for going beyond the PDP model.

Pinker and Prince (1991: 233) suggest a compromise model whereby irregular forms are generated by structure which has connectionist properties, while the regular forms are generated by rule. They compare this connectionist portion of the morphology with the redundancy rules used by Jackendoff (1975). Marcus et al. (1995: 195) sketch a similar suggestion, and link it not only to Jackendoff's redundancy rules, but also to the schemas of Bybee (1985, 1988). Pinker and Prince (1991: 243–244) point out that such a model implies the psychological separation of regular and irregular morphology, but provide some limited evidence from language deficits to support such a picture. Apparently some agrammatic aphasics can read irregular past tenses and plurals much more accurately than phonologically equivalent but regularly inflected words, and this would be accounted for if this type of aphasia attacked the rule part of morphological competence but not the connectionist part. Conversely, some children suffering from a central nervous system deficit known as Williams Syndrome overgeneralise regular morphology and have difficulty with irregulars.

3.12.5 Is a compromise possible?

Discussions in the literature usually assume that it has to be decided whether morphology works by rule or by analogy, and that is the kind of assumption that has been made in the discussion so far, except at the very end of section 3.12.4. For example, Becker (1990) concludes that what we call a rule would be no different if we called it an analogy, and that 'rule' is just a term used by some linguists for 'analogy'. It is worth asking, though, whether there is some way of dividing rule-governed morphology from analogical morphology.

It seems to me that the cases most clearly requiring explanation by analogy come from two areas: from rule-changing innovation with (at least initially) very low type frequency in the new pattern and from certain lexicalised forms.

Rule-changing innovation refers to the making of new bases as in the *sontaient* example above, new paradigms such as the *cheeseburger* type, and back-formations. We might also extend this general type to include the consciously worked out plural forms of the *octopi* type. In these types a new formation is based on a single form (possibly more in the case of

octopi) and, although the new pattern may become common, there is in the first instance a single output.

The relevant lexicalised forms include cases of irregular morphophonemics in words like *dully*, and the irregular forms and meanings of nominalisations. Here we have claim and counter-claim. Those who believe in an analogical approach claim that innovations are based on individual existing words and thus take on the irregular form or meaning of those words. The counter-claim is that if the patterns are considered in abstract enough terms, it will be discovered that there is no irregularity in innovation, and that apparent irregularities arise through the diachronic process of lexicalisation. Each claim needs to be systematically supported; as far as I know, neither has been.

This leaves two arguments, the argument from phonaesthemes and the argument from paradigm pressure. Phonaesthemes can be discarded as irrelevant if it is admitted that the question of phonaesthemes is not a matter for morphology. Paradigm pressure is potentially embarrassing to both sides. On the one hand, where there is innovation because of the force of the general pattern over a large section of vocabulary, it frequently appears that simple rules such as the addition of affixes and the meaning of the whole being derived from the meaning of its atoms will not apply. Recall the Dutch examples of the names of female inhabitants of various places discussed in section 3.11. On the other hand, this does not prevent extremely general rules being formulated to capture the patterns which are available. It would appear that such examples do not weigh more heavily in one direction than the other, although they may have implications for the type of rule we envisage when we talk about 'rule-governed' morphology.

This discussion points to the conclusion that we can equate rule-governed with productive and analogical with creative. Given that it has already been argued that the notion of 'rule-governed' depends on the notion of productivity, such a conclusion would not be surprising. However, the discussion is not conclusive. It is not conclusive because it is still not absolutely clear that the difference between the 'clearer' cases of analogy and the 'clearest' cases of rule-governedness is actually a difference of type and not simply a difference of frequency. Also, it is not conclusive because it fails to answer the claim of someone like Becker (1990) that general rules work by exactly the same procedure as the things which others call analogies, and no distinction should be drawn in principle.

The question of type as opposed to frequency can, in one sense, be answered by definition. If we distinguish between RULE-GOVERNED INNOVATION and RULE-CHANGING INNOVATION (Chomsky 1964: 22; Botha 1968: 135) then we can say that there is a difference of type between them. Rule-governed innovation may change the experience of the individual speaker, may have an effect on the norm, but has no effect on the language system; rule-changing innovation changes the system. Rule-governed innovation is always based on an already existing pattern which is found across at least two items; rule-changing innovation is based on a pattern which is perceived for the first time at the point of innovation, and which may exist in only one model. It is thus possible to answer the questions here by definition, and say that rule-governed innovation is not a matter of analogy, while rule-changing innovation is, and that rule-changing innovation (i.e. analogy) is always creativity. This kind of distinction, and the overt link with analogy, is made by Chomsky (1964: 22). While this is a useful distinction to draw, it does not solve all the problems. In particular, it assumes that all innovation can be put into one of the two classes, although there is much innovation which we might otherwise want to call analogical but which does not appear to change the system. This is probably true of phonaesthemes, for instance, but is also true of new words which do not then form new patterns, partly because they never become part of the norm. A possible example from Tulloch (1991) is *non-ism*, where the use of a prefix and suffix with no base is rule-changing (the only possible model for it that I am aware of is *superette*, which might be seen as suffixation applied to a clipped form) and yet there is no general pattern of prefix + suffix established for the language as a whole, unless neo-classical compounds are perceived in that way.

3.12.6 Examples

Lehnert (1971) lists the words in table 3.5 which end in *-icative*. Those words derived from a verb in *-ate* present no problem for any approach, nor do those derived by a prefix. However, the words apparently derived from a nominalisation in *-cation* may. These fall into two groups: first, there are those where the verb ends in *-ify*, which regularly have nominalisations in *-ication*, and where the *-ic-* could be seen as a regular stem-extender for the verb; second, there are those like *apply* where the *-c-* is harder to motivate. We could argue that *-icative* adjectives are directly derived from *-ication* nouns, so that the source of the *-c-* in the adjective would not be in question (recall that all the *-ate* verbs listed also have *-ation*

Table 3.5: *Words ending in* -icative *(from Lehnert 1971) and correlative facts*

abdicative	-ate]$_V$		
adjudicative	-ate]$_V$		
affricative	-ate]$_N$		
applicative		-cation	
communicative	-ate]$_V$		
complicative	-ate]$_V$		
dedicative	-ate]$_V$		
duplicative	-ate]$_V$		
eradicative	-ate]$_V$		
excommunicative	-ate]$_V$		
exemplificative		-cation	
explicative	-ate]$_V$		
formicative	? -ate]$_{V/A}$		
fricative		-cation	
imbricative	-ate]$_V$		
implicative	-ate]$_V$		
incommunicative			prefix
indicative	-ate]$_V$		
induplicative			prefix
intoxicative	-ate]$_V$		
judicative		-cation	
justificative		-cation	
lubricative	-ate]$_V$		
medicative	-ate]$_V$		
modificative		-cation	
multiplicative		-cation	
nonfricative			prefix
predicative	-ate]$_V$		
prognostificative		-cation	
qualificative		-cation	
reduplicative	-ate]$_V$		
significative		-cation	
uncommunicative			prefix
vellicative	-ate]$_V$		
vindicative	-ate]$_V$		

nominalisations and stems ending in *-c*). This would be seeing the formation of these words in terms of paradigm pressure. Alternatively, we might consider that words such as *applicative* are the ones that make analogy seem like a good option. Note that there is only one such word

in the list, though this is not necessarily of overriding concern. More importantly, *application* is not a noun formed in English, but is borrowed direct from French, and is thus not subject to English analogies or rules. Accordingly, we can account for apparent irregularities in the data in terms of lexicalisation.

Tulloch (1991) lists approximately 1950 new words. The numbers of words in some of the categories exemplified in this work are given in table 3.6. It can be seen there that the clearly rule-governed morphological categories are not in an overwhelming majority. Prefixation, suffixation and compounding between them account for only 1,037 or approximately 53 per cent of the total entries. Just how much of the output of the other types listed is rule-governed may be controversial. For example, Kelly (1998) argues that blending is more regular than has generally been recognised (on blends in Hebrew, see also Bat-El 1996). Nevertheless, most of the other types are generally seen as irregular, and it follows that non-rule-governed innovation is responsible for an extremely large part of the vocabulary innovation in English (which makes Aronoff's [1976: 20] label of 'oddities' in itself an oddity). However regular the rule-governed portion of the innovation may be, there is a large amount of irregular formation of new vocabulary items, although very little of it is ex nihilo innovation: most innovation is motivated.

Table 3.6: *Numbers of new words in various categories in Tulloch (1991)*

acronym, initialism, abbreviation	111
back-formation	21
blend	82
clipping	48
compound	621
extended, specialised or metaphorical meaning for an existing form	247
loan word, calque	53
word manufacture, possible word manufacture	21
syntactic phrase used as a vocabulary item	229
prefixation	178
suffixation	238
conversion (probably under-estimated, since it is not always marked and some conversions are in second or subsequent meanings)	24

Within the words listed in Tulloch (1991), now consider those words which have the suffix *-ation*. There are eighteen such words, of which thirteen are derived from verbs in *-ise* and three are derived from verbs in *-ify*. All of this is totally regular and expected. The other two words are *deforestation* and *lambadazation*. *Deforestation* might not be a genuine case of suffixation: rather it might be derived by prefix replacement from *afforestation*. If it is derived in such a way, we have an instance of paradigm pressure allowing new formations. If it is derived by suffixation, the choice of *-ation* rather than *-ing* may be slightly surprising, but *-ation* is productive in so many places, that the form might not be particularly out of line anyway. *Lambadazation* is a very different case. There is a noun *lambada*, borrowed from the Portuguese, meaning a dance of Brazilian origin, and that has been turned into a verb by conversion. However, that verb appears to have undergone a further shift, to come to mean 'to market ethnic culture in the west', and it is from this verb that *lambadazation* is derived. But by what process? The affixation of *-ation* to *lambada* should give *lambada-ation*, the *z* is introduced – possibly on the basis of all the words in *-ization*, or a re-analysis of words in *-ization* into *i* + *zation* – to break up the sequence of vowels. I know of no parallel case. Here, then, we appear to have an instance where the majority of the observed forms are perfectly rule-governed, but where a minority appear to involve the use of some non-rule-governed process.

Even from these three minor examples (*-icative* adjectives, types of neologisms in English and *-ation* nominals) it should be clear that the dispute between supporters of analogy and supporters of rules cannot easily be settled. It seems again that both have their place, but that neither is as dominant as might appear. The discussion here can be read as implying that a neat distinction can be drawn in terms of rule-changing innovation and rule-governed innovation, with the latter simply generalising change introduced by the former. While I am comfortable with this general picture, it might still be argued that both types of innovation are fundamentally similar, and cannot easily be distinguished in practice.

3.12.7 Conclusion

The arguments for and against analogy seem to cancel each other out to a large extent. Neither side emerges from the discussion in an unassailable position.

We are sure that analogy works some of the time; it is harder to be sure that productivity ever works, although Derwing and his colleagues have argued with some vigour that regular plural formation in English is

rule-governed (Derwing 1980; Derwing and Baker 1980; Prideaux 1984) and there have also been strong arguments put forward for the rule-governed nature of other inflectional morphology (Pinker and Prince 1988, 1991; Clahsen et al. 1992; Marcus et al. 1995). Certainly the possible co-existence of two systems must be allowed for. In Bauer (1983: 296) I suggested that it could be that speakers work with analogy, but that linguists' descriptions of the output of this behaviour are in terms of rules. That option still seems to me to be a possible one. It may also be true that rule systems presuppose analogy: they must start somewhere!

In the same passage in Bauer (1983), I also suggested that describing things in terms of rules was the conservative option. If we give up on rules immediately, and say that everything is analogy, we will never discover the hidden regularities. If we try to describe things in terms of rules, we always have analogy to fall back on. Rule-governed productivity may thus be a better research strategy than analogy, even if analogy, by allowing for greater variation in output, permits the researcher to present a more accurate picture of speaker behaviour.

Becker (1990: 28–30 et passim) argues not that speakers sometimes use analogy and sometimes rules, but that rules simply are analogies, and that they are seen as rules only when they gain productivity. If this is merely a terminological game, it is of no interest. But Becker would claim that the difference between *ils sontaient* and *ils dormaient* is only a matter of degree and not a matter of type. The difference between *Vaxen* and *Vaxes* would then not be a question of analogy or rule but a question of rare or common analogy. This means that there is no distinction to be drawn between productivity and creativity, which is an interesting thought, whose ramifications deserve further consideration. To the extent that people like Van Marle are successful in showing that the two are distinct because there are implications of the division, Becker's position seems weakened. On the other hand, the arguments in favour of the distinction may not in fact be as clear as all that.

In the meantime, the rule vs. analogy debate seems unresolvable without further presuppositions, e.g. about psychological reality. In the next chapter I go on to consider such evidence.

3.13 A working definition of productivity

We are now in a rather better position to give a definition of productivity. Productivity is a feature of morphological innovation. It is a feature of morphological processes which allow for new coinages, but not all

coining necessarily indicates productivity. To be shown to be productive, coining must be repetitive in the speech community: isolated instances of coining from individuals do not in themselves necessarily indicate productivity. Various factors appear to aid productivity: type frequency of appropriate bases, phonological and semantic transparency, naturalness, etc., but these are aids to productivity, not productivity itself. Productivity can be distinguished from creativity, although it is hard to draw a consistent line between the two. It may be the case that productivity can be seen as rule-governed, and creativity seen as rule-changing and equated with the use of analogy, but this is not settled. In sum, the productivity of a morphological process is its potential for repetitive non-creative morphological coining.

3.14 Summary

The following have been introduced, defined and used as technical terms in this chapter:

- availability
- coinage
- coining
- creativity
- established
- existing word
- generalisation
- individual productivity
- innovation
- institutionalisation
- item-familiarity
- lexicalisation (phonological and semantic)
- majority process/pattern/rule/form
- monofunctionality
- neologism
- nonce word
- opacity
- phonaestheme
- playful formation
- potential profitability
- potential word
- probable word
- profitability
- rule-changing innovation
- rule-governed innovation
- rule-governedness
- semantic coherence
- societal productivity
- storage
- token frequency
- transparency
- type frequency
- uniformity

In addition, the term 'analogy' has been discussed in some detail, but no firm definition of it has been given, and neither has analogical formation been clearly distinguished from productive formation. On the basis

of this discussion, the definition of productivity given in section 3.13 has been arrived at: the PRODUCTIVITY of a morphological process is its potential for repetitive non-creative morphological coining. This definition is still provisional, and will need to be modified in the light of later discussion.

4 *Psycholinguistic evidence about productivity*

> Why is it one of the deep-set beliefs in memory research that the ideal to reach for is to store as much information as possible by using as little space as possible? . . . The point, of course, is not that this basic scientific research method is misguided, but that it may be dangerous to speculate that the human learner is designed to 'summarise' information in the same way as the scientist.
>
> Sandra (1994: 247)

4.1 Introduction

Psycholinguists are interested in morphological aspects of language behaviour because of the information that can be gleaned from it about the way in which language is processed, stored and produced. Much of the research that has been done in this area has been carried out from the point of view of research into reading, attempting to discover how readers link the text to their own linguistic competences, for instance. However, not all of this research is relevant for the purposes of this book. Here I review only two of these three aspects: storage and production.

The reasoning behind this is as follows. The comprehension of language in its written form may be complicated by different strategies of dealing with the complexities of orthographic structure that have nothing to do with the way in which meaningful units are stored in the brain. The same is true of phonological structure in dealing with the comprehension of spoken language. If it were true, for instance, that words were stored in alphabetical order in the brain, then knowledge of the first letter of the word would be an important clue to processing that word, but would not necessarily tell us a great deal about what information was actually stored. A more likely scenario is that the phonotactic and syllabic structure of a

word are important in language processing, without this telling us about the precise structure of lexical entries. Storage, on the other hand, is important for production. If we have a full-entry lexicon (as in Halle 1973) with every inflected form of every lexeme listed in the lexicon in full and no items that are not potentially free word-forms listed, then there is no possibility that morphological structures are produced by applying rules to underlying morphemes (because no such structures are stored in the brain). Thus it can be seen that what is stored may limit the possibilities for production strategies. I thus begin by discussing psycholinguistic evidence on what is stored in the brain. I go on to discuss production, which is the topic of central relevance to this book, on the basis of what we can discover about storage.

It should be noted at the outset that much of the psycholinguistic literature assumes one rather traditional approach to morphological analysis. Specifically, it is assumed that the optimal analysis of words is into linear morphemes, which are sign-like. More modern approaches to morphology such as amorphous morphology (where phonological structure is built directly to reflect certain aspects of semantic structure; see Anderson 1992) or various types of non-linear phonology (such as the type which treats reduplication as the generation of a phonological template to be filled with material from the base; see Marantz 1982) are not discussed in the literature being reported on in this chapter. Neither are approaches based on Optimality Theory built into the discussions. To some extent this is determined by the fact that so many of the experiments deal with languages such as English and German, where the morpheme construct is not obviously insufficient. To some extent it is because a fairly traditional view of morphology is presupposed by the very questions being asked. And to some extent there is undoubtedly ignorance among some psycholinguists about the current state of morphological theory (a specific example of that is provided in section 4.2.2).

Much of the time this theoretical lag is of little importance: traditional morphemes often work quite well in derivational morphology and in any case, while theorists may agree that the morpheme is an awkward concept, they do not have any generally-accepted alternative to it. Moreover, questions concerning the fundamental problem of producing one word from another are perfectly compatible with the notion of the morpheme which is being used. Nevertheless, alternative theoretical approaches might be expected and are not dealt with in the literature.

4.2 Storage

4.2.1 A research paradigm

The central question is phrased by Sandra (1994: 228) as: 'Do language users draw on their perception of morphological relations between words for storing these words in their mental lexicon?'

In order to answer this question, there is a general research paradigm within which much recent work has been carried out. In this paradigm, experimental subjects are given a task to do on the presentation of a stimulus. Typically, the task is a lexical decision task, i.e. the subject is asked to signal whether the presented stimulus is or is not a word. The stimulus is presented by a computer, and the subject responds by pressing a 'word' or a 'non-word' button, and the computer notes the reaction time. The stimulus may be presented either in aural or in visual form, depending upon the experiment. If no more than this is done, it turns out that subjects respond more quickly to frequent words than to rare words (Chialant and Caramazza 1995: 65–66). Accordingly, in more complex experiments, it is important that words used are matched for frequency, as well as other properties such as length. Frequency – at least as a perceptual category – is however not as simple as was implied in section 3.4. Perceptual frequency of a base is affected not only by the frequency of related inflectional forms (Nagy et al. 1989; Baayen et al. 1997), but also by the size (Baayen et al. 1997) or the frequency (Nagy et al. 1989) of the associated WORD-FAMILY (i.e. the set of complex words with the same base). Reaction times can also be affected by PRIMING the stimulus in some way as well as by these frequency-related variables. In priming experiments, one word is presented to the subject followed by a second word, on which the subject is expected to carry out the lexical decision task. If the same word is heard or seen this is called REPETITION PRIMING, and it tends to speed up the reaction time in the lexical decision task. Other words may also speed up reaction times; these are discussed below. The priming clearly works well if the priming word is presented immediately before the test word, but also works well if there is a longer LAG before the presentation of the related (or identical) test word, including lags of up to fifty intervening words (Stolz and Feldman 1995: 111).

4.2.2 Inflection

In general, a distinction is drawn in these experiments between what happens in inflectional and what happens in derivational morphology. As

far as English is concerned, inflectional morphology is taken to include verbal *-s*, *-ed*, *-ing* and nominal plurals, possibly comparative and superlative *-er* and *-est*. It should be noted that although this distinction appears to be taken for granted in the psycholinguistic literature, it is less obviously taken for granted in the linguistic literature (for instance, Beard [1982] argues that the English plural is derivational and Baayen et al. [1997: 866–867] show that the arguments can be generalised; Haspelmath [1996] argues that adverbial *-ly* is inflectional; and many linguists including Bauer [1988: 86], Dressler [1989] and Plank [1994] have argued that inflection and derivation are prototypical categories, with considerable deviation possible from the prototypes). Nevertheless, in reporting experiments which use this distinction I assume that the categories are meaningful.

We can distinguish between three different levels of priming. FULL PRIMING is achieved when the degree of priming is equivalent to that obtained by a repetition of the same word. PARTIAL PRIMING is obtained when a significantly lesser degree of facilitation in the lexical decision task is shown, but nonetheless significantly more facilitation than is found by prefixing the test stimulus with totally unrelated words. (The terminology is from Fowler et al. 1985: 241.) In this last case, we can talk of no priming or ZERO PRIMING. It is a regular experimental result that the use of a regularly inflected form fully primes the base word with no inflections (Stanners, Neiser, Heron and Hall 1979: 403; Fowler et al. 1985: 244). It might be thought that this effect could be due either (a) to phonological overlap or (b) to semantic overlap with the stimulus word. However, both of these alternatives have been ruled out. Slowiaczek and Pisoni (1986: 230) found that sharing of initial phonemes in semantically unrelated words did not cause priming (though they comment [1986: 235] that they did not test for priming by complete syllables). While synonyms do have a priming effect, that effect tends to dissipate rather quickly (work only with a shorter lag) in comparison with the priming effect produced by the same stem (Fowler et al. 1985: 251; Marslen-Wilson et al. 1994: 18).

If regularly-inflected word-forms of every lexeme were stored separately from each other, it would be expected that priming would be no greater than for phonologically or semantically related pairs of words. If the priming effect is caused neither by the phonological form of the word, nor by its semantics, it would seem that it must be caused by the morphemic make-up of the word. Accordingly, this set of findings is interpreted as meaning that when a regularly inflected form is produced, there is mental

access made to the base lexeme. Since the lexemes used as bases in such experiments are usually monomorphemic, this is seen as implying a morphemic level of storage, although strictly speaking that does not necessarily follow as far as the base is concerned. It does seem to imply that the inflections are stored as separate items, whose effect can be distinguished from that of the base to which they are attached.

There do not appear to be any experiments where a particular inflectional affix is shown to prime another word with the same inflectional affix. Superficially, this would seem to weaken the claim for separate storage. However, it must be assumed that the largest part of checking whether a word is legal or not goes into checking the validity of the base, so that any priming effect of affixes would be extremely small, and possibly within the margin of error for the experimental method. I thus do not consider this gap to be a particularly significant one. If there were any such effect, it would be interesting to know whether it was caused by morphemes or allomorphs, but it seems unlikely that any effect could be demonstrated.

A recent experiment (Sonnenstuhl et al. 2000) suggests that not all members of an inflectional paradigm are equally good at priming each other. For example, German *ihr lackiert* ('you [plural] paint') is less good at priming *ich lackiere* ('I paint') than *er lackiert* ('he paints'), despite the homophony of the verb forms. This doesn't detract from the basic principle, but does suggest that paradigm structure is another factor to be taken into consideration in attempting to predict the strength of the priming effect.

Stemberger and MacWhinney (1986: 18) suggest that there are also other types of evidence which point to morphemic storage. They say that if we compare monomorphemic words like *Rex* with bimorphemic words of the same phonological structure, like *wrecks*, we find (a) that the /s/ is longer when it represents a morpheme than when it does not and (b) that errors of pronunciation are less likely to be made when the /s/ represents a morpheme than when it does not. Both of these, they say, suggest that there is a separate storage of the morphemes. They also suggest that natural errors like *tell-us-ing* indicate that words are not stored as wholes, but that the individual morphemes are stored. It has also been suggested that slips of the tongue like *I want to get this cash chequed* (for: *I want to get this cheque cashed*), where the inflectional affix remains in the appropriate position for the verb and does not move into the noun slot, indicate that inflections are stored separately. Butterworth (1983: 267–269)

remains unconvinced by this last type of evidence, on the grounds that there is sometimes a switch with something which is not (synchronically) a morpheme at all, e.g. *I've got a lot of cooken chicked* (for: *I've got a lot of chicken cooked*). However, we might conclude that there were two different kinds of slip here only one of which was morphemic. Finally, using Italian materials, Chialant and Caramazza (1995) show that subjects find it harder to rule out non-words in a lexical decision task if those non-words contain actual inflectional affixes than if they contain no morpheme-like sequences. This finding seems to imply that the inflectional morphemes are recognised even in non-familiar contexts, and helps to plug the gap identified above in the priming paradigm.

All these conclusions are based on what happens in cases of regular inflection. What happens in cases of irregular inflection is rather different. Irregularly inflected forms only partially prime their bases, not fully (Stanners, Neiser, Heron and Hall 1979: 405). This is interpreted as meaning that irregularly inflected forms are stored separately from other forms of the lexeme which they realise. This point of view is also well established among descriptive linguists, who point out that a form like *was* is not predictable by any generalisation from the form of the stem *be*, and thus must be listed as a separate fundamental irregularity. It is thus disconcerting to find Fowler et al. (1985: 250) noting that 'we found that visually presented irregular words do prime their unaffixed relatives fully [. . . with] very similar priming in the two modalities [i.e. visual vs. aural presentation, LB]'. (For similar results in Italian, see Orsolini and Marslen-Wilson 1997.) Unfortunately, this is not as clear as it might be, since although the priming words include some irregular inflections such as *fell*, *slept* and *risen*, they include rather more 'irregular' derivatives such as *clarify*, *health* and *wisdom*; it is not clear whether the inflectional instances were in any way different from the derivational ones. While we might wish to say no more than that this case is one which is currently undecided, we can note that there is at least some evidence that irregular inflected forms do not prime regular stems as easily as regular ones.

Evidence of a different kind, however, may seem more conclusive. On the basis of the behaviour of aphasic patients with acquired neurological damage, Marslen-Wilson and Tyler (1997) say that some patterns of brain damage appear to affect the priming effect of regular inflectional morphology (past-tense marking in their experiment) while leaving the effect of irregular morphology intact; a contrasting pattern of brain damage has just the opposite effect. They argue (1997: 593) that this shows that

'regular and irregular past tenses are neurologically dissociable' and that 'the two morphological categories ally themselves with different types of mental computation'.

There is also a certain amount of evidence that token-frequent forms are not stored in the same way as token-infrequent forms. Stemberger and MacWhinney (1986) report, for instance, that there are fewer errors made in producing high frequency inflected forms than in producing low frequency inflected forms, either in natural conversation or in an experimental situation. They suggest that this implies that high frequency forms are stored as wholes, while low frequency forms are generated as required. They point out (1986: 23) that this seems to imply that high frequency forms are *both* stored *and* analysed, since both high and low frequency regularly inflected forms fully prime their bases.

Evidence of a very different kind is presented by Jaeger et al. (1996). They present the result of a positron emission tomographic study, where blood flows in the brain were observed during the production of regular and irregular past tense forms of verbs. Overlapping but different areas of the brain were activated for the production of regular past tense forms and irregular past tense forms. Reaction times were also longer for irregular past tense forms. It is concluded that different mechanisms are involved in the production of regular and irregular past tense forms. This is presented as evidence against the connectionist model (see section 3.12.4) in which a single process accounts for both regular and irregular inflectional forms. The experimental method has been attacked by Seidenberg and Hoeffner (1998). They argue that the results presented by Jaeger et al. (1996) derive from the fact that all the regular forms were presented to subjects in a single list, and then the same thing was done with the irregular forms. While the regular forms all work in the same way, and subjects could thus predict the process they would have to apply, the irregular ones consist of several distinct patterns, and require constant vigilance on the part of the subjects. Although Jaeger et al. (1998: 124) attempt to deny Seidenberg and Hoeffner's argument here, saying that 'there is no evidence' for their contentions, it seems to me that the point is well made, and in the short term at least casts doubt on the interpretation of the original experimental results. Indefrey (2000), looking at results over several similar experiments, concludes that although each individual experiment finds differences between regular and irregular morphology, when the results from the different experiments are aggregated they cancel each other out. Furthermore, he says that when more conservative statistical

measures are used, the reported differences between irregular and regular morphology mostly disappear. Thus, while the results provided by Jaeger et al. (1996) are interesting, it would be premature to conclude too much from them.

In a wider framework, any evidence which points to the analysability of word-forms and the separate storage of morphemes might be interpreted as evidence against Anderson's model of amorphous morphology (Anderson 1992). In that model – as in traditional grammar – word-forms rather than morphemes are basic, and the phonological shape of a word-form is seen to derive from semantically-instigated phonological operations on stems. Morphemes as such do not exist, and thus cannot be stored. But seeing these two as being in direct conflict would be an over-hasty conclusion. Amorphous morphology, like morphemic morphology, allows for the generation of word-forms from stems, and thus requires the listing of a stem (or set of stems). The difference between the two models is that while in the morphemic model the presentation of a word-form like *flicked* primes the morpheme {flick}, in the amorphous morphology model the presentation of a word like *flicked* primes the stem *flick* which is associated with the lexeme FLICK. The two are compatible in terms of the priming experimental method. Where the two differ is with regard to the distinction between regular and irregular inflected forms. It is expected within the morphemic model that these will be dealt with differently (see the discussion above, and much of the anti-connectionist literature discussed in section 3.12.4). In the amorphous framework, an irregular form like *stuck* is simply another stem of the lexeme STICK (Anderson 1992: 132–133), and there should be no difference in the look-up for the stem *stick* and the stem *stuck*. Thus Anderson's predictions are in line with those made by the connectionists, but for very different reasons. Psycholinguistic evidence in favour of a distinct treatment of regulars and irregulars can therefore be seen as evidence against the amorphous model.

4.2.3 Derivation

Within derivation, we need to distinguish between prefixation and suffixation and between various degrees of regularity and transparency. At the most transparent end of the scale, there are those cases of affixes being added to bases with at most a certain amount of resyllabification required in the phonology (see table 3.2), and where the semantic analysis of the new derivative is (relatively speaking) compositional. Since many

psycholinguistic experiments are done in the visual modality (i.e. using the written language) the phonological transparency of such cases is frequently reinterpreted in terms of orthographic transparency. At a less transparent level, there are those cases where the affix is a Level I affix (or, in a slightly different terminology, has a +-boundary) and where there are concomitant changes of stress pattern or important allomorphic changes (including allomorphy arising from the historical changes brought about by the vowel shift, consonantal assimilations and so on). In terms of table 3.2, this can be seen as a merger of Levels IV, V and VI of that table. The semantic analysis of such combinations is frequently rather less compositional. Finally, at a third level of transparency/opacity there are those instances such as *transmit*, which are etymologically analysable, but which may or may not be synchronically analysable in English. Aronoff (1976: 10–14) argues that there are morphemes involved in such cases, and this analysis is frequently adopted without it being clear that those using it are aware of its controversial status (for a statement that such words are no longer analysable, see Marchand 1969: 5–6).

If we begin at the most opaque end of this scale, there is evidence that etymologically analysable words which have no clear synchronic semantic motivation do not prime bases (if these occur) or other members of the same paradigm (Marslen-Wilson et al. 1994: 24). For instance, *conceive* does not prime *receive*, and the implication is that there is no lexical access to a root morpheme {ceive} in such cases. This is probably not very surprising, given the fairly standard acknowledgement that a morpheme involves both a formal side and a semantic side (see e.g. Bauer 1988: 109). Marslen-Wilson et al. (1994: 27) conclude that 'The average listener has no access to the diachronic history of a word and will only mentally represent it as morphologically complex if this gives the right compositional semantics.' Such a conclusion is compatible with other types of experiment carried out by Derwing (1976), for example, where it was found that subjects would agree that a derived form 'came from' another simpler form only if there was formal and semantic overlap (see also Derwing and Baker 1979: 222).

