The EVOLUTION of MORPHOLOGY

Andrew Carstairs-McCarthy

The Evolution of Morphology

Studies in the Evolution of Language

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ANDREW CARSTAIRS-McCARTHY

To Jeremy

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Preface and acknowledgements

In the 1980s I began to publish on morphology, and in the 1990s on language evolution. In this book these two interests converge. One may regard it as a fleshing out of a talk that I gave at the Harvard conference on language evolution in 2002, published in 2005 as a chapter in *Language Origins: Perspectives on Evolution* (Carstairs-McCarthy 2005a).

In my earlier book on language evolution, *The Origins of Complex Language* (1999), I discussed the basic architecture of the clause—rather presumptuously, you may think, since syntax is not my specialism. The general verdict on that book seems to be that it is intriguing but (so far as the central thesis is concerned) unconvincing. Whether or not that general verdict is correct, I would like to take the opportunity to reassure readers that what I say in my present book is logically quite independent of what I said in *The Origins of Complex Language.* There would be no contradiction in finding what I say now totally persuasive even while regarding everything I said in 1999 as rubbish. But probably most readers' judgements will be less extreme in either direction.

For assistance during the long gestation of this book, I would like to thank colleagues at the University of Canterbury Department of Linguistics, who have heard more than one try-out of ideas developed in it. In particular I would like to thank Heidi Quinn, who advised me on the role of morphology in the Minimalist Program. I would like to thank also two visitors to the University of Canterbury: Stephen R. Anderson, who spent much of his study leave in Christchurch during 2004–5, and Joan Bresnan, who was here for the Lexical-Functional Grammar conference in 2004, organized by Ida Toivonen and Ash Asudeh. With Steve I had many enjoyable conversations on morphological issues. For a time, Ida, Steve, and I met regularly to discuss consonant gradation in Saami, which poses tricky questions about the respective roles of phonology and morphology in accounting for complex morphophonological phenomena. Joan Bresnan encouraged me on the grounds that no one else was thinking along the same lines as me, so, if I didn't write this book, no one else would. (I later discovered, however, that there is a welcome convergence between my thinking and that of Dieter Wunderlich (2008).) Naturally, none of these people is to be held responsible for my opinions.

At Oxford University Press, John Davey has been constantly encouraging and admirably prompt in replying to questions.

I am now retired, so it seems appropriate to widen my acknowledgements to include everyone who enabled me to undertake a career in linguistics. These include the British taxpayer, who supported me as an undergraduate at Oxford in 1963–7; the Governing Body of the School of Oriental and African Studies, which awarded me a Postgraduate Exhibition in 1967–9; the Harkness Fund, which awarded me a scholarship that paid for my studies at MIT in 1969–71; the US National Institute of Mental Health, which indirectly supported me at MIT, through a research assistantship, in 1971–2; and the Leverhulme Trust, which awarded me a Senior Scholarship that enabled me to complete a Ph.D. at the School of Oriental and African Studies in 1979–81. Thanks to the generosity of all these agencies, I was able to enjoy eleven years of full-time university study without incurring any debt. Few students nowadays are so fortunate.

Last but not least, I would like to thank my partner Jeremy, who has encouraged me unstintingly during the whole long and often frustrating enterprise.

Christchurch
18 March 2009

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Design in language and design in biology

1.1 A difficulty faced by human linguists

Human beings are peculiarly badly placed to understand why human language is as it is. This is because we are all native speakers of some human language, and none of us speaks any non-human language. Thus, none of us has knowledge of any language-like capacity belonging to any other animal species. (By 'any language-like capacity', I mean any comparably elaborate system for communication and for the mental representation of experience.) No such species exists, after all—that is, no other species with such a capacity. We are increasingly aware, it's true, that many animals can convey to each other in subtle ways detailed information belonging to particular restricted domains. Yet outside science fiction, we have never encountered any other creatures (Martians or Venusians, say) with a capacity of this kind that is as elaborate as our own and also as unrestricted.

The non-existence of such Martians and Venusians means that we have no standard of comparison that might help us to distinguish in human language between characteristics that are expected and characteristics that should be regarded as surprising. For example, should we or should we not regard it as surprising that the notion 'grammatical subject' seems to be applicable to so many languages? This question can be tackled from a variety of angles (syntactic, semantic, logical), and the technical literature on it is huge. But there is one kind of evidence that we cannot bring to bear on it, even though if it were available it would be of the highest relevance. Is the notion 'subject' applicable generally to the grammar of the language-like capacities of other species, such as Martians? The answer to this question would be worth its weight in gold to syntactic theorists and logicians. But it is a question that, in the world as it is, it is fruitless to ask.

The point that I am making can be illustrated with a parallel in a non-linguistic domain. Imagine what it would be like if it were not language but some other characteristic—some characteristic that is widespread in our own world—that was limited to just one species. Let us imagine a world where many species, including our own, reproduce by combining genetic material from two individuals, but in all but one such species there is no distinction between the type of contribution that each parent makes: no distinction between ovum and sperm, hence no distinction of sex. Fans of the writer Ursula Le Guin may be tempted to visualize the humans on this world as like the hermaphroditic inhabitants of the planet 'Winter' in her novel *The Left Hand of Darkness* (1969). Yet, even on Le Guin's Winter, a person does become temporarily a clear-cut male or female during the monthly mating period. In the world that I am inviting you to imagine, by contrast, reproduction involving two distinct sexes, male and female, is limited to just one species: the sea-horse (that small fish with a peculiar upright posture and horse-like head that makes it popular in aquariums). Sea-horses in that other world reproduce exactly as in ours.

Other-world biologists can describe in accurate detail how the female sea-horse inserts her egg into the male, who then fertilizes it with his sperm and carries it inside his body until it is ready to hatch. But can the other-world biologists really be said to understand sea-horse reproduction: what is relatively surprising about it, and what is less surprising? I chose the sea-horse as the sole sexually reproducing species in that other world precisely because it is so untypical of the world we know. The usual pattern of behaviour is for the male to insert his sperm into the female, and for the female to undertake the task of carrying the fertilized egg while it develops into an embryo. Is it just a historical accident that this way of doing things preponderates so heavily over that of the sea-horse, or is there a deeper reason? It seems likely that there is indeed a deeper reason, related to the fact that, by comparison with the male's numerous tiny sperm, a female's eggs are relatively large, few in number, and costly to manufacture. Not being a biologist, I will not presume to say more than that. The important point is that the imbalance between the two patterns of reproductive behaviour is a huge factor to be taken account of in the task of reaching an adequate theory of sexual reproduction in our own world—yet it is a factor that, for biological theory-builders in the imaginary world, is entirely missing. In trying to understand the sea-horse's reproductive behaviour, the imaginary biologists are at a tremendous disadvantage by comparison with our own world's biologists. And linguists in our own world are at just such a disadvantage, unfortunately, in trying to understand language.

1.2 How to overcome the difficulty: The power of
abductive reasoning

The difficulty faced by linguists, and by biologists in a hypothetical world where only sea-horses reproduce sexually, is serious. But it is not totally insuperable. It resembles a difficulty that is fundamental to two scientific disciplines concerned with past events: palaeontology and cosmology. I will focus for a moment on cosmology. Cosmologists, astronomers, and physicists seek to answer basic questions about why the universe is as it is. Why is the universe expanding? Will it carry on doing so forever? Why is matter distributed in small tight concentrations with enormous gaps in between? What is the relationship between space and time? It would help cosmologists tremendously if there were other universes that they could compare with this one. In the absence of such universes (or, at least, in the absence of any access to them), cosmologists have to adopt a different research strategy. They have to devise thought experiments, asking themselves: 'In order for as many as possible of the currently observed characteristics of the universe to fall neatly into place, what assumptions do we need to make about its origin and about fundamental laws governing it?'

   In 1965, a kind of 'white noise', or background hiss, puzzled the designers of a new Bell Laboratories communications antenna in New Jersey. Various possible sources for it were checked: defects in the equipment, electrical or magnetic interference from neighbouring cities, and so on. But none of these apparently plausible explanations for it worked. Robert Dicke at Princeton University then realized that the hiss was most readily explicable on the assumption that it was due to residual radiation from the 'Big Bang' with which our universe began. (For a lively non-technical account of this, see Bryson 2003: 9–13, 131–2.) It is not that the hiss proves conclusively that the Big Bang theory is correct. Rather, it is that the detection of the hiss (along with other observations about superficially unrelated matters) is most naturally explained as a consequence of well-established physical principles, provided that we assume also a primordial Big Bang.

   The form of this argument is at first sight surprising, if one examines it closely. It is different from the form of the prototypical scientific argument, of the kind that underlies replicable experiments in (say) a chemistry laboratory. The prototypical kind of argument has the following **deductive** form:

The hypothesis to be tested is p. The proposition p has as a consequence the claim that if q is true, then r must be true too. Therefore in appropriate experimental conditions we arrange that q holds, and then check whether r holds also. If we observe r, then the

experiment tends to confirm the hypothesis p, whereas if we observe not-r, the experiment disconfirms p.

This form of argument is familiar in linguistic theory, too. Let p be some hypothesized principle of Universal Grammar, let q be the statement that a given set of items are candidates for sentencehood in some language, and let r be a claim about the grammaticality judgements that native speakers will apply to them, in the light of p. If the grammaticality judgements actually offered by native speakers are consistent with r, we regard p as confirmed (at least for the time being). If not, there is at least some work to be done on p, even if we are not prepared to jettison it outright.

The argument concerning the Big Bang has a somewhat different form, however. It runs like this:

The hypothesis to be tested is p. If p is true, then, on the basis of other well-established assumptions, we will expect to observe q, r, s, t . . . as well. If p is false, there is no obvious connection between q, r, s, t. . . . Yet q, r, s, t, . . . are all true. The likelihood that p is true is therefore increased, inasmuch as it explains the otherwise apparently random coexistence of q, r, s, t, ...

This is the kind of reasoning which, following the work of the American philosopher and logician Charles Sanders Peirce, has come to be called **abductive** (Peirce 1940: 150–6). Another label for it is 'inference to the best explanation'. It is the kind of reasoning that I will be using in this book to suggest an explanation for certain puzzling characteristics of language. First, however, I will say a little more about it in general and in application to cosmology.

In cosmology, needless to say, it is not the mysterious hiss alone that established the Big Bang theory as superior to the rival Steady State theory. That would be to reason on the basis of q alone, without any accompanying r, s, t. . . . There was an older observation (corresponding to r for our purposes) that fell into place under the Big Bang theory too: light from very distant stars tends to cluster towards the red end of the spectrum. This makes sense if these distant stars are moving rapidly away from us, because then this 'red shift' can be seen as an instance of the Doppler Shift—the relationship between wave length and relative velocity that explains why the whistle of a passing train seems to rise in pitch as it approaches and fall in pitch as it recedes (Bryson 2003: 127). The Steady State theory suggests no obvious reason why distant stars should be moving away from us, but the Big Bang theory does supply one: the expansion of the universe initiated by the Big Bang is still going on.

In cosmology, therefore, a solid abductive argument can be mounted. As seemingly unconnected observations, we have the red tinge of distant galaxies,

the behaviour of train whistles, and the mysterious hiss picked up by the Bell Lab antenna—and no doubt many more facts that a cosmologist, astronomer, or physicist could adduce. As a well-established assumption, we have the Doppler Effect. And as a hypothesis in terms of which the observations all fall into place, given the Doppler Effect, we have the Big Bang theory. The Big Bang theory is thus established not on the basis of experimental evidence of the kind prototypically associated with a chemistry laboratory, but on the basis of its success in accounting elegantly and economically for a range of apparently disparate facts.

Many important conclusions in linguistic theory are established by investigations logically parallel to the chemistry-lab kind. The proposals put forward in this book, however, are justified on a basis more closely parallel to the Big Bang theory. Abductive reasoning is not foreign to linguistics, particularly not to historical linguistics, in that linguistic reconstruction is mainly abductive in character;[1] but it is less familiar to grammatical theorists, and for that reason I have spent some time justifying its use here.

1.3 Narrowing the focus: Why does morphology exist?

The component of grammar with which this book is concerned is **morphology** (the structure of complex words). I will be applying abductive reasoning to suggest answers to certain fundamental, closely linked questions:

  (a) Why does morphology exist—or, equivalently, why do complex words have a structure that must be described differently (at least in some degree) from how the structure of phrases and sentences is described?
  (b) Why does morphology have the characteristics that it has, motivating the traditional distinctions between morpheme and allomorph, between affix and root, between inflection and derivation, between concatenative and non-concatenative exponence, and between productive and unproductive processes?
  (c) Why is morphology widely thought to be more closely associated with the lexicon than syntax is?

I will attempt to show that these are serious questions, and that they are not too vague to tackle. The answers that emerge (at least in outline) help to make sense of certain otherwise puzzling aspects of how grammar works.

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[1] Andersen (1973) also uses the terms 'abduction' and 'deduction' in a narrower technical sense, in relation to sound change.

In a nutshell, the account that I will offer runs as follows. Morphology exists because **morphophonology** exists—that is, the phenomenon whereby what seems clearly to be the same item, in some sense, appears in more than one shape: *man* and *men*, for example, or *keep* and *kep-* (as in *kept*), or *-sume* and *-sump-* (as in *consume* and *consumption*). In turn, morphophonology exists because of the route that language evolution has taken. To be more precise, morphology exists because of certain accidental characteristics of the raw materials (cognitive and expressive) that natural selection had at its disposal during the period when the biological underpinnings for language in humans were evolving. If these characteristics had been absent (that is, if human brains had in certain crucial respects operated differently and if human bodies had been constructed differently), language could well have evolved, but without anything corresponding to complex word-forms. There would have been grammar, but there would have been nothing like what we know as morphology.

It is not that morphology fulfils no useful functions—'useful' here being informal shorthand for 'relating to either communication or cognition', and 'cognition' being in turn shorthand for 'the mental representation of experience'. Rather, the kind of orderliness that morphology displays, though it is often exploited to fulfil communicative or cognitive functions, is not particularly well designed for that task, and it often not exploited for such functions at all. Morphology is often messy where we might expect it to be tidy, and it is surprisingly tidy in areas where messiness might seem tolerable, given what we think we know about how languages change and how they are learned.

Until I present evidence to back up this claim, however, readers are entitled to be sceptical. Indeed, the very question 'Why does morphology exist?' is likely to provoke at least three kinds of adverse reaction. I will comment on each of the three briefly, foreshadowing fuller discussion in later chapters.

Firstly, the question presupposes that morphology does indeed exist as part of the architecture of language, distinct from phonology, syntax, semantics, and the lexicon. That is by no means an uncontroversial assumption. This whole book is implicitly devoted to showing that it is nevertheless correct. More specifically, attempts to partition morphology and allocate its parts to other areas of grammar are examined in Chapter 2, where I criticize certain attempts to motivate morphology by reference to 'the lexicon', to syntactic movement, and to the existence of linguistic elements that are 'bound' in the sense that they cannot stand on their own.

The second kind of adverse reaction is an impatient shrug. To ask why morphology exists (one may think) is a bit like asking why language in general

exists, or why kangaroos exist, or why Europe exists. 'Those questions are pointless, surely—they are just too vague to get a grip on,' says the impatient objector. 'Questions about how a particular affix originated, for example, or what a particular species of kangaroo feeds on, or how particular European countries have acquired the borders that they have—for such questions one may hope to find answers. But questions about the very existence of affixes or animal species or geopolitical entities such as Europe are bound to lead nowhere.' In answer to this reaction, I ask readers for the time being to suspend their scepticism. The proof of the pudding will be in the eating. If an interesting answer can be supplied to a question such as this, that in itself shows that the question was worth asking. For my question about morphology I hope to provide the outlines of an answer that is not only interesting but also convincing enough to deserve continued exploration.

My mention of natural selection will, for some readers, provoke a third kind of adverse reaction. The origin and evolution of language is notoriously a topic that most serious linguistic scholars since the nineteenth century have regarded as too speculative to be worth discussing. This attitude began in a small way to shift around 1990, the year of two important and (above all) linguistically well-informed forays into this no-go area: Derek Bickerton's book *Language and Species* and the target article 'Natural language and natural selection' by Steven Pinker and Paul Bloom in *Behavioral and Brain Sciences*. For most theoretical linguists, however, it was probably not until 2002 that language evolution leapt suddenly towards centre stage, with the publication of the article 'The language faculty: what is it, who has it, and how did it evolve?' by Noam Chomsky along with two experts on animal communication, Marc Hauser and Tecumseh Fitch (Hauser et al. 2002). That article set in train a vigorous debate (Pinker and Jackendoff 2005; Fitch et al. 2005; Jackendoff and Pinker 2005).

This book, however, is not directly a contribution to that debate. I focus here on aspects of language and its evolution that are scarcely discussed by either the allies or the opponents of Chomsky (see section 1.5 below). That sounds as if it may imply that what I say does not impinge at all on their debate. In fact, it turns out that there is indeed an overlap. If I am right, then important parts of what is said about morphology within Chomsky's Minimalist Program may be off track. However, this may be a positive rather than negative outcome for syntactic theorists. It absolves them from having to make sense of certain aspects of grammar that do indeed make no sense from the point of view of the development of syntax. Besides, my position is already 'Chomskyan' in some degree, because the causal chain that I invoke (phonology gives rise to morphophonology which gives rise to morphology) is

consistent with the spirit of a number of Chomsky's recent comments. For example, Chomsky says (2004a: 405):

... a large range of imperfections [in language] may have to do with the need to 'externalize' language. If we could communicate by telepathy, they would not arise. The phonological component is in a certain sense 'extrinsic' to language, and the locus of a good part of its imperfection, so one might speculate.

Whether phonology is extrinsic to language or not is a matter of how one defines 'language', an issue that I do not wish to get bogged down in here. But the term 'imperfection' is not inappropriate, given what I have said about bad design.

1.4 Design in biology: What it does and does not mean

Issues about design in language specifically will be broached in the next chapter. Before embarking on them, I need to say something about good and bad design in a wider biological context. This is partly because I wish to show that there is nothing eccentric in raising this issue in relation to language. Partly, however, it is because for many readers, quite apart from what they may think about investigating the evolution of language in particular, alarm bells will ring when they hear the term 'design' used in relation to evolution. The next three subsections will therefore be devoted to clearing away distractions and possible sources of misunderstanding.

1.4.1 *'Design' does not mean 'intelligent design'*

In recent years, in relation to evolution, the word 'design' has most often been heard in the collocation 'intelligent design' (ID). This term is associated with the view that, whether or not natural selection may be one mechanism through which organisms have evolved, certain features of many organisms (most notably, intricate interrelationships between how parts of the organisms function) provide evidence for an intelligent designer, that is (presumably) a divine creator. Some organisms display a kind of irreducible complexity (so it is said) that natural selection alone cannot explain.

I am not sympathetic to this argument, for reasons of the kind advanced by many mainstream biologists. (For discussion, with arguments both for and against ID, see Dembski and Ruse 2004.) But in any case it is not necessary for me to take a position here on whether the universe is or is not ultimately the work of an intelligent designer. This is because I will be focusing attention on aspects of the human organism and its behaviour whose design does not seem intelligent at all—whose design indeed seems quite stupid. As preparation,

some clear non-linguistic examples of evolutionary outcomes that could not be deemed intelligent by even the most charitable judge of design will be pointed out in section 1.4.3.