Where the affixes concerned are transparently attached to their bases, a derivative will prime its base. This is true for prefixes (Stanners, Neiser and Painton 1979: 741; Marslen-Wilson et al. 1994: 21) and for suffixes (Fowler et al. 1985: 245; Sandra 1994: 240; although Stanners, Neiser, Heron and Hall [1979: 410] find partial priming rather than full priming). A word made up of a prefix and a transparent base will prime a word

made up of the same transparent base and a suffix or vice versa (Marslen-Wilson et al. 1994: 26) but one suffixed derivative will not necessarily prime another suffixed derivative with the same base (e.g. *manager*, *management*) (Marslen-Wilson et al. [1994: 26] say they do not, though Fowler et al. [1985: 250] find that such pairs do prime each other, but more clearly in the spoken form than in the written). Marslen-Wilson et al. (1994: 19) interpret this as meaning that competing suffixes inhibit each other, while prefix and suffix are not in competition; given the common coining of new words precisely in contexts where the suffixes are being contrasted (see the examples cited in section 3.11) this proposal may not seem terribly convincing, though some justification for the observed difference in behaviour is required. Marslen-Wilson et al. (1994: 17) find both that a derivative can be primed by its base and that the base can be primed by the derivative. The implication of these findings is that transparent derivatives, like inflections, are stored in analysed form, i.e. morphemically.

Perhaps surprisingly, words containing Level I (+-boundary) affixes, which are phonologically less transparent than those dealt with above, also seem to prime their bases, though some researchers find partial rather than full priming in such cases. Stanners, Neiser, Heron and Hall (1979), who find partial priming in fully transparent cases, also find partial priming in these rather less transparent cases; Fowler et al. (1985) found full priming in both cases once their experimental design was modified to avoid the effects of what they refer to as 'episodic' factors such as the result of remembering having seen the particular word in the same experimental situation.

These results are interpreted as indicating that there is little likelihood of there being different ways of processing transparent and less transparent derivatives, since similar priming effects are achieved in both cases. However, words which are related etymologically but which are no longer semantically related appear to be stored independently of each other. Stanners, Neiser, Heron and Hall (1979) conclude on the basis of partial priming by derivatives that derivatives might be separately listed in the lexicon, though possibly with some link back to the base lexeme. In the light of the findings of Fowler et al. (1985) it is less clear that such a model can be supported, although it would make perfectly good sense if (at least some) derivatives were both stored and analysed (like frequent inflected forms).

I have seen no work on potential differences between frequent and rare derivatives, parallel to the work by Stemberger and MacWhinney on

frequent and rare inflected forms. However, Baayen and Neijt (1997) do find differences in meaning between high and low frequency derivatives using the Dutch suffix *-heid* '-ness', and this suggests that such parallels could be drawn.

4.2.4 Compounding

Sandra (1990) finds that either element of a transparent compound like *milk-bottle* can be primed by a semantically related word (say *cow* for the first element and *flask* for the second) in a short-lag experiment. However, opaque compounds (such as *buttercup*, where the meaning is not easily derivable from the meanings of the parts) or pseudocompounds (like *boycott*, where there is no free word *cott*) cannot be primed in the same way. He suggests (1990: 543) that such words have their own independent lexical representation, and that there is no analysis into morphemes in such cases, though there is in the case of transparent compounds.

Zwitserlood (1994) tries using compounds to prime one or the other of their elements. She finds that both transparent and opaque compounds prime either of the elements, with priming of the second element being stronger than priming of the first element. That this is not simply due to the repetition of form is suggested by the fact that apparent words which appear in the compounds but which are not genuine compound elements are not primed. For example, Dutch *kers* 'cherry' is not primed by *kerst·boom* 'christmas tree'. In a second experiment, Zwitserlood (1994) finds that transparent and partly opaque compounds prime a word which is semantically related to one of the elements, but that fully opaque compounds do not. Zwitserlood (1994: 364) interprets these results as implying that even partly opaque compounds are stored lexically as a function of their elements, but that opaque compounds have a listed semantic representation separate from that of the elements involved. This is largely compatible with Andrews's (1986) conclusion that compounds are optionally accessed through their elements.

Libben (1998) found an aphasic patient who showed a tendency to interpret even opaque compounds as though they were composed of their constituent parts. In another set of experiments, Libben et al. (1999) find that in compounds with ambiguous parsing such as *clamprod* (which might be *clam* + *prod* or *clamp* + *rod*), all possible parses are activated and then evaluated. This is compatible with Sandra's results if we assume that impossible parses decay quickly and soon become unavailable, while an accepted parse into morphemes remains available.

4.2.5 General discussion

To the extent that generalisations can be made over all this data, it seems to be a generalisation in favour of some kind of morphemic underliers being accessed for complex words. For example, in a conclusion that can be taken to apply to all types of morphology and not just the derivation that is discussed in their own experiments, Marslen-Wilson et al. (1994: 31) make the following statement:

> At the beginning of this article we asked, what is the basic unit in terms of which the lexicon is organized? Our answer, for derivational forms in English, is clearly the morpheme. This should be understood, however, as a cognitive, or psycholinguistic, concept of the morpheme . . . This cognitive morpheme does not include all entities definable as morphemes on linguistic and diachronic grounds . . .

Given the theoretical problems associated with morphemes at the present time (see e.g. Bauer 1999c), such a cognitive morpheme does not seem as theoretically suspect as might appear.

In transparent cases, such a result may not be very surprising, although Butterworth (1983) resists it strongly. In opaque cases, it seems much more surprising, and the evidence is rather less clear-cut. Marslen-Wilson et al. (1994: 27) may be able to interpret their evidence as showing 'that semantically opaque, morphologically complex words in English are represented as morphologically simple at the level of the lexical entry' and such a conclusion might have good theoretical support, but it is not supported as a general conclusion by all the psycholinguistic data. It may well be the case that there is a separate semantic listing for opaque complex words as well as some acknowledgement of their complex structure, and that it is this mixture of morphological analysis and semantic listing which causes the fuzziness in the results. Such a conclusion would be a theoretically welcome one, as well as a psycholinguistically justifiable one.

It must be remembered that these experiments are all attempts to find out what is actually happening in the brain. Sandra (1994: 244) warns that 'The existence of a logical system in the language does not necessarily imply that the mental structures that develop for dealing with this system will reflect this logic.' While it is well-taken as a general point, this should not be relevant here since the probes are designed to find out about the mental structures rather than about the logical (linguistic) system. And while it is no doubt true, as Smith (1995: 369) says, that 'individual speakers of a particular language probably do not internalize their morphemic knowledge in the same way' (there is plenty of anecdotal

evidence to support this view), the experiments that have been reported on here use large numbers of individual speakers, and are presumed to provide some kind of general picture of how real speakers work.

Sandra (1990: 554) comes to the conclusion that some of his experiments suggest 'that transparent compounds may have no representations in the lexicon because their meaning can be determined each time anew from the constituent meaning'. This bears a striking similarity to Aronoff's (1976: 45) conclusion that forms in *-ness* are not listed (and therefore cannot be blocked) because they are so productive. Given such an interesting coincidence of views, it is unfortunate that the psycholinguistic literature virtually never considers productivity rather than transparency as a possible source for different kinds of lexical behaviour. Chialant and Caramazza (1995: 65) specifically mention productivity as a factor which could be relevant in determining how a word is represented in the lexicon, but they do not follow this up in their paper, though in a later paper Badecker and Caramazza (1998: 400) say that 'productivity may be the determining factor with regard to whether morphologically complex forms are composed in the processing system'. Accordingly, of course, many of the instances of transparent morphology used in experiments are split between productive and non-productive uses of affixes. Given the possibility of finding productive and non-productive incidences of the same affix, it might be the case that this would provide a fruitful line of approach for psycholinguistic experimentation.

4.3 Production and comprehension

The literature on the production of morphologically complex forms is not as theoretically consistent or as coherent as the literature on lexical storage. Accordingly, what we have to deal with here is a very mixed collection of observations about the way in which real speakers (sometimes learners, sometimes presumed competent speakers, occasionally pathological speakers) behave in the production of morphologically complex forms, and deductions from those observations. The observations are interesting, but do not necessarily provide a totally coherent picture.

Inextricably linked with the production of morphological forms is the comprehension of morphological forms. It can be argued that understanding a new morphological construction involves an understanding of the structures and elements involved in the same way that production does. Accordingly evidence from comprehension will also be considered here.

4.3.1 Inflection

The most thorough examination of the production of an inflectional process of which I am aware is the series of experiments undertaken by Derwing and his colleagues following the pioneering work by Berko Gleason (1958), and summarised in Prideaux (1984: 76–82). They consider a number of English inflectional processes, but in particular the formation of regular plurals. They outline six hypotheses (not necessarily an exhaustive list of possibilities) about how speakers form the plural.

(a) The plural could be learned separately for each individual noun; this is termed the List analysis.
(b) The plural could be learned separately for each individual stem, so that only one act of learning would be involved for *supposition* and *presupposition*, for example; this is termed the Whole Stem analysis.
(c) The plural could be predicted on the basis of the rhyme in the final syllable of the noun; this is termed the Rhyme analysis.
(d) The form of the plural could be determined by phonotactic rules which do not permit sequences such as /kætz/ or /hɔːsz/ and so force adjustments to the string; this is termed the Phonotactic analysis.
(e) The form of the plural could be determined by the last segment of the noun; this is termed the Segmental analysis.
(f) The form of the plural could be determined by the featural analysis of the final segment of the noun; this is termed the Feature analysis.

In Berko Gleason's original experiment, a cartoon creature was given a nonsense name (such as *wug*) and then two such creatures were shown to children, who were told 'Now there are two of them. There are two __'. The children would produce the expected form *wugs* /wʌgz/ with no difficulty. This use of a plural form for a noun which has never previously been met appears to exclude the List hypothesis. More recent experiments, including those by Derwing and Baker (1979, 1980) appear to exclude all of the first five hypotheses above. For instance, by including nonsense words for which no rhyming word exists in their set of cues, they are able to exclude the Rhyme hypothesis. As a result, they conclude that hypothesis (f), the Feature analysis, must be the one speakers of English actually operate with. The implication, of course, is that speakers can operate with productive rules in the formation of regular inflectional forms.

In the course of this work quite a lot of information on the acquisition of English inflectional forms has emerged. For instance, the acquisition of the /ɪz/ allomorph of the regular plural is acquired later than the other allomorphs, and the homophonous third person singular of the present tense is acquired later than the regular plural. Whatever the reasons for this, and however interesting the observations might be, this does not conflict with the fundamental claim about the rule-governedness of regular inflectional processes, which is the important factor for present purposes.

4.3.2 Derivation

There is plenty of evidence that people do not find the production of new derivatives as simple and automatic as they appear to find the production of new inflectional forms (see Bauer [1996], material from which is repeated here).

First, consider some examples of what happens in the later stages of language acquisition. Aitchison (1994) reports that there is a big increase in English speakers' ability to coin new words in the adult fashion during the early teenage years. We can relate this to some version of what Marchman and Bates (1994) term the 'critical mass hypothesis': acquirers need a large number of words at their command before they can generalise over them. My own files show examples of words coined by children in this age-group which appear incorrect by adult norms (however effective they might be). For instance, a fourteen-year-old, reporting on a planned excursion from school in order to gain 'work experience', said 'You can choose what you want to work-experi-ize (/ɪkspiəriaɪz/)'. The same child, when playing Monopoly, was heard to say 'You are doing better financially but I am doing better propertarially [i.e. "in terms of properties"].' It appears that even at this relatively late stage in the acquisition of their native language, children have not fully acquired the ability to make new words according to community norms. A similar type of observation has been made with reference to Flemish. In the formation of synthetic compounds in Flemish, even twelve-year-olds have been observed to retain the ontogenetically earlier pattern of verb + noun rather than the adult pattern of noun + verb + *-er*, e.g. *mix-soep* for a machine that mixes soup instead of the preferred adult form *soepmixer* (Smeds 1979, cited in Clark 1993: 153).

It is not only children who may have such difficulties, either. Recall the example cited in section 3.10.5 of how much difficulty one group of native speakers had in coining *chairhood* (Churma 1987). The implication

of this example is that the productivity of morphological processes is not necessarily automatic, even for presumed competent speakers of the language concerned.

There is even evidence that – at least in some languages at some times – speakers choose to avoid making new words. Consider the situation in Mohawk, for instance. Mohawk is an Iroquoian language spoken by about 1,000 people in New York State, Ontario and Quebec (Rudes 1994). It is a polysynthetic language. In general, polysynthetic languages have a lot of productive morphology (Fortescue [1994: 2601] is rather more tentative and merely says that 'such languages do presuppose a certain degree of productivity in their morphology'), and learners of such languages are frequently frustrated by the fact that it is rarely possible to look up an attested word in a dictionary. Superficially, Mohawk is no exception to these generalisations. Yet if learners use affixes productively in Mohawk, they are apt to be told by Mohawk speakers that 'No one ever said it that way before' (Marianne Mithun, personal communication). Making up a new word – at least at the derivational end of any inflectional/derivational cline – is a conscious procedure, and one which is carried out by people with prestige in the community on special occasions, not as a regular thing (Marianne Mithun, personal communication).

There can equally be problems with comprehension. Clark and Berman (1984) report that children generally show monotonic improvement in interpreting Hebrew nonce derivatives with age. However, despite the ability of very young children to deal with the transfixal nature of Hebrew morphology, even eleven-year-olds have difficulty in comprehending in context nonce agentive words of the form *CaCaC* (parallel to established words such as *ganav* 'thief', *balaš* 'detective'). Yet this is one of 'the commonest devices for new agent nouns' (1984: 551) in the adult population. Part of the difficulty may be that such forms are homophonous with past tense forms (*ganav* 'stole', *balaš* 'pursued'), but if this does not cause problems for adults, we would not expect to find it causing problems for eleven-year-olds.

Although it does not illustrate lack of comprehension of the resultant derivatives, a rather interesting experiment which appears to show lack of appreciation of morphological structure is described by Wheeler and Schumsky (1980). In this experiment, respondents were asked fairly overtly to divide a base from its final affix, the experiment being carried out both in written and in oral form. In most cases consultants were able to perform the task in accordance with the expectations of the experimenters,

but with suffixes like *-ship* and *-dom* and *-er*, the suffix is unrecognised in a large proportion of responses. It is not possible to work out precise percentages, or even total numbers of responses, from the data Wheeler and Schumsky give, because of a number of alternative or irrelevant answers, but Wheeler and Schumsky (1980: 11) say:

> For example, *dom* is chosen as the suffix of *kingdom* in 19 written responses and 11 oral responses, but the word is said to have no suffix in 14 written responses and 3 oral responses. For a few of these words, 'no suffix' is actually the majority response in one or both experiments. *Baker*, for example, has 18 'no suffix' responses and only 11 *er* responses in the written experiment.

All of this evidence can be interpreted as showing that real speakers do not have the automatic ability to manipulate morphological structure in derivatives, that derivation is, in some sense, 'hard' for speakers to do. Conflicting with this evidence, there is also some which implies that the formation of new words by derivation is not a particularly difficult task.

For instance, there is an immense amount of evidence that children use derivational morphology innovatively from an early age. MacWhinney (1976: 402) lists a number of derivational affixes of Hungarian that are employed by children learning Hungarian natively from as early as one year eight months in age. Clark and Hecht (1982) report English-speaking children using agentives in *-er* from the age of three years, and the process being almost fully established by the age of six years. We must assume that the children understand these forms before they start using them, but the experiments reported in this paper do not consider the competence of younger children. In a parallel study, children learning Hebrew had difficulty in producing agent nouns at the age of three, but could manage them by the age of four (though, on the other side of the coin, they had difficulty with instrument nouns until the age of eleven) (Clark and Berman 1984). Dressler and Karpf (1995: 105–107) give evidence of diminutive morphology emerging extremely early in child language acquisition in German, Russian, Czech, Polish and Hebrew. While it is clear that not all of derivational morphology is necessarily easy, there is at least some of it which seems to match inflectional morphology in terms of how early and how automatically it is produced. Indeed, Dressler and Karpf (1995: 107) specifically comment that only two inflectional markers are in general use by the time the diminutive appears in the language of the Hebrew child studied.

Other types of evidence are less clear and less convincing. For example, Cutler (1980: 46–47) reports asking a group of 'unpaid volunteer subjects drawn from the University of Sussex community' to produce neologisms, and not only does she not comment that the task provided any particular difficulty, her figures suggest that all subjects were able to do this for all the bases they were asked about. The task is simply assumed to be a possible, indeed a fairly trivial, one. It may be that this assumption is misleading, but the success of the experiment (which was considering how derivatives were formed rather than whether they were formed) suggests otherwise. Clark and Clark (1977: 285) cite a slip of the tongue which gave *peculiaracy* for *peculiarity*, and suggest that the wrong nominalisation suffix has been produced on line, and that this suggests that the nominalisation ending is separately listed and produced. However, consideration of a transcription rather than an orthographic rendition of the two words (/pəkjulıærəsi/ for /pəkjulıærıti/) suggests that the slip is better seen as a substitution of /s/ for /t/ (that is, failure to make a complete closure for the stop, not an uncommon phenomenon in fast speech) rather than replacement of one affix by another. Real instances of affix replacement are apparently rare (Butterworth 1989: 115).

For derivation, therefore, there is conflicting evidence about production: some of it seems hard, some of it seems fairly automatic. There are at least two possible conclusions that might be drawn on the basis of such a distinction. The first is that there are two different kinds of derivative, formed in different ways, perhaps used in different ways. The second is that there is only one kind of derivation, but some of it is more productive (in some vague, unrefined sense) than other parts, and that the apparent difficulty of some derivation is a direct reflection of its lack of productivity. This could be rephrased by saying that it is not clear whether low productivity is the cause or the effect of the observed phenomena.

4.3.3 Compounding

The best evidence for the automatic productivity of compounding in a language like English or German is the amount of it that goes on (see the discussion in section 3.2.1) and how invisible it remains. Readers of the press are not struck every couple of lines by the fact that there is a new word which they have never met before. While the claim that there are so many new words might be controversial for English, it is far less so for German, but the same general comment applies. Clearly this is not a universal statement. Hebrew has far fewer compounds, and because of

the amount of phonological sandhi that takes place between the elements of compounds (Glinert 1989: 440–441) they are likely to be far more striking. Other languages (such as Hixkaryana and West Greenlandic) are sometimes claimed not to have compounds (see Bauer in preparation), and there compounding cannot be automatically productive.

Where compounding is common, though, it is also acquired very early. The earliest agentive nouns coined in English tend to be compounds (Clark and Hecht 1982), and tend to yield later to standard derivatives. Clark (1993: 146) reports for one English-speaking child that all of his innovative words were compounds before the age of two, and between the age of two and five some 70 per cent were compounds. She reports similar figures for Dutch-speaking children (1993: 153), German-speaking children (1993: 154) and Swedish-speaking children (1993: 158). The figures for Hungarian are much lower, with just over 40 per cent of innovative nouns being compounds (Clark 1993: 171). As we would expect, Hebrew-speaking children make almost no use of compounds in coining new forms (Clark 1993: 175). Clark (1993: 162) even attributes the relatively late onset of coining of novel nouns in French-speaking children to the fact that in that language neither compounding nor conversion is particularly productive.

The picture here, therefore, is of variation between languages, but with compounding very easily accessible in those languages which use it widely. Unfortunately, this picture is, to a certain extent, contradicted by a celebrated set of experiments reported by Gleitman and Gleitman (1970). They showed that less educated consultants were less able to give grammatically appropriate paraphrases of three-term nominal compounds than were consultants with PhDs. The following passage (from Gleitman and Gleitman [1979: 109], in which they provide a later discussion of their experiment) shows the kind of error that was made when consultants were asked about the compound *house-bird glass*:

> We can assume that every speaker of English, approximately, knows how to use *glass* both adjectivally (*a glass house*) and nominally (*a piece of glass*; *a glass to drink from*). Yet the less educated subjects often interpreted *house-bird glass* as *glass house-bird*, *a house-bird made of glass*, or even as *glass bird-house*. Why not *glass used to make a house-bird* or *the glass used by a housebird*, solutions which simultaneously resolve the semantic and syntactic properties of the stimulus item?

Gleitman and Gleitman attribute the difference between the two groups to what each group of respondents focused on: the more educated group

focused on the syntax, the less educated on the meaning. The differences were certainly deeply ingrained. In an interesting aside, Gleitman and Gleitman (1979: 108 fn) note that

> It is of some interest that we could find no simple means to teach the clerical [less well-educated] group to perform as the Ph.D. group performed. For instance, clerical workers listened to the stimuli over and over again, with feed-back as to correct choices and a financial reward for each correct choice made. Finally, their performance for a list of 72 stimuli came close to that of the uninstructed Ph.D. group. Then both groups were given a new, but closely equivalent, list of stimulus phrases from which to choose. Now the disparities in performance for the two groups appeared again, and in the same measure. Thus there is no easy way around the fact that these populations differed in their approach to paraphrasing.

Ryder (1994) also found cases in which speakers interpreted noun–noun compounds as though they were left-headed, so that *quilt-horse* was interpreted by one consultant as 'a quilt made of horse-hair'. Ryder (1994: 199) attributes this to an individual 'style' of interpretation. She also notes that one subject was not able to find interpretations for novel compounds.

On the other hand, Perry (1997) found that seven-year-old children were perfectly capable of explaining why fingerpainting was so-called in terms of its being a kind of painting that involved using the fingers: in a small-scale experiment she gained a 75 per cent level of appropriate responses to this item. While other items did not score so well, there is some suggestion that improved experimental method is required to extract the required information rather than improved understanding on the part of the children.

The interpretation of these observations is extremely awkward, especially as it is so reminiscent of the deficit controversy of the early 1970s. If compounding is automatic and productive, it ought to be available to all speakers, independent of educational achievement. If it is not widely available in this way, how can speakers' willingness to coin compounds (from an early age) and accept them in speaking and writing be accounted for?

4.3.4 Morphophonemics

There is quite a large body of literature concerning experimental approaches to English morphophonemics, including in particular alternations like those in (1) that arose because of the effects of the English Great Vowel Shift.

(1)

a.	sane	sanity
b.	obscene	obscenity
c.	profound	profundity
d.	divine	divinity
e.	cone	conical

The results of these experiments can be summarised extremely rapidly: English speakers do not produce such alternations spontaneously, and where they do produce vowel alternations, these seem rather to be determined by the spelling than any phonological or morphological structure of the words concerned.

Jaeger (1986) and Wang and Derwing (1986) provide an excellent summary of the experimental evidence that has been accumulated on this point. As well as experiments relating directly to production, there have been tests based on preferences speakers show for various alternations, tests based on recall of items – some of which show the alternations illustrated in (1) while others show a variety of other alternations – and concept formation tests. In a concept formation test, subjects are given pairs of words and then (after a brief pause) told whether they do or do not form part of the concept they are trying to delimit. Eventually, the subjects are able to predict whether or not a new pair will be a member of the appropriate set or not. Subjects are then tested on a new set of stimuli, typically including pairs which are controversial members of the set, to see whether they believe them to be part of the same 'concept' or not. All of these experimental findings agree that while (1a), (1b), (1d) and (1e) may be part of a unified concept, recognised as good alternations in word-formation and even produced in some appropriate circumstances when neologisms are called for, (1c) is not seen as being part of the same set. The major way in which (1c) differs from the others is that in all the other cases a single letter of the alphabet has two distinct phonemic realisations, while in (1c) there is no orthographic unity to the alternations. The general conclusion reached by scholars such as Jaeger, and Wang and Derwing, therefore, is that there is no living phonological rule of vowel shift in current English, but that there is a recognition of orthographic patterns. Jaeger (1986: 76) concludes that 'Humans prefer to memorise as much as possible and have rule-governed regularities be transparent.'

Similar conclusions derive from studies of the so-called rule of velar softening. Velar softening produces the alternations illustrated in (2), in which velar consonants alternate with non-back consonants:

(2)

public	publicity
demagogue	demagogic

Ohala (1974: 370–371) reports on an experiment in which subjects were asked to say the word and then say what they thought it meant, while the word was presented as e.g. *normal* + *hood*. Where the combination of stem plus affix would (if there was a rule of velar softening) have given rise to velar softening, as for instance in *toxic* + *ism*, 48 out of 78 responses retained the velar plosive, and only 20 responses had the expected non-back consonant.

It is interesting in this context to find McCawley (1986: 30) saying that 'velar softening in English is completely lexicalised and non-automatic'. This 'lexicalised' rule has to be contrasted with the 'productive' rule of allomorphy for the plural marker which, as was seen in section 4.3.1, can be argued to be used by speakers, even children acquiring English. The distinction might be one between derivational morphology and inflectional morphology; but it could equally – and perhaps more convincingly – be taken to be a difference between a productive and a non-productive rule. More complex morphophonemic alternations are found in productive morphology elsewhere. For instance, in Zulu (Doke 1927: 74–75), bilabial consonants are replaced by palatal consonants in the presence of a following diminutive affix, sometimes with some voicing adjustment, as in (3). Despite its complexity, this process is productive, and the morphophonemic accompaniment is also productive. English velar softening is trivial by comparison, and was once automatic (although the phonetic reasons for its presence have now gone in modern English), but it is now not productive. Productive morphophonemics is a concomitant of productive morphology.

(3) **Zulu:**

Base noun	**Gloss**	**Diminutive**
iːkopi	'tin mug'	iːkoʧana
intaba	'hill'	intaʧana
intamo	'neck'	intaɲana

4.3.5 General discussion

It is difficult to see a coherent picture emerging from the various pieces of evidence that have been presented on production and comprehension here. Inflection is used innovatively from an early age, and some kinds of word-formation (varying from language to language, but including both compounding and derivation where appropriate to the language being

learnt) are also used to coin new words early in the acquisition process. On the other hand, some derivation is hard to produce, both for children acquiring the language and for adults; some derivatives (even of the same types that are produced early) are apparently not recognised as such, and the comprehension of many compounds appears to be determined by educational achievement.

Let us begin with the notion that some inflection, some derivation and some compounding is used to coin words that have never been heard before from an early age. This suggests that some morphological processes are stored in the brain independently of the words in which they occur (this is without taking any position on how such processes might be stored). Bases of appropriate types must be recognisable and must be used as such. Appropriate affixes can be added, and appropriate morphophonemic changes can be made. None of this should be surprising. Given that there are languages like Archi which have, in principle, over 1.5 million forms for any verb (Kibrik 1998: 467), there would be an incredible burden on memory if all the forms had to be listed independently, and creating the forms as required seems the only viable alternative. However, as a rider on this, it should be noted that it appears that speakers' ability and willingness to do this is subject to a great amount of individual variation: in an experimental situation Maratsos et al. (1987: 110) found that children varied tremendously in the rate at which they spontaneously produced innovative causative constructions in an environment which had been set up to call them forth.

It does not follow that familiar words are consciously analysed by speakers. It is perfectly possible to use a word like *baker* (Wheeler and Schumsky 1980) or *orange juice* (Derwing 1973: 124 fn 2) in a totally appropriate way without recognising their relationship with *bake* or *orange* respectively. These words may not be lexicalised, but they are established, and are probably learnt as the names for particular entities and without reference to the morphological build-up of the words. We can interpret this in the same way as the results in Stemberger and MacWhinney (1986) are interpreted: some frequent lexemes are stored as wholes, even if they are morphologically analysable. This suggestion would make theoretical sense, would explain some of the experimental findings and anecdotal evidence, and would make sense of the overwhelming impression that in general words are items which speakers know rather than items which speakers create.

The finding from Gleitman and Gleitman (1970) remains to be explained, however. It seems to me that the point that needs to be made about these experiments is that they did not necessarily tap into actual linguistic behaviour. In general, compounds are produced and understood as part of a wider linguistic context. The Gleitman and Gleitman experiments forced reactions which were divorced from any context. In context, the listener knows whether the item under discussion is a bird, a house or a glass, or can deduce it from material other than the ordering of elements in the compound. Aitchison and Lewis (1995) show, for example, that 80 per cent of attestations of the word *wimp* from a journalistic corpus contain information on the meaning of the word in the immediate context. Baayen and Neijt (1997) refer to this phenomenon as CONTEXTUAL ANCHORING. When the compound is presented in isolation, such contextual anchoring is absent, and the test becomes one of finding a set of circumstances in which it would be appropriate to use the compound to describe the prevailing state of affairs. This is a different, and in many ways much harder task. There is good evidence that speakers have difficulty in doing this for sentences. For instance, in one set of experiments, only 89 per cent of responses indicated that *Neither he nor I knew the answer* was definitely an acceptable sentence of English, and only 95 per cent clearly accepted the unobjectionable *You painted your fence blue* (Quirk and Svartvik 1966: 107). In the same set of experiments, only 41 per cent said that *He chose his son a wife* was a clearly acceptable sentence. This last sentence is presumably rated so low because it is culturally inappropriate for most of the respondents (students at the University of London). If we view the failure to interpret *house-bird glass* appropriately in the same light, then we can say that many listeners may not have had a referent for a glass for a bird typically associated with a house, or be so unused to *glass* in the sense of 'mirror' that they did not conceive of a budgerigar's mirror as fitting the description (after all, it is usually called a *mirror*). Out of context and under pressure, it can be hard to work out what must be involved and under what circumstances the description would be accurate. If this point of view is accepted, the test was not a test of the availability of compounding processes, but a test of ability to produce unlikely paraphrases under pressure without any suitable context, and it is perhaps less inexplicable that PhD candidates should perform better than clerical staff, if only because they are more experienced at dealing with strange tests of this kind.

4.4 Summary

In this chapter a number of experiments and observations aimed at discovering the way in which complex words are stored and produced by real speakers have been examined. The most recent evidence seems to suggest that complex words are stored in terms of morphemes provided that

(a) very common morphologically complex words may be stored as wholes;
(b) the term 'morpheme' has to be interpreted in a rather more restricted way than is done in some theoretical linguistic works.

The evidence from production does not so much contradict this evidence as suggest that there are degrees of ease of formation of new coinages, and that some types may be easier to form in some languages than in others. It also suggests that some morphophonemic processes may become unavailable when the morphological processes with which they are associated become unavailable. Furthermore, it has been implied that some of the experiments that appear to show that speakers cannot cope with the complexities of word-formation produce such results because of poor experimental design, rather than because word-formation is inherently 'too hard' for speakers. Nevertheless, it has to be admitted that some word-formation does appear to be either too hard or at least not simple for speakers, and this point will have to be taken into account in drawing up a final picture of morphological productivity.

5 *Scalar productivity*

> Though many things are possible in morphology, some are more possible than others. Aronoff (1976: 35)

> Precise measurement of word formation productivity thus would not seem to be a realistic goal. Bolozky (1999: 3)

5.1 Introduction

Summarising the earlier discussion, we can isolate a number of views to be found in the literature on scalar productivity.

(a) There is no such thing. A morphological process is either productive or not productive.
(b) Productivity is scalar, and this is an important fact about productivity. Somehow this variability is part of the linguistic competence of speakers.
(c) Productivity is scalar, but this is a direct result of variable constraints that are imposed on morphological processes: the more constraints there are, the less productive the process.
(d) Productivity is scalar, but this is nothing to do with linguistic competence, it is purely a matter of performance and should not be seen as an important feature of morphological productivity.
(e) Productivity is scalar, and this is partly a result of morphological constraints on the processes, and partly a matter of linguistic performance. (This is a mixture of positions c and d.)
(f) Productivity is not scalar. All morphological processes are productive, some are not widely used because their products are not needed often, but this is nothing to do with morphology.

Positions (d) and (f) may be difficult to distinguish in their outcomes, but I take it that there is at least a theoretical difference between those scholars who see productivity as being limited only by aesthetic and performance factors, and those who deny productivity any role in morphology, because everything is the result of analogies and analogies are in principle not limited.

The majority of scholars seem, however, to prefer one of the hypotheses that involves some degree of scalar productivity, and it is only under one of these that it makes sense to discuss the question of how productive a given process is, or whether one process is more productive than another. In this chapter I shall consider these views of productivity, first by considering the types of constraints that are recognised by some scholars as limiting productivity (and by others as implying that there is no need to limit productivity because outputs are either possible or not) and, second, by considering ways in which productivity is measured. Ideally, at the end of this chapter we would be able to answer questions such as: Is productivity scalar or not? What does it mean to call productivity 'scalar'? Are the only constraints to be found on word-formation those than can be stated formally? Can productivity be measured or compared? How can we measure productivity? Unfortunately, we will not be able to answer them at the end of the chapter, but we will have seen some of the implications for these questions of current theoretical positions.

5.2 Restrictions or constraints

It is well known that not all affixes can be attached to all bases. This is sometimes discussed as a restriction on the bases that various affixes can attach to (e.g. Bauer 1983), but might equally be thought of as a matter of CONSTRAINTS on collocations of elements. The use of the term 'constraint' has the advantage, in current linguistic parlance, of indicating that the restrictions are not necessarily absolute. While I do not wish in this context to set up an Optimality Theoretic discussion on the conditions under which particular constraints may be broken (though, for an exemplary discussion, see Plag 1999), it does seem to me that it is the kind of area that Optimality Theory might be expected to be able to deal with. My aims in this section are more modest: to show the range of constraints that have been said to apply to the application of morphological processes, and then to ask whether these constraints are sufficient to account for variable or scalar productivity phenomena. It must be

acknowledged at the outset, however, that no claim is being made here about the exhaustivity of the constraints or types of constraint discussed. It is not clear to me whether any exhaustive list of potential constraints is possible and, that being the case, whether we can hope to know whether formal constraints are the major way (let alone the only way) in which productivity is limited.

The constraints discussed at length below deal mostly with matters of detail. Plag (1999: 45–61) deals with ten constraints suggested in the literature which take a broad-brush approach to restrictions on base and affix combinations (see also Scalise 1984: ch. 7). The ten constraints listed by Plag (1999: 45) are given in (1).

(1) a. the word base hypothesis
b. the compositionality hypothesis
c. the binary branching hypothesis
d. locality conditions
e. recursion and repetition constraints
f. the open-class base hypothesis
g. the unitary base hypothesis
h. the unitary output hypothesis
i. blocking
j. stratal constraints.

Plag (1999: 45–47) dismisses the first six of these briskly, and there is little need to add to his discussion. Constraint (1e) will be considered in section 5.2.2; (1i) will be dealt with in section 5.2.8 and need concern us no further here; most of the others can be ignored as being of little relevance to the theme of this book. Aronoff (1976: 48) explains (1g), the Unitary Base Hypothesis, thus: 'The syntacticosemantic specification of the base . . . is always unique. A W[ord] F[ormation] R[rule] will never operate on this or that.' Plag (1999: 47–48) argues that this is meaningless given that a suitable feature system can always link any subset of word-classes. On the other hand, Plag (1999: 49–50) adopts the unitary output hypothesis, which 'does not allow a particular phonological form to be considered a single affix if it produces outputs with different category labels or different semantics' (Scalise 1984: 137). Although Plag adopts this principle, he himself illustrates one of the major problems with it: the fact that one analyst's unitary semantics may be another analyst's disunity (Bauer 1988: 111). Plag finds unity in the semantics of the suffix *-ise* even though it has previously been analysed as multiply ambiguous. This suggests that any decision made because of this hypothesis is always subject to

re-evaluation when a new semantic analysis of a particular affix is made. Despite this drawback, (1h) does not seem to be a particularly controversial principle.

The last constraints, (1j), are stratal constraints such as level-ordering. According to the level-ordering hypothesis, morphological rules are ordered in sets, each set interacting with a distinct corresponding set of phonological rules (Kiparsky 1982; Mohanan 1986; McMahon 1994). Since individual affixes belong to one or another of these sets, the ordering of affixes within the word is determined by the ordering of the sets. There has been so much criticism of the details of applying this system to individual languages, including a vast literature on so-called bracketing paradoxes (which are usually paradoxes only within a level-ordered morphology), that it scarcely seems worthwhile attempting to list it. It seems likely that level-ordering applies least controversially in productive morphology (Bauer 1992b), but problems arise even there. However, Fabb (1988) and Plag (1996; 1999: ch. 4) argue that at least in terms of affix-ordering the level-ordering hypothesis is redundant: other generalisations are needed anyway, and are more powerful than the generalisations which derive from level-ordering.

From the point of view of productivity, this leaves us in the enviable position of being able to ignore questions of level-ordering (though a brief, covert mention is made in section 5.2.2). Overall, this means that the constraints in (1) are not (perhaps despite superficial appearances) ones which have a great deal of effect in determining the productivity of a morphological process, and the most relevant constraints are precisely those which deal with the details.

5.2.1 Phonological constraints

Phonologically conditioned allomorphs illustrate constraints on the collocation of morphological elements. For instance, the /ɪz/ allomorph in the English plural arises through the phonotactic impossibility of clusters of stridents in English, and such clusters arise potentially only over morphological boundaries. Although it is not normal to describe this allomorphy in terms of restrictions on the collocation of morphological elements, we can focus on this aspect of the allomorphy by talking about restrictions on the distribution of the endings /ɪz/, /s/ and /z/, or even about restrictions on the distribution of bases before /s/. The point is that phonological constraints on morphological compatibility are well-attested and not at all unusual.

We can distinguish at least three areas of phonological structure where constraints may operate: there are instances where the segmental make-up of the base is important, instances where the suprasegmental make-up of the base is important, and instances where the number of syllables in the base is important.

In discussing the *-en* in English which forms verbs from adjectives, Marchand (1969: 272) says that

> In all stages of productivity, only stems ending in a stop or a fricative have lent themselves to this type of derivation, and in the last two hundred years, only verbs from adjectives in *t* and *d* seem to have been formed.

He cites examples such as *neaten*, *quieten*, *smarten* and *tighten*. The Dutch suffix *-te*, which forms nouns such as *koelte* 'coolness' from adjectives, cannot be added to adjectives that end in a vowel, and if the adjective ends in a cluster of C_1C_2, C_1 must be a dental sonorant (e.g. *warm·te* 'warmth') (De Haas and Trommelen 1993: 250). In Hebrew, the suffix *-le* which forms hypocoristics from personal names and 'intimate nouns' can only be added to bases ending in vowels, e.g. *ába* 'father' > *ábale* 'daddy' (Glinert 1989: 437). Other examples of this type are not hard to find. As can be seen, the restrictions vary from the extremely narrow to the very liberal, but in each case it is the segmental make-up of the base which determines the application of the process.

As an example where the suprasegmental make-up of the base is important, consider the following from English. The noun-producing suffix *-al* in English (as in *arrival*, *rebuttal*) is added only to verbs which are stressed on the final syllable. The exception of *burial* has a different etymological source and is thus not a real counter-example. A similar example is available from German, where the suffix *-heit* which creates abstract nouns from adjectives is added only to bases which have final stress (Rainer 1988: 177).

Also of interest are those instances where the syllabic make-up of the base is important. In Tzutujil, for example, the suffix -C_1*oj* meaning '-ish' is affixed only to monosyllabic adjectives, as in *q'eq·q'oj* 'blackish' and *rax·roj* 'greenish' (Dayley 1985: 212).

There are also cases where it may not be entirely clear whether a particular restriction is phonological or morphological. The Turkish suffix *-ç*, which forms abstract nouns mainly from reflexive verbs, appears to be restricted to verbs ending in *-n*. This is largely due to the fact that the

reflexive form is *-n*, but the fact that there are non-reflexive instances as well makes it look like a phonological restriction (Lewis 1967: 222). Similarly, the restriction in English that adverbial *-ly* should not be added to adjectives which already end in *-ly* seems at one stage of English to have been a constraint forbidding sequences of homophonous affixes (see section 5.2.2), but now seems to block the appearance of words like *sillily* in which the first *-ly* is not a separate affix (see Baayen and Renouf 1996: 83). What was originally a morphological restriction seems to have become a phonological one.

5.2.2 Morphological constraints

There are at least three ways in which the morphological structure of the base may act as a constraint on subsequent affixation. First, the base may have to belong to a particular morphologically defined class before affixation can occur. Second, the base may have to show a particular morphological structure before affixation can occur. Third, the base may have to end in a particular affix or not end in a particular affix before affixation can occur. Each of these will be treated in turn.

Examples of affixation applying only to a morphologically defined set of bases are found when the base is defined in terms of a category such as gender or in terms of some apparently etymological category. As an example of the former, consider Hebrew, where the sarcastic diminutive of the form CCaCCaC (e.g. *zkankan* 'little beard' < *zakan* 'beard') can only be made from masculine nouns (Glinert 1989: 433). Similarly, the suffix *-iyat* of Punjabi, which creates *shaksiyat* 'individuality' from *shaks* 'person', is used to derive feminine abstract nouns from masculine nouns (Bhatia 1993: 286). Examples of etymological definition of the base are frequent in a language like Dutch, where, for example, the suffixes *-aar* and *-aard* prefer or demand underived Germanic bases (De Haas and Trommelen 1993: 170, 181). Similar examples concern the Modern Greek suffix *-adoros* which is added exclusively to words of Romance origin such as *kombinadoros* 'trickster' from *kombina* 'trick', itself from French *combine* 'trick' (Mackridge 1985: 320), or the Russian suffix *-ant* which, though productive in Russian, is added only to foreign bases such as *kursant* 'student' from *kurs* 'course' (Townsend 1975: 175). It might appear peculiar to call these 'morphological categories' rather than 'etymological categories'. I use that terminology from the conviction that most speakers do not have in their mental lexicons information about the sources of the words they use, and that if there is any reality to these

statements of restriction, it must be because words are perceived as belonging to various synchronic classes. These classes mimic etymological provenance (because that is their origin), but the mental listing involves assigning them to classes which are as random as (perhaps more random than) gender classes. It is typically the case that these apparently etymological categories 'leak', allowing in words of the wrong etymological type. This merely stresses the point that etymology is not necessarily criterial. See Plag (1996: 778) for a similar view of apparently etymological classes. If this argument is rejected, nothing crucial hangs on it: scholars who believe these to be restrictions genuinely based on etymological provenance will simply have to add a further class of possible restriction types.

The morphological structure of the base is relevant where it appears that a particular affix can be added only to a derived or to an underived base. Instances of Dutch affixes strongly preferring underived bases were cited in the last paragraph. An example of the converse case is provided by Punjabi *gair-* 'un-' which is used only with derived bases, e.g. deriving *gairsarkaarii* 'non-governmental' from *sarkaarii* 'governmental', itself derived from *sarkaar* 'government' (Bhatia 1993: 303). Such examples create problems for theories which assume that internal brackets are erased before any further affix is added (Chomsky and Halle 1968: 20; Mohanan 1986: 23), since they demand that an affix should have access to the structure of the base to which it is being attached.

Restrictions involving particular affixes work in several different ways. The simplest is where affixation with a second affix demands prior affixation with another. Using the terminology of Williams (1981: 250), this comes under the heading of POTENTIATION of subsequent affixation, though it is a particularly restricted type of potentiation. Consider the case of the Danish female marker *-trice* which is derived only from bases ending in the suffix *-ør* (Allan et al. 1995: 538), as in *direktrice* from *direktør* 'director'. Similarly the Dutch female marker *-ster* is added only to a small group of derivatives including those in *-aard*, as in *wandelaarster* 'female hiker, tramper' (De Haas and Trommelen 1993: 190). A more complex example of the same phenomenon can be found in Hixkaryana, where the suffix *-mɨ* forms nouns from adverbs, but only from such adverbs as have the prefix *tɨ-*, as in *tɨmporyemɨ* 'thing that ought to be given' from *tɨmporye* 'that ought to be given' (Derbyshire 1979: 168). A more complex type is where the affix being added requires not only a particular affix, but a particular structure with that affix. Again, the Dutch suffix *-ster* illustrates this, since one of the groups of words to

which it can be added is those made up of the suffix *-er* added to a verbal base, as in *zwemster* 'female swimmer' correlating with *zwemmer* 'swimmer' (De Haas and Trommelen 1993: 190). Alternatively, a specific affix may not be a prerequisite, but a specific type of affix may be. For example, the 'gerund' in Sanskrit is marked by the suffix *-tvā* when added to a simple base, but *-ya* when added to a base with a prefix (Whitney 1889: 355). It is the presence of a prefix which is criterial, not the particular prefix involved. Such examples are again problematic for bracket-erasure, and ultimately for the lexicalist hypothesis. Examples from German illustrate the case where a particular type of affix must NOT be present. The suffix *-heit*, which creates abstract nouns from adjectives, can be added to compounds (*Schreib·faul·heit* 'write-lazy-ness = the quality of being a bad correspondent'), prefixed adjectives (*Un·gleich·heit* 'inequality') or adjectival past participles derived with a circumfix (*Ge·schloss·en·heit* 'shut-ness'), but is not added to adjectives derived with a suffix (Fleischer 1975: 148). According to Ettinger (1974: 366–367), German can have diminutives of unmarked nouns, but not of gender-marked nouns, so corresponding to *Hund* 'dog' you have *Hündchen* 'little dog, puppy', but there is no diminutive of *Hündin* 'bitch'. Corresponding to *Ente* 'duck' you have *Entchen* 'little duck', but there is no diminutive corresponding to *Enterich* 'drake'.

The REPEATED MORPH CONSTRAINT (Menn and MacWhinney 1984) or the MULTIPLE APPLICATION CONSTRAINT (Lieber 1981: 173) is a proposed general restriction on the form of morphologically complex words, preventing the same affix being added twice in succession. In this form, it is too strong, since there are cases where the same morph may be repeated or the same type of morph repeated, and these are sometimes predictable (see Mayerthaler 1981: 120). For example, English *re-re-write*, German *Ur-ur-grossvater* seem unobjectionable despite breaking the constraint. In effect this constraint is a very precise restriction on affixation where the same affix is already outermost on the base. In many cases, the constraint appears to apply to homophonous affixes as well as to identical affixes. For instance, the *-ly* at the end of *month·ly* creates adjectives, but cannot usually have the *-ly* that creates adverbs added to it. In the sense that they create different parts of speech, these must be seen as distinct affixes, and yet one constrains the other. Similarly, but in inflectional morphology rather than derivational morphology, no phonological representation of the possessive morph is added to words which end in the homophonous plural marker (*children's* is in order, *adults's* is not; see

Stemberger 1981). The affixes are homophonous (at some level) rather than identical. It is arguable that while *sillily* was originally blocked by the repeated morph constraint, it is now blocked by a more generalised, purely phonological rule.

5.2.3 Syntactic constraints

The distinction between morphological constraints and syntactic constraints may be more illusory than real. Here I draw the distinction in such a way that a morphological constraint concerns the internal structure or inherent class of the word in the base, whereas a syntactic one concerns the way the word is used in context. Since the way in which a word is used depends to some extent upon the class it belongs to, it might be preferable to merge these two. I retain the labels for expository purposes.

The most obvious syntactic constraint in this sense is that most affixes can be added only to certain word classes (e.g. nouns, verbs, adjectives). Indeed, Aronoff (1976: 48) makes this into a fundamental assumption about the way in which word-formation rules (WFRs) function: 'A WFR will never operate on either this or that.' Although the precise status of this assumption is perhaps obscure (is it a hypothesis about the way language works or a hypothesis about when the linguist will claim that there are two homophonous affixes?), it does make the point that the word class of the base is of fundamental importance in morphology. Examples have been cited above, and are so common that they do not require a great deal of elaboration. The basic point holds for both derivational and inflectional morphology, and has been used to distinguish affixes as morphological entities from clitics as syntactic ones (Zwicky and Pullum 1983).

There are many types of deverbal affixation that are sensitive to the transitivity of the verbal base. For instance, the suffix *-baar* in Dutch (De Haas and Trommelen 1993: 291), like its English equivalent *-able*, is used on transitive but not intransitive verbs, so that *becomable* and its Dutch equivalent are not possible words. In Apalai there are two distinct affixes used to denote 'agents of present action': *-ne* is used only on transitive verbs, *-kety* is used only on intransitive verbs, as in (2) (Koehn and Koehn 1986: 90–91).

(2)	parata wo·ne	wa·kety
	rubber cut·AGT	dance·AGT
	'rubber-cutter'	'dancer'

In Kannada, the suffix *-tana* which produces abstract nouns is found on nominal and on adjectival bases, but the nouns it can be added to 'can all function as attributive "adjectives"' (Sridhar 1990: 266), e.g. *mu:rkha·tana* 'foolishness' from *mu:rkha* 'fool'.

An example which appears particularly complex comes from Diyari. The excessive marker *-kaɲʈi* is used on 'the set of common nouns which take the inchoative verbalizer and appear in the ergative case when used predicatively' (Austin 1981: 39), as in *ŋudukaɲʈi* 'powerful one' from *ŋudu* 'power'.

5.2.4 Semantic constraints

In some cases, semantic restrictions on the base are clearly a matter of what it makes sense to have a word for. For example, in Modern Greek there is an affix *-enios* meaning 'made of' which attaches to the names of materials as in *lamarinenios* 'made of sheet metal' from *lamarina* 'sheet metal' (Mackridge 1985: 322). Since the only things that items can be made of are their materials, this is simply a matter of making sense. Similarly, the Mangarayi suffix *-ŋuŋuŋ* 'person originating from' requires a base which denotes a place name or a language, as in *Guwiɲilen·ŋuŋuŋ* 'Queenslander' (Merlan 1982: 170).

In other instances, however, there is less obvious reason for the semantic restriction. For example, the Dyirbal suffix *-baray* meaning 'with a lot of' is usually added to nouns denoting humans (Dixon 1972: 223), as is the German collective marker *-tum*: it is productive only when added to nouns denoting humans, though historically it had a wider application (Fleischer 1975: 163). The Russian collective marker *-stv·o* as in *uchitel′stvo* 'teachers collectively; the profession or duties of a teacher' is even more restricted, being added only to the names of professions (Townsend 1975: 194).

5.2.5 Lexical constraints

All non-productive affixation is in effect lexically constrained, in that the list of words to which the affixes can be added is known and finite. While it may not be possible to get general agreement on which words of English contain the suffix *-th* as in *warmth* (consider *dearth*, *death*, *month*, *wealth*, *youth* as words on which there might be disagreement), nevertheless for any individual speaker it is possible in principle to list all the bases that occur with this suffix. (See section 2.3 on the relation between listedness and non-productiveness.)

More dramatic are those instances where a particular affix or process is found with a very small number of bases, possibly only one or two. In Dutch, for instance, the word *dievegge* 'petty thief, shoplifter' is clearly derived from *dief* 'thief', but there are no other words with the same suffix (De Haas and Trommelen 1993: 230). In English, the element *-ric* is added exclusively to *bishop* (equivalently, *arch-bishop*), and the suffix *-ter* is found only in *laughter* and *slaughter*. In Abkhaz the intensifier *-samsal* is found only in the word *àyk°ac°'a-samsal* 'very black' (Hewitt 1979: 250). And in Punjabi the nominalisation suffix *-aapaa* is found only in the word *kuʈaapaa* 'beating' from the verb *kuʈʈ* 'to beat' (Bhatia 1993: 293).

5.2.6 Pragmatic constraints

The distinction between semantic and pragmatic constraints may be hard to draw in practice, but in principle semantic effects deal with the linguistic nature of the base, while pragmatic effects deal with the way in which the words are used or the nature of the real-world referent of the word. Some examples will illustrate the point.

In Abkhaz the suffix *-c* is a singulative marker, used to denote a single entity of a type normally thought of as occurring in groups (Hewitt 1979: 243). For example, *à-šxə-c* means 'a single bee', while its root means 'bee', but with the implicit understanding that these naturally occur in large numbers. This expectation is part of what we know about the world, rather than part of what it means to be a bee, and it is therefore classed as a pragmatic constraint rather than a semantic one. In Dyirbal, the suffix *-ginay* means 'covered with, full of', but is 'normally used only of "something dirty or unpleasant"' (Dixon 1972: 223), as in *gunaginay* 'covered with faeces'. A similar restriction applies to the Kusaiean suffix *-yak* meaning 'to become affected by' which is typically attached to the names of insects (meaning 'to become infested with') or diseases (meaning 'to be badly affected by') (Lee 1975: 211–212).

A pragmatic restriction of a different kind, because it is a restriction on the output rather than on the input, is reported for Kannada. Here the suffix *-vaːra* (best translated as '-wise') is stylistically restricted or restricted by domain, being used mostly in bureaucratic language, e.g. *koːmu·vaːra* 'community-wise' (Sridhar 1990: 279).

5.2.7 Aesthetic constraints

Aesthetic constraints on the production of morphologically complex forms are hard either to prove or to disprove. Since individuals do not necessarily

share the same set of aesthetic reactions, one person can freely coin a word that others feel awkward with. Adams (1973: 3) lists a number of apparently aesthetic reactions to words which have not prevented their subsequent adoption and use, and in particular cites the comment made in 1909 that 'you could hardly think of a worse word' than *aviation*.

Guilbert (1975: 191) discusses an aesthetic reaction against very long words in French, which might prevent *oppositionnellement* 'oppositionally' while permitting *oppositionnel* 'oppositional', but is guarded about how real such constraints might be. He points out that such forms do not appear to be ungrammatical, and can be perfectly well understood.

At another level, it might be argued that all constraints on word-formation are 'merely' aesthetic. In particular this might be argued for phonological constraints. Is there a phonological constraint forbidding *sillily* in English, or is it just ugly to have a sequence of /(ɪ)lɪlɪ/? It seems to me that the main difference here is a difference of approach (perhaps of belief structures among linguists), and that where such constraints seem to be reliable, they should be termed phonological constraints, while less reliable ones may, indeed, be aesthetic and correspondingly variable.

This brings us back to the – ultimately unanswerable – question of the distinction between competence and performance in morphology (see section 2.8). I know of no really reliable 'aesthetic' restrictions on word-formation, and until such effects are shown, it is perhaps best to be a little sceptical.

5.2.8 Blocking as a constraint

Aronoff (1976: 43) defines BLOCKING as 'the nonoccurrence of one form due to the simple existence of another'. The other form is usually taken to prevent the formation of a new word because of synonymy, although occasionally homonymy, particularly embarrassing homonymy, is the preventative factor. Clark and Clark (1979) thus use the terms PRE-EMPTION BY SYNONYMY and PRE-EMPTION BY HOMONYMY, which are more explicit, but less widely employed. This terminology is new, though recognition of the phenomenon can be found at least as far back as Paul (1995 [1880]: 114). The example of blocking that is usually cited is that there is no word *stealer* in English because it is blocked (or pre-empted) by *thief* (e.g. Bolinger 1975: 109). The example is interesting, not least because it is not strictly true. The blocking does not operate for obvious reasons when *stealer* is not synonymous with *thief* (e.g. *the ten stealers* for 'the fingers'), but neither does it necessarily function in synthetic compounds

and other constructions with an overt direct object: *sheep-stealer, a stealer of sheep*.

It now seems to be generally accepted that, at least for what Rainer (1988) terms TOKEN BLOCKING, blocking does not always work. Di Sciullo and Williams (1987: 13) go further and say 'It remains a mystery what blocking actually is, and it is quite unclear under what circumstances it obtains.' This sporadic failure of blocking is explained neatly by Rainer (1988: 164) when he says

> we may view the blocking force as the result of the antagonism between the pressure exerted by a potential regular word and the resistance offered by the corresponding blocking word, whereby pressure is a function of productivity and resistance a function of frequency.

In other words, the pressures which cause and prevent the application of blocking are both gradient pressures, so that which wins out on any given occasion is determined by the relative pressures being applied in that case.

To the extent that token blocking is a recognisable and predictable effect in morphology, however, it acts as a constraint on the profitability of morphological processes, and is thus relevant here.

Within the theory of Lexical Phonology and Morphology, there is a rather more precisely formulated theory of blocking (see, e.g., Kiparsky 1982). In this theory, operations which apply at one level in a level-ordered morphology prevent the application of operations on a later level which would create synonymous forms from the same base. Thus, for example, the rule which would create a form *oxes* on Level II is blocked by the fact that *oxen* has already been listed on Level I. This is a generalisation of the Elsewhere Condition (see section 3.9): the application of *-en* to the base *ox* is a more specific rule than the default which adds *-s* to nouns to make a plural, and thus overrules the default.

I have two objections to this narrow interpretation of blocking. The first is that precisely the case where blocking in this sense is said to apply is the case where we find the major exceptions to blocking. For instance, the gradual replacement in the course of the twentieth century of *atypical* by *untypical* should not be possible if the existence of Level I *atypical* is blocking *untypical*. More generally, if *oxen* blocks *oxes*, then why is it not the case that all Middle English *-en* plurals remain in modern English, and the only plurals with *-s* are the nouns which have been introduced since *-s* became the productive plural marker (compare with the French case of nouns in *-al* discussed in section 1.2)?

The second is that it implies that a Level II affix can never block a Level I affix. In the very nature of things it is hard to prove that a Level II affix has blocked a Level I affix, since there are often so many reasons why the Level I affix might not have been used in the first place. However, consider the case of *philosopher* rather than *philosophist*, and the general use of *-er* after *graph* (e.g. *cartographer*) rather than *-ist*. Where both forms are listed by *The Oxford English Dictionary* (e.g. for *hagiographer/list*, *orthographer/list*, *pornographer/list*, *stenographer/list*) the *-er* form is usually in use earlier than the *-ist* form and outlasts it. Only in a few cases (such as *epigrapher/list*) is the *-ist* word the earlier and the continuing form of the word. Even alternating with *-ology* (where *-ist* seems to be the default), we find *astrologer* rather than *astrologist* until the 1950s. These examples may not be considered relevant, however, since it might be argued that the *-er* in such words is a Level I *-er*, rather than the expected Level II *-er* which happens to cause truncation when added to a form ending in *-y* (*cartography*, etc.). The same objection cannot be raised against the next example, though. Verbs ending in the phonemic sequence /ɪʃ/ usually make their nominalisations in *-ment* (*accomplishment*, *admonishment*, *banishment*, *establishment*, *nourishment*, *punishment*, *refurbishment*, *replenishment*, etc.). In terms of stress, this *-ment* behaves as a straightforward Level II affix. Yet there are a few verbs in *-ish* (which I assume from the data above is not a morph) which do not have a corresponding *-ment* nominalisation. In some cases, the nominalisation that is found uses a Level I affix (*demolition*, for example); in other cases a word using a Level I affix has become more usual than that using *-ment* (i.e. blocking fails): *diminishment* yields to *diminution*, *distinguishment* yields to *distinction*, *publishment* yields to *publication*. Given what happens when the blocking fails, we can argue here that Level II *-ment* is in general blocking the use of Level I affixes. That being the case, the less specific notion of blocking used by Aronoff can be seen to be preferable to the more specific one used by Kiparsky.

What Rainer (1988) calls TYPE BLOCKING is rather different in nature. Type blocking occurs when there is a general restriction on the availability of at least one of two or more competing affixes, such that only one affix is available for any particular base. The Dutch plurals discussed in section 1.1 illustrate this kind of blocking. This sort of blocking is already covered by the kinds of restrictions that have been discussed in this section and does not require any further comment. Note, however, that the fact that these constraints are termed blocking by some authors makes

the point that the same general type of phenomenon can be seen to be applying in both cases.

5.2.9 Is profitability simply a function of constraints?

It must be noted that although many of the constraints that have been discussed above have been absolute (an affix attaches only to a particular type of base), many of them have not been (an affix prefers a particular type of base or usually attaches to a particular type of base). Accordingly it is extremely difficult to determine how far constraints actually exclude particular bases as potential bases for morphological processes. If all the constraints were absolute, this would not be a problem; when some of them are relative, it is a problem.

There are also several possible interpretations of the way in which constraints operate. At the most restrictive end, we might say that only bases which are not excluded by any constraint can function as potential bases for a particular morphological process. If this were true, then we would be able to make good estimates of the number of potential bases for any morphological process. But the moment more variable constraints are allowed, it becomes impossible to know how large the potential set of bases for a particular process is. We might try to get over this by saying that constraints do not allow us to predict actual numbers, but do allow us to predict relative numbers, in that the number of constraints operating is in itself a guide to profitability. But this seems rather imprecise, given that some constraints are extremely strong (a base must end in a particular segment or affix) while others are extremely weak because they permit far larger classes of words to act as potential bases. If constraints have any value in morphology, it would seem that it must be because we can use them to make predictions about the potential profitability of a process.

It is not clear how any hypothesis about the ways in which constraints function can be tested. If the process on which the hypothesis is tested is still available, then many of the words which fit the constraints will be potential words rather than existing words. This means that textual examples can never actually confirm the hypothesis, though they may be consistent with it. The only possibility is to use psycholinguistic techniques to discover how individuals react to nonce forms which are predicted to be potential words and nonce forms which are predicted not to be. With forms that are no longer available, the problems are even worse, since it is probable that not all potential words became established words

during the period of productivity of the process concerned. Lack of a particular form in a dictionary is therefore not evidence that the relevant form was not possible at some period.

Consider the evidence provided by Bauer (1992a) in a study of adverbs in *-ly* derived from bases ending in *-ly*. Of the five semantic classes of adjective ending in an *-ly* affix discussed in that paper, only two appear to potentiate subsequent adverbial *-ly* affixation. From 473 instances of adjectives ending in *-ly* found in *The Oxford English Dictionary*, only 10 were found with subsequent adverbial *-ly* (for complications caused by the methodology, see the discussion in Bauer 1992a). Even considering the semantic restrictions, this figure seems very low. On the other hand, if we assume a phonological restriction (such as appears to operate in current English, although it clearly did not always operate; see Baayen and Renouf 1996), even 10 examples is a lot. Either there was some kind of preferential constraint operating, or the restrictions that were operating were not sufficient to make solid predictions about productivity, or all the other words were perfectly possible, perhaps even used, but for various reasons did not get listed in *The Oxford English Dictionary*, or the few words which we find break the rule and are not genuine examples of productivity (Baayen and Renouf 1996: 83). (Reasons why such words should not be listed in *The Oxford English Dictionary* are of some interest, albeit marginally here, and include the ways in which lexicographers operate; see Baayen and Renouf 1996: 81.) The point is that it is not necessarily possible to distinguish between these various potential reasons.