1.4.2 *'Good design' is not tautologous*

Trivially, every species is well enough designed to avoid extinction, at least for the time being. Nevertheless, there are some species of which one can reasonably say that they meet higher design standards than that, while other species are so inefficient and clumsy in their ecological niche that one wonders how they manage to survive at all. Respective examples are the grey squirrel and the various species of tree-kangaroo. The North American grey squirrel, introduced into the British Isles, has been so successful as to drive out almost completely the native red squirrel. By contrast, although tree-kangaroos have adapted to arboreal living by acquiring longer fore limbs and shorter hind limbs than most kangaroos, a zoologist is still moved to comment: '[They] are ungainly in trees and their success can only be explained by an absence of predators or of competitors of equal size' (Strahan 1995: 306).

Differences in design quality (if one can put it like that) underlie a phenomenon that has become sadly familiar in the course of human settlement in remoter parts of the globe, particularly islands. New Zealand, before continuous human occupation began within the last thousand years, was home to almost no mammals (only two species of bat) but a huge variety of birds, both flying and flightless. Almost all of those native species are now extinct. That is not solely because their habitats have disappeared. Even in those areas of the country that remain pristine, native bird life is hugely depleted because of competition and predation from introduced species of bird and mammal. Similar stories can be told about many other parts of the globe.

Clearly, one cannot say that the native fauna of New Zealand were badly designed for their habitat. They were designed well enough to survive for millions of years. Yet they were not so well designed, even for their native habitat, as some species that are not native. These introduced species, in their home environment on a large continent (usually Eurasia or North America), are typically spread over a wider variety of habitat types than New Zealand can supply. In achieving this wide geographic spread, they have adapted to take advantage of a wider variety of food sources and to survive a wider variety of challenges from competitors and predators. Can one admit these facts yet at the same time deny that these introduced species are better designed than the native species? Such a position is, it seems to me, mealy-mouthed. To acknowledge that species A is less well designed than species

B does not, after all, require one to be indifferent to species B's extinction, or forbid one to take steps to prevent it, if possible.

What about us human beings: are we well designed or not? Implicitly, we tend to regard ourselves (and human language) as meeting high design standards. After all, according to traditional Jewish and Christian doctrine, we are made 'in the image of God'. Our notorious success must show that we resemble the grey squirrel more than the tree-kangaroo, we are inclined to think. But, given that no similarly intelligent and communicative species such as Martians or Venusians are available for comparison with us, in the way that the squirrel is available for comparison with the tree-kangaroo, this may be self-flattery. We are designed well enough to have survived, and indeed to thrive so far, on Earth, just as the huge flightless moa was designed well enough to thrive for millions of years in New Zealand. The moa were lucky enough to enjoy a benign environment, facing no mammalian predators or competitors. But they quickly became extinct when an energetic mammal species arrived, namely ourselves. Members of some other species may perhaps say of us too in future that we were lucky: humans enjoyed a benign environment, without competition or predation from any other intelligent species using a kind of language better designed than theirs, so that their shortcomings (including the shortcomings of the kind of language they had) were obscured.

1.4.3 *Examples of bad design in vertebrates*

In the previous section I argued that it can make sense to say of a species that it is less well designed than some other species for the environment that it inhabits. In this section I invite readers to consider not whole species but particular characteristics of individual organisms. Does it make sense to ask whether, say, the alimentary tract (whereby food gets from the mouth to the stomach) is well designed? The evolutionary biologist George C. Williams says yes (1992: 7):

Many features of living organisms are functionally arbitrary or even maladaptive. The neck skeletons of giraffe, man, and mouse are all marvels of mechanical engineering for the different ways of life of these divergent mammals. Yet all have seven vertebrae in this region, a functionally inexplicable uniformity. The only acceptable explanation is historical, descent from a common ancestor with seven cervical vertebrae. . . .

The same necks can illustrate persistent maladaptation. All vertebrates are capable of choking on food, because digestive and respiratory systems cross in the throat. This likewise is understandable as historical legacy, descent from an ancestor in which the anterior part of the alimentary tract was modified to form a previously unneeded

respiratory system. This evolutionary short-sightedness has never been correctable. There has never been an initial step, towards uncrossing these systems, that could be favored by selection.

A crucial phrase here is 'historical legacy'. It just so happened that, in vertebrates, the orifice that came to be used for breathing (the nose) was located above, not below, the orifice used for eating (the mouth). This configuration presented difficulties: the relative positions of the stomach and the lungs made it necessary for the air tube and the food tube to cross over.

These difficulties were overcome; in adult humans, the cross-over point is located at the pharynx. But the way in which they were overcome was less than ideal. The crossover inevitably creates a risk that food or air will go down the wrong tube, and in particular that food will enter the respiratory tube, causing choking. A better design could have been achieved by repositioning the two orifices. But natural selection does not plan ahead: it provides no mechanism for backtracking, whereby reproductive success is compromised in the short term for the sake of long-term benefit. Biologists sometimes talk in terms of an evolutionary landscape, with hills and valleys, in which natural selection helps species to ascend the closest hill. But a crucial word here is 'closest'. As Dawkins (1995: 79) puts it: 'Unlike human designers, natural selection can't go downhill—not even if there is a tempting higher hill on the other side of the valley.'

It is important to note that a feature of an organism can be maladaptive (as Williams puts it), yet the species to which it belongs can still thrive. This situation demonstrates the unhelpfulness of the phrase 'the survival of the fittest' as a capsule formulation of Darwinian natural selection. If the criterion for fitness is survival itself, the claim it makes is circular. On the other hand, if we look for criteria independent of survival, it becomes clear that organisms that are not particularly fit do indeed survive. All that is necessary is that they should be fit enough for their environment. For us vertebrates, fortunately, that environment has never contained a rival strain of vertebrates in which the positions of nose and mouth are reversed, so that the respiratory and alimentary systems do not interfere with one another and no individual ever dies of choking.

This design flaw is far from unique. Williams (1992) mentions two others: the vertebrate eye and the mammalian sperm duct. In vertebrates, unlike cephalopods such as octopuses, nerves are connected to the cells of the retina on the inner or lensward side, thus helping to obstruct light from reaching the retina and necessitating a 'blind spot' where the bundled nerves pass through the retina on the way to the brain. And in mammals the sperm ducts that link the testes to the penis are looped back over the ureters that connect the

kidneys to the bladder, and are thus centimeters longer than they need to be. The latter case demonstrates how changing environmental conditions can render an original good design bad, even though (trivially) it still remains good enough for species survival. For warm-blooded creatures there is an advantage in having the testes outside the main body cavity, for the sake of coolness. If only, when blood warming began, the testes had been differently positioned relative to the ureter, the advantage of cool testes might have been achieved with a relatively short sperm duct. As it was, the two tubes became looped around one another, and there was no way in which such a radical flaw could be remedied by the kind of step-by-step improvement that natural selection permits.

1.5 Back to language: Williams and Chomsky

I mentioned earlier a relatively recent development among linguistic theorists: the interest now shown in language evolution by Chomsky and some of his colleagues. There is an intriguing convergence between the ideas of Chomsky and those of George C. Williams. Understanding that convergence will clarify much of the thinking that underlies this book.

In 'Beyond explanatory adequacy' (2001), Chomsky discusses the genetically determined initial state of the faculty of language in the individual (S_0). This is (he says) 'a product of evolution'. S_0 is closely tied to 'the initial conditions on language acquisition'. These initial conditions fall into three categories:

(a) General properties of organic systems, of the kind investigated by the biologist D'Arcy Thompson (1961) and the mathematician Alan Turing (1992). These are physical and mathematical rather than biological in character. They include such disparate phenomena as the role of the Fibonacci series[2] in determining the shape of pine cones, and the role of physics in determining that a mouse the size of an elephant could not exist (because its legs would be too flimsy to support its body).

(b) The 'interface condition' in S_0: its 'principled' part, which reflects the fact that S_0 must interact with the human brain and the human articulatory apparatus (the 'conceptual-intentional' and 'sensory-motor' systems respectively).

(c) 'Unexplained elements' of S_0, that is, any of its characteristics that are not attributable to (a) or (b).

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[2] The Fibonacci series is the series of numbers, starting with zero and 1, such that each is the sum of the two previous numbers: 0, 1, 1, 2, 3, 5, 8, 13, 21 …

Compare this threefold distinction now with one that Williams makes (1992: 6):

  (a) The concept of organism-as-crystal, emphasized by mechanistic biologists such as D'Arcy Thompson.
  (b) The concept of organism-as-artifact, emphasized in studies of adaptation through natural selection.
  (c) The concept of organism-as-document, which 'should also be recognized [by] biologists interested mainly in unique evolutionary histories'.

There is a close resemblance between these threesomes. For example, when Chomsky focuses on the interface condition in S_o ((b) in his scheme), he is concerned with aspects of it that have come to be the way they are through evolutionary adaptation ((b) in Williams's scheme). Yet there is a sharp divergence between Chomsky's and Williams's attitude to the third element in each of their schemes. For Chomsky, anything in (c) (that is, anything that cannot be assigned to (a) or (b)) is not susceptible of 'principled explanation' and is therefore not interesting. For Williams, however, (c) is something that the biologist interested in 'unique evolutionary histories' must pay particular attention to. For example, the biologist interested in why the vertebrate eye is structured differently from (and less efficiently than) the cephalopod eye is bound to be interested in the early history of eyes in the two lineages. It must be that it was sheer accident (sheer bad luck, one might say) that, in vertebrates, light-sensitive cells and their associated nerves were originally so configured that (to use Dawkins's metaphor) the 'tempting higher hill' represented by the cephalopod arrangement was never accessible by small incremental improvements.

Chomsky's position seems to presuppose that any aspect of language that is due to what he calls 'path-dependent evolutionary processes' (2001), that is any aspect attributable to contingencies of human prehistory, is bound to be uninteresting. But this is prematurely pessimistic. What if plausible assumptions about earlier stages of human history (particularly linguistic prehistory) turn out to make sense of aspects of contemporary language that seem puzzlingly ill-designed? Then we may be able to construct an abductive argument to explain them, analogous in form to the Big Bang argument. It is this sort of argument that I will seek to construct.

I mentioned earlier the contrasting twenty-first-century views of language evolution taken by Chomsky and his colleagues on the one hand, and by Pinker and Jackendoff on the other. Chomsky's hunch is that what will yield the most fruitful insights is investigating factor (a) in his scheme. (This view emerges not only in Chomsky's joint work with Hauser and Fitch but also in

various solo essays: Chomsky 2004a, 2004b, 2005.) Pinker and Jackendoff, by contrast, argue for greater emphasis on factor (b). My approach, with its emphasis on factor (c), is therefore different from both of these. It is not accidental that my approach derives from an interest in an aspect of language that Chomsky, Pinker, and Jackendoff all tend to neglect: not syntax (Chomsky's focus) or semantics, neurolinguistics, and psycholinguistics (the main focuses of Jackendoff and Pinker), but morphology.

1.6 Advice to readers

This book is aimed towards two kinds of reader: professionals in linguistics (whether academics or students), and people interested in language evolution from other disciplines and from among the general public. For some non-linguist readers, what I have said already about evolution and natural selection will be very familiar. On the other hand, these readers are likely to find themselves on less familiar territory in later chapters, and already, on encountering terms such as 'allomorph' or 'morphophonology' they may have had to reach for a dictionary or a glossary of linguistic terms. For the sake of these readers, I will need to present some material in a fashion that may seem elementary to linguists. I hope that readers in both categories will be willing to put up with these shifts in level (so far as they are concerned), recognizing them as inevitable in a book of this kind.

2

Why there is morphology: Traditional accounts

2.1 A puzzle as viewed from Mars

A language with a grammar is clearly better designed, both for communication and for the mental representation of experience, than a language without a grammar. By **grammar** I mean a system or systems for encoding the semantic relationships between individually meaningful units in any utterance, so that a composite meaning for the utterance can be reliably inferred. It is thanks to such systematic encoding that in English, for example, *gold coín* (with stress on *coin*) means unambiguously 'coin made of gold (or one that looks as if it might be made of gold)' and not, for example, 'gold used for making coins' or 'coin used for buying gold'. Likewise, thanks to systematic encoding, we can be sure that the sentence *Sarah killed the alligator* describes a reptile fatality, not a human one. By 'a language without grammar' I mean a language with a vocabulary but with no such systematic encoding. That kind of language is essentially what Bickerton (1990) has labelled **protolanguage**.

In my definition of 'grammar' I used the words 'a system OR SYSTEMS'. Why is the plural included here? The answer is that we need to allow English to count as a language with a grammar, yet English has not one but two systems for encoding semantic relationships: syntax and morphology. In this respect, English is like most languages. I will illustrate this in the next section with two examples from English and one from Zulu. But first I will focus on how a hypothetical Martian linguist might view the distinction between syntax and morphology—a Martian being (as envisaged in Chapter 1) a member of a species with a communication system that is as elaborate and versatile as human language but has evolved independently.

Let us assume that Martian 'grammar' consists unequivocally of a single encoding system, not two. Then the Martian's first question would probably be an incredulous 'Why? Given that SOME kind of grammar is useful, what

advantage could there possibly be in having ᴛᴡᴏ kinds?' Assume also that, much as on Earth, the characteristics of Martian 'grammar' are studied by Martian linguists who submit research proposals to funding agencies. What would be the reaction of a Martian funding agency to a request for funds to investigate why Martian 'grammar' constitutes a single system, not a dual one? The funding agency would reject such a request at once. It would be rather as if on Earth a linguist were to request funds in order to study why people whose main speech-control area is in the right hemisphere of the brain (a small minority of the population) do not produce sentences whose word order is, so to speak, a mirror image of the order observed in sentences produced by the left-hemisphere majority. The research would thus be wanting to explain why right-hemisphere speakers do not say *Mat the on sat cat the* rather than *The cat sat on the mat.*

The Earth linguist's question is not entirely bizarre, perhaps, in view of the fact that the two brain hemispheres are in many respects mirror images of one another. Even so, the Earth funding agency would almost certainly reply: 'Handling mirror-image grammars would saddle our brains with an enormous extra processing burden. Given that both hemispheres can evidently operate with the same grammar, it is hardly surprising that a dual system with back-to-front syntax never emerged as a rival. Please direct your research efforts towards more worthwhile questions!' The smidgeon of plausibility that attaches to the idea of mirror-image syntax would not be enough to justify allocation of scarce research funds to that project.

Contrast this with the Martian funding agency's reaction to the proposal for research on the dual-grammar possibility. In the agency's eyes, no corresponding smidgeon of plausibility would mitigate the utter bizarreness of the proposal. Given that the Martians are used to single-system grammar in their counterpart to language, why would any reasonable Martian regard as puzzling (and therefore as worth investigating) their lack of a more complex kind of grammar? Yet linguistic researchers on Earth are confronted in actuality with precisely what to the Martian funding agency would seem so bizarrely improbable.

I hope this parable has helped to persuade readers that the distinction between syntax and morphology constitutes a genuine puzzle that needs to be investigated. It is one that it is easy to overlook, however, just because we are so used to languages in which syntax and morphology can be more or less clearly distinguished. It is just like the puzzle of sea-horse reproduction, which would be easy to overlook in a world where sea-horses are the only animals with two distinct sexes.

2.2 The two systems within grammar: Are they genuinely distinct?

A possible reaction to the Martians' incredulity is that it is a sign that we Earth linguists are after all mistaken. If having two systems is really so pointless (or such a bad design feature), perhaps we have been wrong in assuming that there are two distinct systems within the grammar of human languages. So this section is devoted to showing that we have not been wrong. Consequently the problem of accounting for the distinction is genuine. The remaining sections of this chapter will be concerned with various approaches to this problem, some new and some old.

Syntax achieves the purpose of grammar (as defined in section 2.1) through the positioning of meaningful elements (such as words) in a linear sequence, hierarchically structured into larger units (such as phrases and clauses). A linear sequence requires at least two elements. It would seem to follow that, in order to show that syntax does not constitute the whole of grammar, it is sufficient to invoke systematic semantic differences that involve only single meaningful elements, not sequences of two or more. And that is easy enough to do in English. Consider the following pairs of word forms:

(1)   a. wait   waited
      b. sing   sang
      c. cling clung
      d. give   gave
      e. fly   flew

In (1a), the difference between the present- and the past-tense forms of the verb WAIT is expressed by the presence of the element *-ed* in the latter, following the verb root *wait*. In the (1a) pattern, therefore, one might perhaps argue that we are dealing with a syntactic phenomenon. We are used to thinking of words as the minimal meaningful elements that syntax deals with, so one might object that the suffix *-ed* is not traditionally classified as a word. But against this, one can argue that *-ed* is all the same a minimal meaningful element (in linguistic terminology, a suffixed **morpheme**), and we should not allow a terminological distinction between 'words' and 'morphemes' to mask the essential sameness in status of *wait* and *-ed* as syntactic units.

In respect of examples (1b–e), however, a syntactic analysis is harder to justify. In these examples, the past-tense form contains not a sequence of two elements but a single element, it seems, that differs from the present-tense form in its vowel. Yet there is a long-established tradition in structuralist

linguistics (particularly American versions of it) that blurs the distinction between (1b–e) and (1a) in that it treats (1b–e) as consisting of a string of two elements, just as (1a) does. (An older representative of this tradition is Bloch (1947), but it is still alive in Distributed Morphology, as developed by Halle and Marantz (1993) and subsequently.) Let us suppose that *sang* at (1b) is to be analysed as merely the superficial expression of a string consisting of a root and a suffix that is realized phonologically as zero: *sing* + Ø. The zero suffix has the effect, in this instance, of triggering a vowel change in the root. More important for our purposes, though, is just the fact that the zero suffix is a suffix, thus restoring the structural parallel between *sang* as *sing* + Ø and *waited* as *wait* + *-ed.*

Whatever the merits or demerits of such an analysis, however, it does not suffice to cast serious doubt on the reality of the syntax-morphology distinction. This distinction rests on more than differences in shape (whether superficial or not) such as been *waited* and *sang*. I will illustrate this from English (again) and from Zulu, turning to some facts of quite a different kind.

How is the relationship expressed in English between an action and the person or thing that undergoes the action—what is typically, in semantic terminology, the 'Theme' of the clause? It turns out there is no single answer. Consider the following:

(2)   Elizabeth opened the can.

(3)   The can opened easily.

(4)   Surprisingly, that rusty old opener of ours opened the can without any difficulty.

In (2), the verb *open* is accompanied by two noun phrase **arguments** (expressions identifying participants in the state of affairs), namely *Elizabeth* and *the can*. The phrase *the can*, the object of the verb *open*, expresses the argument with the semantic role Theme, while *Elizabeth*, the subject of the verb, expresses the Agent. In (3), by contrast, *the can*, while it still expresses the Theme, is the subject of the verb rather than the object. In (4), as in (2), the Theme *the can* is again the object, but this time the argument expressed by the subject is not an Agent but an Instrument, expressed by *that rusty old opener of ours*. What this illustrates is that the semantic roles that can be expressed by the grammatical subject include at least the Agent, the Theme, and the Instrument, while the Theme can be expressed sometimes by the subject, sometimes by the object. And it is not only the Theme that is syntactically so fluid. Consider an elaboration of (2):

(5)   Elizabeth opened the can with that rusty old opener of ours.

Here, the phrase *that rusty old opener of ours*, expressing the Instrument, appears neither as subject (as in (4)) nor as object of the verb, but in a prepositional phrase.

   This mismatch between semantic roles (such as Agent, Theme, and Instrument) and syntactic or grammatical functions (such as subject and object) warrants investigation. Why does syntax work this way? Many answers have been suggested. It is not the purpose of this book to offer another or to choose between existing ones. I will merely say that a possibly correct answer is: 'For no good reason, in terms of design; it reflects an accident of linguistic evolution, just as the design defects of the vertebrate eye reflect an accident of anatomical evolution.' For present purposes, what is important is that we have not yet exhausted the ways in which semantic relationships can be expressed. Consider (6):

(6)   that rusty old can-opener of ours

In this phrase, the Theme *can* precedes *open* (or rather, it precedes *opener*, which contains *open*), just as in (3). Yet the semantic relationships expressed by (3) and (6) are not the same. In (6), unlike (3), the semantic make-up of the expression contains an Instrument; indeed, an Instrument (the opener) is what the whole expression denotes. What's more, expressions of this kind, where one noun precedes another noun formed from a verb with the suffix -*er*, are possible even when no sentence parallel to (3), with the Theme as subject, exists:

(7)   Many pig-hunters come to this forest.
(8)   *Pigs hunt plentifully in this forest.
(9)   People hunt pigs in this forest.