As another example, consider the case of the *-en* which forms verbs from adjectives in English. This was cited in section 5.2.1 as having a phonological restriction on the base, such that the base must end in an obstruent and, more recently, either /t/ or /d/. The second edition of *The Oxford English Dictionary* gives rather more detail of such words than is discussed by Marchand (1969), and neither of these constraints appears to be absolute. For example, *dimmen* is recorded from 1828 and, although *safen* itself is not recorded, *safener* is recorded from 1942. However, the phonological constraints mentioned are generally maintained, and I shall assume that at least the restriction to obstruents holds. There is another phonological constraint not mentioned by Marchand, namely that the bases should be monosyllabic. I have found only one exception to this generalisation, *naken*, an obsolete form from *naked*, cited from 1240. There is also a morphological restriction, that the base should be unanalysable. The only examples which break this constraint are the

irregular comparatives found in *lessen* and *worsen*, although it should be pointed out that some *-en* forms are based on morphologically analysable nouns rather than adjectives, apparently to allow them to fit the phonological constraint: *heighten*, *lengthen*, *strengthen*. To account for such forms, the constraint could be reformulated to say that only Class I morphology is permitted within *-en*, but this is a rather more permissive formulation than is required to account for the attested forms.

At least 100 examples of the appropriate *-en* can be found in *The Oxford English Dictionary* (precision is difficult, since some relevant words are apparently said to contain the wrong *-en* affix, which *-en* affix is involved is not consistently specified, and so on). This figure is not very meaningful, however, without some idea of how many potential bases there are that fit the constraints, and to which this affix might in principle apply. To gain some idea of this, I selected the monosyllabic adjectives occurring in the 1.1 million words of the Wellington Corpus of Written New Zealand English (Bauer 1993). This is not an exhaustive list of such adjectives in English. The word *hoarse*, for instance, does not occur in the corpus. Nevertheless, it gives a good basis for discussion. From this list, all past participles were deleted, all nouns used attributively were deleted, all adjectives which demand capital letters were deleted (*Dutch*, *French*, *Thai*, etc.) since such adjectives never occur with a verbal *-en*. Adjectives ending in vowels and sonorants were also deleted. This left 172 monosyllabic, monomorphemic adjectives ending in obstruents. Of these, some 18 may be considered to be prevented from forming *-en* verbs either because of pre-emption by homonymy (*broken*, **deften* – which would coincide with *deafen*, compare *soften*) or because of pre-emption by synonymy (*badden* by *worsen*, *liven* by *enliven*, *wetten* by *wet*). This leaves 154 adjectives which might provide bases for *-en* suffixation, ruling out as much as seems at all possible. Of these, precisely 80 have *-en* verbs attested in *The Oxford English Dictionary*. Of course, other verbs may well have been used at various stages in the history of English – indeed, others verbs are bound to have been used. But for *The Oxford English Dictionary* to have missed almost half of the relevant words seems rather surprising. Some of the missing words sound perfectly natural, and acceptable. Even though they do not have bases which end in /t/ or /d/, I would not be surprised to find things like: *He gruffened his voice to disguise it*, *If we want to attract more customers*, *we'll have to poshen up the premises.* Others seem totally impossible: What would be the use of an *-en* derivative from *hind*, for example? How would you use an *-en* verb

derived from *loath*? There may be a general phonological constraint against bases ending in a sequence of nasal + voiced plosive to rule out *blonden*, *granden*, *kinden*, *rounden* and possibly also *strangen*, etc., though the fact that forms like *dampen*, *pinken* and *milden* are attested does not lend strong support to such a suggestion.

Although such evidence cannot be absolutely conclusive, it seems that constraints in themselves are not sufficient to rule out all the non-occurring forms. At least, this is true unless the overriding, undoubtedly important and, I suspect, unformalisable constraint is added: words will not be formed unless they will be useful. Even then, there are plenty of potentially useful forms which seem not to have become established, even if they are used. With *-en* it seems likely that lexicographical practice would have noted the relevant words, and the suffix is not so automatically productive as to make new forms invisible, as seems to happen with adverbial *-ly* and *-ness* for example. Rather, there are gaps not explained simply by considering constraints.

5.2.10 Constraints on bases or constraints on affixes?

The presentation in this chapter has been in terms of constraints which allow or prevent the attachment of a particular affix to a specific base. Plag (1996), however, argues convincingly that although affixes may be subcategorised for the bases they attach to where some kinds of constraint are involved (specifically, where phonological or semantic constraints are in operation), in other cases it is the base which selects the affix. He talks in terms of BASE-DRIVEN selectional restrictions. For example, he points out (1996: 776) that all verbs using the suffix *-ify* productively take nominalisations in *-ification* (see also section 6.4), so that it is a fact about *-ify* what nominalisation ending it takes, rather than a fact about *-ation* that it may be attached to *-ify*.

Not only is Plag's hypothesis convincing, it has added unexpected benefits in cutting down the number of morphological constraints which might otherwise have to be considered. For example, Plag (1996: 788–789) argues that there is no need to have a class of English suffixes which may be attached to only one other suffix, as proposed by Fabb (1988).

However, these perfectly valid conclusions need not disturb the discussion of constraints provided here. The constraints discussed above are ones which limit the potential set of base–affix combinations, without any implication that one is selecting the other as part of an efficient statement of the functioning of a productive process. Where there are

lexical restrictions, if they were not irrelevant because they are lexicalised, the base would clearly have to select the affix; in most phonological cases, the affix seems to select the base (as suggested by Plag), except in cases of allomorphy. Whichever way round the restrictions are most efficiently stated, they constrain potential combinations, which is what is crucial in this chapter.

5.2.11 Concluding remarks on constraints

On the whole, constraints on the productivity of word-formation processes have to be based on formal aspects of the potential bases. These constraints certainly limit the number of potential words that can be formed by a particular morphological process, but they almost miss the point. The main point is that words are only formed as and when there is a need for them, and such a need cannot be reduced to formal terms. It is because needs continue to arise that morphological processes can remain productive for considerable periods of time. The conclusion must be that although constraints may be valuable in determining the domains of potential productivity, that is under what circumstances morphological processes will be available, they cannot tell us everything about the profitability of morphological processes. That is not as easily describable in terms of formal factors. High numbers of constraints may limit the domain of application of a morphological process to such an extent that the type frequency of the resultant words will be extremely low. But type frequency can be low independent of constraints. We need to distinguish between these two possible sources of low type frequency, which we can call CONSTRAINT-RESTRICTED TYPE FREQUENCY and USAGE-RESTRICTED TYPE FREQUENCY. Constraint-restrictions are only one factor affecting type frequency, an important one, but not the only thing that needs to be considered.

This leaves open the possibility that there are measurable differences in the productivity of different morphological processes, and it is to this measurement that we now turn.

5.3 Measuring productivity

In this section we have to consider two things: whether productivity can be measured, and how it has been measured in the past. We will begin by looking at those measures which have been proposed and some of the problems with them.

5.3.1 Productivity as equatable with the number of analysable words

We have already seen that there are problems associated with equating the productivity of a process with the number of words that have been produced by that process. We can bring the objections together here, though we need to note that this version of a measure of productivity is something of a straw man.

First, then, such a count might possibly tell us something about the degree of generalisation of a morphological process, but it does not tell us anything about the availability of the process. It might be that the last word using that process has already been accepted by the language community, and that the process is no longer productive in this sense. It certainly says nothing about potential words, which is a crucial part of the notion of productivity. So a straight count can only tell us about past productivity.

A straight count, will, in many cases, include words which were not actually formed by the relevant morphological process. Consider, for instance, a count of English adjectives with the suffix *-able*. The words in (3) might provide a part of the corpus of words considered relevant for such an undertaking.

(3)

acceptable	analysable
changeable	disposable
desirable	enjoyable
measurable	renewable

The difficulty is that the words in the first column in (3) are all early French loans, and not words produced by English word-formation at all (Marchand 1969: 229). It may not even be possible to tell with some early words whether they were loans or created in English. Words such as *payable* are treated by *The Oxford English Dictionary* as English formations, but have French counterparts known in the right circles at the right period; a word like *regrettable*, first attested in English much later, is probably a direct loan; and even *understandable* (one of the earliest English formations), which formally cannot be a loan, might be calqued on Latin *comprehensibilis*. To summarise, a straight count may capture forms which were in use before any word-formation process had arisen. This is the danger of confusing the process of word-formation (sometimes called *Wortbildung* in German) from the analysis of the results of word-formation (sometimes termed *Wortgebildetheit* – word-formedness or analysability) (see, for example, Dokulil 1968).

With a process such as *-able* suffixation, we can get an answer of a kind by consulting a work like Lehnert (1971) or by searching the electronic version of *The Oxford English Dictionary*. What is not clear is how much material any such answer misses. With any morphological process which is available, we must assume that some material is missed; this may become a problem in cases where two processes are being compared, and significantly more is missed in one than another. In other words, the more profitable a particular process is, the less possible it is to give an accurate idea of the numbers of words in the community created with that process. This is the uncertainty principle applied to morphology.

Aronoff (1976: 36) has another objection to equating type frequency with productivity: 'it isn't fair'. Since, as we have seen, there are innumerable possible restrictions on the bases that can undergo particular processes, it may be that one type of process can apply to a very small class of bases while another can apply to a very large class of bases. Do we want to say that productivity is a function of the size of the input class of bases? I take it that this is an open question, but Aronoff clearly does not want to take this point of view. Contra Aronoff, it can be said that type frequency does have something to do with the perceived productivity of a particular process. The addition of *-ter* (as in *laugh·ter*) is not seen as ever having been very productive because there are so few words containing the suffix. We must keep this in mind when attempting a measure of productivity.

5.3.2 A first attempt: Aronoff 1976

Aronoff (1976: 36) suggests that a better measure of productivity is the ratio of the actual words produced by a word-formation rule (WFR) to potential words produced by the rule.

> There is a simple way to take such restrictions into account: we count up the number of words which we feel *could* occur as the output of a given WFR (which we can do by counting the number of possible bases for the rule), count up the number of actually occurring words formed by that rule, take the ratio of the two and compare this with the same ratio for another WFR.

Baayen and Lieber (1991: 803) formalise this as

$$I = \frac{V}{S}$$

where I is the index of productivity, V is the number of actual types and S is the number of types which the word-formation rule could give rise to. At the same time, they point out that there are problems with Aronoff's ratio measurement.

First, although we have seen that the notion of actual word is a theoretically necessary one, it is not one which is easily measured in any simple way (Baayen and Lieber 1991: 802; Bauer 1994a: 3355; see also section 3.2.1 above). The only way a firm number can be provided is in terms of some fixed corpus (whether an electronic corpus of data or a dictionary), and such a corpus is likely to miss some of the uses of productive processes in particular. Aronoff (1976: 36–37) himself draws attention to this problem with his ratio. He claims that the outputs of the most productive word-formation rules are never listed, and in later work (Anshen and Aronoff 1997) provides historical evidence which appears to support his position.

Second, given the number of potential limitations on bases, it is not always clear what the number of potential words is from any given process. Baayen and Lieber (1991: 804) argue that S remains in principle enumerably infinite, and that thus all productive word-formation processes must have an index of productivity of zero. Although it is not true that the number of possible bases is infinite unless we assume that bases can be of infinite length (which is pragmatically clearly false, even if there is no hard-and-fast limit on the length of possible bases), nevertheless the point can be taken that productive processes cannot in principle differ greatly in their productivity by this measure. For that reason, as Baayen and Lieber point out, Aronoff's formula can only measure non-productive cases of word-formation.

In any case, as we saw for the simpler notion of equating productivity with frequency, this measure can only tell us about past productivity, it cannot tell us whether a particular morphological process is available or not.

Aronoff (1976: 36) presents another problem with his own ratio, a problem which he claims 'is not crucial'. It is that

> one cannot speak absolutely about the productivity of a WFR. Rather one must ask how productive an affix is when attached to words of a particular morphological class.

This is the notion of domains of productivity, which is later developed by Van Marle (1985). We will return to this point later (section 6.5).

5.3.3 Including token frequency

Aronoff (1983) makes the point that the mean token frequency for derived words formed by productive processes is often lower than that for corresponding words formed by non-productive processes. He contrasts words in *-ivity* and those ending in *-iveness*, and on the basis of frequencies given by Kučera and Francis (1967) provides the figures given in table 5.1. Baayen and Lieber (1991: 805) confirm this general pattern from a larger database.

Table 5.1: *X*ivity *vs. X*iveness *(from Aronoff 1983: 168)*

	Mean frequency of base words	SD	Mean frequency of derived words	SD	N
X*ivity*	27.261	29.218	9.565	23.891	23
X*iveness*	13.117	20.445	0.641	3.214	103
	Z-score = 2.23		Z-score = 1.79		
	significant (one-tail test)	0.02	significant (one-tail test)	0.04	

The explanation of this pattern runs as follows. The words belonging to non-productive patterns must, by definition, be lexicalised. One aspect of lexicalisation is semantic specialisation (idiomatisation). Thus, lexicalised words may have specialised meanings – possibly several such meanings. Productively-produced nonce words, on the other hand, can only have one meaning, the compositional meaning that arises from the morphological process. Token frequency is argued to reflect semantic complexity, with greatest semantic complexity belonging to non-productively formed words.

The implication is that token-frequency can be used as an indirect measure of semantic complexity, which in itself correlates to a certain degree with productivity. Thus, we must expect to find relatively large numbers of words which are used infrequently formed by the most productive processes. At the same time, token frequency cannot be taken as a direct measure of productivity, since semantic complexity is a factor which also applies to simplex words.

Developing from this, Baayen (1989; see Baayen and Lieber 1991 and Baayen 1992) suggests a new measure of productivity. He suggests that an appropriate measure of the productivity of a process is

$$\mathcal{P} = \frac{n_1}{N}$$

where n_1 is the number of words formed by the appropriate process occurring in a corpus precisely once (the hapax legomena) and N is the total token frequency of words created by that morphological process in the corpus.

To illustrate the application of this formula, consider the affix *-th* in English which forms ordinals and fractions. There are two ways of viewing the productivity of this affix. Either this affix only applies to a very limited number of words (*4–20*, *30*, *40*, *50*, *60*, *70*, *80*, *90*, *100*, *1,000*, *1,000,000*) and cannot form any new words (so that in *67,586th* it occurs in the familiar word *sixth*), or this number can be added to an infinite number of bases (like *67,586*). In the first case, the affix is not productive; in the second case it is extremely productive. If we calculate the productivity of ordinal *-th* using Baayen's formula, we should get markedly different results under the two different analyses of *-th*. To do so, it is necessary to examine a corpus. I chose the Wellington Corpus of Written New Zealand English for reasons of local loyalty, though that has the disadvantage of the corpus being not much bigger than one million words. The results can be seen in Figures 5.1a and 5.1b. Figure 5.1a shows the productive hypothesis (*67,586th* is a different word from *6th*), and Figure 5.1b shows the unproductive hypothesis (*67,586th* is an instance of *6th*).

Thus in figure 5.1a, there are thirteen types with just one token each, while in 5.1b, there are only two types with one token. Note that Figures 5.1a and 5.1b are two analyses of the same data. On the productive analysis of *-th*, we have a relatively large number of hapaxes and the distribution is skewed to the extreme left. On the non-productive analysis, we still have a skewed distribution, but the mode is further to the right. With a larger corpus, we would expect the mode to move further to the right, and the difference to be clearer. Because the productive hypothesis expands the number of possible types enormously, we have increased the number of types which may only be realised by a single token in any corpus.

If we apply Baayen's formula to the total of 434 tokens in the data, the following results emerge:

On the productive hypothesis: $\mathcal{P} = \frac{13}{434} = 0.03$

On the non-productive hypothesis: $\mathcal{P} = \frac{2}{434} = 0.005$

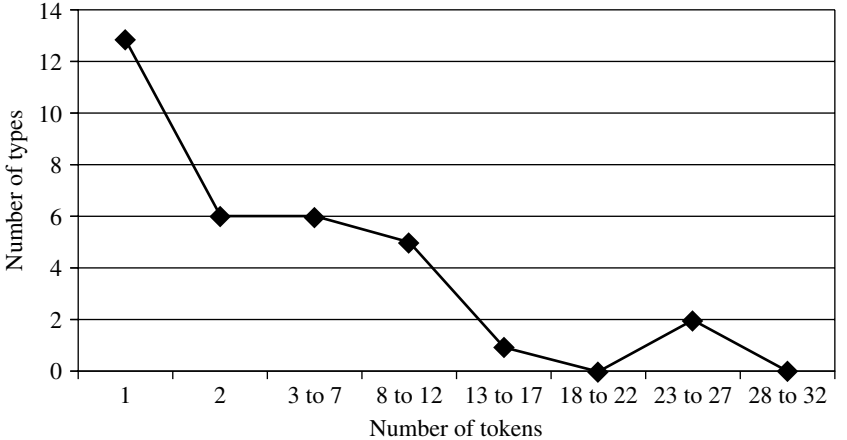

Figure 5.1a: Productive hypothesis

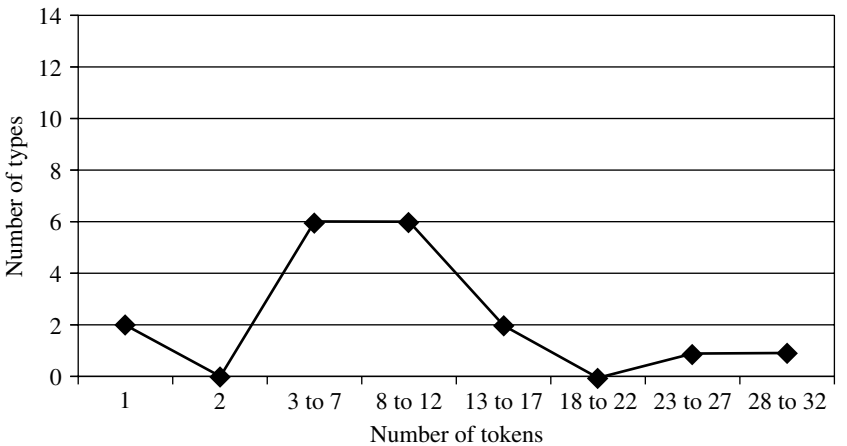

Figure 5.1b: Unproductive hypothesis

Figure 5.1: Ordinal *-th* in the Wellington Corpus of Written New Zealand English

These figures are not directly comparable with other figures given by Baayen in his various publications, since most of Baayen's figures come from a corpus of eighteen million words, and the number for productivity falls as a function of increasing sample size; that is, this is not a fixed measure of productivity, but only a measure of productivity at a given sample size. To get a baseline measure for comparative purposes, Baayen and Lieber (1991: 815) suggest that the corresponding figure for simplex words in the relevant sample should be calculated. New simplex words do arise

(through borrowing, back-formation and the use of acronyms, for instance), but they provide what Baayen and Lieber (1991: 815) call 'a base-line condition for the assessment of productivity' because 'a frequency-based analysis of morphological productivity should be sensitive to what the morphology adds to a distribution', not just to the possibilities which would exist without any morphology. Unfortunately, I do not have the appropriate figures from the Wellington Corpus, but it can nevertheless be seen from the figures given above that $\mathcal{P}$ is much higher under the hypothesis that ordinal *-th* is productive than under the hypothesis that it is not.

Baayen and Lieber (1991: 809) claim that this measure $\mathcal{P}$ has as one of its advantages that it reflects linguists' intuitions about productivity. If that is so, we need to know why it should be so. We need to ask why hapaxes in a corpus should correspond in any meaningful way to coinages in real use. The answer lies in the number of possible forms there are which display the results of a particular morphological process. The larger the number of possible types, the more likely it is that they will not all have been sampled in a particular corpus, and also the higher the chances that some of them will have been sampled only once. Consider the pronouns: we would be very surprised not to see all of them in a corpus of any size at all, and even rare words like *whose* recur several times in the Wellington Corpus. But there is a very limited number of them. If we had thirteen case-forms of each pronoun, we might not be too surprised if some combinations did not occur even in reasonably large corpora. The larger the number of potential types, the less likely it is that they will all occur in a given corpus. If a particular morphological process is very productive, the number of possible formations it can give rise to is very large. That being the case, it is extremely unlikely, perhaps impossible, to have all possible types in the sample, and some of the many possible types are likely to have been sampled only once. Thus, although the hapaxes in a particular corpus may not be new coinages, they should give a guide to the number of new coinages that are expected to be possible (Baayen and Lieber 1991: 813).

Inevitably, any suggestion of a measure of productivity of this type gives rise to innumerable questions. Some of the questions are relatively easily answered, while others are not. A few of the possible questions are dealt with here.

In the Wellington Corpus of Written New Zealand English, the suffix *-iana* occurs just once, in the word *Victoriana*. The number of hapaxes (n_1) is one, the number of types is (N) is one; the suffix *-iana* appears to be

totally productive. This is, of course, total nonsense, and is nothing to do with Baayen's measure. Rather the absurd result is the outcome of insufficient sampling. Generally speaking, the smaller the sample we are dealing with, the less confident we can be about the precise accuracy of the conclusions we can draw on the basis of that sample. If we want to be relatively sure that the picture we have got from the sample is accurate, then we need a reasonably large sample. Moreover, the more accurately we want to be able to state the result, the bigger our sample must be. However, there is not enough information available to be able to give a precise estimate of the size of the sample that would be required to give a reliable statistic in this case. For example, for a full mathematical understanding, the statistical properties of $\mathcal{P}$ and its precise relationship to productivity would need to be elucidated. In other contexts, Sigley (1997: 219) suggests various levels of accuracy with different sample sizes, and in his contexts samples of under 50 seem very unreliable. Those figures do not necessarily apply here, and seem conservative for this type of sample. All we can say is that samples under 50 are very unlikely to be accurate. That is why the conclusion based on the single example of *Victoriana* is not a statistically sound one.

We also need to ask whether the measurement $\mathcal{P}$ confuses degree of generalisation of a morphological process with productivity of that process. In theory, the answer to this is no, since with a widely generalised but unproductive process, each type should have a high token frequency, which should keep the productivity index low. One affix that seems like a good one to test this on is *-ment*, since it is a very highly generalised suffix (well over 400 types are listed by Lehnert 1971), but it is suggested in Bauer (1983: 55) that *-ment* may no longer be productive. Accordingly, the distribution of *-ment* in the Wellington Corpus was considered. The contention that this affix is no longer productive (taking 'productive' to mean 'available') is given some support in that of the three types out of a total of 166 in the Wellington Corpus which were not immediately item-familiar, two are listed in Lehnert (1971) and one is marked as a loan by being italicised in the original. The type to token profile is indicated in figure 5.2, which shows only words with 1–20 tokens, although *government* had over 700 tokens.

There are some problems in deciding which words should count as hapaxes for these purposes. For example, does *underdevelopment* count as a hapax, or as a token of *development*? In figure 5.2, it was counted as a hapax, in line with Baayen's (1993: 200) practice. If we take it that it is a token of *development*, then there are up to 12 fewer hapaxes in the data.

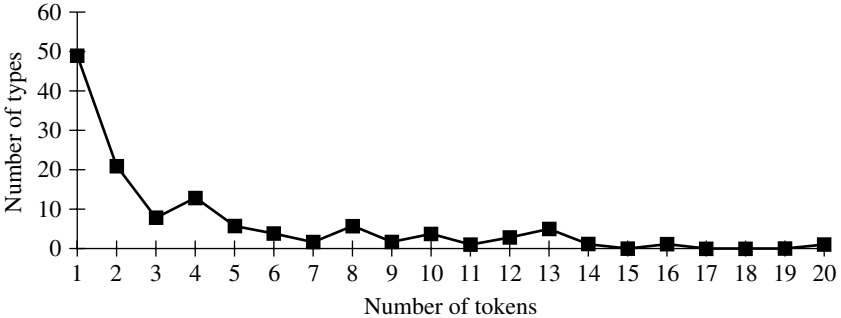

Figure 5.2: Productivity of *-ment* in the Wellington Corpus of Written New Zealand English

In either case, the general tendency of the graph is a maximally left-skewed distribution, which is supposed to correlate with productivity.

The productivity index for this data is as follows for the two ways of counting hapaxes:

$$\mathcal{P} = \frac{49}{4471} = 0.011 \qquad \mathcal{P} = \frac{37}{4471} = 0.0083$$

While the second of these figures does not make *-ment* look very productive in comparison with other things in the corpus, the first figure does make it look as though it might be productive, despite our evidence that it is not available to any great degree. Baayen and Lieber's (1991: 830) own figures – over a much larger corpus – suggest that *-ment* is 'barely productive', so perhaps the boundary of productivity goes somewhere around 0.01 for the Wellington Corpus. If this is the case, then it appears that Baayen's measurement does not confuse generalisation with productivity, despite the evidence of the graph in figure 5.2.

Van Marle (1992: 156) asks why the denominator in the fraction should be the token frequency of the morphological process in the corpus. The answer to this is that there is an assumption that lexicalised types have a higher token frequency than unlexicalised types, and that a high average token frequency should therefore indicate a lesser degree of productivity. We see that affect *-ment* nouns in that a single word with a token frequency of over 700 reduces the productivity value considerably. Without giving precise figures, we can see that the word *government* has in itself a considerable influence on the productivity rating of *-ment*: the affix looks more productive if the tokens for *government* (with its high token frequency) are omitted.

$$\mathcal{P} = \frac{37}{3770} = 0.0098$$

Van Marle (1992: 156) comments on Baayen's formula:

> I do not see what kind of direct relationship there is between the chance that a given rule is put into action and the frequency with which the words that have already been produced by that rule are used. Once a word is coined, the frequency of the use of that word, it seems to me, is more or less irrelevant to the degree of productivity of that rule.

This seems like a good point, and Baayen's (1993: 188) response is not as clear as might be wished. It seems to relate to the way hapaxes typically pattern in corpora, which implies that atypical examples may be found. Plag (1999: 112–113) comments on some of these. In a large corpus from *The Times*, Baayen and Renouf find a surprisingly low value for $\mathcal{P}$ for adverbial *-ly*, suggesting it is less productive than negative *in-* or verbal *-ate*, a level which they characterise (1996: 86) as 'hardly productive'. This is despite the fact that various scholars have suggested or implied that adverbial *-ly* is actually inflectional (Haspelmath 1996; Plag 1999: 113 fn 21), and thus, presumably, as productive as it is possible to get. Plag also criticises the figures for the verbal suffix *-ate*, pointing out that 7 of the first 10 hapaxes in an alphabetical list derived from the twenty million words of the COBUILD Corpus are actually formed in the sixteenth, seventeenth and eighteenth centuries. Plag suggests that doubt is cast on the value of hapaxes as a measure of productivity by data of this kind. It certainly seems to be the case that $\mathcal{P}$ is an indirect measure of productivity.

Since the numbers that are produced by using Baayen's formula for $\mathcal{P}$ are relative to the size of the corpus involved, comparisons cannot be made across corpora of different sizes. This limits the usefulness of $\mathcal{P}$ as a measure, without in the slightest invalidating it.

The measure $\mathcal{P}$ ignores the number of potential bases that are available for a particular morphological process. This means that the problems raised by Aronoff with respect to other measures of productivity are valid here as well. The same is true of notions of type-frequency. However, Baayen provides another measure which he terms 'global productivity' which does pay attention to type-frequency, and we turn to look at that next.

5.3.4 Reintroducing type-frequency

Baayen's measure of 'productivity in the strict sense' (Baayen and Lieber 1991: 817), $\mathcal{P}$, is an attempt to give a statistical measure of the probability of encountering new types (Baayen 1993: 183). But it ignores

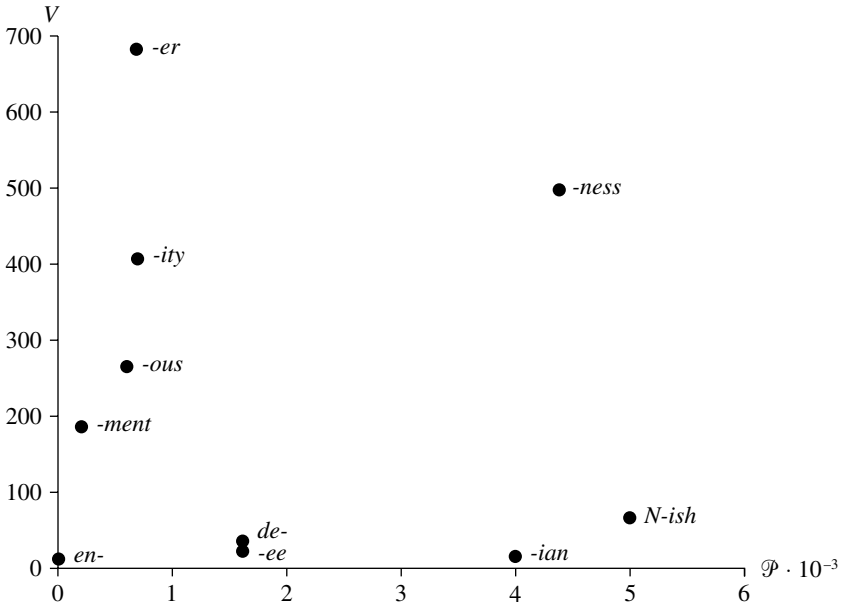

Figure 5.3: Global productivity (P*) of a number of English word-formation processes from an 18-million-word corpus (from Baayen and Lieber 1991: 819; Baayen 1992: 124)

type-frequency. Baayen (1989; Baayen and Lieber 1991: 817ff; Baayen 1992: 122–125) reintroduces this in a measure of 'global productivity' which he calls P*. P* is shown on a two-dimensional chart with $\mathcal{P}$ on the horizontal axis, and V, the number of different types in the corpus, on the vertical axis. A chart plotting some English affixes in such a space (Baayen and Lieber 1991: 819; Baayen 1992: 124) is reproduced as figure 5.3. If a process shows up in the bottom left-hand corner of such a display, we can say that the process is unproductive, and if it ends up in the top right-hand corner we can say that it is extremely productive. Unfortunately, as Baayen (1992: 124) admits, it is not possible to weight the relative contributions of the vertical and horizontal dimensions in such a chart. 'Is *-ian* more productive than *-ity*?' is not a meaningful question given their disparate positions on the chart.

What this seems to imply is that type-frequency is something separate from productivity in the strict sense ($\mathcal{P}$). To my mind, it fails to show that there is a vital relationship between the two measures, and rather implies that the two should be kept entirely separate. Yet, intuitively, the type-frequency would be thought to influence the probability of encountering new types.