What (8) shows is that *hunt*, unlike *open*, cannot have a Theme as its subject (that is, not unless the sentence is rendered passive with the auxiliary BE: *Pigs are hunted in this forest*). Even so, *pig-hunter* in (7) is just as well-formed as *can-opener* in (6): it means 'someone who hunts pigs', just as *can-opener* means 'something that opens cans'. In (7), *pig* and *hunt-* (in that order) seem to have the same semantic relationship as *hunt* and *pigs*, in the opposite order, in (9).

   The near-universal conclusion of linguists (though with some qualifications, discussed in section 2.3 below) is that the structure of the expressions *can-opener* and *pig-hunters* belongs to a different component of grammar from the structure of expressions such as *The can opened easily* and *People hunt pigs*. The former expressions belong to morphology, which deals with the

structure of complex words; the latter belong to syntax, which deals with the structure of phrases and sentences.

Now for some Zulu data. (For description of relevant aspects of Zulu grammar, see Doke 1973, and Rycroft and Ngcobo 1979.) Compare the following eight sentences:

(10)	Izintombi	ziyaphuza	ubisi	'The girls are drinking milk'
	girls	are.drinking	milk	
(11)	Izintombi	ziyaphuza	amanzi	'The girls are drinking water'
	girls	are.drinking	water	
(12)	Amakati	ayaphuza	ubisi	'The cats are drinking milk'
	cats	are.drinking	milk	
(13)	Amakati	ayaphuza	amanzi	'The cats are drinking water'
	cats	are.drinking	water	
(14)	Izintombi	ziyaluphuza	ubisi	'The girls are drinking the milk'
	girls	are.drinking.it	milk	
(15)	Izintombi	ziyawaphuza	amanzi	'The girls are drinking the water'
	girls	are.drinking.it	water	
(16)	Amakati	ayaluphuza	ubisi	'The cats are drinking the milk'
	cats	are.drinking.it	milk	
(17)	Amakati	ayawaphuza	amanzi	'The cats are drinking the water'
	cats	are.drinking.it	water	

The basic word order of a simple sentence in Zulu is the same as in English: Subject Verb Object (SVO). But the form of the expression glossed as 'are drinking' in (10)-(17) varies considerably. Examination reveals two common elements in all the forms: a terminal element -*phuza* and an internal element -*ya*-. These are respectively the verb root, meaning 'drink', and an aspectual element which can be glossed as 'Progressive' or 'be . . . -ing'. But preceding -*ya*- and sometimes following it are pieces that vary. Preceding -*ya*- we find *zi*- when the subject is *izintombi* 'girls' and *a*- when the subject is *amakati* 'cats'. This illustrates the fact that verbs in Zulu must agree with their subjects in gender and number, or in a category combining gender and number that Bantu scholars call 'class': *izintombi* is Class 8 while *amakati* is Class 6. Following -*ya*- we sometimes find an element -*lu*- when the object is *ubisi* 'milk' and -*wa*- when the object is *amanzi* 'water'. This illustrates the fact that a verb in Zulu will agree with its object if the object is definite, as indicated by the presence of 'the' in the English gloss, whereas if the object is indefinite (e.g. 'milk' or 'water' as opposed to 'the milk' or 'the water'),

this object marker (*-lu-* or *-wa-* in these examples) is missing. Again, class is relevant: *ubisi* is Class 11, *amanzi* Class 6 (like *amakati*).

Now consider the following four sentences:

(18)   Ziyaluphuza   'They are drinking it'
(19)   Ayaluphuza   'They are drinking it'
(20)   Ziyawaphuza   'They are drinking it'
(21)   Ayawaphuza   'They are drinking it'

The fact that all these four examples have the same English gloss reflects a variation in form according to the class of the drinker and of what is drunk. But more puzzling is the fact that the way in which they are written suggests that they consist of only one word, without any trace of a preverbal subject corresponding to 'they' and a postverbal object corresponding to 'it'. What has happened to my earlier claim that Zulu has SVO order, like English?

I have been careful to say that these expressions are written as if they are single words, not that they are single words. As it happens, Bantu scholars (more precisely, the missionaries who first devised writing systems for Bantu languages such as Zulu) have disagreed vehemently on this issue. Some have preferred the following style of writing (so-called 'disjunctive' or 'disjoined'):

(22)   Zi   ya   lu   phuza   'They are drinking it'
       SUBJ.CLASS8   PROG   OBJ.CLASS11   drink

Here, what were represented as prefixes in the single-word rendering are implied to be separate pronouns and a verbal aspect marker. But this does not solve the problem about SVO order, because the order here seems to be S-PROG-O-V, with the verb at the end. Why is there this inconsistency?

The standard reply is that there is no inconsistency because only the order illustrated in (10)–(17) belongs to syntax. The order illustrated in (18)–(21), by contrast, belongs to morphology. When the Theme argument in all these sentences is represented by a noun phrase, as in (10)–(17), that noun phrase appears as a syntactic object, following the verb. When however it is represented by what in English is glossed as an unemphatic pronoun 'it', there is no object following the verb; instead, there is a pronominal element (*-wa-* or *-lu-*) before the verb. What's more, this pronominal element is bound, not free: it cannot appear on its own as an elliptical sentence, unlike a noun such as *ubisi* or *amanzi*. Further still, this pronominal element is not mutually exclusive with a noun phrase object, as is shown in (14)–(17): when both occur, the implication is that the object is definite rather than indefinite.

This kind of co-occurrence option is unavailable to the pronoun *it* in English, as is shown by the contrast between (14) on the one hand and (23) on the other:

(23) a. *The girls are drinking it milk.
      b. *The girls are drinking it the milk.

To say that the structure of (18)–(21) is morphological rather than syntactic does not explain anything, admittedly. All we have done so far is assign a label to a set of differences in behaviour: differences with respect to (10)–(17) in the order of constituent elements, in their freedom of occurrence, and in their combinability. But that suffices for present purposes. I am concerned in this section only to show that a difference between morphology and syntax exists, not to explain why it exists. And evidence of the kind cited from English and Zulu could be duplicated from innumerable languages.[1]

Now we are in a position to begin to address two questions, the first of them obvious, the second less so:

 (i) Why does the morphology-syntax distinction exist?
(ii) Given that a distinction between morphology and syntax exists, how are they respectively exploited?

Question (i) can in turn be divided into two subquestions which, in the light of our discussion, we can nickname the *sing-sang* question and the *pig-hunter* question. The *sing-sang* question relates to how a given approach handles relationships such as those between the pairs of items at (1b–e)—relationships of the kind traditionally called 'morphophonological'. These (on the face of it) involve differences in shape but not in sequence or hierarchical structure. By contrast, the *pig-hunter* question (which I might equally well have called the *ziyaluphuza* question) relates precisely to sequence and hierarchical structure. What are we to make of the kinds of difference in sequence that we observed in *pig-hunters* and *People hunt pigs* at (7) and (9), and between ... *ziyaphuza ubisi* (with the object *ubisi* 'milk' following *-phuza* 'drink') and *ziyaluphuza* (with the object marker *-lu-* preceding *-phuza*) at (10) and (18)?

In sections 2.3 to 2.7, I will present a variety of directions from which linguists have approached, explicitly or implicitly, the problem of the morphology-syntax distinction. I say 'explicitly or implicitly' because, as I have already argued, most linguists take the distinction for granted, so any reasons

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[1] Arguments with a similar thrust are presented by Joseph and Smirniotopoulos (1993) in respect of Greek and Sells (2005) in respect of Korean and Japanese. Koopman (2005), however, tries to counter Sells's argument in respect of Korean.

they offer for it must necessarily be implicit rather than explicit. I will argue that all these approaches are inadequate as answers to question (i) above, even though each of them may have merit in tackling some aspects of question (ii).

2.3 Morphology as syntax below the word level

Given that we do not need more than one system within grammar, the conclusion that there is really only one system—despite appearances and despite evidence of the kind presented in section 2.2—would be welcome if it could be established. The idea that there really is only one system is proclaimed in the title *The Syntax of Words* given by Elisabeth Selkirk (1982) to a monograph about morphology within the generative framework. Another linguist who has devoted considerable effort to defending this viewpoint is Rochelle Lieber (1983, 1987, 1988, 1992), and it has been taken up again more recently by Joseph Emonds (2002). I will discuss here principally the work of Lieber, because her coverage of the issue is fullest and also because only she squarely addresses the *sing-sang* question as well as the *pig-hunter* question. I will suggest that this viewpoint leaves unanswered our crucial question, or rather a suitably amended version of it: even if morphology is construed as a variety of syntax, why does it differ so substantially from other kinds of syntax?

2.3.1 *Lieber and the* sing-sang *question*

On the *sing-sang* question, Selkirk and Emonds say nothing, at least in the works I have mentioned. But Lieber (1987, 1992) does not shirk the problem; instead, she answers it by invoking mechanisms first developed by McCarthy (1981). What if a word form such as *sang* is indeed analysable into two meaningful elements, not *sing* and Ø but effectively *s-...-ng* and *-a-*? True, the grammatical relationship between them does not happen to be expressed by concatenating them in a linear sequence. In *waited* at (1a), we can reasonably say that two items are concatenated: a verb root *wait* and a suffix *-ed* expressing 'past', sitting side by side on a single tier, so to speak. But McCarthy argued that, to accommodate a mode of word formation that is pervasive in Semitic languages such as Arabic and Hebrew, a root and an affix often need to be thought of as situated on distinct but associated tiers. The Arabic words that are borrowed into English as *Islam, Muslim,* and *salaam* all contain the triconsonantal root *s...l...m* meaning 'peace' or 'submission'. However, the affixes that accompany it are for the most part not concatenated alongside *s...l...m* on the same tier; rather, they are linked with it by way of a

'skeleton' of consonantal and vocalic slots. For the word *Islam*, this skeleton can be thought of as VCCVC. The root *s...l...m* is linked with the consonantal slots in the skeleton, and the affix *i...a* is linked with the vocalic slots. McCarthy's apt term for this sort of morphology is 'non-concatenative'. Within Lieber's framework one can say, in effect, that non-concatenative morphology is just as syntactic as concatenative morphology is; the syntax in question merely happens to be a non-concatenative kind applying below the level of the word as a syntactic unit.

As a way of reconciling the *sing-sang* phenomenon with the idea that morphology is really only syntax, this is ingenious. But it works only at the cost of provoking a question that recurs in one form or another with all attempts to explain away, rather than explain, the morphology-syntax distinction. If *-a-* and *s-...-ng* in *sang* belong in a hierarchic structure of an ordinary syntactic kind (albeit not concatenated on the same tier), we should expect to observe patterns of hierarchy-without-concatenation not just in what is traditionally called 'morphology' but also in grammatical phenomena that are uncontroversially regarded as syntactic. Let us give the name 'Pervasively Non-concatenative English' to a hypothetical variety of English that has this characteristic.

Pervasively Non-concatenative English (PNE) differs from actual English in that its grammar allows larger units, too, to express meanings (grammatical or other) not through concatenative structures but through occupying particular slots in a phonologically defined skeleton. In PNE as I imagine it here, the skeleton consists of phonological words, defined as metrical units, each one containing one and only one main stress. To illustrate what I have in mind I will first illustrate the contrast between syntactic phrases and phonological words in English.

Consider (24), in which phonological words are bracketed, with main stresses indicated by acute accent marks:

(24)   [The óverdue] [chéque'll] [arríve] [in tomórrow's] [máil]

This bracketing is different from a conventional syntactic bracketing, which can be represented (sufficiently accurately for our purposes) as follows:

(25)   [[The overdue cheque]$_{NP}$ [-'ll arrive [in [tomorrow's mail]$_{NP}$]$_{PP}$]$_{VP}$]$_S$

In actual English, the order of the phrases can be altered to some degree without altering their grammatical relationships:

(26)   [[In [tomorrow's mail]$_{NP}$]$_{PP}$ [the overdue cheque]$_{NP}$ [-'ll arrive]$_{VP}$]$_S$

However, altering the order of the phonological words will not yield an acceptable sentence unless, fortuitously, it corresponds to permissible reordering of phrases, as in (27d):

(27) a. *[in tomórrow's] [the óverdue] [chéque'll] [arríve] [máil]
      b. *[máil] [the óverdue] [chéque'll] [arríve] [in tomórrow's]
      c. *[the óverdue] [arríve] [chéque'll] [in tomórrow's] [máil]
      d. [in tomórrow's] [máil] [the óverdue] [chéque'll] [arríve]

In the imaginary language PNE, by contrast, some grammatical functions are expressed through a particular position in the tier of phonological words. In (24), where five phonological word positions are occupied, none of the occupants has any particular grammatical function in virtue of its position. But in PNE, let us suppose that the second phonological word position is always occupied by the phonological word containing the main verb of the clause. Thus, in PNE, (27c) may perhaps be a well-formed sentence, but (27a), (27b), and (27d) (which is the same as (26) in actual English) cannot be, because [arríve] is in the wrong position.

Some readers may notice a superficial similarity between PNE and actual German. In the main clause of a German sentence, an inflected verb form (whether a lexical verb or an auxiliary such as *wird* 'will') has to occupy the second position, in the sense that it has to follow immediately the first phrase:

(28) a. Der überfällige Scheck wird morgen   mit der Post ankommen.
          the overdue cheque   will tomorrow   by post   arrive

      b. Morgen   wird der überfällige check mit der Post ankommen.
          tomorrow   will the overdue cheque   by post   arrive

      c. Ankommen wird der überfällige Scheck morgen   mit der Post.
          arrive   will the overdue cheque   tomorrow   by post

However, this second-position requirement is expressed in terms of syntactic constituency, not phonological constituency. In (28b) the auxiliary *wird* follows *morgen*, which happens to consist of a single word (both grammatical and phonological); but it is the status of *morgen* as a phrase that matters. Similarly, in (28a), *der überfällige Scheck* consists of three grammatical words and two phonological words ([der überfällige] and [Scheck]), but, crucially, it constitutes a single noun phrase. Indeed, there is no language (so far as I know) where the factors crucially affecting the expression of grammatical meanings above the level of the word can include phonological as opposed to

grammatical constituency, in the way illustrated by PNE. But why not, if Lieber's approach is correct?

One possible response to this question is impatient dismissal. 'Why should we expect a language like PNE to be possible? Syntax deals in syntactic objects, so why would a language encode syntactic information by reference to phonological objects such as phonological words?' The trouble with this response is that it is exactly like the response that the Martian (and in particular the Martian funding agency) might be tempted to make if asked to contemplate as a hypothetical possibility the kind of phenomena illustrated by (1b–e). Yet we know that the data of (1b–e) do indeed occur in actual English. So, whatever the answer may be to the question why no language such as PNE exists, impatient dismissal is not an appropriate response to it. What's more, it is a particularly embarrassing question in Lieber's framework, where the *sing-sang* phenomenon is treated as a kind of non-concatenative syntax that just happens to be restricted to words.

There is another embarrassment that arises from (1b–e). It is bad enough that non-concatenative syntax (if we call it that) is restricted to below the word level. What is worse is that it seems to display so much more diversity in English than concatenative below-the-word syntax does. Concatenatively, we have just two patterns for past-tense formation: with the *-ed* suffix, as in *waited* at (1a), and with the *-t* suffix, as in *kept*, *lost*, and *built*. But among their non-concatenative counterparts there are at least four patterns, as in *sang*, *clung*, *gave*, and *flew*—and indeed more than four, if we examine all the varieties of past-tense formation in English. Again, why should this be so, if morphology is really just a kind of syntax, with *-ed* in *waited* and ... *-a-* ... in *sang* as fundamentally the same kind of object?

The claim that PNE does not exist is, at one level, trivial. But the claim that grammar is so constituted that syntax is 'phonology-free' (Pullum and Zwicky 1988), so no language resembling PNE in relevant respects could possibly exist, is an adventurous one. If it is false, it should be easy to show it to be false. It should be easy to find languages where there are phrase-level constructions, involving multi-word constituents, whose description crucially requires reference not merely to other syntactic units but to phonological units such as phonological words. So, if it is a true claim, it cries out for explanation—an explanation that will be impossible in a framework, such as Lieber's, that denies the fundamental validity of the morphology-syntax distinction.

2.3.2 *Lieber and the* pig-hunter *question*

Why does *pig* precede *hunt* in *pig-hunter* but follow it in *They hunt pigs*? Lieber (1983) provides an explanation that is again ingenious, though again

(in my view) unsatisfactory. She invokes what she calls an **Argument-Linking Principle**, which imposes a constraint on the semantic role that can be fulfilled by the sister constituent (call it X) of a verb V in any syntactic configuration of the form [X V] or [V X]. (The Argument-Linking Principle applies to prepositions as well as verbs, but a discussion in terms of verbs only will suffice here.) Application of the Principle depends on the distinction between two kinds of argument that a verb may have: its external argument (basically, the argument expressed by its subject) and its internal arguments (basically, any obligatory argument other than the subject, such as the Theme argument *pigs* in *They hunt pigs*). Specifically, the X position must be occupied by (must 'link', as Lieber puts it) an internal argument of the verb in the V position.

The application of this Principle to *pig-hunter* follows from the structure that Lieber posits for it: [[pig$_N$ hunt$_V$] -er$_N$]. Here, *-er* is a noun-forming suffix attached to the verbal configuration [pig$_N$ hunt$_V$], which is a verb by virtue of the fact that its right-hand element, *hunt*, is a verb. Because [pig$_N$ hunt$_V$] is of the form [X V], *pig* here must be an internal argument of *hunt*, namely its Theme. (The same applies to [[can$_N$ open$_V$] -er$_N$].) The Argument-Linking Principle thus predicts that the following sentences, with the interpretations given, could not be grammatical in any variety of English, because X in the relevant structure [X V] (*tourist* in (29a) and *reserve* in (29b)) is not an internal argument of V:

(29) a. *There are tourist-hunters in that reserve.
          'Tourists hunt in that reserve.'
      b. ?Reserve-hunters do not generally favour pigs.
          'People who hunt in reserves do not generally favour pigs.'

This prediction seems correct in respect of (29a). In respect of (29b) it is less clearly correct, because *reserve-hunter* sounds (to me, at least) a plausible term to use for someone who hunts in reserves rather than elsewhere. Let us put that problem aside, however. A more obvious problem is that the structure posited for *pig-hunter* contains the element [pig$_N$ hunt$_V$], analysed as a verb. Yet English has no verb *pig-hunt*, as the ungrammaticality of (30) shows:

(30) *The tourists pig-hunted all afternoon.
       'The tourists hunted pigs all afternoon.'

This is not a random fact, as is shown by (31) and many similar examples:

(31) a. *John door-opened with a key.
          'John opened the door with a key.'

    b. *Martin novel-writes one a year.
       'Martin writes one novel a year.'
    c. *No thanks, I never coffee-drink.
       'No thanks, I never drink coffee.'