In response to this criticism, Baayen (1993: 192) proposes yet another measure of productivity, which he confusingly labels $\mathcal{P}^*$, and which he terms 'the hapax-conditioned degree of productivity'. The measure $\mathcal{P}^*$ considers the number of hapaxes in an appropriate category in a corpus as a proportion of the total number of hapaxes in the corpus. Baayen formalises this as

$$\mathcal{P}^* = \frac{n_{1,E,t}}{h_t}$$

where E indicates the appropriate morphological category (e.g. nouns ending in *-ism*), t indicates the number of tokens in the corpus and h is the number of hapaxes, so that the top line of the formula reads 'the number of words of the appropriate morphological category occurring just once in the corpus'. According to this ranking, *-ian* in English is considerably less productive than *-ity* (Baayen 1993: 193). This measure, then, allows the ambiguities of figure 5.3 to be eliminated. Baayen (1993: 194) notes, however, that simplex words are given a very high ranking on this measure, and cannot be taken as representing a threshold of productivity.

Baayen himself sees this measure as having a rather different function from the measure $\mathcal{P}$. 'The primary use of $\mathcal{P}$ is to distinguish between productive and unproductive word formation processes as such, whereas $\mathcal{P}^*$ . . . [is] particularly suited to ranking productive processes according to their degree of productivity,' he claims (1993: 205). This distinction is not evident in earlier discussions of $\mathcal{P}$.

The main question to be asked here is whether $\mathcal{P}^*$ is measuring the right thing. Since it is dependent upon the notion of hapaxes, previous criticisms directed at their use apply even more strongly here. More fundamentally, if we assume that the number of hapaxes does correlate significantly with the number of possible new coinages, this measure asks 'What proportion of new coinages use affix A?' rather than asking 'What proportion of words using affix A are new coinages?' It is this latter which seems a more relevant question to ask.

Similar objections can be made against the ratios calculated by Bolozky (1999). Bolozky (1999: 5) introduces the ratios in the following terms:

> The first ratio (R1) contrasts the number of neologisms in a pattern with the total number of realizations of that pattern within some category in the lexicon. It reflects the pattern's potential to 'regenerate itself' in the total lexicon, regardless of the pattern size. The other ratio (R2) compares

> the number of neologisms in a pattern with the total number of neologisms in the semantic category concerned. It reflects the productivity of that pattern within that semantic category in the new lexicon, and consequently the mechanism most readily used by the synchronic innovator.

Not only does R2 look at the proportion of new coinages using a particular form (like Baayen's measure), but the ratios also presuppose, in a manner compatible with the separationist views of Beard and others, that there are semantic notions to be conveyed by word-formation (notions such as agency or nominalisation). While this may be possible in some areas (such as nominalisation – and such a measure will be used in section 6.4), there are places where it would seem dangerous to make such assumptions. Plag (1999: 227) argues that the success of his method of dealing with affixes casts doubt on the separationist hypothesis as a whole. On a much less imposing scale, consider the suffix *-ery* in English which occurs in words such as *greenery*, *machinery*; *nursery*, *fruitery*; *snobbery*, *thievery*. Here we can distinguish three semantic types: 'collective noun', 'locational noun' and 'abstract noun'. Yet it is not clear to me that a word like *fishery* is ambiguous rather than vague between two of these meanings, and meaning is the only factor which distinguishes the three, all formal indicators being the same for all three meanings.

5.3.5 An alternative view

Another way to view productivity is in terms of rate of additions. The more productive a morphological process is, the more coinages that occur created by that morphological process in a given time period. If we take a morphological process like *-ness* affixation, the productivity of the process can be equated with the average number of new words in *-ness* that are used in the language within a specified time period. We can portray this as in figure 5.4, where ρ is the rate of increase, and the actual increase in words between t_1 and t_2 is (a–b).

The difficulty for the linguist with the view illustrated in figure 5.4 is that it is impossible to provide accurate figures for a or for b, and, indeed, that it is impossible to estimate accurately the time differential between any such two sets of measurements. If accurate measurements cannot be provided for a and b, then estimated figures are the best we can hope for. These estimates should not, however, be simple guesses; they need to be based on the best data available. One possible source of such data is standard dictionaries. Standard dictionaries provide, as it were, a snapshot of the state of the language at the date of editing; and a dictionary such as *The Oxford English Dictionary* provides a vast amount of

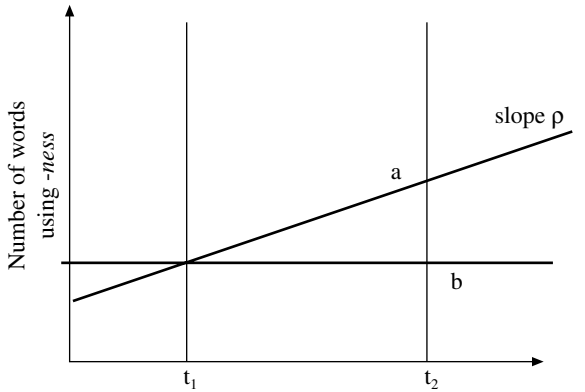

Figure 5.4: Productivity as rate of increase

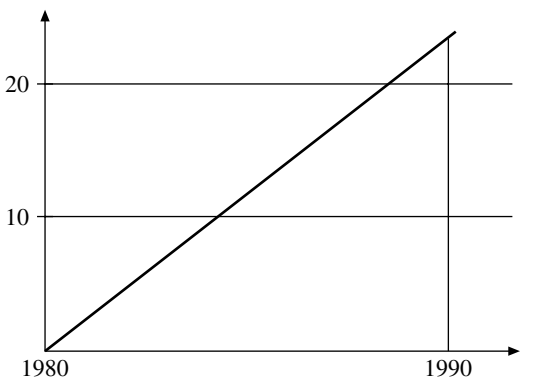

Figure 5.5: Productivity of *-ness* according to Tulloch (1991)

historical information as well. Since many standard dictionaries are now available in multiple editions, it would be possible to gain from them a picture of the increase in the number of words illustrating a particular word-formation process over a relatively well-specified time period. Alternatively, dictionaries of neologisms allow a similar statement of rate of increase (although Baayen and Renouf [1996: 69] argue that such dictionaries present an inaccurate picture). For example, *The Oxford Dictionary of New Words* (Tulloch 1991) lists the words with the suffix *-ness* in table 5.2 as being ones of which 'the general public was made aware . . . during the eighties and early nineties' (Tulloch 1991: v). Although various questions are ignored here, we could graph the data from (4) as in figure 5.5, giving us a rate of two words per year.

(4) **New words in *-ness*, 1980–90 (from Tulloch 1991)**

environment-mindedness	unfriendliness
deafenedness	user-unfriendliness
heterosexism awareness	yuppiness
eco-awareness	niche business
naffness	glitziness
nerdishness	scuzziness
wimpishness	greenness
friendliness	global consciousness
environmental friendliness	environment-consciousness
user-friendliness	ecoconsciousness
environment-friendliness	Nimbyness

As I said, various questions have been avoided here: *Friendliness* and *greenness* are both familiar forms with new meanings: Should they count as productive uses? Do the six instances ending in *-friendliness* represent six new cases of *-ness* formations (for a positive answer, see Baayen 1993: 200)? But in principle these questions can be answered, and a rate can be given. The rate is surprisingly low because such a method allows us to consider established words only, and thus omits precisely the data which might be considered to illustrate the real productivity of productive morphology.

Alternatively, two electronic corpora could be used, and the words in *-ness* listed in each of them could be listed, and if all the words in *-ness* appearing in the earlier corpus were deleted from the list of *-ness* words in the later corpus, the increase in words in *-ness* in the newer over the older (assuming there to be an increase) could be seen as the rate of productivity of *-ness*. There are practical reasons why this is not currently possible, to do with the lack of availability of large enough, strictly parallel, electronic corpora of an appropriate type. An alternative, a practical possibility at a few sites, is to take a corpus created by the regular addition of newly produced materials, and to consider the rate of addition of new forms.

What is the difference between these two approaches? Fundamentally, consulting a dictionary will give a very conservative idea about what is 'part of the language'. Neologisms are likely to be ignored, rare words are likely to be ignored and, of the established words which are considered, those words which the publishers consider unlikely to be of value to the target audience are likely to be ignored. This means that a dictionary will give a smaller number of words than are known in the language community. Consulting a corpus will allow rare, easily-interpretable, words and nonce words to be included, but the coverage is likely to be less complete

than that of a dictionary – at least, unless the corpus is extremely large indeed, or the dictionary very small.

Given this difference, the use of a dictionary as a starting point and the use of a (suitably large) corpus as a point of comparison could be a positive compromise. It is likely to prevent relatively rare established words being taken as neologisms, and it allows the genuinely productive use of morphological processes to be seen. This can be seen as a way of maximising the likelihood of showing the full extent of productivity of productive morphological processes without exaggerating it unnecessarily. It is likely that such a process will provide some kind of compromise on the time dimension, since neither the dictionary nor most corpora can be given an absolute date. However, this may not be a major difficulty. Dressler (1997: 15) comments that any such approach is 'of little relevance' because it refers to 'language as norm' and omits reference to true potential. I would argue that such an approach does permit us to look beyond norms, because a corpus allows the discovery of words which are not authenticated by the norm; furthermore, true potential is limited by what Baayen and Lieber (1991: 818) call the 'pragmatic usefulness' of the morphological process, and this is one of the factors that prevents all morphological processes being of equal productivity.

We are now in the position to return to figure 5.4 and to reconsider it in such a way that t_1 represents a view of the vocabulary taken by a dictionary, and t_2 represents a view of the vocabulary taken by a corpus. The measurement of (a–b), the number of words of the appropriate type which are found in the corpus but not in the dictionary, gives something which may be taken as a measure of the increase in appropriate forms over the time period between the completion of the dictionary and the final date of material in the corpus. This measure in itself is relatively meaningless, but a comparative measure of productivity can be gained if two (or more) similar measures are taken. Consider figure 5.6.

To the extent that

$$\frac{(a - b)}{(c - d)} > 1$$

we can say that Process X is more productive than Process Y. Following Baayen, we can say that if Process Y in such a comparison is the creation of simplex words, we can give a value for the productivity of Process X in this comparison. Clearly, the larger the corpus, the higher the value given for the productivity of any particular process is likely to be. This is in

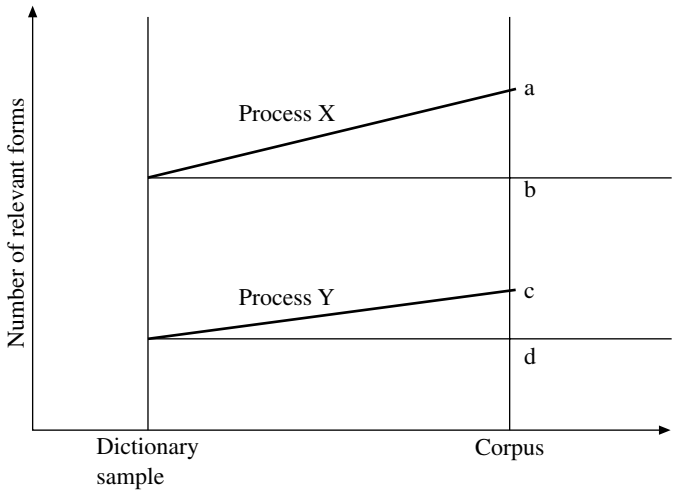

Figure 5.6: Comparison of the productivity of two morphological processes

direct contrast with Baayen's measure of productivity, $\mathcal{P}$, which becomes smaller as corpus size increases. But the comparative ratio with the rate of increase in simplex words need not grow. In this sense, the figures provided by this measure are more stable than Baayen's, though they are dependent on the size of the dictionary used for the initial comparison.

What has been said here may seem overly dismissive of the value of dictionaries in the measurement of productivity. At the same time, it cannot be denied that some significant achievements in the measurement of productivity have been made on the basis of dictionaries, including some of the most recent and most important work in the area (Bolozky 1999; Plag 1999). The reason for my attitude here is the conviction that the most productive processes (such as *-able*, *-er* and *-ness* in English) are not regularly included in dictionaries, and that any dictionary-based measure will thus underestimate productivity in these crucial cases.

An interesting exception to this general rule is provided by Anshen and Aronoff (1997). They use the quotation material in *The Oxford English Dictionary* to show when the pattern of preposition + verb ceases to be productive for different prepositions (e.g. words like *ofask*, *ofstand* and *ofswink* are not used after 1400, while words using the preposition *over*, such as *overcompensate*, continue to the present day, and this pattern continues to be used to create new words). Because the treatment of the

relevant material is consistent through the dictionary with no possibility of current productivity clouding the data, this piece of work avoids the problems of dictionary-based studies in general, and indeed exploits the strengths of *The Oxford English Dictionary*.

Plag (1999: 97–100) argues specifically that weaknesses which hold for other dictionaries in this regard do not hold for *The Oxford English Dictionary*. He does, however, admit that lexicographers for *The Oxford English Dictionary* appear to be less aware of some types of coinage than of others. While I would agree that use of *The Oxford English Dictionary* minimises the problems, it still seems to me that there are problems of principle here, especially – as Plag acknowledges – when it comes to ranking productive processes. It should also be noted that a resource as complete as *The Oxford English Dictionary* exists for very few languages, and that advocating such a method as a general rule may not be as successful as it can be in English.

Dictionaries, then – and especially a dictionary like *The Oxford English Dictionary* – provide a mine of information about the productivity of morphological processes, and some of it not easily obtainable elsewhere (for example, data on conversion). Nevertheless, they provide rather biased information, and this has to be borne in mind. It is, thus, encouraging to find both Plag (1999: 117) and Bolozky (1999: 194) agreeing that (to use Bolozky's words) 'comparative findings from multiple methodologies would constitute a reliable evaluation measure of morphological productivity in any language.' Bolozky's work also illustrates how far such comparative findings support each other. While this many-pronged approach to the measurement of productivity seems useful in the short term, it is unlikely that it will generate any more robust measure of productivity than those we already have.

5.4 Summary

We began this chapter by considering the major types of constraint that can affect the extent to which a particular morphological process can be used in the production of new words and concluded that even taking into account the large number of possibilities provided by such constraints, it did not seem justifiable to see productivity as the inverse of the number of constraints applying to particular affixes. What is left unexplained by this approach provides the core for attempts to measure productivity in a more rigorous manner. So we then considered various proposals from the

literature for measuring productivity. We saw that the best measures currently available are indirect, and that there are problems which even the best available methods do not solve, in particular comparability between corpora (or languages, etc.). Perhaps what we are seeking is a measure which will tell us for any morphological process M

> What is the likelihood that the next word I meet which is formed by M will be a new word (or, an alternative formulation, a word which is not item-familiar)?

Unfortunately, there does not seem to be any simple way of assigning reliable values to such a measure.

6 *Exemplification*

> One picture is worth ten thousand words.
> Frederick R. Barnard, *Printers' Ink* (10 March 1927)

> Few things are harder to put up with than the annoyance of a good example. Mark Twain, *Pudd'nhead Wilson* (1894: ch. 19)

6.1 Introduction

In this chapter I introduce a number of examples whose function is to illustrate various aspects of the discussion that have taken place in earlier chapters. It is my hope that the discussion of these examples will provide a focus for some of the theoretical points which have been discussed, and point to conclusions about what productivity is and how it should be dealt with.

6.2 Proto-Germanic **-dōm*

Mention was made in section 1.2 of the fact that productivity of a given process can vary diachronically. The example of Proto-Germanic **-dōm* is chosen to illustrate this point. It will be shown that while some formations using this process are common across different Germanic languages (and it is thus possible, though not definite, that they are shared formations from the Common Germanic period), the use of the suffix in the daughter languages shows a great deal of variation – even though it has never become wildly productive in any of them. In some cases it seems that one meaning has ceased to be productive at all; in others some type has thrived at the expense of others. By starting with the Proto-Germanic form, we are able to show how different patterns of productivity have

shaped the vocabularies of the daughter languages. This indicates the importance of productivity in the long-term development of vocabularies, and also suggests the importance of considering productivity to be a linguistic matter, since it can affect systems as well as norms.

6.2.1 Background

Proto-Germanic had a nominalisation suffix *-*dōm*, which has passed down to many of the modern Germanic languages. In Germanic times this element may have been a free-standing word, so that original Germanic formations using it were compounds (Voyles 1992: 273). Certainly, a word *dōm* is found in Old English with the meaning 'judgement, statute, jurisdiction' (Onions 1966: 282), and this has passed into modern English as *doom*. A similar word is found in Middle High German (Fleischer 1975: 163). This element has reflexes in several modern Germanic languages. Here Danish, Dutch, English and German will be considered.

6.2.2 Danish

Holmboe (1978: 142) lists forty-six words of Danish containing the suffix *-dom*, half of them compounds with a second element in *-dom*. The list is apparently not complete (see e.g. Hansen 1967: 277), but very nearly exhaustive. We can distinguish the following sets of words:

(a) adjectival bases, with the noun in *-dom* having a collective sense: *rigdom* ('riches' from *rig* 'rich'), *visdom* ('wisdom' from *viis* 'wise');
(b) adjectival bases, with the noun in *-dom* having a stative meaning: *dårligdom* ('misery, wickedness, illness' from *dårlig* 'bad'), *fattigdom* ('poverty' from *fattig* 'poor'), *sygdom* ('sickness' from *syg* 'ill'), *ungdom* ('youth' from *ung* 'young');
(c) adjectival bases with the noun in *-dom* having a concrete meaning: *helligdom* ('sacred object or place' from *hellig* 'holy');
(d) verbal bases with the noun in *-dom* having an abstract meaning: *lægedom* ('cure' from *læge* 'cure'), *lærdom* ('scholarship' from *lære* 'learn'), *spådom* ('prophecy' from *spå* 'prophesy'), *straffedom* ('sentence, judgement' from *straffe* 'punish');
(e) verbal bases with the noun in *-dom* having a concrete meaning: *ejendom* ('property' from *eje* 'own (verb)');
(f) nominal bases with the noun in *-dom* meaning a period of time: *alderdom* ('old age' from *alder* 'age'), *barndom* ('childhood' from *barn* 'child');

(g) nominal bases with the noun in *-dom* meaning a quality: *jomfrudom* ('virginity' from *jomfru* 'virgin'), *manddom* ('manhood' from *mand* 'man'), *trældom* ('slavery' from *træl* 'bondsman, slave');
(h) nominal bases with the noun in *-dom* being an abstract noun with some other meaning: *jødedom* ('Jewry, Judaism' from *jøde* 'Jew'), *trolddom* ('magic, wizardry' from *trold* 'troll').

There is a separate suffix *-dømme* which means 'power, area of power' as in *bispedømme* ('bishopric' from *bispe* 'bishop'), *hertugsdømme* ('duchy' from *hertug* 'duke'). Holmboe (1978: 48) lists nine basic words with this suffix, although several of those nine also occur in compounds or further derivatives. This suffix is derived from the suffix *-dom*, with which it was identical in the Old Middle Danish period (1100–1350) (Skautrup 1944: 290), but from which it had become distinguished before 1500 (Skautrup 1947: 53).

According to Hansen (1967: 277) the suffix *-dom* is seldom used in new coinages, and those he cites seem to be intended humorously, and not to have become established. In terms of societal productivity, therefore, it appears that *-dom* is no longer available in Danish, having been used only sporadically since the sixteenth century (Skautrup 1947: 236), and there are no new forms registered in the period 1968–72, which is the only recent period for which I have data (Holmboe, personal communication).

6.2.3 Dutch

De Haas and Trommelen (1993: 253) distinguish between three groups of nouns with the suffix *-dom* in Dutch:

(a) nominal bases denoting living creatures (typically but not exclusively persons) with the noun in *-dom* having a collective meaning: *mensdom* ('humankind' from *mens* 'person'), *lekendom* ('lay people' from *leek* 'layperson'), *studentendom* ('students as a body' from *student* 'student'); the plural or linking *-en* marker is used on the end of the base noun when the noun would otherwise end in a plosive;
(b) nominal bases denoting people with the noun in *-dom* denoting the jurisdiction or area of jurisdiction of the person named in the base: *hertogdom* ('duchy' from *hertog* 'duke'), *prinsdom* ('princedom' from *prins* 'prince');
(c) adjectival bases with the noun in *-dom* denoting something abstract: *eigendom* ('ownership' from *eigen* 'own (adjective)'; also

used in a concrete sense to mean 'property'), *rijkdom* ('wealth, riches' from *rijk* 'rich').

As a special case under (c), de Hass and Trommelen (1993: 254) mention the word *wasdom* 'growth, development' derived from the verb *wassen* 'grow'. They claim (1993: 253) that none of these patterns is available for new coinages.

6.2.4 English

Marchand (1969: 262–264) distinguishes several different senses of the English suffix *-dom*, though the various meanings may overlap. He notes in particular that the suffix is 'very productive' and that in modern words (which are often not established – Marchand [1969: 263] says 'not exactly recognized as standard vocabulary') there is frequently a depreciatory tone. The senses identified are:

(a) nominal bases with the noun in *-dom* denoting a territory: *duke-dom*, *earldom*, *kingdom*;
(b) nominal bases with the noun in *-dom* denoting a collectivity: *flapperdom*, *gangsterdom*, *officialdom*;
(c) nominal bases with the noun in *-dom* denoting neutrally a state or condition: *bachelordom*, *stardom*, *wifedom*;
(d) nominal bases with the noun in *-dom* meaning the 'world of' the item denoted by the base, frequently in a figurative sense, and clearly deriving from the meaning shown in (a): *boydom*, *fairydom*. This meaning also occurs with the names of animals in the base: *cattledom*, *puppydom*. Some of these nouns have a base in which the noun denotes a person, where the *-dom* derivative characterises these people as a class (see (b) above): *spinsterdom*, *teacherdom*, *yankeedom*. The set of words denoting a group with a common interest need not always have a person in the base: *crossword-puzzledom*, *moviedom*, *taxidom*;
(e) a few rare *-dom* derivatives based on adjectives (*awaredom*, *free-dom*, *topsy-turveydom*, *wisdom*) and verbs (*boredom*, *listendom*).

6.2.5 German

Erben (1975: 77) distinguishes between two meanings of the German suffix *-tum* (the cognate suffix): a noun of quality and a noun of collectivity.

He illustrates the former group with the word *Kennertum* ('connoisseurship' from *Kenner* 'connoisseur'), which he equates with *Kenner-Sein* ('being a connoisseur'), and the latter type with *Bürgertum* ('the middle classes' from *Bürger* 'citizen'), which he contrasts with *Bürgerschaft* which he glosses as 'the totality of all the citizens in a town'.

Fleischer (1975: 163–164) points out that although the majority of instances of words in *-tum* have bases denoting persons (as in the two instances illustrated above and the territorial meaning associated with *Herzogtum* ['duchy' from *Herzog* 'duke']), there are some older formations which show different patterns, though these other patterns do not appear to be societally available. He cites:

(a) adjectival bases: *Deutschtum* ('the German character, the Germans' from *deutsch* 'German'), *Eigentum* ('ownership, property' from *eigen* 'own (adjective)'), *Heiligtum* ('relic, sanctuary' from *heilig* 'holy'), *Reichtum* ('riches' from *reich* 'rich').
(b) verbal bases: *Irrtum* ('error' from *irren* 'err, wander'), *Wachstum* ('growth' from *wachsen* 'grow').
(c) non-human noun bases: *Altertum* ('antiquity' from *Alter* 'age'), *Besitztum* ('possession(s)' from *Besitz* 'possession'), *Schrifttum* ('literature' from *Schrift* 'writing').

6.2.6 Comparison

According to De Vries (1971: 125) the suffix *-dom* is a west Germanic suffix which was borrowed into north Germanic; it is nevertheless quite widespread within north Germanic and its development there can be treated as a separate development of the same affix. The suffix was originally used indifferently on bases which were adjectives, nouns or verbs (Voyles 1992: 273). However, *-dom* formations on verbal bases are extremely rare in all the languages considered (the more so since Danish *ejendom* cited above as a relevant example is actually a German loan word rather than a direct formation on a verb), and are apparently no longer productive in any of the languages considered. The set of words with adjectival bases is also very small. Those with a collective sense (e.g. Danish *rigdom*, Dutch *rijkdom*, German *Reichtum*; Danish *visdom*, English *wisdom*, German *Weistum* [now obsolete, formerly a specialised legal term, replaced in its general sense by *Weisheit*]) appear to go back to common west Germanic. *Heiligtum* is also found throughout west and

north Germanic. The development of the series of abstracts like Danish *ungdom* appears to be peculiar to north Germanic.

Formations with nominal bases have persisted much more in all the languages considered and show a number of distinct semantic developments. There is the meaning of 'territory, jurisdiction', in Danish denoted by a separate development of the suffix, found throughout west Germanic. In English this territorial meaning has become figurative and spread to the world as seen by particular groups (including *fandom*, *puppydom*, etc.). A general collective meaning is found in English, Dutch and German, but only sporadically in Danish (*jødedom*); in German at least this has become the main meaning, but with bases restricted to nouns denoting humans, while the same restriction has not applied in English, and, indeed, the bases have extended to the occasional adjective as in *topsy-turveydom*. A third meaning is a general abstract noun meaning, emotionally neutral (which many of the other derivatives, especially in English and German are not), as in Danish *manddom*, English *stardom*.

Of these meanings, the general abstract noun meaning is the most general and must be the oldest. The meaning of 'territory, jurisdiction' appears to be a separate development in west Germanic, with the collective meaning being derived from that (which explains why the collective meaning is not in general use in Danish).

6.2.7 Implications

The main point that emerges from this example is that restrictions on bases can change as part of the diachronic process of language change. In the common Germanic period, there appear to have been few restrictions on the use of *-dom*, even in terms of the part of speech involved (although we do not have enough information to be sure of how widely the process was used at this stage). In the modern languages this freedom has been severely curtailed: in Danish and Dutch the suffix is no longer productive at all (the maximum possible curtailment), in German it is productive only with bases which are nouns denoting human beings. In both German and English the collective meaning has become the main meaning of the suffix, but in English this has not led to the same restriction to bases denoting humans that is apparent in German. The collective meaning was also an important one in Dutch before the suffix became unproductive. In Danish the general abstract meaning was the most important one – or, at least, was the most important one in those words which have survived.

In all of the languages considered, many words have survived from earlier periods of the productivity of the affixation process, so that *rigdom*, *rijkdom*, *Reichstum* appear to represent a formation common to three languages, as do *visdom*, *wisdom*, *Weistum* (albeit a different three), but in each case the pattern upon which the word was formed is no longer available in the modern language, and in these cases has probably not been for several centuries. These words thus provide an excellent example of the process of lexicalisation, even though all of the 'riches' words remain analysable in their own languages (*wisdom* is not always psychologically linked with *wise*, presumably because of the non-productivity of the vowel shift rule, and *Weistum*'s peculiar history was commented on above).

It is not possible to tell from the available sources just how widely the suffix might have been used at earlier periods of the various languages considered. Skautrup (1944, 1947), for example, lists very few examples from older periods of Danish which have not persisted to the present day, yet it seems unlikely that all established forms (let alone nonce forms) have been captured in relevant texts. One need only compare the ninety-six basic words listed by Lehnert (1971) for English with the over 150 listed with the same suffix by Marchand (1969) and the over 300 listed by Wentworth (1941) to see that lexical listings are no accurate guide to potential – if any proof of this statement is required. Nevertheless, if we compare the twenty-three basic words in *-dom* listed by Holmboe (1978) for Danish (with another twenty-three further derivatives or compounds), with the ninety-six listed by Lehnert (1971) for English (with another eleven further derivatives or compounds), with the 111 basic words in *-tum* listed by Mater (1970) for German (with another fifty-five further derivatives or compounds) we get a picture of different levels of exploitation of potential, different degrees of generalisation, of the same diachronic suffix in the different languages. Note, moreover, that although we have the most strict restriction on possible bases in the German case, we also have a higher number of lexically listed attestations in that language, so that there is no clear relationship between constraints on possible bases and overall level of generalisation.

Even if we look only at the formations with the 'territory, jurisdiction' meaning, we find differences, as shown in table 6.1. Words equivalent to *Christendom* have been omitted in table 6.1 since the suffix in Danish is *-dom* rather than *-dømme*. With some other words it is not clear whether the 'territory, jurisdiction' meaning is the main meaning or not, and in general such words have been omitted.

Table 6.1: *Comparison of* -dom *formations with the meaning 'territory, jurisdiction' from three Germanic languages (from Mater 1970, Lehnert 1971, Holmboe 1978)*

Danish	English	German
bispedømme ærkebispedømme		Bistum* Erzbistum
	dogedom	
		Führertum
fyrstendømme	princedom	Fürstentum Erbfürstentum Kurfürstentum Großfürstentum Sedezfürstentum Duodezfürstentum
		Grafentum
herredømme eneherredømme masseherredømme overherredømme voldsherredømme verdensherredømme	lorddom	Herrentum Fronherrentum
hertugdømme ærkehertugdømme storhertugdømme	dukedom archdukedom	Herzogtum Großherzogtum Erbgroßherzogtum Erzherzogtum
	Kaiserdom	Kaisertum
kongedømme	kingdom	Königtum Wahlkönigtum Zwischenkönigtum
	peerdom	
pavedømme	popedom	Papsttum
præstedømme	priestdom	Priestertum Erzpriestertum
	queendom	
sheikdømme	sheik(h)dom	

Table 6.1: (*cont'd*)

Danish	English	German
	sheriffdom	
zardømme	tsardom czardom tzardom	Zarentum

* The irregular abbreviation of *Bischof* to *Bis* in *Bistum* is common to German and Dutch.

Some of the gaps in the derivations shown in table 6.1 are due to the lack of the appropriate base in the relevant language. For example, there is no real equivalent of English *peer* in Danish or German, and so no need for a word corresponding to *peerdom*. England did not have any *Kurfürsten*, and so no need for the word *Kurfürstentum*. In other cases, though, there is no apparent reason why a word which has apparently been needed in one of the three languages should not have been needed in one or more of the others: *queendom* in Danish and German, *sheikdom* in German, *grevedømme* (corresponding to German *Grafentum*) in Danish. These differences do seem to be differences in the extent to which this particular process has been used and, as such, differences in productivity in one sense.

One point which needs to be made in this connection is that in some cases the productivity of this suffix is limited by the productivity of another. The reason there is no *bishopdom* in English (corresponding to Danish, Dutch and German uses of the suffix) is that such a word is blocked by *bishopric*. An apparently potential English *countdom* (corresponding to German *Grafentum*) is blocked by *county*. *Princedom* coexists with *principality*, and the precise division of labour between the two words does not seem to be entirely settled. The reason there is no *emperordom* in English is because of *empire*. In this case, the use of *-dom* seems always to be blocked by quite specific words rather than by any single competing morphological process, but that need not be true in all cases.

6.2.8 Concluding remarks

It has been shown here that a single morphological process can, with the passage of time, take on quite distinct patterns of productivity: some patterns (such as the use of *-dom* on adjectives) can be discarded, others

added (such as the collective meaning); constraints can also change. The diachronic changes in patterns of productivity seem to provide one of the strongest reasons for seeing these as being part of what is covered by competence. The resultant patterns in Danish and German are very different, and would presumably be described by different sets of rules, yet each of these situations has emerged gradually from the same fundamental Germanic process. For the differences to emerge, the rules must change with time, and if the differences are shown by changes in the rules, they cannot be merely performance based. On the other hand, it is not clear whether the differences in the level of exploitation of a particular suffix (such as the 'territory, jurisdiction' meaning of *-dom*) are system differences or performance differences (and the status of blocking needs to be considered here, too): they certainly lead to differences in norms. What the German evidence shows is that profitability is not directly related to the number of constraints imposed on a particular process.

6.3 Nominalisations of colour words

The point of this next example is to look at the limits of constraints on bases in derivation. Constraints have been discussed in detail in section 5.2, where it was concluded that there might be two types of constraint: absolute constraints which work reliably and are part of competence, and variable constraints which work less reliably and may be part of performance. Here we consider some constraints which appear to be on the borderline between the two. Although some of them are types which are (or are taken to be) absolute elsewhere, here they are not absolute. At the same time, we see that the constraints interact with each other in such a way as to make some coinages less probable than others.

6.3.1 A problem and an experiment

In a passage already cited in section 2.3, Matthews (1974: 221–222) points out that people seem to have different attitudes to some nominalisations of colour words such as *purpleness* than to other words on the same pattern such as *whiteness*. He asks why it should be that if *the wall is white* and *the wall is purple* are both fully acceptable potential utterances, *the whiteness of the wall* should be so much better than *the purpleness of the wall*, and concludes that the difference lies in the semi-productivity of the *-ness* suffix (and, by implication, of all derivational affixes). In Bauer (1983) I stated that the difference was simply one of familiarity: a difference between words and sentences is that we expect the former but not

the latter to be item-familiar, and we are less happy with words which are not item-familiar even if there is no grammatical reason for this reaction.

Just recently I decided to test this conclusion further by asking students to rate various *-ness* nominalisations of colour words on a scale from 1 to 5, where 1 was totally acceptable, and 5 was unacceptable. Over a two-year period twenty-eight native-English-speaking third-year linguistics students took part in the experiment, the results for the two cohorts being indistinguishable from each other. The combined results are reported here.

It seemed to me that there were several potential sources of the difference Matthews observes, assuming that Matthews's observation is indeed correct. First, it might be a result of the familiarity or otherwise of the base colour-term. Second, it might be connected with the etymology of the base colour-term, with Germanic colour names being more available for *-ness* suffixation than Romance or Greek ones. Third, it seemed to me as I considered the available colour-terms, that there might be a difference between those which are monomorphemic and those which are derived. Fourth, it seemed to me that there might be a distinction to be drawn in terms of the length of the base. In order to test these assorted hypotheses, I came up with the list of colour terms shown in table 6.2 to use as base items.

Table 6.2: *Colour words used as bases in the experiment*

		One syllable	Two syllables	Three syllables	Longer
Familiar	Germanic	white	yellow golden†		
	Non-Germanic	pink	purple		
Less usual	Germanic	tan	snowy†	silvery†	
	Non-Germanic	beige	scarlet	violet	
Rare	Germanic	dun	sallow creamy†		periwinkle
	Non-Germanic	puce	cerise	magenta	heliotrope

† These forms are derived.

The status of *pink* is a bit odd; it doesn't appear to be Germanic, despite its phonological structure, but its origin is unknown. It is perhaps misleadingly counted as non-Germanic. Most of the blanks in the table are the result of colour-terms of the appropriate structure being unavailable in English.

Familiarity in the above table is based on frequency in a number of corpora (I should like to thank my colleague Ave Coxhead for running these words through her collection of corpora for me). The familiar terms

had a frequency of over 220 in a corpus of seven million words. The less usual words had frequencies of 20–120 in the same corpora (except *beige*, which had a higher frequency in the first corpus I tried, but came out lower overall). The rare words had frequencies of less than ten. Of course, some of the frequencies were made up by non-colour words (e.g. *White* as a surname, *Snowy* as the name of an Australian river), but in most cases it seemed that the frequencies held taking these factors into account. There were border problems between the rare and less usual words, which were clearly divided in small corpora, but which were not absolutely clear-cut once very large corpora were considered.

The various hypotheses will be considered individually below.

6.3.2 Familiarity of the base

First consider the question of the familiarity of the base. The relevant figures for one- and two-syllable words are provided in table 6.3. The Wilcoxon Ranked Pairs Test is used rather than a parametric test since ordinal-scaled data is used and the assumptions required for a parametric test are questionable over this type of data.

Table 6.3: *Acceptability ratings depending on the familiarity of the base*

	Mean	SD	Asymptotic Z statistic for Wilcoxon Ranked Pairs Test		
Familiar	1.80	0.46	Familiar – Less usual	–4.511	$p < 0.000$
Less usual	2.85	0.66	Less usual – Rare	–0.752	$p = 0.452$
Rare	3.17	0.41			

While it is significantly easier to form *-ness* words from the most familiar words than the others, there is no monotonic increase in difficulty with even less familiar words. The addition of *periwinkle* and *heliotrope* to the calculation might give rise to the expected gradient here, but their difficulty could be due to the number of syllables they contain and so they have been excluded from consideration here to keep the statistics unbiased.

6.3.3 Etymology

Next consider the etymological hypothesis. The acceptability ratings for the non-derived Germanic and non-Germanic bases are given in table 6.4. The hypothesis that different etymological classes of base provide derivatives of differing degrees of acceptability does not appear to be correct.

Table 6.4: *Acceptability ratings for words with different etymological classes of base*

	Mean	SD	Asymptotic Z statistic for Wilcoxon Ranked Pairs Test	
Germanic	2.78	0.38		
Non-Germanic	2.93	0.44	–1.721	$p = 0.085$

6.3.4 Morphology of the base

Third, consider the question of derived versus non-derived bases. Strictly there are only two pairs of really comparable words, here: *yellow* and *golden* and then *sallow* and *creamy*. All the others differ in terms of their etymology as well. In table 6.5 the acceptability figures for the class made up of *yellow* and *sallow* are compared with those for the class made up of *golden* and *creamy*, and they are not distinct. However, if we consider trisyllables only, with *violet* and *magenta* on the one hand and *silvery* on the other, then there is a marked difference (table 6.6).

Table 6.5: *Acceptability ratings depending on the morphological status of the base, matched data*

	Mean	SD	Asymptotic Z statistic for Wilcoxon Ranked Pairs Test	
yellow and *sallow*	2.11	0.61		
golden and *creamy*	1.79	0.59	–1.635	$p = 0.102$

Table 6.6: *Acceptability ratings depending on the morphological status of the base, trisyllabic bases*

	Mean	SD	Asymptotic Z statistic for Wilcoxon Ranked Pairs Test	
magenta and *violet*	3.45	0.60		
silvery	2.38	0.95	–3.883	$p < 0.000$

This creates a minor paradox, since neither etymological origin nor derived status has been found to make a difference on its own (tables 6.4 and 6.5), yet in trisyllables the effect of the combination is striking.

6.3.5 Number of syllables

Finally, consider the number of syllables of the base. It can be seen from table 6.7 that there is a general tendency for words to become less acceptable as they get longer, but it is not maintained at the short end of the scale. Non-derived two-syllable words are slightly better bases than monosyllables.

Table 6.7: *Acceptability ratings for words with different length bases*

	Mean	SD
Monosyllabic	2.69	0.43
Disyllabic (not derived)	2.51	0.41
Trisyllabic	3.45	0.60
Longer	3.75	0.80

If the overall significance of this distribution is tested by a Friedman Test (again a non-parametric test, for the reasons outlined above), then the probability is less than one in a thousand that the distribution arises purely by chance. But we then need to consider the results of Wilcoxon Ranked Pairs Tests to pin the difference down more accurately. This is done in table 6.8. Thus monosyllabic and disyllabic bases are not clearly distinct types for forming *-ness* words and neither are trisyllabic and longer bases; but there is a clearly significant difference between these two larger groupings, such that shorter bases give higher acceptability ratings. Base length is a critical factor in determining acceptability judgements.

Table 6.8: *Asymptotic Z statistic for Wilcoxon Ranked Pairs Tests; data from table 6.7*

Monosyllabic – Disyllabic	–2.131	$p = 0.033$
Disyllabic – Trisyllabic	–4.449	$p < 0.000$
Trisyllabic – Longer	–1.635	$p = 0.102$

6.3.6 Discussion and implications

On the basis of these results it seems likely that all of the hypotheses suggested had some truth to them. Even though etymology and morphological

make-up of the base do not show up as individually significant factors in contributing to the acceptability of abstract *-ness* words, in combination they appear to affect trisyllabic bases. Familiarity and base length (which may, of course, be linked) are both significant factors over some part of the range. The implication of this is that there are discoverable reasons why people accept *whiteness* but not *purpleness*: predictions about acceptability can be made on the basis of the various categories discussed here. Not only are these distinctions linguistic, mainly systemic distinctions, they are also distinctions of degree, not a simple matter of yes or no. To the extent that there are discoverable constraints in operation, it seems that we do not need to work with a notion of semi-productivity, as Matthews (1974) suggests. Rather, it seems, even variable constraints may be matters of competence and interact with each other in such a way as to bring about gradient productivity.

The evidence provided by this small-scale experiment makes a case for a more complex view of constraints on bases than was presented in section 5.2. There, the individual constraints were presented as though each was independent of the others. Here we see that the various constraints may interact in complex ways, making the actual outcomes more difficult to model and predict. Also, a new kind of constraint has been introduced here, that of the familiarity (measured in terms of token-frequency in some supposedly representative corpus) of the base. Although token-frequency is known to influence language (e.g. in protecting some items from language change), it would seem very odd to attribute it to the language system in any way. To this extent at least, then, performance factors do seem to influence profitability.

6.4 Nominalisation endings in English

The next example is concerned with nominalisations of English verbs and is based on a sample of *The Oxford English Dictionary* (second edition). Every page whose number was divisible by 20 was read for nominalisations. A total of 574 nominalisations were found. This provides a dictionary corpus for the discussion in sections 6.4.1–6.4.5.

This extended example considers in detail a case of competing morphological processes. In his study of competing verbalising processes in English, Plag (1999) concludes that they are largely in complementary distribution. This being the case, the coiner has virtually no choice of form when coining a new verb. This explains how it is possible for the speaker to create a particular word in the face of an apparent plethora

of choice. Plag (1999: 228) also concludes that where complementary distribution fails and there are no constraints forcing the choice of one morphological process above another, either may be used and doublets are possible. He cites cases such as the synonymous *dandify*/*dandyise* and *plastify*/*plasticise* to illustrate his point. With the nominalisations to be considered here, there is a greater range of apparent models for the coinage of new forms, and it becomes clear that the strict complementary distribution found by Plag in his data has not always held.

Later in the section, the different measures of productivity discussed in section 5.3 will be evaluated against data from English nominalisations.

6.4.1 Minor nominalisation types

Starting with the sample from *The Oxford English Dictionary*, I report first on those nominalisation types which were infrequent and whose productivity is, for a number of reasons, clearly low.

(a) Suffix *-t*

This suffix was found in five words. Three of these were cases of prefixation from other words and it cannot be assumed that the affix is added independently to the prefixed form. Such examples (*counterweight*, *overdraft* and *overslaht*), therefore, cannot be taken to illustrate productive use. One of the other examples (*perceit*) seems to have been based directly on the word *deceit*. The only word left for serious consideration, *draught*, was formed by 1398. There is no evidence of more recent productivity with this suffix, though it is widely used in other Germanic languages.

(b) Suffix *-ter*

As is well known, this suffix occurs clearly only in the word *laughter*, which was formed before the Old English period. There is no evidence of more recent productivity for this suffix.

(c) Suffix *-s*

This suffix (if it merits the name: it might be better viewed as an instance of consonantal apophony) was found in five words only: *consense*, *excess*, *misdispense*, *summons* and *suspense*. *Misdispense* is formed by prefixation from an earlier *dispense*. The others are borrowings from French. All are attested by 1450. There is no evidence of any productivity for this process in English.

(d) Suffix *-th*

Although this suffix is rather more widespread than the others mentioned here, it is not particularly common when added to verbs. *Mowth* 'mowing' (an eighteenth-century reworking of an older *math*) was the only relevant word in my sample, though words such as *stealth* are better-known examples. The formation of *mowth* is perhaps surprisingly late, but the word never seems to have been used a great deal, and was perhaps never fully established.

(e) Suffix *-is*

This suffix was found only in the Greek loan words *emphasis* (compare with the verb *emphasise*), *metastasis* and *reanalysis*. It has probably never been an English affix.

(f) Suffix *-ency*

This suffix was found with five words, all of which have competing forms in *-ence*. The most familiar of these words is *insurgency*. In most cases, they could be derived from adjectives (e.g. *insurgent*) rather than verbs (though the verb *insurge* is also listed).

(g) Suffix *-ancy*

This suffix was found in only two words. The first, *delirancy*, might be derived from an adjective or a verb and might, in any case, be a direct loan from Latin. *Perceivancy*, which does seem to be an English-coined word, appears to have had only a short life, with competing nominalisations winning out, and eventually rendering it obsolete.

6.4.2 Moderately successful nominalisation affixes

The suffixes *-age*, *-al*, *-ance*, *-ence*, *-ery*, *-ure* and *-y* appear on more words than those illustrated in section 6.4.1. Examples of the relevant formations are *blockage*, *appraisal*, *observance*, *insurgence*, *jugglery*, *exposure*, *adultery*. For all of these affixes it is true that:

- Many of the examples of the affixes are on words which have become obsolete.
- Many of the words apparently containing these suffixes are in fact loans.
- Many of the words containing (or apparently containing) these affixes have competing forms with other suffixes (frequently more productive ones).

Table 6.9: *Numbers of nominalisations from the sample in various categories*

	-age	*-al*	*-ance*	*-ence*	*-ery*	*-ure*	*-y*
Total number in sample	14	12	16	11	7	23	8
Obsolete words in sample	4	2	8	2	0	9	3
Loan words in sample	2	0	7	7	2	12	5
Words with competing nominalisations in sample	6	6	13	7	5	19	4
Instances of prefixation in sample	2	4	1	2	0	4	1

Details are given in table 6.9. Although the small numbers involved make it difficult to be precise, we can attempt to date the maximal productivity of each of these suffixes as nominalisation markers (bearing in mind that none of them has ever been particularly productive in this use):

-age late nineteenth century
-al late seventeenth century, early eighteenth century
-ance fifteenth century
-ence early nineteenth century
-ery late seventeenth century
-ure early seventeenth century
-y seventeenth century

Note that the competition between these nominalisation processes appears to have been greatest during the seventeenth century, at the time when the so-called 'ink-horn' terms were being deliberately introduced to expand the vocabulary of English.

6.4.3 Successful nominalisation processes

Three nominalisation processes stand out as having been particularly successful in the history of English: conversion, *-ment* suffixation and suffixation of various forms ending in *-ion*. These will be discussed in turn, and then compared.

(a) Conversion

A total of 104 nominalisations in my sample were instances of conversion; of these only thirteen were loan words, and thirteen were cases of prefixation on established nominalisations. Conversion is found in all

periods of the history of English, from the Old English period to the present day; however, in the sample, it appears to have been particularly successful in the late sixteenth and early seventeenth centuries. The vast majority of the verbs which become nouns by conversion are morphologically simple, phrasal verbs and other verbal compounds including a preposition/adverb providing a major class of exception to the general rule. With such verbs, there is sometimes a stress distinction between noun and verb (*overdose*) and sometimes not (*overload*). Morphologically complex words except for these verbs frequently have other nominalisations competing with the form arising by conversion, with the suffixed form the more successful (e.g. *disappointment* competes with *disappoint*, *implantation* competes with *implant*).

(b) Suffix *-ment*

There are 69 nominalisations in *-ment* in my sample, of which 24 are loans. As an English affix, this suffix appears to have been productive between the mid-sixteenth century and the mid-nineteenth century. Twenty-four of the words in the sample are now obsolete, a rather higher proportion than for instances of conversion, but there are only four cases of prefixation on a known base. Only six of the words in my sample show *-ment* being attached to a monosyllabic base, including the word *basement* which may be derived from a noun rather than a verb. Bases in general are either etymologically complex (especially with allomorphs of the Latin prefixes *ad-*, *con-* and *de-*) or morphologically complex (especially with the prefixes *be-*, *dis-* and *en-*). However, a word like *blandishment* shows that the affix can be applied in other cases as well.

(c) Various suffixes ending in *-ion*

It seems probable that all these affixes should be treated as allomorphs of the same morpheme, as argued by Aronoff (1976: 99–105). The form *-tion* occurs in only two words in the sample (*mixtion* which is a loan word, and an instance of prefixation from an established base); the form *-ition* occurs in only six, of which at least five are loans; the form *-ution* occurs in two words in the sample, one a loan and one a case of prefixation from an established base. This leaves *-ion*, *-ation* and *-cation* to be discussed.

There are 177 examples of *-ion* in the sample, of which 39 are cases of prefixation and 106 are loan words. The remaining forms show productivity from the beginning of the seventeenth century through to the twentieth century, but always on Latin or French bases. A word like

alition is of interest since it clearly shows the suffix being used on a Latin base, even though there is no corresponding verb. Occasional examples show the affix being used on bases other than verbs (*mammillation*). The vast majority of cases of English word-formation show *-ion* being added to a base ending in *-ate*.

The form *-cation* is added almost exclusively to bases ending in *-ify*. The sole exception in the sample is *canonication* (synonymous with the more widely used *canonisation*, which is older). It may be that this is not a regular formation at all.

The form *-ation* is most productively added to bases in *-ise*. Of the instances of English word-formation in the sample that are not cases of prefixation, 41 show *-ation* added to *-ise*, while 13 show *-ation* added to something else. Of the 41 which show *-ation* added to *-ise*, the majority are formed in the nineteenth and twentieth centuries, although *anathemization* (1549; obsolete) and *meliorization* (1599) show that the pattern is older. The instances where *-ation* is added to a base not ending in *-ise* are more varied, as can be seen in table 6.10.

Table 6.10: *Words from the sample with* -ation *added to a base which does not end in* -ise

Example	First citation	Comments
bouleversation	1667	competes with *bouleversement*
chatteration	1862	competes with *chatter* and *chatterment*
consultation	1548	
cutization	1882	no corresponding verb; based on Latin *cutis* 'skin'
elicitation	1656	
encystation	1869	competes with *encystment*
gelatination	1796	
nonplussation	1833	
screation	1658	no corresponding verb
severation	1648	
siltation	1932	
spreadation	1810	competes with *spread*
transphosphorylation	1943	no corresponding verb

The nineteenth century examples here are either (a) technical terms or (b) terms which are attempts to sound more technical than is called for

by the context. I assume that *chatteration* is stylistically marked with respect to *chatter*, being jocular because too technical in style. This is in line with the comment made by Plag (1996: 779), when he says

> The jocular and vulgar words among the counterexamples [to the constraint that Latinate suffixes should occur only on Latinate bases, LB] even support the etymological constraint, because their pragmatic effect originates in the systematic violation of the constraint. Thus the use of a learnèd suffix with a nonlearnèd stem usually has a humorous effect.

6.4.4 Comparison

Synchronically, in present-day English, there are two ways of forming nominalisations: by conversion, or by suffixation of the relevant form ending in *-ion*. Of these, *-cation* is added only to verbs ending in *-ify*; *-ation* is added predominantly to verbs ending in *-ise*; *-ion* is added predominantly to verbs ending in *-ate*. Since all such verbs are morphologically complex and affixed, it is no surprise to find conversion being used mainly in cases where no affixation is involved. At this level we have complementary distribution of the various processes, and thus no choice of nominalisation process in the majority of cases.

Problems arise with morphologically or etymologically complex words which do not have an established nominalisation. In the seventeenth and eighteenth centuries, these would normally have received nominalisations in *-ment*. Today, with *-ment* on the wane, it may be that *-ation* is taking over the slot. While *-ation* is sometimes possible in such places, it seems that it is restricted because of the style of such words: *-ation* added to a base which is not a suffixed verb seems to give a slightly jocular because over-formal impression, as with *chatteration* cited above.

If we go back to earlier times, the situation may not have been so simple. In the second half of the sixteenth, the first half of the seventeenth and the first half of the nineteenth centuries in particular, nominalisation processes appear to have been in direct competition for the available verbs. It should be noted that these are times of rapid vocabulary expansion, and in particular the earlier of these periods coincides with the period where ink-horn terms were deliberately introduced into English to raise the status of the language (as was seen at the time). Latinate-sounding nominalisations were thus in particular demand, with the French not far behind. Many of the nominalisations noted by *The Oxford English Dictionary* appear to have had very short lives, sometimes being used by an individual writer, or making a brief bid to oust an already established

nominalisation, which proved to be too strong. What appears to have changed is the degree of standardisation in language these days. If we look in texts rather than in dictionaries, we can still find some unexpected nominalisations; consider the following:

> a ground mist had delayed the departure of his flight by some three hours and the 'embuggerance' factors had simply multiplied from there on. (Egleton, Clive, *Picture of the Year*. London: Hodder and Stoughton, 1987: 137)

> the memoirs of Koko . . . This recountal completes a new troika of captive great ape sagas. (Sebeok, Thomas A., *I think I am a Verb*. New York: Plenum, 1986: 205)

Furthermore, it has presumably always been the case that an alternative nominalisation ending will be used to avoid the particular connotations of an established one. But, on the whole, the system appears to be far more settled now than it was in the seventeenth century, no doubt because the eighteenth century grammarians and lexicographers were more successful than we like to believe in 'fixing' the language.

6.4.5 Relative productivity of different processes

If we consider the productivity of the processes discussed in sections 6.4.1 and 6.4.2 above through history, the figures provided by the sample are meaningless, since there are so few examples. Even a relatively successful English suffix like *-age* rarely has more than two new coinages being registered by *The Oxford English Dictionary* in any fifty-year period of history. With the most productive patterns, though, some conclusions can be reached. Figure 6.1 is a graph showing the relative productivity of five nominalisation processes from 1600 to the present-day. Raw numbers are not used, since these are not particularly helpful, being subject to the different rates of registration of new nominalisations by *The Oxford English Dictionary*: fewer nominalisations are registered in the early 1800s than in the preceding or following periods. Instead the percentage which each formation-type contributes to the new nominalisations registered in each period is used as a measure – compare Bolozky's (1999) R2. This does provide enough evidence to show something of a trend: a rise in the productivity of the *-ation* variant as *-ment* declines in productivity. This is presumably to be linked to the fate of the *-ise* suffix. Judging by the figures provided here, *-ify* seems to be becoming less important as a verbaliser, while *-ise* is increasing in use. Conversion seems to maintain a relatively constant rate of use right throughout the period, only once

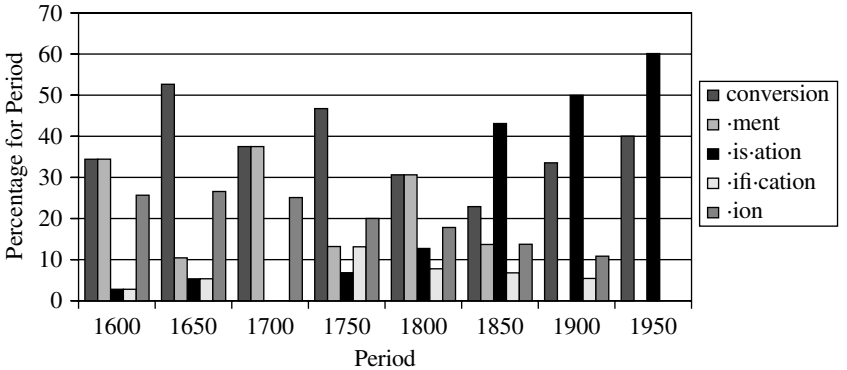

Figure 6.1: Relative profitability of five nominalisation processes

dropping below 30 per cent. While the figures for the latest period may be misleading because of the size of the sample registered in *The Oxford English Dictionary*, it is interesting that it is only in that period that conversion has been overtaken as the most profitable nominalisation process.

With a larger sample, and looking purely at numbers not percentages, Biese (1941) shows a similar pattern of peaks and troughs for conversion, but with the major peak slightly earlier than in figure 6.1 and another peak in the nineteenth century. These peaks correlate closely with the overall peaks of new word-formation so that a graph of number of cases of conversion divided by the number of recorded neologisms would remain relatively stable. Even if we use the data provided by Biese to get a function based on the overall number of neologisms in *The Oxford English Dictionary* (again based on a sample), my data still shows a boost for conversion in the eighteenth century and a rise for *-isation* in the nineteenth. This can be seen in figure 6.2, which plots for each period the ratio of

$$\frac{\text{Number of new nominalisations of relevant type in my sample}}{\text{Number of neologisms in Biese's sample}}$$

No figures are given for the twentieth century, since Biese's figures are based on the first edition of *The Oxford English Dictionary*. Since the samples are of different sizes, the figure for the ratio should not be invested with any particular relevance, but the relative figures may be meaningful.

6.4.6 Measures of productivity: corpus-based

We now move from a dictionary-based study of English nominalisations to a corpus-based approach, considering measures that have been discussed

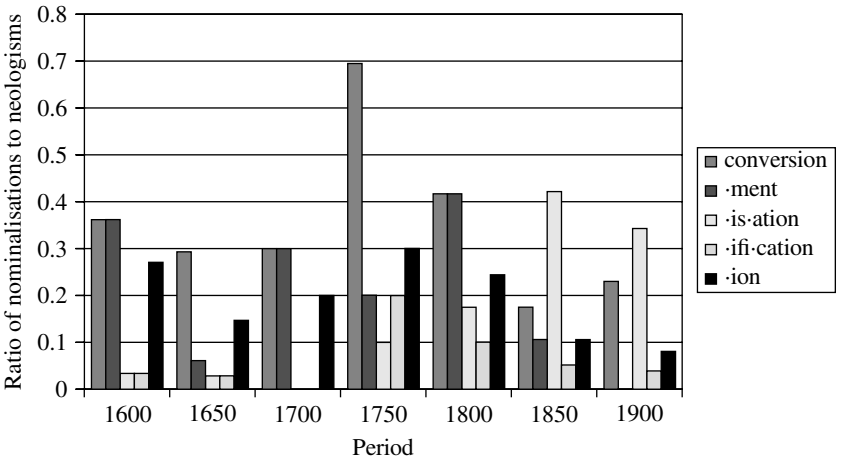

Figure 6.2: Ratio of new nominalisations to neologisms at different periods (for details see text)

from a theoretical perspective in section 5.3. Although it is not possible to quantify conversion in an un-parsed corpus, it is possible to quantify some of the affixal types of nominalisation. In this section quantification will be undertaken on the basis of the 1.1 million words of the Wellington Corpus of Written New Zealand English, ignoring those affixes which have a very low number of occurrences in the corpus. While a million words of running text is a small corpus for this kind of research (Baayen's results for English are based on a corpus of eighteen million words), the intention here is to consider the processes rather than to give definitive measures for individual affixes.

In section 5.3.2 it was pointed out that one possible measure of productivity is

$$I = \frac{V}{S}$$

where I is the index of productivity, V is the number of existing types, and S is the number of types which the word-formation rule could potentially give rise to. In that section, a number of theoretical problems with such a measure were discussed, in particular the impossibility of providing a figure for S. Without wishing to minimise the difficulties involved in any way, I should like here to propose a practical way round the obstacle to make the measure useable. When a corpus study is being done, V in

the corpus does not represent anything like the real number of existing types, but a selection from the existing types. Suppose we assume – and it is not a trivial assumption – that, for any affix, the corpus is likely to have sampled potential bases in the same proportion as it sampled existing types with that affix. Then, we can use the selection of possible bases provided in the same corpus as the number for S.

There are many instances where it is not possible to count the number of possible bases in this way. For example, there is no simple way of counting all possible bases for *-ment* suffixation in this way, since the relevant bases are not easily identifiable in terms of their morphological make-up. However, some of the occurrences of suffixes ending in *-ion* are predicted by the ending of the base, and these can be compared. The three types are those verbs ending in *-ate*, those ending in *-ify*, and those ending in *-ise*. Each of these will be considered below.

In the Wellington Corpus there are 271 types ending in *-ation*. In the majority of these types, the *-ate* is an etymological or phonological unit only, not a representative of a synchronic morpheme {ate}. Only 44 types might be said to contain the verbalising morpheme {ate} (the hedge is because it is not always clear with words like *evaluate* whether the root should be taken to be *value* or not; the figure of 44 is arrived at on the controversial basis that such words are indeed morphologically complex in today's English). Although a conservative analysis would have to work on the basis of this limited number of *-ate* words, it is not clear whether it is important that the *-ate* should be a real suffix: Barnhart et al. (1990) list *adenylation* as a new word from *adenylate*, even though *adenyl* is a bound base. Being unconservative, there are 321 verbs ending in *-ate* in the Wellington Corpus (again this does not distinguish those instances where the *-ate* is a genuine morpheme from those where it is an etymological or phonological string, though it does exclude monosyllabic roots). There are problems with taking these numbers at face value, since not all of the words in *-ation* are derivatives of the verbs in *-ate* that are in the corpus. Nevertheless, if we use the figures as a guideline, we get the result that

$$I_{ation} = \frac{V_{ation}}{S_{ation}} = \frac{271}{321} = 0.84$$

This figure in isolation must be treated with care, and it must be compared with other numbers before it can be fully interpreted. Nonetheless, it is a high figure, given that ex hypotheosi the lowest number that could occur in the denominator is the number in the numerator, so that the maximum productivity (read 'profitability') of a process should be 1.

There are 36 types ending *-ification* in the Wellington Corpus, and 57 types ending in *-ify*. Again the same warnings apply, but this gives the following result:

$$I_{\text{ification}} = \frac{V_{\text{ification}}}{S_{\text{ification}}} = \frac{36}{57} = 0.63$$

There are 56 types ending in *-isation* (or spelled with a <z>) in the Wellington Corpus, and 165 types ending in *-ise*. This gives:

$$I_{\text{isation}} = \frac{V_{\text{isation}}}{S_{\text{isation}}} = \frac{56}{165} = 0.34$$

This process thus suggests that the addition of *-ion* to *-ate* is more productive (profitable) than the addition of *-cation* to verbs ending in *-ify*, which is in turn more productive than the addition of *-ation* to verbs ending in *-ise*. We need to ask whether this result makes sense in terms of other things we know.

One point of comparison is provided by the new words listed in Barnhart et al. (1990). This listing is now rather dated, and in any case older than the Wellington Corpus (which collects data from 1986 onwards), nevertheless it provides the most recent substantial listing of neologisms which is easily available. The relevant figures from that dictionary are presented in table 6.11. If we merely consider the number of nominalisations listed in table 6.11, we see that *-isation* is by far the most profitable of the types in that corpus, in direct contradiction of the figures given above. We also see that there are more nouns listed ending in *-ification* than there are verbs listed in *-ify*, which contradicts one of the fundamental assumptions of this measure of productivity. This problem of overgeneration (Allen 1978) is not one which has been considered a great deal in relation to the question of productivity.