Lieber's answer to this problem is ingenious. As a verb, *pig-hunt* (and likewise *door-open*, *novel-write*, and *coffee-drink*) would be subject in English to a general syntactic requirement on verbs, namely that their argument structure must be 'satisfied' in their immediate syntactic context. To see what this means, consider the simple verb *hunt*. Its argument structure (the semantic roles of noun phrases that must accompany it) includes an internal argument expressing the Theme. So, for the argument structure of *hunt* to be satisfied in its immediate syntactic context, it needs to have a direct object designating what is hunted, e.g. *pigs* or *unicorns* or *that fox that escaped last week*. (It is true that sometimes *hunt* can appear with no overt direct object, as in *She enjoys hunting*; but there is still an object 'understood' here, unlike in examples which genuinely lack any object argument, such as *The door opened* and *The water boiled*. Thus we can ask 'What does she enjoy hunting?' but not 'What does the water boil?')

    So far, so good. But now think about a hypothetical verb *pig-hunt*. This too is a verb, so it too needs to have a direct object designating what it is hunted:

(32)   a. *The tourists pig-hunt pigs.
       b. *The tourists pig-hunt unicorns.
       c. *The tourists pig-hunted that fox that escaped last week.

But these examples are all bad because there is one argument too many. If *pig-hunt* means what it has to mean as a constituent of *pig-hunter*, then the Theme is already 'satisfied' inside the verb itself, and cannot be 'satisfied' also by a phrase outside the verb, such as *pigs* or *unicorns*. Thus *pig-hunt* can never appear as a verb on its own. However, the addition of the suffix *-er* to *pig-hunt* converts the whole word into a noun (*pig-hunter*), and as such it is no longer subject to general syntactic requirements on verbs. In the context [[pig$_N$ hunt$_V$] -er$_N$], therefore, the configuration [pig$_N$ hunt$_V$], even though it is a verb, escapes the requirement that yields one argument too many. It is therefore free to observe the Argument-Linking Principle: [pig$_N$ hunt$_V$] is of the form [X V], and *pig* in the X position can indeed 'link' the internal Theme argument of *hunt*.

    As I said, this is ingenious. It is still unsatisfactory, however. When the verb *hunt* appears in the environment ___-*er*, so as to yield *hunter*, is it appearing in a syntactic environment or a morphological one? According to

Lieber's view of morphology as 'word syntax', this is not an either-or choice: a morphological environment is simply one kind of syntactic environment. However, it must be a special kind of syntactic environment. After all, -*er* has the effect of overriding the verbal status of *hunt* and hence obliterating its need for an internal (Theme) argument to be expressed. So even if we do not recognize morphological environments as fundamentally different from syntactic environments, we have to recognize what one may call 'status-overriding' environments (ones where words can be shifted from one lexical category to another) as distinct from 'non-status-overriding' ones. Thus one kind of morphology—the kind exhibited by words with suffixes such as -*er*, which change verbs into nouns—re-enters by the back door: not as morphology explicitly, but as a special kind of syntax, creating a differentiation between what happens when a verb is sister to an affix such as -*er* and what happens when it is sister to a word or phrase such as *pigs* or *that fox we saw yesterday*.

This counter to Lieber is by no means a counter to all the arguments that have been advanced in favour of morphology as word-level syntax. It illustrates however a pervasive characteristic of such arguments. They typically do not deny that there are some differences between what is traditionally called 'syntax' and what is traditionally called 'morphology'. They argue however that such differences are marginal, or flow from some simple overarching principle such as the one that Emonds (2002) calls the **Domain Size Restriction**:

(33)   No phrase . . . appears within an X^0 (word).

Such a principle predicts correctly that, for example, although a noun can be formed from a bare adjective by suffixing -*ness*, as in (34), no noun can be formed by suffixing -*ness* to an adjective phrase, as in (35):

(34)   happy → happiness 'characteristic of being happy'

(35)   very happy → *very happiness 'characteristic of being very happy'

However, any principle of this kind is problematic in two ways. The first problem is empirical: is it correct? In relation to the Domain Size Restriction, this is doubtful, on the basis of evidence noted by Lieber herself (1992):[2]

(36)   a. over-the-fence gossip
       b. a couldn't-care-less attitude
       c. a Monday-morningish reluctance to get out of bed

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[2] Facts of this kind will be discussed in Chapter 7.

The second and more important problem is: does it suggest any reason why the morphology-syntax distinction should exist in the first place? Without it, would grammar be in some way less efficient at coding semantic relationships, or less easily learned? It seems fair to say that this question—the central one, for our purposes—is barely noted by advocates of morphology-as-syntax, let alone answered.

2.4 Morphology as a driver for syntactic displacement

In section 2.3 we considered the view that morphology does not really exist as a distinct pattern of grammatical organization. In this section we consider a view that has gained ground more recently in the context of Minimalist syntactic theory inspired by recent work of Noam Chomsky. This is the view that some aspects of morphology are needed in order to restore to grammar a kind of 'perfection' that it risks losing because of a conflict between how language is structured, in some fundamental sense, and what language is used for.

It would not be sensible for me to attempt an extended critique of Minimalism in relation to morphology, partly because Minimalism has so little to say about morphology for its own sake. All that Minimalist syntacticians have done is invoke some aspects of morphology to help deal with a certain kind of grammatical anomaly. Other aspects of morphology they are happy to leave to out of consideration altogether, or to relegate to 'Phonological Form', which lies on the fringe of grammar, or perhaps even outside grammar proper. This applies especially to the *sing-sang* question.

A second reason not to embark on an extended critique is more nebulous, but nevertheless valid. Minimalism is presented by its proponents as not a theory but a research programme, based on the hunch that the basic principles of grammar should be simple and economical. So is there good evidence to support this hunch? With disarming frankness, Chomsky admits that there is not (2000: 11):

> As anyone familiar with recent work will be aware, there is ample empirical evidence to support the opposite conclusion throughout.... [A] core assumption of the work within the Principles-and-Parameters framework [which preceded Minimalism] is that everything I have just proposed is false—that language is indeed highly 'imperfect'..., as might well be imagined.

What is suggested, however, is that these imperfections (departures from ideal simplicity and economy) arise through the need for the grammar to interface with the world outside grammar. Specifically, the grammar has to interact

with the human conceptual-intentional system (our brains) and with the human sensory-motor system (the organs through which we perceive and produce utterances). Because of our poor understanding of both these interfaces, we are told, apparent counterevidence to Minimalist assumptions is only to be expected. This is especially so at such an early stage in the development of the research programme. Thus critics of Minimalism are likely to encounter either a frustrating wall of bland agreement, or else an accusation of not understanding what it is to conduct inquiries into the nature of language at an appropriately fundamental level in the light of current knowledge.

That said, Minimalism invokes morphology in an intriguing way to serve an explanatory purpose with regard to syntax. Minimalism explores reasons for apparent 'imperfections' in language. One such apparent imperfection is syntactic displacement. Examples of this are the displacement of a topicalized item and a question word to the front of a sentence in examples such as (37a) and (38a), if (as is argued) the structure that a 'perfect' grammar would supply for such sentences is more akin to (37b) and (38b):

(37) a. Beans I like [*implying e.g. . . .* but not spinach].
      b. [I [TENSE [like beans]]]

(38) a. Who did you see?
      b. [you [PAST [see who]]]

Such displacement may even be observed in a simple clause such as (39a), if one is persuaded by the reasoning of some syntactic theorists to the effect that grammatical subjects originate internally to the verb phrase (or rather, internally to 'vP', a phrase headed by 'little-v'), as indicated in (39b):

(39) a. John kissed Mary
      b. [PAST [John [kiss Mary]]$_{vP}$]

Such reasoning would imply that the structures given in (37b) and (38b) need correction, on the lines of (40) and (41):

(40) [TENSE [I [like beans]]$_{vP}$]

(41) [PAST [you [see who]]$_{vP}$]

Fortunately we do not need for our purposes to have an opinion on how convincing the evidence is for abstract grammatical structures such as (39b), (40), and (41). The point is that the displacement illustrated in (37a), (38a), and perhaps (39a) counts as a grammatical imperfection unless there is something within grammar itself to drive it. This is where morphology may come in (it is

claimed). All lexical items (or words: Chomsky treats these terms as more or less equivalent) have phonetic features, which are interpreted at the sensory-motor interface (that is: which make them pronounceable). They also have semantic features, which are interpreted at the conceptual-intentional interface (that is: which make them meaningful). But many words also have features that are interpretable at neither interface, such as the 'person' feature on verbs. For example, *seems* is 'third person' in (42), agreeing with *Clinton*, yet the third-person suffix *-s* is uninterpretable in that it has no semantic or phonetic function beyond the grammatical requirement for its presence:

(42)   Clinton seems to have been elected.

To make matters worse, apparently, *Clinton* in (42) has no direct semantic relationship with the verb *seem*; rather, its relationship is with *elected*, as a Theme argument. This suggests an underlying structure of the kind represented (in simplified form) in (43):

(43)   [seems [___ [elect Clinton]]$_{vP}$]

The underscore indicates the absence of any overt noun phrase to indicate who has done the electing—what in school grammar lessons a couple of generations ago would be called the 'logical subject'. The absence of an overt 'logical subject' allows *Clinton* to be displaced to the grammatical subject position in a passive sentence such as (44):

(44)   Clinton has been elected.

But (42) seems to illustrate yet further displacement of *Clinton*, to a position as subject of the verb *seem*.

We are thus faced with two imperfections, uninterpretability and displacement. But, says Chomsky, the design of language may nevertheless be optimal, because the two imperfections are related: 'uninterpretable features are the mechanism that implements the displacement property' (2000: 12; 13–14). Uninterpretable features have to be 'erased' (or 'checked'), and this erasure or checking is achieved by bringing uninterpretable features into a 'local relation' with a constituent that has the same features in an interpretable form. The feature 'third person' is uninterpretable on verbs, but it is interpretable on a noun phrase such as *Clinton*, whose meaning inherently excludes the features 'first person' (belonging to *I* and *we*) and 'second person' (belonging to *you*). As Chomsky puts it (2000: 14–15): '. . . the matching [person] features of the agreeing phrase "Clinton" are attracted to the offending features of the main verb "seems," which are then erased under local matching. . . . [Thus] a complex array of phenomena . . . reduce to the

simple fact that uninterpretable formal features must be erased in a local relation with a matching feature, yielding the displacement property required for semantic interpretation at the interface.' In this ingenious way, two grammatical imperfections cancel each other out. Furthermore, a function that lies outside grammar is achieved, in that, by being located at the start of the sentence, the noun phrase *Clinton* is identifiable as 'topic' or 'shared information'.

Does this ingenious approach explain satisfactorily why morphology exists alongside syntax? Some aspects of morphology it certainly does not account for—but, if we point that out, it is likely that we will merely encounter the kind of bland agreement that I mentioned earlier. Minimalists will happily admit that it does not answer the *sing-sang* question, which they lump in with many other unanswered questions concerning the sensory-motor interface. And, insofar as this approach concentrates on inflectional features, it says nothing about derivational issues such as the relationship of *hunter* to *hunt* or of *pig-hunter* to *hunt pigs*. Minimalists would probably say that, because both *hunter* and *pig-hunter* are words or 'lexical items', and thus have no internal structure so far as syntax is concerned, it is unreasonable to expect principles of syntax to shed any light on how they are formed. But it is reasonable to ask to what extent Minimalism answers the question why inflectional morphology, at least, should exist.

The Minimalist account of inflection offered by Chomsky in the work cited here emphasizes the link between inflection and displacement. It is therefore pertinent to ask whether the two always go together. Can we have uninterpretable features (so-called) without displacement, or displacement without uninterpretable features? And what about when interpretable features have no uninterpretable features to be matched with? An example of that would be plural marking on nouns that never imposes any requirement on accompanying determiners, adjectives, or verbs to 'agree in number'. Any of these would seem to be an embarrassment for someone who seeks an explanation for morphology's existence by way of Chomsky's account of how inflectional morphology contributes to optimal grammars. Yet it is easy to find examples of all of them.

Uninterpretable features without displacement can be observed in many highly inflected languages with free word order, such as Latin. Latin expresses on verbs the person and number of subject noun phrases, in a more thoroughgoing fashion than English does—yet Latin does not insist that the subject should appear in an overtly local relation with the verb. Notoriously, all possible reorderings of the words in (45) are grammatical:

(45) Puer   puella-m   ama-t.   'The boy loves the girl.'
      boy.NOM   girl-ACC   love-s

(46) Puer amat puellam.

(47) Puellam puer amat.

(48) Puellam amat puer.

(49) Amat puer puellam.

(50) Amat puellam puer.

The suffix *-t* on *ama-t* expresses the feature 'third person', uninterpretable on verbs, just like English *-s* on *seem-s*. But there is no way in which the subject *puer* can be deemed in all of (45)–(50) to be in a local relation to *amat*, unless 'local relation' is defined in such a capacious fashion that it risks becoming empirically empty.

   The same data illustrate also displacement that is not associated with matching uninterpretable features. If one takes the view that many case features on nouns are uninterpretable (because they relate to the grammatical function of a noun phrase rather than to its wider 'conceptual-intentional' meaning), then feature-matching for those uninterpretable features must take place in local relation with case-assigning items such as verbs and prepositions. Let us assume that in (45) the uninterpretable accusative case of *puellam* is 'matched' through its local relation with the verb *amat*. Case feature-matching cannot then explain in purely grammatical terms why *puellam* is moved to the front in (47). Yet central to Chomsky's account of displacement is the claim that purely grammatical mechanisms are available to account for it, within the framework of an optimal grammar. In defence of this claim, one could perhaps posit, at the start of the sentence in (47), an uninterpretable grammatical feature such as 'Topic'; *puellam* is then moved to the 'Topic' feature for the sake of erasure. But since 'Topic' has no morphological manifestation in Latin, uninterpretable features thereby drift yet further away from explaining the existence of morphology.

   What about interpretable features that never participate in 'matching' and 'erasure' of uninterpretable counterparts? A strong candidate for this is plural marking on nouns in Afrikaans (Donaldson 1993). Afrikaans distinguishes singular and plural forms of nouns, as do Indo-European languages generally. However, there is no agreement for number of the kind we observe in English. This is illustrated by the examples in the pairs of examples at (51) with their English glosses.

(51) a. hierdie boek   'this book'
      b. hierdie boeke   'these books'

(52)   a. My broer werk.   'My brother is working.'
       b. My broers werk.   'My brothers are working.'

Morphology of this kind, because it takes no part in 'erasure', lends no support to a Minimalism-inspired account of why morphology exists.

Even without these difficulties, Chomsky's account of how displacement and uninterpretable features interact has an air of paradox. Consider an analogy. When I go cycling, I carry a puncture repair kit. This is an 'imperfection', because it increases my load. On the other hand, the repair kit comes in handy when I encounter another kind of 'imperfection', namely when I suffer a puncture. The two imperfections thus, in a sense, cancel each other out. But would I wish to say that my cycling is not 'optimal' unless I have punctures? That would seem perverse. Yet it is akin to what Chomsky seems to say in his efforts to reconcile the simplicity and economy of grammatical structures with the constraints imposed by the two interfaces, semantic and phonetic.[3]

The upshot of this discussion is that, even if we grant to Chomsky the benefit of all legitimate doubts concerning what the Minimalist program has so far achieved, one cannot find in it a motivation for why all inflectional morphology behaves as it does, let alone derivational morphology (the other side of the *pig-hunter* question) and the *sing-sang* question. At best, Minimalism may tell us something about why morphology is exploited in certain ways, given that it exists. Yet this conclusion need not be seen by Minimalists as a negative one for them. As I noted in Chapter 1, Chomsky speculates that the locus of a good part of the 'imperfections' of language is the phonological component (in his broad sense of that term). My conclusions support that speculation, provided we say not 'is in the phonological component' but rather 'originated prehistorically in the phonological component'. If that is correct, we are entitled to see most of morphology as a 'documentary' characteristic of language (in George C. Williams's terms), and are thus no longer obliged to seek 'crystalline' explanations for it.

Chomsky himself (2004b) speaks not of crystals and documents but rather of snowflakes and spines. A snowflake is 'perfect', owing its fascinatingly regular structure to principles of physics. A vertebrate's spine is far from perfect, as is shown by the pervasiveness of back pain in humans and (it seems likely) other mammals too. The human spine is the way it is as a result of evolutionary tinkering, most of its characteristics being a product of historical accident. Chomsky's hunch is that language (or at least the peculiarly human aspects of it) are more snowflake-like than spine-like.

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[3] It is only fair to add that some supporters of Chomskyan Minimalism are not convinced that syntactic displacement is driven by morphology, e.g. Hinzen (2006: 208–20) and Moro (2008: 211–26).

However, it would not prejudice the spirit of the Minimalist enterprise if morphology—its existence, that is, rather than the use that languages currently make of it—were relegated to the spine category.

2.5 Morphology as the grammar of bound items

Some linguistic items are free, some are bound. A bound item is one that cannot occur on its own, even in the most elliptical utterance. Many bound items are what are traditionally called 'affixes'; indeed affixes are by definition bound. There exist also roots that are bound in all contexts, as illustrated in (53), and roots that have some bound allomorphs and some free, as in (54):

(53) a. <u>cran</u>berry <u>grue</u>some <u>ruth</u>less
      b. <u>aud</u>ible, <u>aud</u>itory, <u>aud</u>ition

(54) a. *Bound root:*   *Free root:*
          <u>wive</u>-s   wife
          <u>ridd</u>-en   ride, rode
          <u>fif</u>-th   five

The bound roots *cran-*, *grue-*, and *ruth-* in (53a) are of the kind often called 'cranberry morphs': not only are they bound but they occur in only one word, unlike *aud-* in (53b).

These bound items all occur in combinations whose internal structure would traditionally be analysed morphologically rather than syntactically. Could it be, then, that the existence of bound items gives a clue to why morphology exists? That might be so if it turned out that no bound items behave in a way that lends itself to an ordinary syntactic analysis—ordinary, that is, in the sense of not requiring any significant departure from the principles that govern the behaviour of words and phrases. If that is the case, the question still remains why it should be case, and also why bound items exist in the first place. Nevertheless, a neat correlation between morphology and boundness would blunt the urgency of the question why two patterns of grammatical organization exist rather than one.

Connections between morphology and boundness have been explored by Stephen R. Anderson (2005) in particular, building on work by Zwicky (1977) and Klavans (1985). Anderson's project is the inverse of Lieber's, as discussed in section 2.3. Lieber tries to show that morphology really is just syntax below the level of the word. Anderson, by contrast, argues that principles of morphology (as opposed to syntax) play a part in the grammar of some phrases and sentences, so that certain phenomena traditionally regarded as syntactic are

really morphological. Thus, for example, he suggests that the English possessive -'s does just what inflectional affixes do, namely express or spell out grammatical content, but it does so at the level of the phrase rather than the word. This is shown by the fact that -'s is always positioned at the extreme right edge of the bracketed noun phrases (or determiner phrases) in (55):

(55) a. [Fred]'s taste in wallpaper
     b. [the man in the hall]'s taste in wallpaper
     c. [every man I know]'s taste in wallpaper
     d. [that brother-in-law of mine that I was telling you about]'s taste in wallpaper

(Similar arguments with respect to -'s have been advanced by Stump (2001).) Furthermore, Anderson suggests that the behaviour of many superficially word-like bound forms (so-called 'clitics') is best handled in terms of morphological constraints rather than syntactic ones. Thus the tendency on the part of many clitics to occupy the second position within a phrase or sentence is due to the same factors whereby in some languages certain prefixes are not attached at the extreme left edge of a word but are 'infixed' after some leftmost element such as the first consonant, the first consonant cluster, the first syllable or the first metrical 'foot'.

   For our purposes, it is not necessary to examine in detail Anderson's analysis of such clitics, because even Anderson agrees that not all bound forms need morphological rather than syntactic treatment. Consider the English auxiliaries *would* and *will*, illustrated in (56), and the corresponding reduced forms illustrated in (57):

(56) a. In those days I would go to Brighton every weekend.
     b. Tom will eat all the muffins.

(57) a. In those days I'd [aid] go to Brighton every weekend.
     b. Tom'll ['tʰɒml] eat all the muffins.

The reduced forms are clearly bound. The status of *would* and *will* as free forms is clear from examples like (58):

(58) a. You said 'would'—so does that mean you don't go their any longer?
     b. 'Will'? He's already eaten them!

One might argue that the reduced forms are merely effects of fast-speech phonology, so neither syntax nor morphology plays a part in accounting for them. But this cannot be correct, because even in fast speech we never

pronounce *plywood* as [plaid] or *Cromwell* as [ˈkɪɒml]. As Anderson says, it seems inescapable to regard *-'d* and *-'ll* as bound items phonologically distinct from and competing with the free forms *would* and *will*. They are thus bound items whose distribution is handled syntactically, disconfirming the hypothesis that boundness presupposes morphological structure. So, even if Anderson is right in arguing that morphological principles can affect phrases as well as words, it is not the case that all bound elements play by morphological rather than syntactic rules.

2.6 Morphology as lexical structure

I said earlier that the *pig-hunter* question might equally well have been called the *ziyaluphuza* question, alluding to the Zulu difference in order between noun phrases with meanings such as '(the) water' or '(the) milk', which follow the verb, and the elements glossed as 'they' or 'it', which precede the verb. Zulu resembles most Bantu languages in having elaborate agglutinative morphology—or should it be called agglutinative syntax below the level of the word? The syntax-versus-morphology issue has been discussed extensively in relation to another Bantu language, Chichewa, by Bresnan and Mchombo (1995). They conclude that Chichewa evidence supports the view that 'words are built out of different structural elements and by different principles of composition than syntactic phrases' (1995: 181), in other words they too deny that morphology is merely a kind of syntax.

There is a more immediate reason for mentioning Bresnan and Mchombo here, however. It is the label that they choose for their viewpoint: they call it the **lexical integrity principle**. They thus imply that morphology is in some sense 'lexical' rather than syntactic. But what does 'lexical' mean here? It is related historically to the word 'lexicon', which is another word for 'dictionary' and which has long been used in linguistics as a technical term in the sense introduced by Leonard Bloomfield (1933: 274): 'an appendix of the grammar, a list of basic irregularities'. Is it, then, that the existence of morphology as a distinct pattern of grammatical organization is derivable from the fact that not only simple monomorphemic items such as *cat*, *high*, and *laugh* but also many complex items such as *catty*, *highness*, and *laughter* are in some respect irregular or unpredictable, and must therefore be listed in the lexicon?

The word 'lexical' has unfortunately acquired in linguistic theory two meanings that are logically entirely independent. On the basis of one of these meanings, it is inevitable that any kind of language, whether human or Martian, should display phenomena that one can call 'lexical'. On the basis of the other meaning, it is by no means inevitable that anything 'lexical'

should exist.[4] But because of the terminological confusion, one may be tempted to think that phenomena of the latter kind must exist in consequence of the former. Although no one has explicitly argued on these lines—indeed, the fallacy of doing so is obvious as soon as one tries to make such an argument explicit—, I suspect that many linguists have slipped into accepting the existence of morphology as inevitable through precisely this sort of mistake.

In the first sense, 'lexical' means 'concerning the lexicon', where 'the lexicon' denotes Bloomfield's 'list of basic irregularities'. Di Sciullo and Williams (1987: 3) say in a similar vein that the lexicon is 'like a prison—it contains only the lawless, and the only thing that its inmates have in common is lawlessness'. For example, among simple words, there is no law or regularity in English grammar on the basis of which we can predict that *cat* rather than *dog* shall mean 'feline animal'. It is in this sense that lexical phenomena are inevitable. They will be found in any kind of language whose vocabulary includes items whose meaning is not predictable on the basis of their composition (whether auditory or visual, depending on the medium for linguistic expression)—that is, any kind of language whose vocabulary is not entirely onomatopoeic or iconic.

The sphere of the lexicon extends beyond simple words, however. Among phrases, nothing allows us to predict that *to keep tabs on Harrods* shall mean 'to monitor Harrods closely' rather than 'to have an account at Harrods'. Among complex words, nothing allows us to predict that the adjectives *curious*, *glorious*, and *various* shall have as their corresponding nouns *curiosity*, *glory*, and *variety* rather than (say) *cury*, *gloriety*, and *variosity*, or that the verbs *arrive* and *derive* shall have as their corresponding nouns *arrival* and *derivation* rather than *arrivation* and *derival*. And 'lexical' has come to be used in a second sense, too, meaning 'concerning relationships between words'. In this sense, facts such as the relationships between *curious* and *curiosity*, between *glorious* and *glory*, and between *various* and *variety* are at the heart of what is 'lexical' in English. It is precisely such relationships that were explored by Ray Jackendoff in his classic article 'Morphological and semantic regularities in the lexicon' (1975)[5] and by Mark Aronoff in his monograph *Word Formation in Generative Grammar* (1976).

Jackendoff's title seems to leave open the possibility that there may be morphological regularities not only in the lexicon but also outside it. Yet

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[4] Harley and Noyer (2003: 466–7) make a similar point.

[5] For more recent comments on this knot of issues, see Jackendoff (1997: 109–51) and (2002: 152–67).

both Aronoff's and Jackendoff's works belong in a tradition inaugurated by Chomsky in his 'Remarks on nominalization' (1970). This is a tradition of posing the issue of how to treat relationships of the *arrive-arrival* kind as a choice between only two options: a syntactic treatment and a 'lexical' treatment. A syntactic treatment that was popular around 1970 was one on the basis of which the noun phrase *John's arrival*, say, would be derived from a clausal structure such as [John$_N$ arrive$_V$]$_S$. This was held to explain why, for example, suitable alternatives for *John* as subject of the verb *arrive* are also suitable alternatives for *John's* as possessor of the noun *arrival*. Chomsky proposed instead a treatment in terms of which *John's arrival* is only ever a noun phrase so far as the syntax is concerned, and the semantic and other parallels between *arrive* and *arrival* (and within other verb-noun pairs) are handled in terms of features shared by the verb and the noun in the 'lexicon'.

The reader is entitled to be puzzled at this point. The sharing of features between *arrive* and *arrival* is systematic in the sense that it parallels the sharing of features between other verbs and nouns, such as *believe* and *belief*, *defend* and *defence*, *involve* and *involvement*, and *derive* and *derivation*. But how can such systematicity belong in the lexicon if the lexicon is merely a list of irregularities, the members of which have in common only their lawlessness? Why did Chomsky not allow for the possibility that the regular, systematically patterned, aspects of such relationships should be handled in some third component of grammar: neither the lexicon nor the syntax but (let's say) the morphology, or a component concerned with semantic patterns?

This is a question about the history and sociology of linguistics as much as about linguistic theory. For our purposes, it is enough to say that two impetuses within the generative linguistic tradition favoured Chomsky's preference in 1970 for 'the lexicon' as the locus for relationships such as between *arrive* and *arrival*. One was the long-established tradition, dating back to *Syntactic Structures* (1957), of using syntactic rather than morphological means to construct complex words. A phrase such as *have been drinking* was constructed by means of an Auxiliary Transformation (later nicknamed 'Affix Hopping'), whereby a morpheme string *have - en - be - ing - drink* was converted to *have - be - en # - drink - ing #*. This transformation not only rearranged the order of elements but also inserted word boundaries (indicated by #). A salient characteristic of Chomsky's earliest approach to syntax thus emerges: it could operate on morphemes as well as on whole words. There is no hint here of any third mechanism to handle the relationship of morphemes within a complex word-form—a mechanism distinct from both syntactic manipulation and lexical listing.

Such a mechanism might in principle have been supplied by generative grammarians' approach to the *sing-sang* problem, that is to the problem of allomorphy. But that did not happen because allomorphy of the *sing-sang* kind was regarded as part of the phonology. The absence in contemporary English of any phonological motivation for such vowel change in irregular verbs (for example) was no embarrassment. Within the framework of Chomsky and Halle's *The Sound Pattern of English* (1968), the conditions under which a phonological process could be stipulated to apply could include lexical and grammatical conditions as well as phonological ones. Furthermore, *The Sound Pattern of English* even allowed for the 'readjustment' of the phonological features in a morpheme's lexical entry before it undergoes any phonological processes. Thus the phonological information in the lexical entry for *sing* would, in past-tense contexts, be assigned a feature specification indicating that it was subject to the same rule of Vowel Shift that (according to Chomsky and Halle) plays a part also in the allomorphy exhibited in *divinity* and *divine* and in *satisfy* and *satisfaction* (1968: 11, 201, 243).

Readjustment rules are not strictly part of the 'phonological component' of the grammar as conceived by Chomsky and Halle (1968), so it is not entirely clear whether the representations that they operate on deserve to be called 'phonological representations'. Nevertheless, a readjustment rule of the kind needed to create the past-tense form *sang* is purely phonological in its effects, and is not conceived as belonging to a fully fledged component of grammar distinct from both phonology and the lexicon. We noted in section 2.2 that, in Distributed Morphology (Halle and Marantz 1993), *sang* is derived from *sing* through a phonological process triggered by the presence of the affix Ø representing 'past tense'. This sort of phonological process is identified by Embick and Halle (2005) with the 'readjustment rules' of *The Sound Pattern of English*, and they make the further point that, by recourse to such rules, much allomorphy disappears. In particular, all the stem alternations of the kind illustrated in (2b–e) (*give~gave, fly~flew,* and so on) can be handled phonologically, unlike the choice of different suffixes in *wait-ed, kep-t,* and (according to their analysis) *sang-Ø*.

The absence of an explicit morphological component in generative grammatical descriptions provokes the question that formed the title of an article by Stephen R. Anderson (1982): 'Where's morphology?' The label 'Distributed Morphology' implies one answer: the analysis of phenomena traditionally labelled 'morphological' is distributed between the syntax, the phonology and a list of vocabulary items. Other generativists have been inclined to follow Chomsky (1970) in giving pride of place to the lexicon. This is reflected in the title of the reply to Anderson (1982) by Jensen and

Stong-Jensen (1984): 'Morphology is in the lexicon!'. But there is a tension in all these approaches. How can one at the same time maintain the traditional association of the lexicon with exceptionality and 'lawlessness'?

This difficulty was masked by the fact that, in many areas of morphology, exceptionality (or, at least, unpredictability) is the norm: thus, as we have already seen, no principle allows one to predict that the nouns associated with *arrive* and *derive* are *arrival* and *derivation* respectively. Nevertheless, the tension between the lawlessness of the lexicon and the existence of morphological regularities had a big effect on generative morphologists' priorities in the 1970s and 1980s. Perhaps, as Anderson suggested (1982), one can distinguish between derivation, which is 'inside the lexicon', and inflection, which is 'outside' it. Or perhaps, as Aronoff suggested (1976), one can distinguish between 'productive' processes, which are outside the lexicon, and 'unproductive' ones, whose output needs to be lexically listed. As an example of a productive process, Aronoff suggested the formation of abstract nouns from adjectives by the suffixation of *-ness*, while an example of an unproductive process is the formation of abstract nouns with the suffix *-ity* (e.g. *curiosity*, *sensitivity*). This distinction, says Aronoff, helps to explain **blocking**, whereby the existence of a word can block the existence of another that would have the same meaning: thus the existence of *glory* blocks **gloriosity* because **gloriosity*, if it existed, would have to be 'in the lexicon', but *glory* cannot block *gloriousness* because *gloriousness*, being formed by the productive process of suffixing *-ness*, is 'outside the lexicon'.

I have already mentioned Chomsky and Halle's 'readjustment rules', which tinker with phonological features without being part of the phonological component. One can think of readjustment rules as exporting some kinds of 'lawless' behaviour from the lexicon into a limbo between the lexicon and the phonology. Kiparsky (1982), inaugurating the framework of **Lexical Phonology**, proposed a converse manoeuvre: much phonology was imported into the lexicon, to account for phonological aspects of Jackendoff's 'morphological regularities' and Aronoff's unproductive rules. This was not so strange a move as it may at first seem, given that generative phonology already made generous provision for phonological rules to be restricted not only grammatically (applying in some morphological contexts but not others, or to some syntactic categories of words but not others) but also lexically (applying to syntactically and semantically arbitrary classes such as 'strong verbs' or 'first declension nouns'). In terms of Lexical Phonology, the two senses of 'lexical' fuse inasmuch as many relationships between words (e.g. between *wait* and *waited*, between *sing* and *sang*, between *curious* and *curiosity*, and between *help* and *helpful* and *helpfulness*)

are handled in terms of morphological and phonological rules that operate 'in the lexicon'.

This approach has some subtle attractions. I have already mentioned Aronoff's invocation of lexical listing to explain why *glory* can block *gloriosity* but not *gloriousness*. In Kiparsky's framework, blocking effects operate between rules rather than between words or word forms, yet similar effects can be achieved through allocating rules to different levels or 'strata' within the lexicon, and through the ingenious device of treating words themselves ('lexical entries') as rules—but rules with unusually specific content that apply at the transition between successive strata. For example, when the lexical entries *dog*, *cooker*, and *organizer* enter the stratum on which regular plurals are formed, they do so in the form of identity rules: $dog \rightarrow dog$, $cooker \rightarrow cooker$, $organizer \rightarrow organizer$. These lexical entries are then free to undergo the regular plural rule $N_{Plural} \rightarrow N+z_{Plural}$ so as to yield *dogs*, *cookers*, and *organizers*. However, the irregular plural form *teeth* does not acquire the regular plural suffix *-s* or /z/ (*teeths*) because its identity rule $teeth_{Plural} \rightarrow teeth_{Plural}$, applying at the end of an earlier stratum, is more specific than $N_{Plural} \rightarrow N+z_{Plural}$ and thus blocks the application of this rule to *teeth*. The form *teeth* is itself the product of a specific rule, something like $[CuC]_{Plural} \rightarrow [CiC]_{Plural}$, that applies on the earliest lexical stratum—a rule lexically restricted so as to apply only to *goose, tooth, foot*, and perhaps *woman*—which forestalls any possible application of $N_{Plural} \rightarrow N+z_{Plural}$ to *tooth* so as to yield *tooths*.

Kiparsky's approach thus accounts ingeniously for certain blocking effects. However, it requires the term 'lexical' to be used in a way that is fundamentally inconsistent with Aronoff's, as we will see in a moment. This forces us to conclude that the habit of using the same word 'lexical' for the two independent notions that I distinguished earlier is indeed seriously misleading, despite its popularity.

Aronoff assumes that 'only words which are arbitrary in some way must be entered in the lexicon' (1976: 45). This is why *gloriousness* is not in the lexicon: its meaning is entirely predictable ('the characteristic of being glorious'), and it is not subject to lexical blocking by *glory* (unlike *gloriosity* which, if it existed, would have to be in the lexicon because of the idiosyncrasies of the suffix *-ity*). Let us apply Aronovian reasoning now to *cookers* and *organizers*. The word *cooker* must be in the lexicon, because its meanings are unpredictable: it means not 'person who cooks' but either 'appliance for cooking (incorporating oven and hob)' or 'variety of fruit (e.g. apple) that is suitable for cooking'. On the other hand, *organizer* need not be in the lexicon, because its meaning is predictable: 'person who organizes'. This difference is reflected

in how the two words are handled in dictionaries: for example, the Concise Oxford Dictionary does not contain a separate entry for *organizer*, merely noting the existence of this derivative within the entry for the verb *organize*, but it does contain a separate entry for *cooker*. Yet both these words are treated on a par within Kiparsky's Lexical Phonology: both of them are created by suffixation of *-er* on 'level 2', so both leave level 2 as 'lexical entries' via 'identity rules' *cooker* → *cooker* and *organizer* → *organizer* en route to level 3, on which they can acquire the regular plural suffix *-s*.

Recall the example *keep tabs on*, as in *keep tabs on Harrods*. This illustrates one kind of mismatch between the two senses of 'lexical'. It is lexical in the first sense (it must be listed in a Bloomfield-style lexicon) because its meaning is unpredictable, but it is not a 'lexical entry' in Kiparsky's sense in that it is not an item produced by the operations of Lexical Phonology and morphology. In short, like all phrasal idioms, its internal structure is syntactic, not morphological. But examples such as *gloriousness* and *organizer* are mismatches of the opposite kind: they are complex items whose internal structure is morphological rather than syntactic, yet they do not need lexical listing. There is certainly much overlap between the denotations of 'lexical' in its two senses, but there is leakage in both directions.

Let us consider some further examples involving the suffix *-ness*, which is Aronoff's principal example of a suffix that is (he says) fully productive and whose products are consequently outside the lexicon. We have seen that lexically listed items range in size and complexity between items such as *dog* which are unanalysable and items such as *keep tabs on* which are syntactically structured. Given the variety of phrasal idioms in English (*red herring, down in the mouth, take a shine to, put that in your pipe and smoke it*, etc.), it looks as if there are few constraints on the kind of syntactic structure that a phrasal idiom may display.[6] Is it really likely, then, that there are some morphological structures (more precisely, some products of word-formation processes) that are never lexically listed? Is *-ness* suffixation such a case?

It turns out that it is not hard to find nouns with the suffix *-ness* that are semantically arbitrary in some degree. The abstract noun corresponding to the adjective *high* is not *highness* but *height*. Yet it is not that *highness* does not exist; rather, it has a special unpredictable meaning 'royal personage', as in *His Royal Highness the Prince of Wales*. Even such an apparently straightforward noun as *goodness* is semantically arbitrary in contexts such as *If you*

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[6] However, for evidence that there are indeed some syntactic constraints on idioms, see O'Grady (1998) and Kuiper and Everaert (2000).

overcook vegetables, they will lose their goodness. Here *goodness* does not mean 'quality of being good' but rather 'nutritional value'. Notice that this special 'nutritional' sense inheres in the abstract noun only, not in the adjective *good*: thus *good cooking* means only 'cooking that produces appetizing meals', not 'cooking that produces nutritious meals'. And consider the nouns *fastness* and *tightness*. If *-ness* suffixation were always semantically transparent, these nouns should overlap in meaning in just the way that the adjectives *fast* and *tight* do, as in *My finger was stuck fast* and *My finger was stuck tight*. Yet the noun *tightness* has no lexically listed senses (at least in common uses), whereas *fastness* must be lexically listed inasmuch as it can mean only 'absence of susceptibility to bleaching or fading' (in application to colours) or, in archaic usage, 'fortified hideout', as in *mountain fastnesses*. And the reader can no doubt think of other idiosyncratic quirks among nouns with the suffix *-ness*.

The upshot is that the inevitability of 'lexical' phenomena in the first sense by no means entails the inevitability of 'lexical' phenomena in the second sense. How, then, have the two senses come to be seen as linked? Why is there so much overlap, in the sense that so many complex words are indeed idiosyncratic, and so many idiosyncratic items are complex words? This is a far from trivial question, unfortunately muddied by the traditional fluid usage of the term 'lexical'. Some new suggestions for an answer will be offered in Chapter 7.

2.7 Morphology as the detritus of linguistic change

It is hard now to think back to the time, before the influence of Saussure (1973) in the early decades of the twentieth century, when all serious linguistic theorizing dealt with language change rather than with the structure of languages at particular points in time. Diachronic linguistics has indeed enjoyed a revival in recent years, not in the sense that more scholars are engaged in it (though that may be so), but in the sense that it is increasingly seen as appropriate to offer purely diachronic explanations for at least some synchronic facts about how syntax and phonology operate. For example, the question has been raised whether a theory of synchronic phonology is really needed, if it turns out that many or all typological generalizations about phonology can be attributed to how sound systems change over time (Juliette Blevins 2004).