Table 6.11: *Number of nominalisations in three patterns listed in Barnhart et al. (1990), along with number of new verbs which form potential bases for these formations*

	-at(ion)	*-ifi(cation)*	*-is(ation)*
Verbs	14	5	36
Nominalisations	14	11	30

The extent of the problem is considerably greater than is indicated by the figures given in table 6.11. Fourteen, or almost half, of the nominalisations in *-isation* are derived from verbs which do not feature on the list. This phenomenon is, after all, the very reason that we need to have a category of back-formation – and three of the verbs in *-ate* on the list are created from *-ation*-words by back-formation. In terms of an overgenerating morphology that works on the basis of potential words this might not matter; the *-ise*-words are part of the linguistic potential, and thus part of what is permitted by the system. In terms of a more psychologically based morphology that derives words from existing words, this has important implications. It might be argued, for example, that data of this kind illustrates that some kind of analogy is more important in the formation of new words than rules are, since stages in the rule-governed sequence of forms can be missed out. This argument holds only, it seems to me, if it can be shown that the restrictions on what can take the suffix *-ise* are different in the two cases, and that seems unlikely (for the discussion of a similar case, see Bauer 1999a). An alternative formulation might be that nouns in *-isation* have two possible sources: the suffixation of *-ation* to a base in *-ise* or the suffixation of *-isation* directly to the nominal or adjectival base. In either case it suggests that a simple formula such as

$$\mathrm{I} = \frac{\mathrm{V}}{\mathrm{S}}$$

will not work for practical reasons as well as for theoretical ones.

If this measure does not work, we can move on to consider Baayen's measures. It will be recalled from section 5.3.3 that Baayen's basic measure of productivity is

$$\mathcal{P} = \frac{n_1}{N}$$

where n_1 is the number of words formed by the appropriate process occurring in a corpus precisely once (the hapax legomena) and N is the total token frequency of words created by that morphological process in the corpus. It will also be recalled that on the basis of a discussion of the productivity of *-ment* in the Wellington Corpus it was suggested that the base level for measurements of productivity in that corpus was round about 0.01 (the level of productivity shown by *-ment* itself). Against that

background we can go on to discuss the productivity of other nominalisation processes using this same measure.

Consider first the suffix *-age*. There are 29 different lexemes found in the Wellington Corpus which are nominalisations of verbs and which are formed with this suffix, including one (*miscarriage*) which could be seen either as the nominalisation of a verb or as a prefixed nominalisation. All of these 29 are item-familiar. The only recent words ending in *-age* that Marchand (1969: 234–236) lists are those where the *-age* derivative, formed from a nominal base, denotes a location (e.g. *orphanage*). All this would lead us to suspect that *-age* is no longer available in English, and so has no profitability at all. There are 12 relevant hapaxes in *-age* in the corpus, with the total number of occurrences of the suffix being 386, which gives

$$\mathcal{P} = \frac{n_1}{N} = \frac{12}{386} = 0.03$$

This suggests that the suffix is indeed productive. The result may be due to the small size of the corpus, but ought not to be, given that the size of the corpus has already been taken into account in setting the level at which affixes are deemed to be productive. It may be that the number of tokens of *-age* is not high enough to provide a reasonable measure. In any case, the result casts doubt on the measure being used.

The suffix *-ance*, with 49 different lexemes (all item-familiar), a total of 899 tokens and 7 hapaxes, provides a much more realistic value on this measure:

$$\mathcal{P} = \frac{n_1}{N} = \frac{7}{899} = 0.008$$

On the assumptions made above, this suffix is shown to be unproductive, which is what would be expected. A similar result is provided by *-ence* with 37 different types, 6 hapaxes and 820 tokens:

$$\mathcal{P} = \frac{n_1}{N} = \frac{6}{820} = 0.007$$

Again this is an expected result.

The values for deverbal *-th* are more difficult to calculate, given the small number of relevant types, the number of such types which involve compounds, and the controversy in deciding what is or is not a relevant type. There are only 10 relevant types: *aftermath*, *berth*, *birth*, *childbirth*,

growth, *death*, *health*, *stealth*, *tilth*, *undergrowth*. Together these provide 701 tokens, with only one hapax, giving

$$\mathcal{P} = \frac{n_1}{N} = \frac{1}{701} = 0.001$$

While the omission of *berth* and *birth* (both etymologically related to *bear*) would not make any significant difference to this, the omission of *death* (with 201 tokens) would increase the overall value of $\mathcal{P}$ to 0.002 – still, however, clearly non-productive.

The suffix *-ure*, with only 23 types (all item-familiar), provides less data on which to base a measure, but with 6 hapaxes and a total of 599 tokens, it still emerges as being unproductive:

$$\mathcal{P} = \frac{n_1}{N} = \frac{6}{599} = 0.010$$

While these results are probably in line with intuitions and expectations (except for the result for *-age*), it is not necessarily the case that an approximately equal degree of (non)-productivity would be assigned to *-ure* and to *-ment* on an intuitive basis. That is where Baayen's global productivity P* comes in, with *-ment* occurring in many more types than *-ure*.

Conversion cannot be considered here since, by definition, there is no form that an untagged corpus can be searched for which will indicate conversion. This leaves us with the major nominalisation suffix to consider, namely the one with various allomorphs ending in *-ion*.

There are 786 types in the data ending in some variant of this affix, with a total number of tokens of 14,622 and 171 hapaxes. Applying the formula, we get

$$\mathcal{P} = \frac{n_1}{N} = \frac{171}{14622} = 0.012$$

which implies that affixation with *-ion* is barely productive in modern English. However, we have already seen that there are various subtypes within this which do not show the same degree of productivity. Accordingly, we need to consider at least the following subtypes separately:

(a) the type where *-ion* is added to a base in *-ate*;
(b) the type where *-cation* is added to a base in *-ify*;
(c) the type where *-ation* is added to a base in *-ise*;
(d) the type where *-ion* is added to some other base;

(e) the type where *-ation* is added to some other base;
(f) the type where some other variant (*-tion*, *-ition*, *-ution*, etc.) is added to the base, sometimes with complex morphophonemic modifications to the base.

(a) *-ion* is added to a base in *-ate*

There are 271 types in the corpus ending in *-ation*, with 70 hapaxes and a total of 3,543 tokens.

$$\mathcal{P} = \frac{n_1}{N} = \frac{70}{3543} = 0.020$$

However, within this, we can distinguish two types: the type where the *-ate* to which the *-ion* is added is clearly a separate morpheme (as in *vaccination*), and the type where it is simply a phonological element (possibly etymologically analysable, but not synchronically analysable as a morpheme, as in *violation*) in the base. Although, as has been indicated above, a division between these two classes is not necessarily absolutely clear-cut, a serious attempt at distinguishing gives the figures in table 6.12.

Table 6.12: *The influence of morphemic status on the productivity of the* -ation *patterns*

	Morphemic {ate}	Non-morphemic <ate>
Hapaxes	16	54
Tokens	295	3,248
$\mathcal{P}$	0.054	0.017

The indications are, then, that there is a real difference in productivity between *-ion* added to a base which ends in morphemic *-ate* rather than in an *-ate* which is purely phonological, and that while both types might be said to be productive, the addition of *-ion* to a base ending in the verbaliser *-ate* is much more productive than any other affix we have considered so far.

(b) *-cation* is added to a base in *-ify*

There are 36 types ending in *-ification* in the corpus, all but one item-familiar, with 14 hapaxes and a total of 217 tokens, which provides another figure suggesting high productivity (although the low number of types may make this figure rather unreliable):

$$\mathcal{P} = \frac{n_1}{N} = \frac{14}{217} = 0.065$$

(c) *-ation* is added to a base in *-ise*

The figures for *-isation* (variously spelled) are very similar, with 56 types in the corpus, 21 hapaxes and 342 tokens:

$$\mathcal{P} = \frac{n_1}{N} = \frac{21}{342} = 0.061$$

(d) *-ion* is added to some other base

With 200 types in the corpus and a total token frequency of 4,850 with 30 hapaxes, this type of formation comes out as being non-productive:

$$\mathcal{P} = \frac{n_1}{N} = \frac{30}{4850} = 0.006$$

(e) *-ation* is added to some other base

There are 155 types where *-ation* is added directly to a word base which does not end in *-ise*. These types provide a total of 3,652 tokens, with 26 hapaxes, so that this process is seen to be unproductive by this measure:

$$\mathcal{P} = \frac{n_1}{N} = \frac{26}{3652} = 0.007$$

(f) some other variant is added to the base

There are 74 types in the corpus with complex morphophonemics or irregular variants ending in *-ion*, with a total of 1,952 tokens and 10 hapaxes. Again, this is seen to be unproductive by this measure:

$$\mathcal{P} = \frac{n_1}{N} = \frac{10}{1952} = 0.005$$

Note that while *-ion* overall is measured as being of marginal productivity by this method, certain patterns within this are measured as being of relatively high productivity, namely the addition of *-ion* to morphemic *-ate*, the addition of *-cation* to verbs in *-ify*, and the addition of *-ation* to verbs in *-ise*. What we are seeing here is the importance of the domain in discussions of productivity: rather than say that *-ion* is of marginal productivity, we want to say that *-ion* is only productive when added to certain types of base, but that it is highly productive in those few selected

places. The conclusion is not new, being overt in the works of, for example, Aronoff (1976) and Van Marle (1985); this is simply another illustration of the point. It will be recalled, though, from section 5.3.2 that Aronoff claims that this question of domains is not crucial in providing a measure of productivity. It seems to me that examples like the one discussed here show that it is in fact crucial, and that a measure of productivity which does not pay attention to these domains will inevitably be giving a false impression of the productivity of particular processes.

While the three productive patterns appear to be very similar to each other in terms of the measurement $\mathcal{P}$ (at least in the corpus), they can be differentiated in terms of global productivity P*. With 36 types ending in *-ification*, 46 ending in *-ation* (with morphemic *-ate*) and 56 types ending in *-isation*, this last has the greatest global productivity, which is also in line with the numbers gained from Barnhart et al. (1990) and cited in table 6.11.

It would be expected from section 5.3.4 that Baayen's measure $\mathcal{P}^*$ would be the most effective method of grading the various productive processes. The formula for this measure is

$$\mathcal{P}^* = \frac{n_{1,E,t}}{h_t}$$

The number of hapaxes in the Wellington Corpus is 18,134, so for the construction *-at·ion* the appropriate value is calculated by

$$\mathcal{P}^* = \frac{70}{18134} = 0.0039$$

The corresponding value for *-ifi·cation* is

$$\mathcal{P}^* = \frac{14}{18134} = 0.0008$$

And the value for *-is·ation* is

$$\mathcal{P}^* = \frac{21}{18134} = 0.0012.$$

This gives the ranking that *-ion* added to *-ate* is more productive than *-ation* added to *-ise*, which in turn is more productive than *-cation* added to *-ify*. Not only does this not correspond to the ranking according to $\mathcal{P}$, it is just the inverse.

For the sake of completeness, we should also consider a measure based on the procedures outlined in section 5.3.5. The Wellington Corpus was created with texts from 1986, so to see what nominalisations are used productively in it, we need to compare it with a dictionary published before 1986, but not too long before 1986. The 1982 edition of the *Concise Oxford Dictionary* (Sykes 1982, referred to in what follows as *COD7*) seems to fit the bill, although we will need to consider evidence from other dictionaries as well. Although *COD7* is a British dictionary and the corpus is a New Zealand corpus, there are no particular grounds for believing that there are likely to be nominalisations in New Zealand English that are not part of British English as well. In any case, there is no corresponding New Zealand dictionary. However, any forms which are questionable can be traced through New Zealand dictionaries published at a later date but with etymological information, including dates of first occurrence, should this be necessary.

It comes as no surprise that all the words ending in *-age*, *-ance*, *-ence*, *-th* and *-ure* in the corpus are listed in *COD7*. This confirms these as being unproductive or of very low productivity (the corpus is too small for us to be absolutely sure of a complete lack of productivity on this basis alone), and is in line with everything else that has been said here.

Where *-ment* is concerned, there are three forms which do not appear in *COD7*: *impoundment* and *refurbishment* which are listed in *Collins English Dictionary* (Hanks 1979) and are thus not 'new' words, and *changement*. This word occurs in the following context:

A35 044 |^The dancers worked tirelessly on. ^There are no made-up
A35 045 faces, elegant head-dresses or tutus today; it's just plain
A35 046 working clothes.
A35 047 |^Some are in old black shorts, torn at the ends, others
A35 048 wear long white T-shirts with Chinese inscriptions on the back,
A35 049 mementoes of their China tour.
A35 050 |^One dancer pirouettes around the stage in a black leotard
A35 051 and chiffon skirt.
A35 052 |^One male dancer in a tight-fitting glittering purple
A35 053 unitard receives curious glares from the audience.
A35 054 |^After 16 changements Harry dismisses the class. ^For some
A35 055 it's time for a quick smoke backstage. ^Others rush off to
A35 056 retrieve a glass of water.
A35 057 |^Five minutes later it's back on stage for the first
A35 058 rehearsal of the day, *1Portraits of Desire.

From this extract it is clear that *changement* is a technical term in ballet, and it is in fact pronounced in the French manner and is still considered a loan, though it has been in use for at least a couple of hundred years. It thus neither classifies as a new word, nor as a case of English word-formation. Overall, then, although there are two relevant forms which do not appear in *COD7*, they are not new coinages, and do not represent the productive use of the affix, and *-ment* appears to be as unproductive as the suffixes considered above. This fits with the claim made in Bauer (1983: 76), and probably also with the claim made by Baayen and Lieber (1991: 830) that *-ment* is 'barely productive'.

Where words ending in *-ion* are concerned, the picture is less clear. A complete list of words in this category found in the corpus but not listed in *COD7* is provided in table 6.13. Although the list in table 6.13 looks superficially impressive, there are many words in the list which are prefixed versions of words which are listed in *COD7*, and which should be considered in discussing the productivity of the individual prefixes rather than in this context. They are marked with an asterisk in table 6.13. There are also several words which, though not listed in *COD7*, are listed in Hanks (1979) (an older dictionary than *COD7*, note), and which thus show up as accidental gaps in the dictionary corpus rather than genuine new formations. These words are marked with an obelisk in table 6.13.

Some of the remaining words are of inherent interest. Some are too technical to be in ordinary dictionaries: *discretisation* and *parameterization* are both listed in *The Oxford English Dictionary* as mathematical terms coined in the twentieth century. *Infuscation* is listed as obsolete by *The Oxford English Dictionary*. *Adaption*, *patronisation* and *privatisation* are all listed in *The Oxford English Dictionary*, the last only since 1959.

If all of these are discounted, we are left with *colonialisation* (rather than *colonisation* which is listed), *corporatisation*, *globalisation*, *habitualisation* and *marginalisation*, all of which end in *-isation*. This gives us the result that four types out of the 56 in the corpus on this pattern are new. The difference in time between the dictionary and the corpus is at least four years, so this suggests a rate of increase of one per year (or less). None of the other patterns is shown to be productive. The low numbers and the low rate of increase observed are probably the result of the restricted size of the Wellington Corpus, but the method can be seen to

Table 6.13: *Words ending in* -ion *with a verbal base found in the Wellington Corpus and not listed in* COD7

acidification†	marginalisation
actualisation†	mechanisation†
adaption	mineralisation†
advocation†	oxidation†
autocorrelation*	parameterization
bilocation*	patronisation
categorisation†	pedestrianisation†
colonialisation	petrification†
commercialisation†	preallocation*
corporatisation	premodification*
defamiliarisation*	privatisation
dehumanisation*	reapplication*
deregulation*	reassertion*
discretisation	redefinition*
gentrification†	regularisation†
globalisation	reinfestation*
habitualisation	reintermediation*
infuscation	reintroduction*
initialisation†	relitigation*
institutionalisation†	reregulation*
introgression†	subcompensation*
liberalisation†	theorization†

* and †: These words are probably not genuine innovations; see text for details.

provide a clear answer: the most productive nominalisation affix in current English is *-ation* added to a base in *-ise*.

6.4.7 Comparing measures of productivity

It was reported in section 5.3.5 that both Bolozky (1999) and Plag (1999) see the best measure of productivity arising through agreement between various dictionary- and corpus-based methods. Unfortunately, the experiment carried out here suggests that no such agreement can be expected: almost any ranking of items can be obtained from statistics which are specifically designed to rank processes in terms of their productivity. The results from this section are summarised in tabular form in table 6.14.

Table 6.14: *Comparative productivity rankings*

	OED	Neologisms	I	$\mathcal{P}$	P*	$\mathcal{P}$*	*COD7*/ Corpus
-ment	NP	NR	NR	4	NR	NR	NP
-is·ation	1	1	3	2	1	2	1
-ifi·cation	NR	3	2	1	NR	3	NP
-at·ion	NR	2	1	3	NR	1	NP
conversion	2	NR	NR	NR	NR	NR	NR

NP = not productive; NR = not ranked

We have seen in section 5.3 that none of these measures is without its own theoretical difficulties. There are also practical difficulties in applying the measures, some of which have been seen in this section. And it must be admitted that the corpus used for corpus-based results is rather small, with the number of *-ification* words in particular being rather low (though smaller corpora have been used, apparently successfully). Nevertheless, these conflicting results raise serious questions about the status of current best attempts to measure productivity. If all the measures are attempts to capture the same intuition, albeit indirectly, most are clearly failing at some point. Of course, the various statistics measure different things, and thus cannot be expected to agree all the time. The degree of disagreement we find is still alarming and suggests that we need a better way of approximating the intuition in the first place. I made a preliminary suggestion at the end of chapter 5. That suggestion has various problems associated with it, but might form the kernel of a new view.

It might be objected that the figures in table 6.14 are irrelevant. They do not, for the most part, show differences between different morphemes, but between different allomorphs of the same morpheme. As such they indicate more about the productivity of the input-classes (verbs in *-ate*, *-ify* and *-ise*) than they do about anything else. There are at least three counter-arguments to this position. The first is that we have seen at various points that the output classes are not entirely based on the input classes, so that the measures should be useful anyway. The second is that we have seen the importance of domains of productivity, so that we ought to be able to measure comparative productivity of the same affix in different domains. The third is that the results do not seem to reflect consistently the productivity of the input classes. According to Plag (1999:

104, 115) *-ise* is more productive than either *-ate* or *-ify*, *-ate* coming out as more productive than *-ify* on dictionary-based measures but vice versa according to $\mathcal{P}$ or P*. This result is best reflected by the statistic P* in table 6.14 or by the dictionary-based measures. It thus seems to be the corpus-based measures which are most in need of clarification. Since it was argued earlier that dictionary-based measures contain fundamental theoretical flaws, the conclusion must be that we still have no reliable measure of productivity.

6.5 Agentive and non-agentive *-er*

With the final example, we raise the question of exactly what should be considered the same process when we are measuring the productivity of a process. The same question can be reformulated in terms of morphemes (though it does not depend on that notion), or can be seen as a return to the question raised in section 2.2 of whether it is formation-types or paradigm slots which are productive. Ryder (1999) makes a convincing case for all relevant *-er* formations being seen as the result of the same process. In what follows, the implications of such a stance will be considered, though no firm conclusions will be reached.

In Bauer (1979), I provided an analysis of new formations in *-er* denoting persons from Barnhart et al. (1973). In that paper I commented on the number of new forms that were formed on compound bases. I did not, however, analyse those *-er*-derivatives which did not denote persons. I now want to take up the comparison between human and non-human *-er* derivatives in the light of the suggestion (reported in Kastovsky 1986: 596–597) that different meanings of the same form should be treated separately where questions of productivity are concerned.

This suggestion can be seen as the natural extension of another suggestion which is sometimes made, that the *-er* which produces agent or human nouns is a separate affix from the *-er* which produces instrument or non-human nouns (on two homophonous *-able* suffixes see e.g. Aronoff 1976: 48). This latter view is compatible with the view expressed by Beard (e.g. 1995: 81–83) that the meanings underlying derivational patterns are primary, not the affixes which happen to realise those meanings.

The views mentioned above lead to the analysis of a large number of homophonous affixes. The opposing view, which is less often overtly espoused, is in some ways an extension of Humboldt's Universal, i.e. the expectation that one form should correlate with a single meaning and

vice versa. In this view, identity of form would be taken as a guide to identity of morphological structure unless there is good reason to argue for the contrary. Precisely what counts as a 'good reason' then becomes crucial. Some arguments that can be used in this way are discussed in Bauer (1988: 110–115), but the arguments themselves are frequently open to multiple interpretations, especially where identity or non-identity of meaning is concerned. According to this view, all the derivative *-er*s in English should be taken to be tokens of the same morpheme, whose meaning should thus be far less specific than 'agent' or 'instrument', and rather more like 'entity having some relationship to the base' (see the analysis in Ryder 1999).

The important question for present purposes is the following:

> Is it only if we are dealing with two distinct *-er* morphemes/affixes/constructions that we can really ask whether they differ in productivity, or can we talk about distinguishable uses of the same morpheme/affix/construction differing in productivity?

In the case in hand, there are approximately two thirds as many non-human nouns ending in *-er* listed by Barnhart et al. (1973) as there are human nouns with the same affix. This gives prima facie evidence of a difference in profitability of the two uses of *-er*. However, such a difference could arise from a number of different causes. For example:

(a) It might be the case that speakers talk about humans more than about inanimate objects, and thus need words to make finer distinctions between human entities than between inanimate entities, with the result that there is a greater need for the *-er* derivatives with human denotata than for those with inanimate denotata.

(b) It might be the case that the difference arises from typological pressures (which may in turn be related to the pragmatic pressures outlined above). Dressler (1986: 526) claims that where 'agent' formations are polysemous

> The agent polysemy does not consist of an unordered set of meanings but seems to have a hierarchical structure: agent > instrument > {locative, source}. This hierarchy is reflected not only in the relative frequency of these meanings but also in the primacy of agentivity in language acquisition . . . and in Breton language decay . . . , and in diachrony meaning extension seems to go in the same direction [references omitted, LB].

The implication is that there is a natural (in some technical sense) pattern here, such that instrumental readings for markers which also have agentive readings must be less frequent, and thus less profitable, than the agentive reading. This view is presented in cognitive terms by Ryder (1999). (A possible counter-example to this generalisation is presented by deverbal conversion in English, where a *whisk* is an instrument and a *cheat* is a person. However, both Marchand (1969) and Szymanek (1989) treat the agentive as being more productive than the instrumental here as well.)

(c) It might be the case that the discrepancy in Barnhart et al. (1973) is exaggerated by the sampling procedures used in collecting material for that dictionary. While it is true that there could be some kind of sampling error involved, the use of dictionaries as sources of data involves making assumptions about the representativeness of the dictionaries involved, so that this is not a serious objection, except in the sense that a larger data-base is always preferable if available.

Points (a) and (b) above would involve us in saying that some distinguishable meanings of a particular process are less exploited than others without us necessarily wanting to say that there was more than one process involved. Thus there is a perfectly coherent answer to our question according to which productivity (in the profitability sense) does not relate to particular processes, but to particular uses of processes. Note that this is entirely compatible with previous conclusions that the productivity of certain processes is dependent upon the linguistic environment (e.g. *-ation* is productive added to *-ise*, but not added to a simplex base).

Having drawn that conclusion, we may now consider the less important question (for present purposes) of whether there are two distinct *-er* processes involved here or just one. It is not clear to me that this question is answerable in the abstract; it is a matter of preferred analysis rather than ultimate truth. However, there is some data which might be relevant.

One piece of evidence which might lead to the conclusion that two distinct processes were involved rather than a single process is if the two uses show different grammatical behaviour (in Bauer [1988: 111] I talked about the category of the base, but the point here is slightly more subtle). One of the points that was made in Bauer (1979) was that the agentive *-er* was often added to bases which were complex in some way, sometimes even to phrasal bases. In table 6.15 a comparison is drawn between the

percentages of relevant formations which are produced on various types of base. The figures for the agentive words are taken from Bauer (1979), those for instrumental words are new. The figures do not add up to 100 per cent since only the most important categories are commented upon. The important thing to note in table 6.15 is the discrepancy between the percentage of agentive nouns formed on the basis of a non-compound verb and the percentage of instrumental nouns formed in that way. Such a discrepancy (and some of the other less striking differences) might be taken to indicate that the two types are following different grammatical patterns, and are therefore distinct types, i.e. distinct processes.

Table 6.15: *Comparison of base-types used in different kinds of* -er *nouns*

Base type	Agentive	Instrumental
Compound verb	7%	6%
Compound noun	14%	6%
Synthetic compound*	31%	27%
Noun phrase	12%	5%
Non-compound verb	14%	47%
Non-compound noun	7%	2%

* The term was not used in Bauer (1979), and is here used to cover all compounds of the form N+V-er where the initial N is an argument of the V, whether or not it is a direct object of the V.

There is, of course, another side to this argument. First, it can be pointed out that although there are differences in weighting, the same patterns are found, so that the two types have at least as much in common as they have distinguishing them. Second, it might be possible to explain the difference. To do this, we need to refer back to Dressler's (1986) typological claims about agentives. One of these (Dressler 1986: 524) is that there is a cline of preferred bases, such that verb > noun > adjective (this is again elaborated in cognitive terms by Ryder 1999). Now, if it is the case that instrument nouns are more marked than agentive nouns (when using the same affix), we might postulate that this increased markedness is compensated for by reducing markedness on the cline of preferred bases, such that preferred bases (read 'more natural bases') are more strongly preferred for the semantically more marked type. In the light of such arguments, the suggestion that the different grammatical patterning might indicate distinct processes seems less convincing.

The ultimate proof of morphemic distinctness ought to be contrast. If two contrastive units replace each other in the same context, there ought to be a difference of meaning between them. Given that *spoiler* is listed in Barnhart et al. (1973) with two meanings – (a) a third political candidate or (b) an airflow deflector on an automobile – it would seem that the two do contrast and that there is thus evidence for there being two different morphemes. Unfortunately, the notion of contrast is not as clear here as we might expect it to be.

If we were to consider the word *spoiler* alongside a putative *spoilee*, we would not be in doubt that the two words contrasted with each other and that the difference lay in the choice of suffixes. This seems very much in line with the way in which Harris (1951: 29–41) sets up phonemic distinctions. In what might appear a similar case, no one will deny that the sentence *I was impressed by how effective the spoiler was* will gain a very different interpretation depending on whether *spoiler* is interpreted as being a political candidate or an airflow deflector. The problem is whether that difference in interpretation is to be localised with the morpheme {er} or with the lexeme SPOILER. In the former case, we will wish to distinguish two different *-er* affixes; in the latter, we will only need to say that adding *-er* to *spoil* has not been pre-empted by homonymy (as is normal, given the lack of confusion between the two under most circumstances). And since in either case we might want to say that we are dealing with an entity which spoils something (either the political campaign or the smooth flow of the air), there is an argument for seeing the homonymy as being a feature of the lexemes not the morphemes. However, such arguments may be slippery: it seems to me that the distinction between *diner* ('person who is dining') and *diner* ('railway carriage or small restaurant in which one may dine') is more likely to be attributed to a distinction between *-er* morphemes than the difference between the two kinds of *spoiler*. I would link this with different paraphrase possibilities (the railway carriage is not 'an entity which dines'), but a different type of paraphrase (e.g. 'entity associated with dining') might lead to a different conclusion. In this sense there is no absolute answer to the question of how many different *-er* affixes there are in English, there are only different possible analyses of the data.

6.6 Summary

In this chapter a number of examples of derivational word-formation have been considered: the fate of Proto-Germanic **-dōm* in North and

West Germanic, the formation of nominalisations from colour terms in English, nominalisations of English verbs and agentive and instrumental nominalisations in *-er* in English. Methods for investigating these processes have varied from direct experimentation in the case of the colour terms to dictionary searches. The following major conclusions have been reached:

- Restrictions on bases taking part in particular processes can change diachronically.
- The productivity of one process can restrict the productivity of another process.
- Because of these factors, patterns of productivity with the same affix can change from one period to another.
- Patterns of productivity which superficially appear unpredictable can be reduced to a number of overlapping restrictions on what is permitted in word-formation, and correspondingly a superficially confused or inexplicable pattern should not immediately lead to the assumption that no explanation in principled terms is possible.
- Statements of domains are crucial in determining the productivity of morphological processes.
- The productivity of English nominalisation endings is far more constrained than would be expected from consulting a dictionary; this was not necessarily always the case, and standardisation appears to have a role to play in fixing such constraints as we find.
- Different measures of productivity do not always agree, and there is thus still room for a generally-agreed measure of productivity.
- Productivity can differ for different uses of the same morphological process.

7 *Conclusion*

> Productivity is one of the central mysteries of derivational morphology.
> Aronoff (1976: 35)

In this conclusion it is my aim to tease out some of the threads that have been discussed in the previous chapters and present a unified view of morphological productivity. I shall also consider briefly the extension of the notion of productivity into other areas of linguistics.

7.1 Productivity summarised and defined

7.1.1 Availability and coinage

We are now in the position to divide morphological productivity into two distinct themes: availability and profitability. A morphological process is available if it can be used to produce new words as they become necessary. Availability is a yes/no question: either a morphological process is available or it is not. We might, however, ask more precise questions about availability and ask whether or not a particular morphological process is available with a certain type of base. For example, we might ask whether *-ment* is available with bases ending in *-ise* (answer in current English: no), or whether *-ation* is available with the same set of bases (answer in current English: yes). In principle a yes/no answer will be available for classes of base defined by any of the constraints discussed in section 5.2. In practice it might be more difficult, or there might be cases of uncertainty. This does not alter the fundamental principle that availability is an either-or, and that things are either productive or unproductive in this sense, with no middle ground. It also has to be noted that statements of availability are temporally limited. What is available in one period may not be in the next. Availability can change diachronically,

and valid statements about availability in one period do not necessarily apply to any adjacent period.

This broad statement of principle is clear, and probably uncontroversial, but there are difficulties in applying it, because not every morphologically complex coinage is brought about through the productive application of available morphological processes. There are several reasons for this.

First, there are types of innovation which are excluded from the domain of productivity for a number of reasons. Word manufacture, for instance, is not considered to be relevant because it does not involve any morphological process. Some irregular innovations are viewed as creating their effect precisely because they are not standardly regular morphological processes, and these are also excluded from consideration. There is currently no general agreement on where the boundaries of such non-productive innovations lie. We can probably agree to term such coinages 'creative' as opposed to 'productive', but a precise definition of the difference proves elusive (see section 3.10).

Second, we have the problem that availability is usually taken to be a phenomenon of the speech community rather than a phenomenon associated with a particular individual speaker. Individual speakers may coin new words which are not congruent with currently predominating customs in the community as a whole. *The Oxford English Dictionary* credits Walpole with coining *gloomth* and *greenth* in the mid-eighteenth century, some 150 years after the end of the period of societal availability for *-th*; *greenth* appears to have survived into the late nineteenth century, but neither is now used, and neither can be taken to illustrate genuine productive use of *-th*. Accordingly, it may be the case that a word coined by one individual shows nothing about the productivity (availability) of the relevant process within the speech community as a whole. One implication of this is that we must establish availability in the wider community, not just in the individual. Implicitly, then, there is reference to a threshold of usage below which any given morphological process is not considered to be available, and above which it is considered to be available. Baayen and his colleagues suggest that this threshold should be taken to be equivalent to the rate of coinage of morphological simplex forms. While this measure is certainly a logical one, it is one that is restricted in value to the kind of corpus study that Baayen is involved in, and a more generally applicable measure is required. Another implication of this is that the use of a particular morphological process by a particular writer or speaker

(see e.g. Strang 1968) will not necessarily generalise to the whole community of which that individual is a part.