This approach, now being tentatively investigated in other areas of grammar, is well established in morphology. Underlying it is an assumption, usually implicit rather than explicit, that morphological phenomena have a

special need to be explained, or to be explained away—a need not shared with syntactic and phonological phenomena. This assumption echoes the central problem addressed in this book. However, historically based explanations for morphology covertly assume a kind of prehistoric linguistic 'Golden Age', as I will try to demonstrate.

A purely historical explanation for why morphology exists amounts to an assertion that all morphological phenomena can be traced back to ancestral phenomena that were entirely non-morphological, involving only syntax or phonology. It is as if the present stage of any language, in exhibiting divergences between morphological and syntactic structure (the *pig-hunter* problem) or allomorphy that is no longer phonologically conditioned (the *sing-sang* problem), represents a falling away from an earlier state of greater uniformity and regularity. But there is an implicit contradiction here. For any contemporary language that exhibits morphological phenomena of either the *pig-hunter* or the *sing-sang* varieties, no reputable historical linguist seriously aspires to reconstruct some earlier stage at which all such phenomena are simultaneously absent. To do so would be to reject the uniformitarian hypothesis that has traditionally guided historical linguistic reconstruction since the late nineteenth century: the hypothesis that the time depth within which reconstruction is feasible is too shallow for us to reach back to a period at which the human capacity for language manifested itself in fundamentally different ways from now. Because morphology as a component of grammar exists in contemporary languages, it is not something that we should expect to be able to probe beyond (so to speak) by the methods of historical comparison and reconstruction.[7] That being so, why should we imagine that each individual morphological phenomenon is something that a historical linguist should in principle be able to probe beyond, so as to locate for it a syntactic or phonological origin?

This hesitation of mine would be answered if it could be shown that, in practice, non-morphological origins can indeed be identified for all or nearly all the morphology of languages with reasonably old written records. But, as the remainder of this section will show, this is far from the case. Historical changes may indeed provide opportunities for morphology to be exploited, given that it is a kind of grammatical patterning that the human language faculty makes available. But nothing in the manner of language change during the historical period (that is, language change of a cultural rather than

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[7] A similar point is made by Lass (1977, 1997). As he puts it: 'this [i.e. the attempt to probe beyond all morphology] ends up being an indefensible claim about the (general) priority of one morphological type: the "isolating"' (1997: 235).

biological nature, dating from after the emergence of fully modern *Homo sapiens*) supplies an answer to the question that concerns us in this chapter: the question why the syntax-morphology distinction exists.

2.7.1 *Linguistic change and the* pig-hunter *question*

Much research has been devoted in recent years to the kind of semantic change whereby words with lexical or open-class meanings such as 'wish', 'possess', or 'belly' can shift over a few generations towards grammatical or closed-class meanings such as 'future', 'past', or 'in'. Examples of this are easy to find in English: thus, although the verb *will* originally meant 'desire' (a meaning which survives in the corresponding noun, as in *He lacked the will to succeed*), the phrase *I will come* no longer means 'I wish to come' but rather 'I am going to come'. Similarly, French *J'écrirai la lettre*, in which the suffix *-ai* derives etymologically from the Latin word *habeo* 'I have', no longer means 'I have the letter to write' but simply 'I will write the letter'. This phenomenon is called **grammaticalization** (Heine et al. 1991; Hopper and Traugott 2003).

The semantic changes in grammaticalization are often accompanied by formal changes. Words that become semantically bleached in this way often become attenuated in shape too: they may lose their phonological independence (so as to become clitics), or they become 'morphologized' as affixes. Thus, alongside *I will come* we have the more usual *I'll come*, with *will* reduced to the simple clitic *-'ll* (as discussed earlier), and for French *écrirai* '(I) will write' Romance scholars can trace a lineage of attested forms going back to Latin *scribere habeo*, literally 'to-write I-have'. Is it the case, then, that all affixes originated as separate words that have been morphologized in this way?

Givón (1971) added a further twist to this idea when he proposed an explanation for the kind of sequencing anomaly that we have observed in *pig-hunter* versus *(they) hunt pigs*. Perhaps the order *pig-hunt(er)* reflects an earlier stage of English syntax at which objects preceded verbs, indeed at which verbs were sentence-final. And there is indeed some evidence that Proto-Indo-European may have had verb-final syntax. The morphological construct *pig-hunter* may therefore encapsulate in frozen form an earlier syntactic pattern $[\text{pigs}_N \ \text{hunt}_V]_{VP}$, before the shift to modern English $[\text{hunt}_V \ \text{pigs}_N]_{VP}$. From this he derived the slogan: 'Today's morphology is yesterday's syntax.'

It is certainly true that for some affixes a historical origin can be traced in a free word-form, and it is also certainly true that in some morphological

constructs the order of elements represents an earlier syntactic pattern. But as a full explanation for the *pig-hunter* question, grammaticalization has two deficiencies. The first deficiency affects in particular Givón's proposal about the order of elements. Consider the weak object pronouns in modern French and Italian. These are morphologized to the point of being clitics, and perhaps even (by some criteria) prefixes attached to the verb. Examples are in (59) for French and (60) for Italian:

(59)   Jean le lui donne.   'John is giving it to her'
        John it to.her gives

(60)   a. Giovanni glie-lo da.   'John is giving it to her'
            John to.her-it gives
        b. Glie-lo da.   'He is giving it to her.'
            to.her-it gives

In (59) and (60), the present-tense verbs (*donne* and *da*) follow the pronominal objects. This order differs from the one that is usual when objects are nouns, as in (61) and (62):

(61)   Jean donne le cadeau à Marie.   'John is giving the present to Mary.'

(62)   a. Giovanni da il regalo a Maria.   'John is giving the present to Mary.'
        b. Da il regalo a Maria.   'He is giving the present to Mary.'

However, the order in (59) and (60) does reflect the order that is usual in classical Latin, where there was a strong tendency (overridable stylistically) for verbs to come at the end. According to Givón, therefore, the morphologization (at least partial) of the object pronouns is reflected in their retention of an earlier word-order pattern.

Unfortunately, things are not so simple. The route towards the consistent position of weak object pronouns before finite verbs in French and Italian is tortuous. At older stages of both languages a different order was dominant, reflecting a reluctance to permit weak pronouns in sentence-initial position. At that stage, if a sentence began with a finite verb, any accompanying weak pronoun had to follow it, not precede it (Meyer-Lübke 1897). Thus, at that stage in the history of Italian, (60b) would be ungrammatical; instead, we would expect something like *Da glie-lo*—which is, as it happens, is the order found in imperative sentences in contemporary Italian: *Daglielo!* 'Give it to him!'. This earlier order is preserved in the indicative in some frozen expressions, such as *affitta-si* '(house) to let' (literally 'lets-itself') (Lausberg 1972: 124–5). Crucially, however, this earlier order deviates from the usual Latin verb-final pattern more than the

contemporary French and Italian order does. It is hardly likely, then, that the contemporary order is due solely to retention of 'yesterday's syntax'. So, regrettably, Givón's attribution of ordering anomalies of the *pig-hunter* type to earlier syntactic stages turns out to be too sweeping.

The second deficiency is a deficiency only from the point of view of someone who thinks that diachronic change may supply a reason for all morphology, not just some of it. Many researchers in grammaticalization would not go so far as to say this. For example, Hopper and Traugott (2003: 141) state: 'Where long written histories are available, MANY [but, by implication, not all] bound morphemes can be shown to go back to independent words. OFTEN, too, [but, by implication, not always,] a historical source in independent forms can be assumed through inspection of synchronic divergent forms' [emphasis added]. Spencer (2006: 128), however, dispenses with these qualifications, saying flatly: 'Morphological patterns are the result of processes governing grammaticalization. In one sense, this is all there is to morphology...'. In a similar vein, Comrie (1992) has proposed that at an early stage of language evolution there was some kind of syntax but no morphology (that is, all language would have been 'isolating', with roots but no affixes), and no morphophonological relationships of the *sing-sang* kind. This idea is developed further by Heine and Kuteva (2002, 2007), who argue that one can legitimately contemplate a time when 'these processes [of grammaticalization] took place *for the first time*, that is, when there were, for example, verbs but no auxiliaries—hence, when human language was less complex than it is today' (2007: 32). Dahl (2004: 109) proposes the nickname 'Garden-of-Eden language' for this hypothetical stage of language evolution, when complexity was lacking.

But reasons to be wary of the view that grammaticalization is 'all there is to morphology' are supplied by Hopper and Traugott. Directly after the passage just quoted, they add (2003: 141): '... [N]ot every instance of grammaticalization involves morphologization'. This qualification is certainly necessary. For example, alongside the morphologized reflex of Latin *habeo* 'I have' in the suffix -*ai* of *j'écrirai* 'I will write', there still exists a grammaticalized but not morphologized reflex of *habeo* in *j'ai écrit* 'I have written'. Alongside the English expression *I'll come* (consisting of two syllables, morphologized at least to the extent that -*'ll* is a bound form), there still exists *I will come* (three syllables), where *will* is sufficiently independent phonologically to be stressable for contrastive purposes (*I WILL come*!) and detachable from the main verb that it normally precedes (*I've said I'll come, and come I will!*). Thus it is not merely the non-grammaticalized *will* meaning 'desire' that retains the status of a complete phonological word-form; the same can be true of the

auxiliary *will* that expresses 'future'. Innumerable further examples could be cited.[8]

Heine and Kuteva are fully aware of such facts. The effect, however, is to weaken fatally any claim (on the lines of Givón's and Spencer's) that grammaticalization can supply a complete explanation for why morphology exists. Grammaticalization may well supply many illustrations of how morphology has been exploited in linguistic change. However, it is perfectly compatible with an imaginable kind of language in which no morphology exists (or in which all languages are of the 'isolating' type). Thus it can hardly explain fully the origin of morphology as a pattern of grammatical organization distinct from syntax.

2.7.2 *Linguistic change and the* sing-sang *question* [9]

When the Neo-Grammarians proposed the principle of exceptionless sound change in the late nineteenth century, a simple explanation for the *sing-sang* question seemed within reach. Such alternations always had a purely phonological source (it was said), through 'sound-laws' operating in particular speech communities at particular points in time. Sound-laws, with their assimilatory or dissimilatory effects, have in principle nothing to do with morphology and syntax, although they may have morphological consequences. By virtue of one such sound-law, in the prehistory of English, the vowel -*i* in a plural suffix exerted a fronting effect (known usually by the German term 'umlaut') on the stem vowel [o] in the ancestor of the noun *foot,* so as to yield a pronunciation something like [fø:ti]. The stem vowel [ø] was later unrounded and raised to [i] while the suffix was dropped, so as to yield the modern plural form *feet*. Other such sound-laws, operating at an earlier historical period, can account for the vowel alternation (traditionally called 'ablaut') observed in the forms *sing, sang,* and *sung*: they consisted originally of an invariant root that we can represent sufficiently accurately for present purposes as *sng combined with, in *sing* and *sang,* infixed elements that we can represent as *e and *o respectively. (On umlaut in English, one may

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[8]   Quite apart from grammaticalized syntactic collocations that do not go all the way to morphology, some morphological phenomena have a historical origin that is demonstrably independent of any syntactic collocation, as when part of a stem is reanalysed as an affix (Heath 1998). Such instances are less common, but they reinforce the point being made here.

[9]   In this subsection the asterisk has its traditional function in historical linguistic contexts, namely to indicate an unattested but reliably reconstructed form, i.e. a form that is assumed to have occurred at a linguistic stage for which no written records exist. For the purpose that the single asterisk serves elsewhere in the book, namely to indicate a non-occurring or ungrammatical form, a double asterisk is used here.

consult a history of the English language, such as Pyles (1971), and, on ablaut, any handbook of comparative Indo-European grammar, such as Szemerényi (1996).)

If we apply the Neo-Grammarian view of sound-change to the *sing-sang* question, it may seem that the question evaporates in a satisfying fashion. Assuming that sound-changes always operate independently of grammar and that grammar is (so to speak) powerless to resist them, then there is perhaps nothing more to say: *sing-sang* phenomena arise out of historical phonology, even at the cost of creating a morphological phenomenon (namely allomorphy) that language as a communicative instrument could well have done without. This answer is indeed superficially similar to an answer that I will be developing in later chapters. The similarity is only superficial, however, because one of its assumptions is mistaken. It is not the case that grammar is powerless to resist sound-change. A neat example of successful resistance is supplied by so-called 'sigmatic aorists' (certain past-tense forms involving the suffix -*s*-) in Ancient Greek.

Ancient Greek was subject to a sound-law whereby [s] (the sound of the Greek letter sigma, hence the term 'sigmatic') disappeared between vowels. Because of this law, a reconstructable pre-Greek form *genes-os with the genitive case suffix -*os*, meaning 'of the kind', appears in Attic Greek of the fourth century BCE as *genous*, with -*ou*- arising from -*e-o*- by a regular process of vowel coalescence. (In Latin, meanwhile, the same reconstructed form *genes-os yielded *generis*, as in the expression *sui generis* 'of its own kind', i.e. 'unique'. The original intervocalic *s survived as -*r*- by yet another phonological development that was exceptionless within the Latin speech community.) But when a suffixed -*s*- was in Attic Greek a marker of a particular past tense known as the 'aorist', it survived as -*s*- not only after consonants, as we would expect on phonological grounds (e.g. *e-deik-s-e* 's/he showed') but also after vowels (e.g. *e-phile:-s-e* 's/he loved',[10] not e.g. ***e-phile:-e*). Here, grammatical motivation (more precisely, the function of indicating the aorist tense) successfully protects the vulnerable -*s*- from disappearance (Chantraine 1973).

Instances of this kind have been well known for as long as the Neo-Grammarian view of sound-change has been discussed. How can it be claimed that sound-change is exceptionless, then? The traditional explanation is analogy. It is said, for example, that forms such as *ephile:se* preserve the suffixal -*s*- by analogy with forms such as *edeikse*, where the -*s*- survives for

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[10] The prefix *e*- helps to express certain tenses, including the aorist. The endmost suffix -*e* indicates 3rd person singular.

solidly phonological reasons. But is this perhaps too capacious a loophole for apparent exceptions to the 'exceptionlessness' doctrine? That depends on whether appeals to analogy can be suitably circumscribed. This issue has occupied theorists of historical linguistics for over a century. For present purposes what matters is merely the fact that sound-changes can indeed be successfully resisted where there is grammatical purpose to be served in doing so (in this instance, to preserve a more uniform mode of expression for the aorist tense). So the question must now be faced: why is not such resistance always successful?

English supplies another instance where resistance has been at least partially successful, namely in verb morphology. The verbs *heave*, *seep*, and *ooze* conform to the pattern of most English verbs (I will call it the 'regular' pattern) in that their past-tense forms involve a suffix *-(e)d*, pronounced [t], [d], or [ɪd] according to the context, so as to yield [hiːvd], [siːpt] and [uːzd]. On the other hand, clusters of consonants in the coda of a syllable, as in these words—particular clusters consisting of a voiced fricative and a voiced plosive, as in *heaved* and *oozed*—are universally disfavoured.[11] This shows up in the fact that many languages allow no codas at all, many others allow only single consonants in coda (not clusters), and many impose severe constraints on which consonants are allowable. (For example, German requires plosives and fricatives in codas to be voiceless.) And *heaved*, *seeped*, and *oozed* have not only a coda cluster but also a long vowel in the syllable nucleus, yielding altogether a particularly 'heavy' rhyme. These characteristics make them 'bad' syllables, from the point of view of how phonological systems generally behave. English tolerates these 'bad' syllables, clearly, inasmuch as these verb forms exist; but even in English their phonological oddness or markedness reveals itself in the fact that these words could not possibly crop up as simple suffixless morphemes. An advertising agency might suggest a name such as *Dreft* for a washing powder (indeed, a washing powder with that name exists), but no advertising agency would suggest a name such as *Dreevd* [driːvd] for a washing powder or any other product, not even a product with negative connotations such as a weed killer or a rat poison. English-speaking customers would feel shy about asking for it because its name would sound strange, even if (not being linguists) they lacked the technical vocabulary to pinpoint the strangeness.

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[11] In accordance with mainstream phonological theory, I assume syllables to have maximally three components: onset, nucleus, and coda. The nucleus and coda together form a subconstituent, the rhyme. For example, the monosyllabic word *bland* is structured [bl [æ nd]], where [bl] is the onset, [æ] is the nucleus, and [nd] the coda. For more discussion, see Blevins (1995).

It would not be surprising, then, if some sound-change or combination of changes in English were to remedy defects such as these in regular past-tense forms. And indeed something like that is precisely what has happened in the past-tense forms of the irregular verbs *leave*, *keep*, and *lose*, namely *left* [left], *kept* [kept], and *lost* [lɒst]. These syllables still have consonant clusters in the coda, but at least the consonants are all voiceless, and the nucleus vowels are short rather than long. This phonological improvement comes at a price, however: the morphology of these verbs is more complex, in that their stems appear in not one shape but two, according to the context: [liːv], [kiːp], and [luːz] or [lef], [kep], and [lɒs]. Is this a price worth paying for the sake of ensuring that the relevant sound-changes are exceptionless? That may seem a silly question: what matters is that it is a price that the English language has been willing to pay. But the question may not seem so silly when we consider the history of one of the regular verbs I have cited: *heave*. The Elizabethan poet Spenser, in his *Faerie Queene* (1596), uses a past-tense form *heft(e)*, exactly parallel to *left* from *leave*:[12]

(63)   His raging blade he hefte (IV. iii. 12).

(64)   The other halfe...Cambell fiercely reft, And backe at him it heft (I. xi. 39).

What has happened since Spenser's time is that, for the verb *heave*, the effect of the sound-changes that yielded *heft* as a past-tense form has been undone, so as to regularize the morphology of this verb. Similar regularizations are under way with verbs such as *cleave* 'split', *dream*, and *kneel*, for which old past-tense forms *cleft* (or *clove*), *dreamt* [dremt], and *knelt* [nelt] are being regularized in many speakers' usage as *cleaved*, *dreamed*, and *kneeled*.

The point about these examples is that they show that the Attic Greek inhibition of intervocalic loss of [s] in aorist verb forms is nothing unusual. It is true that in English there are still about fourteen verbs in whose past-tense form a suffix -*t* is (or can be) combined with a peculiar version of the verb stem so as to yield a 'better' syllable than if the normal version of the stem were combined with the regular -*ed* suffix: *creep, deal, dream, feel, keep, kneel, lean, leap, leave, lose, mean, sleep, sweep, weep*. Yet we have seen that some of these are on the way to regularization, and we have noted one verb, *heave*, that has already been fully regularized. So the only difference between the English and the Greek example is that the regularization in English is not complete.

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[12] My source is the entry for the verb *heave* in the Oxford English Dictionary Online.

Defenders of Neo-Grammarian exceptionlessness will be unfazed by these English examples. 'The very fact that the form *heft(e)* is attested in Spencer shows that relevant changes sound-changes went ahead in that verb form,' they will say. 'Only later was a regular form *heaved* created, by analogy with the great mass of regular verbs in English.' That may be true but, for present purposes, it is irrelevant. The point is that languages can and often do counteract sound-changes where their untrammelled operation either increases or would increase morphological complexity. And what languages can do sometimes they could conceivably do always. Therefore the fact that sound-changes occur does not suffice as an answer to the *sing-sang* question.

There is nothing outlandish in envisaging a language in which sound-change is never permitted to interfere with meaning–form relationships. Such a language could have elaborate morphology, even. In it, however, meaning–form relationships (such as between the aorist tense and its affixal expression in Greek) are always straightforward. Such a language may present the *pig-hunter* problem in one guise or another, but it will not present the *sing-sang* problem. An example is Esperanto. This is, admittedly, an artificially constructed language, but it its grammar is firmly based on that of the European languages that its inventor, Ludwig Zamenhof, was familiar with. Furthermore, over the years many children of Esperanto enthusiasts have acquired it natively. Yet one characteristic of European languages that Zamenhof saw no need to incorporate in it was allomorphy: each Esperanto morpheme appears in one and only one shape. Likewise, George Orwell in his novel *Nineteen Eighty-Four* envisages a 'Newspeak' that is modelled on English but which has a more regular morphology than English, so that the slogan *All men are equal* is rendered in Newspeak as *All mans is equal*, and the past-tense forms of *bring*, *sing*, and *feel* are not *brought*, *sang*, and *felt* but *bringed*, *singed*, and *feeled*. Esperanto and perhaps Newspeak thus exhibit what has been called the 'One-Form-One-Meaning' (OFOM) characteristic. This, according to proponents of **Natural Morphology** (e.g. Dressler et al. 1987; Dressler 1985), represents a natural state towards which all morphological systems tend, even if they never reach it because other tendencies (both inside and outside morphology) conflict with it.

Let us suppose that not just invented languages like Esperanto and Newspeak but all natural languages complied with the 'One-Form-One-Meaning' principle. In such a world, the exceptionlessness of sound-laws would be less obvious, inasmuch as in their operation they would have always to negotiate (so to speak) with the brain's insistence on a straightforward relationship between sound and meaning. In some instances this would involve compromising the phonological generality of a sound-change, as when intervocalic

s-deletion is inhibited in the Greek sigmatic aorist. In other instances it might mean extending the change beyond those phonological contexts which originally motivated it. In this world, sound-change would not be 'irregular' in the sense of applying haphazardly; however, its operation would be tightly circumscribed by the requirement for uniformity in morphological sound–meaning relationships.

Imagine in this world a researcher who raises the question why and how these inhibitions and extensions occur. The researcher asks: 'Why are not sound-changes allowed to apply exceptionlessly in language change, even if the effect is that a single meaning is expressed morphologically in two or more distinct ways?' Again, it seems likely that the research proposal would receive short shrift from funding agencies. 'Imagine the cognitive and communicative drawbacks involved in having multiple morphological expressions of one and the same meaning!' the agencies would reply. 'Those drawbacks might in principle be compensated for by phonological advantages in perception or production; but, because of the subservience of sound to meaning in spoken language (and the subservience of sign to meaning in manual Deaf language), those compensatory advantages could never be sufficient to outweigh the all too obvious disadvantages, namely increased complexity in form–meaning relationships and extra cognitive burdens in both speech processing and speech production.'

Here again what emerges is that a characteristic of language that we take for granted—allomorphy as a by-product of phonological change—could, in an only slightly different world, appear totally outlandish. The fact that in our own world our brains are so constructed at to accommodate morphological complexities that arise from sound-change is therefore a fact that needs explaining. Given that our brains are constructed this way, individual instances of allomorphy (such as in *keep* and *kept*) are often explicable in terms of divergence due to the different effects of regular sound-change in different environments. But we need to understand not just individual instances but the cognitive basis of the phenomenon in general.

2.8 The puzzle remains

Where does this discussion leave us? We have explored five directions in which reasons for the existence of morphology might be sought, and come back empty-handed from all of them. In frustration, one may be tempted by a suggestion mooted once by Chomsky (1986) when he introduced the distinction between the 'core' and the 'periphery' of grammar. Perhaps morphology (or at least its *sing-sang* aspects) is located in grammar's periphery, as a

residue of linguistic change. Speakers learn it, with or without the help of patterns that manifest themselves in it (for example, the pattern whereby *find*, *bind*, and *wind* have 'irregular' past-tense forms that exhibit a kind of regularity: *found, bound, wound*). There may be material there for the developmental psycholinguist, perhaps, interested in all aspects of how vocabulary and grammar are acquired. However, for theoretical linguists there is nothing that needs to be said about such phenomena, because they are outside the 'core' that is the theoretical linguist's business.

We should not rest content with that view, however. Merely by introducing a terminological distinction (such as between 'core' and 'periphery') one cannot demonstrate an empirical conclusion, such as that certain questions about language (the *pig-hunter* and *sing-sang* questions) will yield no interesting answers. It may indeed turn out that no interesting answers emerge. But I will hope to show in the remainder of this book that that pessimistic guess is unlikely to be correct.

3

A cognitive-articulatory dilemma

3.1 Setting the scene: Speech with vocabulary but no grammar

In Chapters 1 and 2 I argued that the existence of not one but two components of grammar (morphology alongside syntax) is a serious puzzle. Moreover, traditional reasons given for this duality, whether explicit or implicit, are not satisfactory. It is time now for me to try to offer something better. In this chapter I will argue that plausible assumptions about early human cognitive and communicative capacities would have presented the brains of our pre-linguistic ancestors with a dilemma. In later chapters I will argue that dilemma was resolved through the development of morphology, and further-more that certain otherwise puzzling details of morphology—details that do not look well designed for any cognitive or communicative purpose—fall into place naturally if this account of why it came into existence is correct.

However language originated, it is clear that a vocabulary must have been part of it from the beginning. It is not so clear that any kind of grammar—in particular, any kind syntax—was there at the beginning. Some scholars have argued that, because syntax is an outgrowth of some pre-linguistic cognitive or neural capacity (social intelligence, say, or tool-making, or the neural prerequisites for accurate throwing), one can legitimately say that syntax pre-existed language. But, even if this is true, syntax could not have mani-fested itself linguistically without a vocabulary. (Similarly, it may be true that Rembrandt was a great artist from birth, because of certain innate gifts; even so, these gifts could not manifest themselves without canvases, brushes, and paint.) In any case, for the purpose of my argument, it does not matter whether syntax arose in language 'early' or 'late'. This is because I will be arguing that syntax and morphology originated independently.

The idea of a kind of early language with a vocabulary but no grammar is hardly new. It is essentially what Derek Bickerton (1990, 1995) calls 'proto-language'. Bickerton envisages protolanguage vocabulary items as encoding simple concepts of the kind that in modern languages are expressed by nouns,

verbs, and adjectives, such as *mammoth, bear, eat, sleep, young, old*, plus some deictic terms corresponding roughly to *here, there, you, me, now*, and so on. An alternative vision is that of Alison Wray (2000, 2002): individual vocabulary items in protolanguage were 'holistic', with a content linked to a whole situation rather than elements or participants within it. Thus a single protolanguage vocabulary item might have a meaning such as 'Father has killed a mammoth', and this vocabulary item might not resemble at all the item that means 'Brother has killed a mammoth'. In support of this, Wray points to the formulaic character of many expressions in everyday discourse, such as *What time is it?*, *(I'll) see you later*, or *Have a nice day*. It is plausible to suppose that the hearer's brain perceives and interprets these expressions holistically (i.e. as wholes), unlike the way in which the brain deals with expressions that are similar but less formulaic, such as *What place is this?*, *I'll see her later*, or *Have a nice lesson*. What Wray suggests is that the earliest manifestations of language resembled holistic expressions such as *Have a nice day* more closely than Bickerton's suggested protowords such as *mammoth* or *sleep*. Only gradually would individual portions of these expressions have come to be analysed as (in some sense) recurrences of portions of other expressions, making a consistent semantic contribution wherever they occur.

My own view is that Bickerton's view of protolanguage is more plausible than Wray's. Wray's account of how holistic expressions came to be analysed into components or 'words' relies implausibly on accidental similarities of both form and content between expressions that (according to her) started out as entirely distinct wholes. (For criticism of Wray's view, see Tallerman (2007).) However, my argument does not require me to make a choice between Bickerton and Wray. All that is necessary for my argument is that, at some point, vocabulary items came to be juxtaposed in utterances. It does not matter whether the juxtaposed items had Bickerton-style meanings (e.g. *brother sleep late*) or Wray-style meanings (*brother-is-sleeping it's-late*); in either case, the items uttered in sequence would have rubbed up against each other in the manner discussed in section 3.3.

There is a second issue on which I am entitled to be equally agnostic. In the title of this section I allude to speech with vocabulary but no grammar. The choice of 'speech' rather than 'language' here was deliberate. It may seem, then, that I am taking sides on the issue of whether or not language originated in gesture. Again, I have a view on this issue—I think that the arguments in favour of a primarily gestural stage in language evolution are not as strong as those against—but, again, this view of mine is not crucial to the argument that I present in this book. This is because, even if language was at first primarily gestural, there came a point at which speech began to predominate,

and at that point the factors that I discuss in section 3.3 would have begun to have an effect. Whatever brain mechanisms evolved to underpin morphology in spoken language would in principle be available to support similar patterns of grammatical organization in Deaf sign language. Does that mean that all the phenomena that are called 'morphological' in sign language have the same evolutionary origin as morphology in spoken language? An appropriately cautious answer would be 'Not necessarily'. But in this book I will say no more about that issue.

3.2 Synonymy avoidance: A broader-than-human trait

In order for early humans to use protolanguage (of whatever type), their brains had to be able to categorize their experiences. But in categorizing, those early humans were not doing anything fundamentally different from what other mammals do (or indeed other vertebrates generally), even though they may have been doing it more elaborately. The more we discover about animal thinking, the less unique humans appear to be. Dorothy Cheney and Robert Seyfarth, in their classic study of vervet monkeys (1990), liken the vervets' social hierarchy to that of the segment of English society described in the novels of Jane Austen. Cheney and Seyfarth are joking, of course, but the very fact that the joke is apt shows how unexpectedly close the resemblance is between human and monkey cognition, at least in some domains.

For the purposes of my argument, it is enough to emphasize one surprising fact about animal cognition. This is the propensity observed in apes and in at least one dog to avoid synonymy: that is, to assume that anything with a meaning has a different meaning from anything else with a meaning. But before discussing the evidence regarding non-humans, I will say something about synonymy avoidance among contemporary humans.

3.2.1 *The elusiveness of exact synonymy in human language*

In normal children, vocabulary expands extraordinarily rapidly. Through babyhood and up to the age of ten or eleven, as the brain and the capacity for language mature, children achieve a feat of memorization that must seem mind-boggling to many an adult struggling to learn a second language. A considerable amount of research has been done on how this feat is achieved. It turns out to be assisted by a massively time-saving assumption, namely the synonymy-avoidance assumption that I have already mentioned. Eve Clark (1993: 64) calls this the Principle of Contrast: 'Speakers take every difference in form to mark a difference in meaning.'

It is easy to see how the Principle of Contrast would facilitate learning the meanings of words. The child (or rather the child's brain) does not have to waste time experimenting with the possibility that a word it has not encountered before means the same thing as a word it already knows. The new word must be associated with a new thing or property or action. Using pragmatic cues, the brain seeks out what is new in the environment in which the new word is heard, and links the two. Sometimes this association is wrong; children make mistakes. But often enough the association is correct, which from the child's point of view means that she or he subsequently encounters no reason to alter it.

Developmental psycholinguists (experts on the study of language acquisition in childhood) will recognize that the picture presented here is a bare outline. More than just the Principle of Contrast guides vocabulary acquisition. For example, there is evidence for a Whole Object Assumption, whereby (for example) *rabbit* is more likely to be taken to refer to the whole animal than to just its legs or its ears (Markman 1989: 26–38). (This is how children's brains cut the Gordian knot of the philosophical riddle posed by Willard van Orman Quine (1960): on hearing the word *gavagai* in some alien language applied to a rabbit, how do we know that the meaning is 'rabbit' and not 'assembly of rabbit parts?') There is also evidence that, at a certain stage, many children observe a Mutual Exclusivity Principle (Markman 1989: 187–215); on the basis of this Principle, they are at first reluctant to accept that (for example) the terms *pet* and *dog* and *spaniel* can be applied to the same animal, even though these terms respect the Principle of Contrast. Only gradually do children learn that the vocabulary of English and indeed all languages is organized in terms of superordinate and subordinate categories. However, for our present purposes, all that matters is that developmental psycholinguists generally accept that vocabulary acquisition in childhood is guided by an expectation on the part of the brain (if one can put it like that) to the effect that a novel word cannot have exactly the same meaning as some word that the child has already encountered (Bloom 2000: 65–73).

At this point, many readers may be uneasy: 'That may well be true for early childhood, but what about adult language? In many areas of adult vocabulary there are exact synonyms: for example, *nearly* and *almost*, *rancid* and *addled* (as in *rancid butter* and *addled eggs*), *courgettes* and *zucchini*, *thumbdrive* and *memory stick*.' I will respond in two ways. First, even in adult language exact synonyms are hard to find. Secondly, what matters for the purposes of the application to morphology is what happens in childhood, not in adulthood.

What exactly do I mean by 'exact synonyms'? A comparison with chess will help. Consider a set of chess pieces in which one of the bishops is missing.

Provided that the players using this chess set agree, any suitably sized object (say, a thimble) can stand in for the lost bishop, with no effect on the validity of the moves that the players make. The lost bishop and the thimble are completely interchangeable; the use of one or the other does not affect chess games played with them in any way. Similarly, we are entitled to call two words exactly synonymous if they are as completely interchangeable as the chess bishop and the thimble: the use of one or the other does not affect in any way the meaning or the acceptability of the expressions containing them. But none of the English pairs of purported synonyms just cited is like that, as I shall show.

Consider first *nearly* and *almost*. One can easily construct pairs of sentences that differ only with respect to these two words, and that seem to mean the same thing:

(1)   a. We're nearly ready.
      b. We're almost ready.

(2)   a. In Cincinnati nearly everybody reads the *Inquirer*.
      b. In Cincinnati almost everybody reads the *Inquirer*.

(3)   a. I got up late this morning and nearly missed my train.
      b. I got up late this morning and almost missed my train.

But this interchangeability breaks down with adverbs carrying the suffix *-ly*:

(4)   a.   Our team will almost certainly win.
      b. *Our team will nearly certainly win.

(5)   a.   That species is almost completely extinct.
      b. *That species is nearly completely extinct.

It seems that *nearly*, which itself has the *-ly* suffix, cannot modify another word with the same suffix. My own reaction to (4) and (5) is that more than just stylistic awkwardness is involved: there is genuine ill-formedness here. However, this ill-formedness does not extend to other *-ly . . . -ly* combinations, as the acceptability of (6) and (7) demonstrates:

(6)   The college is amazingly richly endowed.
(7)   She arrived late, slightly surprisingly.

Moreover, there is nothing wrong with (8) and (9), in contrast to (4b) and (5b):

(8)   Our team's victory is nearly certain.
(9)   Its extinction is nearly complete.

So the constraint is restricted to the collocation *nearly . . . -ly*: a narrow context, but enough to establish that *almost* and *nearly* are not freely

interchangeable. And there is a further difference between them with respect to *not*. Consider the following:

(10)   a.   We're not nearly ready.
         b.   ?We're not almost ready.

The difference is that *not nearly* has an idiomatic interpretation; thus, (10a) means 'We are still very far from being ready'. Such an interpretation is unavailable for (10b), however, which sounds natural only with strong contrastive stress on *almost*, as in the expanded version (11):

(11)   We're not ALMOST ready, we're COMPLETELY ready!

What this illustrates is that, apparently at random, one word can acquire idiosyncrasies that are not automatically transferred to words that seem to mean the same thing. To transfer the idiosyncrasies would protect interchangeability; but that is something that our brains are not interested in doing, it seems.

As regards *rancid* and *addled*, the fact that they are regularly collocated with *butter* and *eggs* respectively shows that they are not interchangeable. The phrases **rancid eggs* and **addled butter* are ill-formed. One can imagine a variety of English that has just one word, applicable to food, with the meaning 'gone bad through having been kept too long'. However, actual English is unnecessarily complicated (one might say); it has a variety of words mean 'gone bad' (*rancid, addled, sour, rotten, stale*), but each is limited in the foods that it can be applied to.

The remaining two pairs of apparent synonyms illustrate two further factors that typically affect adult language use rather than childhood learning, namely dialect differences and competing innovations. The terms *courgettes* and *zucchini* for a kind of small vegetable marrow (loanwords from French and Italian) are more usual in Britain and the USA respectively, like other well-known transatlantic rivals such as *railway* and *railroad*, *boot* and *trunk* (of a car), *drawing pin* and *thumbtack*. Each of these pairs would illustrate exact synonymy only if they were totally interchangeable, with no sense of strangeness or foreignness. As for *memory stick* and *thumbdrive*, they are competing terms for a computing device that did not exist ten years ago. I expect that, before long, one of these terms will come to dominate (perhaps not the same one in every variety of English), rendering the other obsolete. A similar pattern of competition and obsolescence has affected in recent years the competing internet terms *bookmark* and *favourite*. Although some internet browsers may still use *favourite* in their technical literature, the only one in my own active vocabulary now is *bookmark*, both as noun and verb (as in *I've*

bookmarked that website). (Does anyone ever say 'I've favourited that website'?)

My second response is that, even if exact synonymy can be found in adult usage (which is doubtful), that is irrelevant for the purposes of the argument in this book. The acquisition of grammar (including morphology), unlike vocabulary acquisition, essentially ceases in childhood. Adolescents or adults may learn normative grammatical 'rules' such as the English 'rule' against split infinitives (prescribing *to go boldly* rather than *to boldly go*), and they may learn unusual modes of plural formation such as suffixing *-im* in *kibbutzim* and *-ta* in *stigmata*; but these, being consciously learned in the way that a foreign language is, tell us no more about the brain's capacity for spontaneous language learning in childhood than does an adult's conscious acquisition of the word *zucchini* from a cookery book.

3.2.2 *Synonymy avoidance among animals*

It is easy to see why the human brain, faced with the huge task of acquiring a vocabulary of thousands of words, should rely on a no-synonymy expectation to accelerate the process. What is more surprising is that something like the no-synonymy expectation has been observed in chimpanzees and even in a border collie dog, as I will shortly explain. This has striking implications for language evolution. It is hardly likely that a no-synonymy expectation evolved independently in species so closely related as humans and chimpanzees. It is even possible that a homologue of the no-synonymy expectation exists in some mammals other than primates. So there is evidence that this expectation was already established in the human brain before any protolinguistic precursor of modern human language arrived on the scene. Implications of this will be discussed in section 3.3—implications that do not seem to have been noticed before now, but, once noticed, cannot be ignored.

Evidence for synonymy avoidance among chimpanzees emerges from long and careful investigations by Sue Savage-Rumbaugh (1986), working with two common chimpanzees (*pan troglodytes*) called Sherman and Austin. Sherman and Austin had become good at using lexigrams (arbitrary keyboard symbols) to communicate with humans and in due course with each other, especially about food. Savage-Rumbaugh and her colleagues at first assumed that, when a new kind of food was introduced to the chimps' menu, they would need to be taught a new symbol to represent it. But to their surprise they noticed that the chimps seemed able to 'name' the first novel food item—that is, to assign a so far unused lexigram to it—spontaneously, without any training. The researchers therefore decided to test this spontaneous naming capacity further.

For one or two weeks before the introduction of a new food item that would eventually have to be named, several new unassigned lexigrams would be placed on the lexigram keyboard. However, the two chimpanzees would ignore these until the new food item appeared. At that point, one chimpanzee would spontaneously choose one of the unused lexigrams to designate it, and the other chimpanzee would follow his lead (Savage-Rumbaugh 1986: 174–5). In other words, the chimpanzees did not spend time worrying about the possibility that the unassigned lexigrams might be synonyms for lexigrams with which they were already familiar.