7.1.2 Profitability

After we have asked the question about the availability of a particular morphological process, there is a further question to be asked about available processes: how profitable are they? We have seen that there are various ways of measuring productivity in this profitability sense, both direct and indirect, but with no general agreement on how it should be done and no genuinely problem-free procedure available (see sections 5.3, 6.4). We are perhaps in a better position now to understand why this variable profitability arises and what it is that the measures referred to above are trying to capture.

As was shown in section 5.2, there are a number of constraints which limit the types of formation which are possible in particular languages. When these constraints are absolute, they can be seen as part of the language system, and they can be built into questions of availability (see section 7.1.1). However, as we saw in section 6.3.6, some of these constraints are not absolute, but show preferences – preferences which may interact in complex ways, moreover. Where these preferences interact, a frequent result is that possible words are not all equally probable (see also section 3.2.4). In the example of the nominalisations of colour words discussed in section 6.3, colour terms which have three or more syllables are less likely to be used as the base of a nominalisation than shorter, more familiar ones. We cannot necessarily say that such words are excluded by the system (are ungrammatical), but we can note that speakers seem to disprefer them (consider them unacceptable) on some kind of variable scale. This dispreference shows itself in a low type frequency for any words at the unacceptable end of the scale, which in turn, of course, means that such items are unlikely to be listed in standard dictionaries. This gives the impression that the morphological process by which such words can be coined cannot (as well as does not) exploit all members of the semantically-relevant set of potential bases fully. Where there are many such constraints, a morphological process may appear to be of extremely limited profitability; where there are few it may appear to be of high (or at least higher) profitability.

Although the data presented in section 6.2 on the later fate of Proto-Germanic *-*dōm* was not sufficient to allow us to draw firm conclusions on this point, we can speculate that variable constraints have the potential

to change diachronically into absolute constraints, and vice versa. Marchand's (1969: 272) discussion of de-adjectival *-en* suggests that the phonological constraints have changed over time in this way, although we have seen (section 5.2.9) that there may be reasons to doubt the analysis presented there.

Variable constraints of this nature are not the only factor limiting the profitability of particular morphological processes. We have seen that morphological processes are frequently in competition with each other. A well-recognised example of this is the competition between the German diminutive suffixes *-chen* and *-lein* over the majority of bases (Fleischer 1975: 178–180). Similarly, in section 6.3 it was shown that at various periods in the history of English, alternative nominalisations were available for the same verbal base. Under such circumstances, the competition must inevitably affect the profitability of both or all of the morphological processes involved. It is not clear whether such competition is the norm or the exception. In section 6.3.6 it was suggested that the competition between English nominalisation patterns has been reduced as a function of language standardisation. The same is not obviously true of English adjectivalisation processes, so that X*ian* and X*ist* may be equivalent adjectives where X is a proper name as the base (Bauer 1983: 268). This suggests that, even in highly codified languages like English, competition between morphological processes goes on. In some cases such competition will eventually lead to semantic or stylistic division between the processes; while it continues it leads to loss of profitability. We should probably assume that this is a normal situation rather than an exceptional one.

As well as variable constraints and variable competition, we should also consider variable need (for some discussion, see Bauer 1999b). Words are not coined in a pragmatic vacuum, but to fulfil a pragmatic demand (as is stressed, amongst others, by Baayen and Lieber 1991: 818; Štekauer 1996). For instance, Carstairs-McCarthy (1992: 187) cites a Polish suffix *-ówka*, which derives the name of a vodka from the name of the raw material from which it is made. While virtually any vegetable matter can be fermented, it does not follow that this suffix will be found in as many nouns as there are nouns for types of fruit or vegetable in Polish, because Poles are likely to have little use for a word meaning 'vodka made from Brussels sprouts' or – for different reasons – 'vodka made from kiwifruit'. This is one of the reasons why morphological processes which are used for transpositional purposes are likely to be more productive (profitable)

than those used purely for purposes of lexical innovation: there are fewer pragmatic constraints of this type on transposition than on lexical creation.

Finally, it may be the case that speakers simply choose not to use particular morphological processes very often, although it is hard to distinguish this from the other causes of variability listed above. However, even such imponderables should be considered. For instance, Haberland (1994: 347) comments specifically on the lack of a derivational diminutive in Danish, and signals compounding with the word *små* 'small (plural)' as being the Danish equivalent of diminutive marking. Now in related languages such as Dutch or German, diminutives are used extremely widely; but in Danish the productivity of compounding with *små* is very limited: Petersen (1984) lists only two neologisms formed in this way in the twenty-year period on which she reports. It seems that, although this diminutive word-formation process is available, Danes choose not to use it very frequently and prefer syntactic methods for marking size and/or emotional closeness.

7.1.3 The interaction of availability and profitability

Availability and profitability interact to constrain the number of words in the lexicon of the individual speaker which are coined according to the pattern provided by any particular morphological process. If a process is not available, then the expectation for the adult speaker will be that no words using that process will be met unless they are item-familiar. The number of item-familiar words realising the process may be small or may be enormous. Where a morphological process is available, then the number of coinages using that process has the potential to expand at a rate not less than the rate of expansion of the class of possible bases (possibly much higher if there are many potential but unused bases available in the lexicon). The fewer absolute constraints there are on the base, the greater the number of words that is likely to be created, and the greater the number of encountered words which are not item-familiar is likely to be. Equally, the fewer variable constraints there are on the base, the greater the number of words that is likely to be created, and the greater the chance of encountering words which are not item-familiar. Here we have the crux of the confusion which has haunted so much of the discussion on productivity in the past. Availability and profitability both have the same apparent effect on the number of coinages, but the causes of these apparently similar effects are distinct: availability is a matter of what the language system determines; profitability is a matter which is determined

by language norms. In principle, it is clearly important to distinguish between these two sides of morphological behaviour. In reality, it has been common practice to look at the effects of both as a single amalgamation.

The discussion in this chapter so far has all been in terms of a rule-governed approach to morphology and not in terms of an approach based on analogies. What happens if we approach the same material from the assumption that morphological processes are fundamentally analogical processes?

One of the main differences between the two approaches is that while, in a rule-governed approach, it is possible to say that some words are ungrammatical formations in that they break rules, in an analogy-based approach there is no such thing as an ungrammatical formation; formations are simply better or less well supported by a network of parallel structures (see e.g. Becker 1990: 17–18), and thus more or less likely. However, if that is the case, the distinction between availability and profitability cannot be drawn, since it depends on a difference between systemic and non-systemic factors, factors which are merged in the analogy-based approach. In an analogy-based approach, therefore, there is no concept of availability: everything is a matter of profitability (which can then, unambiguously, be termed 'productivity'), and productivity is purely a matter of which analogies happen to have been or happen to be exploited. Equally, under this approach, there can be no possible words, since that notion depends upon the notion of the system: there can only be attested words and the possibility of creating new words supported by a network of pre-existing formal and semantic relationships.

Now, clearly, neither approach is inherently impossible. Nevertheless, we have to ask what we would be giving up if we lost the notion of availability, and the main answer seems to be that we would have to give up the notion of domains of productivity. That is, any observations that a particular morphological process is used differentially with bases of different classes, or depending upon its meaning (cf. section 6.5), etc., cannot be more than informal observations with no theoretical status within the analogy-based approach. Within the rule-governed approach, in contrast, such matters may affect the availability of the suffix, and so be seen as crucial information regarding the system within which the rules are exploited. Furthermore, if there has been any clarification of the notion of productivity arising from the separation of availability and profitability (and I believe that this has helped), then that gain is immediately wiped out within the analogy-based approach. For these reasons it

seems to me that a study of morphological productivity sheds light on the question of the debate on rule-governedness versus analogy within morphology, and argues in favour of the rule-governed approach.

At the same time, it must be recognised that the exceptions mentioned in section 7.1.1 are not in the least problematical within the analogy-based approach. There is some element of roundabouts and swings here, and the linguist has to decide which set of problems to live with.

7.1.4 Productivity defined

A provisional definition of productivity was given at the end of section 3.13: 'the productivity of a morphological process is its potential for repetitive non-creative morphological coining.' Much of this stands. But in the light of subsequent discussions we can now modify the definition.

First we have to recognise that (within the rule-based approach which is being espoused here) the term 'productivity' is ambiguous between availability and profitability, and that the definition above deals only with the availability half of that. We can now, however, build the notion of rule-governedness into the definition without any circularity, since to be coherent the notion of availability presupposes the notion of rule-governedness. Profitability also has to be defined separately.

So we can now define productivity in the following terms.

'Productivity' deals with the number of new words that can be coined using a particular morphological process, and is ambiguous between the sense 'availability' and the sense 'profitability'. The availability of a morphological process is its potential for repetitive rule-governed morphological coining, either in general or in a particular well-defined environment or domain. Availability is determined by the language system, and any process is either available or unavailable, with no middle ground. It creates psychologically real distinctions between available ('living') and unavailable ('dead') processes, which can be tested in a number of ways. The profitability of a morphological process reflects the extent to which its availability is exploited in language use, and may be subject unpredictably to extra-systemic factors. Where a single morphological process has easily distinguishable meanings or sub-uses, these may be assessed independently for both availability and profitability.

7.1.5 Discussion and implications

Although the view of productivity which has emerged here is one which has been based upon a fairly abstract level of argumentation, it has the

advantage of fitting both with a commonsense approach to morphology and with the psycholinguistic evidence discussed in chapter 4.

Assume morphological process M is available at time t_1. A word coined at time t_1 becomes institutionalised, which implies that it becomes item-familiar to a large section of the relevant speech community (in the case of technical language, this community may be considerably smaller than the community of English speakers, but the point remains unaffected). At time t_2 subsequent to t_1, another word will also be coined using M. This word will not necessarily be item-familiar to the same portion of the speech community. If we assume that t_2 is some considerable time after t_1, then measures taken shortly after t_2 will show the token-frequency of the second word to be lower, because one word has spread throughout the community and the second has not yet had the chance to do so. The word coined at t_2 must have been produced and interpreted in terms of its morphemic make-up. Item-familiar institutionalised words may be stored as wholes, although this is likely to be most economical for words with a high frequency of occurrence, or for words which are learnt early enough to be in place before the development of the full morphological parsing facility. This is certainly not before the age of 15 (section 4.3.2; Tyler and Nagy 1989), and explains why early-learnt words are often more easily accessed than later-learnt ones (Brown and Watson 1987). Thus it is that at any time t_3 subsequent to t_2, various words historically formed by M (or giving the appearance of having been formed by M) are not necessarily treated identically in terms of storage, comprehension, production, etc. In more theoretical terms, words produced by available morphological processes may nevertheless become lexicalised.

Words coined by M at t_2 are coined because of some perceived need, whether that need is for transposition or for lexical innovation. Since that need is not predictable in advance, the degree of exploitation of available processes is not predictable, either. In particular, lexical innovation is not automatic since the needs that drive it are not automatic. In itself, this leads to variable profitability of morphological processes. This variability is also exaggerated by a number of other factors, some of which are systemic and others of which may not be. The interaction of factors which make some coinages more probable than others may be too complex to be computed, but leaves traces in the form of variable levels of exploitation (variable profitability). This profitability is in principle measurable, although we do not yet have a robust measure of it, but depends upon domains of availability and on the interplay of the systemic and

non-systemic factors discussed in chapters 5 and 6. This variability – as is the way of variability – can in turn underlie language change so that some patterns (not necessarily unprofitable ones) can cease to be available, and so on.

At the end of chapter 1 we posed a number of questions which have now been answered.

We do need to distinguish productivity from creativity, but the distinction may well be a matter of definition rather than something which can be determined from data.

'Productivity' is ambiguous between 'availability' and 'profitability', as long as a rule-governed approach is taken to the subject. Availability is a yes/no matter, but profitability is a matter of degree, and as such it can be measured in various ways.

By implication we have concluded that within the approach taken in this book, there is more than a difference of degree between a plural form such as *Vaxes* (rule-governed) and one such as *Vaxen* (non-rule-governed, creative), but this makes the point that words formed by non-rule-governed processes may still be accepted as part of the language system, and so does not in itself predict ungrammaticality (unless that is defined very narrowly).

Productivity, which varies diachronically, is influenced by a large number of factors, not all of them systemic.

Finally, in section 3.4.3 we asked whether the word-formation process using *step-* was still productive in English. It can be available only if there are bases with which it can be used. If there are no such bases – and especially if there is no possibility of coining new relevant bases – there can be no availability. If it has used all the bases which used to be available it shows 100 per cent generalisation over that (constrained) set of bases.

7.2 Productivity in other areas of linguistics

This book has been concerned with morphological productivity, but it seems a useful envoi to consider how the same notions apply in other areas of linguistics, particularly in phonology and syntax.

Where syntax is concerned, we can start from the statement by McCawley (1999: 158 fn 11) that a core principle of much of generative grammar appears to have been 'that there is no irregularity in syntax'. We can rephrase that as meaning that the general notion of a syntactic

rule covers only such rules as are available. On the whole this is because syntactic rules which become unavailable fail to leave residues which still require analysis: unavailable phonological rules and morphological rules (such as *-th*]$_N$ affixation) leave multiple residues which are frequently met in synchronic language use. Such residues as are left by syntax (and they are few in number; see (1) for some examples) are not generally taken as a reason to build non-productive procedures into synchronic grammatical rules.

(1) *Come of age*. [**Come of maturity*.]
Few and far between. [**They are far between*.]
How do you do? [**I do very well*.]
If you please [i.e. *if it pleases you*]
Use of second person singular forms, both in religious language and in jocular contexts.

On the whole, it seems to be assumed that syntactic rules are not subject to the same kind of constraint that was illustrated in section 5.2 for morphological rules. This may be justified as a general picture, or it may be no more than the result of inadequate exploration of the possibility of constraints. Certainly, in one case which has been considered from this point of view, namely Dative Shift which links sentences such as *She gave some socks to Paul* and *She gave Paul some socks*, there is some evidence for constraints of the type we have seen operating in morphology. The discussion here is based on Pinker (1989: 45–48), where a summary of earlier research is provided.

First, and most obviously, Dative Shift applies only where the verb in the construction has the appropriate semantic structure: in Pinker's (1989: 48) terms, 'they must be capable of denoting prospective possession of the referent of the second object by the referent of the first object' or they must be communication verbs which use this pattern metaphorically. This characterisation may be a little too narrow, depending on whether the double-object construction or the prepositional construction is considered to be the fundamental one (it excludes, for instance, *My mother baked me a cake for my birthday* where the corresponding preposition would be *for* rather than *to*); nevertheless, it is clear that there is a limited set of verbs which can take part in this construction. While (2a) may be acceptable, (2b) is not.

(2) a. He walked the dog for me while I was on holiday.
b. *He walked me the dog while I was on holiday.

This is precisely parallel to the kind of semantic constraint that was discussed for morphology in section 5.2.4.

Less obviously, it appears that this class of verbs is etymologically limited, being restricted to Germanic verbs or early Romance loans. Since etymology is unlikely to be part of the synchronic competence of most speakers, it is likely that this constraint is perceived in either phonological or morphological terms. A morphological view would exclude from the relevant class verbs made up of the pseudo-prefixes and obligatorily bound bases such as *transfer*, *remit*, *announce*, etc.:

(3) *She transferred me the money.
*He remitted me twenty dollars.
*She announced me the news.

The morphological view, however, is unlikely to be the correct one, given that some words of this type and some genuinely polymorphemic words do take part in Dative Shift:

(4) He reserved me a seat.
She telegraphed me the money.

Precisely how any phonological constraint is to be formulated is controversial. Monosyllabic verbs with appropriate meanings can undergo Dative Shift, but so can some disyllables. The disyllables are either stressed on the first syllable, following a fundamental Germanic pattern (*offer*, *promise*, etc.), or have a first un-footed syllable containing only a schwa (*allot*, *assign*, etc.). This second pattern arises from Germanic prefixation. What is not clear – to me at least – is whether a phonological criterion can be economically stated which includes all those verbs and just those verbs with the appropriate semantic structure which can undergo Dative Shift. This is irrelevant however to the fundamental principle, which is that there seem to be phonological constraints applying to the heads of constructions which can undergo particular syntactic rules. It may even be the case that there is individual variation, either caused by the complex interaction of constraints or simply due to different amounts of exploitation within certain formally-defined constraints: Pinker (1989: 47) accepts (5a) where I could only have (5b).

(5) a. She referred me a patient.
b. She referred a patient to me.

As well as the absolute constraints discussed above, there is plenty of evidence that syntactic rules are exploited to a greater or lesser degree. In

English, even in scientific texts, passive verb groups account for only 30–35 per cent of all verb groups, and the percentage is much lower in non-scientific texts (Svartvik 1966: 46). Even if this were to be reformulated in terms of verbs which are passivisable, it would be clear that the passive is used variably in different text types, and that the passive rule is avoided more often than it is used. The very fact that we talk in terms of a rule of Dative Shift shows that both the double object construction and the prepositional construction are found, so that whichever is the derived form is one which is derived by a variable rule.

It thus appears that profitability is a factor which applies to syntactic rules as well as to morphological rules, but a factor which is largely ignored. It is much easier to ignore in syntax than in morphology because the products of the rules are entirely ephemeral in syntax, while the products of morphological rules are more likely to become part of the norm of the language. In this context it is worth noting that those linguists who have dismissed profitability as merely a performance factor in morphology (see sections 2.2, 2.8) are largely those who are approaching morphology from the view-point of syntacticians, and that there is thus a coherence to their views, in that they are treating the two areas in the same way. From the point of view of morphologists looking at syntax, however, we might want to ask whether it might not be the case that the profitability of syntactic rules is constrained by the interplay of the kinds of linguistic factors we have seen operating in morphology (see in particular the discussion of nominalisations of colour words in section 6.3). It may be that morphology and syntax are more like each other in this way than is commonly noted.

It is certainly the case with syntactic rules – as it is with morphological and phonological rules – that the productivity of the rule may change diachronically. We would expect to find the route to non-productivity being reflected in language use by a diminishing level of exploitation of the rule over time until the rule stopped being used at all. Again, this would show up as variation in the profitability of any rule in a synchronic grammar. While syntactic change is notoriously slow, this does not mean that such patterns cannot in principle be traced, although the amount of variability that can be attributed to this source at any given point in time might be small. As an example of syntactic rules in competition consider the vexed question of English relative clause formation, with *which*/*who(m)*/*that* and a null complementiser in competition, the precise members of the competing set depending on the syntactic context (for

a detailed survey, see Sigley 1997). The variation here is relatively stable, though exploited subtly differently by different communities of English speakers, but still indicates the variable profitability of syntactic rules.

One type of example provides an interesting perspective on morphological and syntactic productivity. It is the type of example where a morphological process is in complementary distribution with a syntactic one, either to represent the same category or in the representation of categories of similar types. For example, in English the comparative of adjectives is morphological for most monosyllabic adjectives (at least in formal styles) and syntactic for all trisyllabic adjectives (with the curious exception of *curious*), while disyllabic adjectives have comparison marked variably by morphological or syntactic means. The change from Classical to Vulgar Latin included a change from a synthetic passive to an analytical form (which has come down to modern Romance), and a change from a synthetic future tense to an analytical one: *scribam* 'I shall write' yields to *scribere habeo*, which becomes synthetic again in modern French in the form *écrirai* (Fox and Wood 1968: 17–18; Bichakjian 1990). This last form is currently in variation with an analytical form *vais écrire*, which, under appropriate circumstances, may be totally equivalent from the semantic point of view to the more formal synthetic form (see Judge and Healey 1983: 110).

Such examples are not particularly exceptional. For ease of exposition, consider the English comparative instance. However we characterise the class of adjectives which takes a synthetic comparison in modern English (and the class may not be entirely well defined; see Bauer 1994b: 51–60), we are going to require some kind of phonological constraint on bases which may undergo the process. There is a sense in which the phonological constraint(s) will be seen as limiting the productivity of the morphological comparative, since most new derived adjectives will be of three or more syllables, and thus fall outside of the domain to which the morphological comparative can apply. It should surely follow, then, that there is a complementary phonological constraint limiting the number of adjectives which can undergo the syntactic comparative with *more*. While the first type is regularly formulated in this way, the second is not. To a certain extent, it might be argued that this divergence is due to the data, where the innovative syntactic form is permitted even with monosyllables, while the conservative morphological form is never permitted with trisyllables. While it is true that we find things such as

> I've never heard anything more mad. (Wallace, Irving. *The Guest of Honour*. London: Michael Joseph, 1989: 163)

> The old man's deaf . . . Even more deaf than he admits. (Deighton, Len, *Spy Line*. London: Hutchinson, 1989: 230)

it is also true that we find things like

> You're the one-woman-needing-est man I know. (Schopen, Bernard. *The Big Silence*. New York: The Mysterious Press, 1989: 166)

so that the counter-examples go in both directions (albeit not with the same frequency). Thus, a discussion in parallel terms for the two types of productivity would seem to be required, but is often not provided.

Lack of productivity in syntax can also be seen in other ways. Recall that Lipka's label for semantic lexicalisation is 'idiomatisation'; in syntax, too, idiomatisation – here interpreted literally as the creation of idioms, with idioms often defined as being made up of several words – leads to the formation of new lexical chunks which do not correspond to what can be produced by synchronic rules. Lexicalisation works in syntax as well as in morphology, and leads to lack of productivity in both places. There may be a difference in the ease with which a compositional reading for a lexicalised chunk is achievable. In general, lexicalised words are not easily used in other than their lexicalised senses, but it does occur occasionally:

> I try very carefully to avoid saying the word 'housewife' because I think it's very insulting – it makes it sound as if a woman is married to a house. (*The Observer*, 21 October 1973)

> Rygebordet stod og røg,
> Hostesaften hosteded.
> [The smoking table was smoking,
> The cough medicine coughed. My translation, LB.] (Rasmussen, Halfdan, *Børnerim*. København: Schønbergske, 1964: 90)

It seems to me that it may be easier to use the literal sense of an idiomatic phrase:

(6) She tripped over a mop and kicked the bucket as she fell.

But if this is a difference, it is one of degree not of type, and the parallels between morphology and syntax are strong.

Now consider the situation in phonology. The general principle that has been applied there is that not all rules are fully productive, but that all partial regularities are to be described in terms of rules. As early as

1969, Zimmer pointed out that some phonological constraints isolated by the linguist do not appear to be used by speakers of the language, while others do. He discusses this in terms of disparities between a linguist's grammar and a native speaker's grammar. He specifically terms those processes which are used by native speakers 'productive rules' (Zimmer 1969: 319) and suggests that excluding the disparities 'can only serve to make the notion of competence more realistic' (1969: 321). A number of subsequent papers also questioned the psychological reality of many phonological rules (Ohala 1974; Steinberg and Krohn 1975; Hsieh 1976). Mainstream phonological theory ignored these problems, so that Mohanan's (1986) treatment of Lexical Phonology still has rules corresponding to the Great Vowel shift in its synchronic derivations (see e.g. Mohanan 1986: 10). In natural schools of phonology, distinctions were drawn between, for example, processes and rules (Stampe 1979: 46), or between P-rules and via-rules (Hooper 1976: 14, 17). Via-rules are specifically stated to apply where a process is 'not productive' (Hooper 1976: 17), although productivity per se is not appealed to by Stampe (and Hsieh [1976] divorces productivity and psychological reality). Via-rules are very like the redundancy rules used by Jackendoff (1975). Jackendoff, too, seems to see these in terms of (non-)productivity, in that he claims (1975: 668) that 'the normal mode for syntactic rules is creative, and the normal mode for lexical rules is passive.' However, much of the evidence presented in chapter 4 points to a conclusion diametrically opposed to this: many morphological rules are frequently used in the production and comprehension of morphologically complex words. The interpretation that has been put on this in this book is that non-lexicalised words in productive series make use of rules. The same form of words would suit the syntactic cases.

Consider now, for a moment, the process familiarly known as Velar Softening by which /k/ alternates with /s/, and /g/ alternates with /ʤ/ in pairs such as *electric/electricity* and *analogue/analogy*. A precise statement of the environment in which this alternation takes place is notoriously difficult. Although we find cases of 'soft <g>' or 'soft <c>' before close front vowels in words like *gill* ('a measure') as opposed to *gill* ('respiratory organ'), such observations are based on orthographic criteria rather than morphophonemic criteria. While in the model of phonology presented in Chomsky and Halle (1968) it might have been possible to give /ʤɪl/ a 'free ride' to the surface on the back of the Velar Softening rule, in more recent phonological models there is no reason to

assume an abstract underlier for the surface /ʤ/. Hyman (1975: 198) suggests that Velar Softening may be a rule which applies to specific morphemes, saying overtly that 'it appears that the only productive conversion of [k] to [s] is when the word ends in *-ic*' and suggesting that the environment for the rule might be better framed in terms of the specific morphemes '{ity, ism, ify, ize}' (the omission of *-ist* is presumably an oversight). But there is evidence that even this might be too generous. Ohala (1974) reports a small-scale study with just twenty-six subjects where, in an experimental design that allowed the relevant forms to be masked by a number of irrelevant ones, subjects were asked to pronounce the result of adding the suffix *-ism* to bases ending in *-ic*, with a resulting non-item-familiar word. The result was only about 30 per cent of the responses showed Velar Softening. In *toxic+ism* only four respondents used the expected [s]. Ohala concludes that Velar Softening is not available as a phonological rule in the Chomsky and Halle sense, although some speakers may use it in an analogical rather than in a productive way.

In a similar way, there is abundant evidence that the English Vowel Shift rule (which is postulated by Chomsky and Halle [1968] to account for those instances of morphophonemic variation which arose because of the operation of the Great Vowel Shift) is no longer a productive phonological rule (Ohala 1974; Steinberg and Krohn 1975; McCawley 1979; Jaeger 1984; Wang and Derwing 1986). Most of the phonological processes which are dealt with in Level I in a level-ordered phonology could probably be added to the list.

Against this background McCawley's (1986: 30) statement that 'velar softening in English is completely lexicalised and non-automatic' makes perfect sense. The implication here is that lexicalisation affects phonological structures just as much as it affects morphological and syntactic ones. A consistent strategy would be to make productive phonological rules part of the synchronic grammar of a language and to list the results of non-productive ones. This view, espoused in Bauer (1978) and derivable from many of the works discussed above, has not been widely accepted in phonology. Yet it would not only be in line with what is done elsewhere in grammar, it would be a step towards a more cognitively-based phonology. Some kind of via-rules or lexical redundancy rules might be retained for the unproductive phonology, but they would not be part of the grammar as such.

Certainly we can see in phonology the same kinds of restrictions that we find in morphological productivity, with morphophonemic rules applying only in specified phonological environments, only in certain morphological environments (e.g. the fricative-voicing rule that gives us *sheaves* from *sheaf* applies only before the plural), or only in certain syntactic environments (marked by such factors as word-class and complexity of boundaries, etc.). Even pragmatic factors such as required clarity of speech (associated with such things as the social environment or the individual who is being addressed) may influence the application of phonological rules. There is probably no influence of semantic context, but this is not unexpected given the nature of phonology.

And while it is the case that morphophonemic rules either apply or do not apply in any given word-form, there are many post-lexical rules which may apply to a greater or lesser extent in the speech of individuals or social groups; this is, after all, the very basis of sociolinguistic variation at the phonetic and phonological levels. In New Zealand English, for instance, /t/-tapping, aspiration of plosives and /h/-dropping or retention, among several other such features, have been found to be variable and correlated with social factors. While this kind of variation may not be precisely the same as the variation found in the morphological data, nonetheless it can have the same effect, namely that one particular phonological rule is exploited more often than another, or that any given innovative string of elements is more or less likely to show the application of a particular phonological rule.

In all these ways, the productivity of phonological rules can be seen as parallel to the productivity of morphological rules. There are at least two ways, though, in which the two differ. First, in the case of morphological rules we found cases where two or more morphological processes were in competition for some specific effect (e.g. in the nominalisations discussed in section 6.4). I am not aware of any case in which phonological rules can be said to be in competition with each other. For example, I know of no case where two sounds assimilate to each other either in terms of place of articulation or in voicing but not both. Variation between glottalisation and aspiration of intervocalic plosives in English is not quite parallel to the morphological case if it is a case of individual variation and not a matter of differing norms. The second point is a related one. In the morphological instances there were examples where the morphological rule which applied depended upon the prior application of some other

morphological rule (*-ity* is added just in case the previous morphological process involved the addition of *-able*, for example). While it is quite clear that one phonological rule can feed another, and in principle at any rate, the application of phonological rule A may create the sole environment in which rule B may operate, the two cases do not seem to be entirely parallel. This difference may be no more than the status of the intermediate form, which is a grammatical and acceptable surface word in the morphological case, but not necessarily in the phonological case, but it is not particularly obvious whether such a difference is trivial or not. We can simply note that there does seem to be a difference here.

However, if we consider again the definition of morphological productivity given in section 7.1.4, we can see that it can apply to phonology as well as to morphology, and that this view of productivity can be used to argue for a more psychologically-real version of phonology than is available in some phonological models today.

A further point of interest is that some scholars (e.g. Bat-El 1996; Plag 1999) are suggesting that some of the constraints restricting the productivity of morphological processes may be phonological constraints affecting the overall phonological structure of the coinages. The view of productivity as an output constraint is an interesting one, not incompatible with what has been said in this book. The notion that phonology may have a larger part to play in morphological grammaticality than has previously been thought suggests that a parallel treatment of the two would be beneficial.

Overall, it seems that the ideas often discussed in relation to morphological productivity are also of interest when applied to phonological and syntactic productivity, but that productivity has not been treated in a uniform way across all three areas. A reconsideration of some of these other areas in the light of the morphological study of productivity has the potential to raise some interesting questions.

7.3 Summary

In this chapter we have provided a definition for morphological productivity based on the discussions earlier in the book, in which an attempt has been made to reconcile the conflicting notions reported earlier. We have also considered the extension of notions of morphological productivity to phonology and syntax and suggested that such a move might well create new insights into the way in which those components of the grammar function.

7.4 Looking back and looking ahead

Although there has been a great deal of discussion of morphological productivity in the literature, there has not been a coherent picture presented of what it is and what its implications are. The present book is a contribution to the debate, giving a rather more organised picture of what morphological productivity entails and how it should be dealt with than has been explicitly laid out before. Of course, there are weaknesses in the picture as presented here; it is to be hoped that they will lead to further developments in the field. In particular, perhaps, no attempt has been made here to anchor the view of morphological productivity within any particular linguistic theory. Partly this is because the general problems that have been discussed are problems that have to be solved in every linguistic theory; partly it is the result of the author's agnostic attitude to the available linguistic theories. It is to be hoped, however, that a better theory will emerge because some of the principles espoused here have been built into it. Linguistic theory should not simply ignore morphological productivity as an uncomfortable epiphenomenon of something not yet understood, but take it seriously as a crucial part of the way language works, and confront the issues that productivity raises.

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