Another researcher with long experience of research on the cognitive capacities of chimpanzees is David Premack. He has used not lexigrams but plastic tokens, which the chimps must select and place in a linear sequence in order to communicate with the experimenter. But in one essential respect Premack's tokens resemble Savage-Rumbaugh's lexigrams: they are arbitrary in that they do not resemble what they designate. With regard to the way in which chimpanzees learn what individual tokens mean, Premack makes the following emphatic comment in a discussion with fellow scholars—a comment that squares exactly with Savage-Rumbaugh's observation:

. . . one can ask whether the animal has at least . . . the idea that things can be named. You can do the following experiment with even the least capable of the animals: you include in the set of words a potential word, that is, a piece of plastic which is demonstrably a potential word in the sense that it has all the properties of the class, but it has never been used as a word. You also use an item which is familiar but which has never been named; even the stupidest animal rapidly constructs the sentence, 'Give X [the name of the animal] this new piece of plastic.' In other words, the animal requests the unnamed item with the so far unused piece of plastic. Thus the chimpanzees recognise that the potential word, which has not yet been so employed, is the appropriate thing to use in requesting the desired item, which is however not yet named.   (Piattelli-Palmarini 1980: 229)

Yet more striking, though more isolated, is the evidence gleaned from a remarkable border collie dog, Rico. Rico was trained by his owners in Germany to fetch items from around the house, and had acquired thereby a vocabulary of about 200 German words at the time when a systematic word-learning experiment began (Kaminski et al. 2004). The experimenters tested Rico by asking him to fetch objects with names that he had never heard before, while at the same time adding novel objects to his familiar repertoire of fetchable items. Rico's reaction was nearly always to fetch one of the novel objects. He almost never matched the new name with an object for which he already had a name in his vocabulary. So, as Kaminski and her colleagues put

it (2004: 1683): 'our findings corroborate the assumption that listeners' ability to attach meaning to specific sounds evolved much earlier than, and independently from, a flexible production of specific sound patterns. That is, some of the perceptual and cognitive mechanisms that may mediate the comprehension of speech WERE ALREADY IN PLACE BEFORE EARLY HUMANS BEGAN TO TALK' [emphasis added].[1]

Some readers may be surprised that I have not said anything about Kanzi, a bonobo (or pigmy chimpanzee) that Savage-Rumbaugh worked with after Sherman and Austin. Bonobos have different social habits from common chimpanzees, so the innate cognitive tools that Kanzi brought to bear on his experience of humans may well have been significantly different. At any rate, Kanzi's achievements were startling (Savage-Rumbaugh et al. 1993; Savage-Rumbaugh and Lewin 1994; Segerdahl et al. 2005). He had been allowed to tag along while the experimenters were trying with little success to train his mother, Matata. Then it dawned on the experimenters that Kanzi, left to his own devices, had acquired a good understanding of spoken English. They therefore turned their attention to him. Under rigorous experimental conditions, Kanzi (aged 8) showed that he could follow spoken instructions slightly more accurately than a human child aged two and a half. It would be surprising, then, if Kanzi did not exploit the no-synonymy expectation when learning English vocabulary, just as Sherman and Austin did when learning lexigrams. It is just that we have no evidence of this. No careful observations were made of how he acquired English, only of how accurately he understood it later.

What emerges from these animal studies is a high likelihood that in early humans, before language or even protolanguage developed, learning was

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[1] Kaminski and her colleagues describe Rico's achievement as illustrating 'fast mapping' in a non-human species. The term 'fast mapping' was originally used by Carey (1978) to label the characteristic whereby children quickly assign a meaning to a word that they hear for the first time, and are surprisingly good at remembering this new word and the meaning they have assigned to it even if they do not encounter it again until weeks later (see also Bloom 2000: 25–53). But fast mapping does not presuppose synonymy avoidance (a child could, after all, in principle fast-map a new word on to a concept for which she already has a word), whereas what is most striking in Rico's performance is his apparent assumption that there are no synonyms.

If we put that detail aside, however, it is notable how closely what Kaminski and her colleagues say echoes a comment by Markson and Bloom (1997) about fast-mapping in humans. Markson and Bloom argue that not just new words but certain kinds of non-linguistic information can be fast-mapped, in the sense that they can be associated rapidly with a novel object firmly enough to be remembered a week or a month later. They conclude (1997: 815): 'The finding that fast mapping is not restricted to word learning is therefore consistent with evidence from different sources which suggests that word learning is mediated by processes of human learning and memory THAT ARE NOT SPECIAL TO THE DOMAIN OF LANGUAGE' [emphasis added].

facilitated by a no-synonymy expectation. But, in vocabulary acquisition, this expectation will be in general reliable only if different vocabulary items do indeed regularly have different meanings. So what will happen if early humans are encumbered with a communication medium that has a built-in tendency to create different items with the same meaning? The origin of that dilemma is the topic of the next section.

3.3 A dilemma: The development of 'synonyms' due to assimilation

My argument from this point on depends crucially on the assumption that, before morphology arose, the articulation of spoken language (or protolanguage) was already essentially modern in character. In particular, individual items in the speech chain could be produced smoothly in rapid succession, without pauses, just as in contemporary speech. How plausible is this assumption?

If one believes that morphology is in evolutionary terms an outgrowth of syntax (so that at least some aspects of syntax are a prerequisite for it), this assumption will be entirely plausible.[2] A pre-modern stage of language in which syntax already existed, yet the phonological apparatus was capable of only slow, jerky delivery, so as to impose pauses between words, seems unlikely. No one, so far as I know, has suggested it. But I argued in Chapter 2 that the view that morphology evolved from syntax leaves too many questions unanswered. The purpose of this book is to argue that morphology evolved independently of syntax. It will therefore help my case if it can be shown that morphology could well have appeared on the scene alongside syntax or even before it. That is the purpose of section 3.3.1.[3]

That said, it is important to emphasize that what is crucial for my argument, as regards the evolutionary origins of syntax and morphology, is not their relative chronology but their independence. In section 3.3.1 it is argued that the cognitive dilemma underlying morphology would have arisen independently of syntax. However, even if one believes that only syntactically structured language would have displayed the kind of fluent-speech characteristics that my argument relies on, one is still faced with the problem of explaining why morphology arose, given that (as explained in Chapter 2) the reasons traditionally offered or hinted at are inadequate.

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[2] Jackendoff (2002: ch. 8) offers a thoughtful, nuanced account of possible stages through which language may have passed in the course of its evolution. He locates morphology relatively late in the process, when syntax has reached the stage of distinguishing grammatical functions such as 'subject' and 'object'. But he takes no account of the factors that we are focusing on: phonological processes and synonymy avoidance, and the tension between them.

[3] The view that morphology arose separately from syntax is also defended by Wunderlich (2008), for reasons that overlap in part with mine.

3.3.1 *The speed of speech production in protolanguage*

A minority of utterances in contemporary languages consist of just one meaningful item (one morpheme, in traditional linguistic terminology): for example, *Stop!*, *Sarah!*, or *Ready?*. No doubt in protolanguage, whether of Bickerton's kind or Wray's kind, many utterances would equally simple, glossable as (for example) 'Mammoth!' or 'Brother is sleeping'. (Recall that the Wray-style item that I gloss as 'Brother is sleeping' would not be decomposable in the way that the gloss implies.) But would all protolanguage utterances be of this kind? That is hardly probable. Protolanguage was, after all, not baby language. Our ancestors at the protolinguistic stage were not defective versions of modern humans. Even if it is appropriate to think of protolanguage as having a vocabulary but no syntax, we need not suppose that it was unusual for adult protolinguistic utterances to consist of two, three, or more vocabulary items.

There are occasions when, even today, we produce utterances with two or more vocabulary items but no grammar. Such utterances are a feature of 'touristese', for example: *Bus—Athens—where?* or *Money—stolen!*. Other features of touristese are loud, hyperarticulated, slow delivery and exaggerated gesturing, deployed like heavy artillery in an effort to batter down barriers of incomprehension. Most pertinent for our purposes is the way the speaker pauses between words. There is no question in touristese of slurring over the boundary between *money* and *stolen*, for instance. And, because touristese is a variety of grammarless language that is familiar to us, it is all too easy for us to visualize protolanguage (another kind of grammarless language) as having all its characteristics, including that of slow, careful delivery. But, again, to visualize our ancestors' protolanguage in this way is to fall into the trap of thinking of it as a defective version of something else—of fully modern language, in fact. As soon as one makes the effort to avoid that trap, it becomes clear that there is no reason to suppose that the speed of delivery in protolanguage was markedly slower than in most contemporary languages.

Bickerton (1990) has argued that protolanguage survives today in a number of forms: the 'two-word' stage of early childhood speech; pidgins (contact languages) that have not yet stabilized; 'touristese' (a sort of ad hoc pidgin); language as used by people who are mentally impaired (as when drunk or extremely tired); and perhaps also Deaf sign vocabulary as used by chimpanzees such as Washoe (Gardner et al. 1989) and Nim (Terrace 1979). But, even supposing that these phenomena really are homologous with early human protolanguage, they all differ in a crucial respect from protolanguage in its prehistoric human context: none of them constitutes the ordinary everyday

means of spoken communication between human adults. That everyday quality is what is most relevant to the probable speed and fluency of protolanguage speech production. Precisely this point is made by Dana McDaniel, contrasting prehistoric protolanguage with contemporary pidgins:

Today's pidgin speakers...have a modern human syntactic system. Aspects of their production will, therefore, be affected by this system. Pidgin speakers may speak slowly and haltingly due to an attempt to represent thematic structure with limited means and due to their knowledge (which is mostly unconscious, but possibly also conscious to some extent) that comprehension is guided by the syntactic system. If the protolanguage did not have a syntactic system, then the production system at that time would not have been restricted by the same considerations. In other words, protolanguage speakers would not have been disturbed by any sense of how language is 'supposed to' work.   (2005: 159)

It would be ludicrous to regard (say) bee-'dancing', or the alarm calls of vervet monkeys, as defective versions of contemporary human language. It would be similarly ludicrous to regard an archaeopteryx as a defective bird, even though it (or a species like it) is the evolutionary ancestor of modern birds. Prehistoric protolanguage deserves the same respect. We should visualize it as being used confidently and fluently, with most utterances consisting of several 'words', each of which follows hard on the heels of the previous one.

3.3.2 *Assimilatory effects of fluent speech*

It is well known that the acoustic signal associated with normal speech is not segmentable into chunks corresponding neatly to successive sounds. In articulatory terms, it is equally well known that the pronunciation of one word or one morpheme (root or affix) can affect its neighbours. Even though phrases such as *a white cloud* or *a short path* may be analysed phonologically at one level as the phoneme strings /ə wait 'klaud/ and /ə ʃɔt 'paθ/, they are likely (even in quite careful speech) to be pronounced [əwaik'klaud] and [əʃɔp'paθ], with the final alveolar plosive of the adjectives assimilating to the place of articulation of the initial plosive of the noun.[4] This is just one of many kinds of assimilation familiar to phonologists. And the same kinds of assimilation would have occurred not just in the earliest forms of syntactically organized language but probably even earlier, if (as I have just argued)

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[4] The symbols have their International Phonetic Alphabet values. In conformity with normal phonological usage, square brackets enclose symbols that are meant to represent an actual pronunciation, while slashes enclose something more abstract: an underlying or 'phonological' representation that the actual pronunciation may deviate from because of the effect of phonological processes. The pronunciation implied by the transcription here is a British variety, but that does not affect the point being made.

protolanguage expressions were articulated as fluently as expressions in contemporary languages.

Let us pretend now that /wait/, /klaud/, /ʃɔt/, and /paθ/ are not English words but vocabulary items in a prehistoric form of language that predated the existence of morphology as a component of grammar. This form of language may already have had some sort of syntactic organization, or it may have been pre-syntactic—that is, it may have been protolanguage. I will assume the latter; however, this assumption is not crucial. The important point is that, in this pre-morphological stage of language, phonological assimilation has generated at least two phonological shapes for /wait/, namely [wait] and [waik], and at least two phonological shapes for /ʃɔt/, namely [ʃɔt] and [ʃɔp]. Yet these pairs of shapes are synonymous. How did our ancestors cope with this situation, given that their brains (like those of contemporary chimpanzees and the dog Rico) already relied on the Principle of Contrast? Were they not programmed to expect that [wait] and [waik], being distinct, should have distinct meanings?

This question may at first seem ludicrous. Most linguist readers' reaction will be to reply: 'The alternants [wait] and [waik] are hardly two distinct synonymous items in the manner of (say) English [wait] and German [vais] (*weiß*) meaning 'white'. The difference between them is purely a matter of low-level allophony. Our ancestors' brains still had to cope with only one form here, the basic or underlying shape /wait/. And the same goes for [ʃɔt] and [ʃɔp]: they are merely low-level variants of a basic shape /ʃɔt/.' And so far as these particular examples are concerned, that brisk dismissive reply seems plausible enough. But many linguist readers will be able to foresee the sort of example that I will invoke next— a type of example for which such a brisk reply will not work.

Let us endow our hypothetical version of protolanguage with two further vocabulary items: /flut/ 'stream' and /ina/ 'several'. Imagine them combined in a fluently uttered complex expression that might be uttered by an excited little boy on stepping outsided the family shelter on a hillside after a night of heavy rain:

(12)   /ˈflut ina/ '(Look, there are) several (new) streams!'

In (12) the vertical mark indicates stress on /flut/, which (let us assume) is more prominent than /ina/ is in the way the boy renders the complex expression. But that need not be the only phonological effect of combining these two items. Let us suppose that the high front vowel /i/ of /ina/ has an assimilatory effect on /flut/, so that the high back rounded vowel /u/ is fronted to [y]:

(13)   [ˈflytina] '(Look, there are) several streams!'

The adults are less impressed by the effects of the rain (they are past the age of splashing in puddles), so the boy redoubles his efforts to engage them:

(14)   [eeee! ˈflytina! ˈflytina ˈflytəna ˈflytna ˈflyʔna!]

In the boy's excited rapid delivery, a succession of further phonological processes take effect. The vowel [i] in the unstressed syllable immediately following the stressed syllable is reduced to [ə] (schwa) and then deleted. The three-syllable utterance [[fly][tə][na]] thereby becomes a two-syllable utterance [[flyt][na]], with the [t] occupying a weaker position than before, being at the end of a syllable (in coda position), rather than at the beginning (in onset position). This triggers its replacement by a glottal stop—a consonant that is 'weaker' than [t] in that it involves no articulation above the larynx.

Phonologists will recognize nothing unusual in the processes that I have posited here. The question now is: Is it still plausible to say that a form such as [ˈflytna] or [ˈflyʔna] is merely a low-level variant of a basic or underlying shape /ˈflut ina/? The answer is not immediately obvious. Within current phonological theory, views on questions such as this cover a wide spectrum. But situations can certainly arise where the answer is clear—where we are certainly dealing with not just more than one surface shape but more than one underlying shape. Readers who know German will already have been reminded of the word *Fluss* 'river', pronounced [flʊs], whose plural form *Flüsse* [ˈflʏsə] contains a front rounded vowel similar to the [y] of [ˈflytina]. Readers who also know something of the history of German will be aware that the [ʊ/ʏ] contrast (an instance of 'umlaut') came about for reasons much like what I posited for the [y] of [ˈflytina], namely that a high front vowel in the original form of the plural suffix (the ancestor of the modern German suffix [ə]) triggered an assimilation in frontness on the part of the stem vowel. But the suffixal vowel later became schwa, just as [ˈflytina] becomes [ˈflytəna] in the boy's increasingly excited protolinguistic speech. So, as regards contemporary German, no one any longer seriously suggests that [ʊ] becomes [ʏ] in *Flüsse* by a phonological process of assimilation. The vowel [i] that once motivated the shift of [ʊ] to [ʏ] lost its frontness centuries ago. Instead, one must analyse the word meaning 'river' as having today two phonological shapes, /flʊs/ and /flʏs/. Perhaps both these shapes are equally 'basic', or perhaps one of them (/flʏs/) is derived from the other (/flʊs/) by a 'readjustment' rule. But that does not matter for our purposes. The important point is that any such rule must apply in contexts that are not phonologically definable. Therefore, whichever analysis we choose, the multiple shapes of the

German word that means 'river' cannot now be ascribed to low-level allophony.

Is a similar style of analysis imposed on us for the hypothetical protolinguistic pair [flut] and [flyt]? Perhaps not, so long as the reduction of /ina/ to [əna] or [na] remains a phenomenon only of particularly rapid or excited speech. In that case, children will get plenty of opportunities to hear the more careful rendering ['flytina], preserving the /i/ which provides the assimilatory motivation for the replacement of [u] by [y]. But there is a further point to be made about /ina/: its meaning ('several') is such as to make it readily combinable with a wide range of vocabulary items—combinable, in fact, with any item that would be glossed by a count noun in English. In this respect, /ina/ differs from all the three items in the protolinguistic utterance that I represented in section 3.1 as *brother sleep late*. Occasions on which *stream several* might appropriately be uttered are easy to visualize; on the other hand, for *stream brother*, *stream sleep* or *stream late*, one has to exercise one's imagination more strenuously in order to concoct a plausible pragmatic context. This characteristic of /ina/ is crucial for demonstrating that phonological assimilation really would have given rise to a synonymy dilemma. I will show why in the next section.

3.3.3 *Cliché patterns and the loss of phonological conditioning*

In a discussion of grammaticalization, James A. Matisoff says (1991: 384):

One crude way of approaching [grammaticalization] is in terms of what I have called *juxtapository productivity* . . . , an index of a morpheme's combinatory possibilities in collocations—the fewer or more general the semantic features of a morpheme, the less likely they are to conflict with those of others. A full verb meaning '*send someone on an errand*' will combine with fewer other lexemes than a bleached verb that means *causative*.

'Juxtapository productivity' is a cumbersome but apt term for what I have just drawn attention to in connection with /ina/ 'several': it is relatively freely combinable. Let me introduce **unselective** as a less sesquipedalian alternative for 'juxtapositorily productive', and correspondingly **selective** for an item that is not juxtapositorily productive.[5]

Because protolanguage is grammar-free, the combination of vocabulary items in it is constrained only by semantic and pragmatic factors. There is no

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[5] Matisoff's 'juxtapository productivity' is similar to what Joan Bybee (1985) labels 'generality'. However, in Bybee's discussion of generality and its contrast with what she calls 'relevance', her emphasis is on semantic effects rather than on the juxtaposition of morphemes, which is a crucial factor in my argument.

sense in which *stream brother* is less grammatical than *stream several*, or in which (say) *wake pond* is less grammatical than *wake earlier*. Nevertheless, the chances of a protolanguage-speaking child hearing *stream several* or *wake earlier* would have been much greater than her chances of hearing *stream brother* or *wake pond*.[6] The collocations *stream several* or *wake earlier* would not necessarily be clichés in the sense of being expressions that have been institutionalized individually, nor do they have unpredictable or unexpected meanings. Yet they exemplify what one may call **cliché patterns**, that is collocations involving one relatively unselective fixed item and one variable one, with a consistent semantic relationship between the two. Thus *stream several* exemplifies a cliché pattern that one can represent as 'COUNTABLE *several*', and *wake earlier* exemplifies a cliché pattern that one can represent as 'ACTION-OR-PROCESS *earlier*', where COUNTABLE and ACTION-OR-PROCESS are semantic umbrella terms each covering a wide range of vocabulary items. Even in protolanguage, then, without anachronistically invoking syntactic labels such as 'noun', 'verb', 'plural number', or 'past tense', one can legitimately distinguish between the two kinds of item that appear in a cliché pattern: the relatively unselective element, exemplified by *several* and *earlier*, and the relatively selective element, represented by the umbrella terms COUNTABLE and ACTION-OR-PROCESS.

Again, because protolanguage is grammar-free, no systematic significance can be attributed to linear order. So far as interpretation is concerned, *wake earlier* could just as well be *earlier wake*, and *stream several* could just as well be *several stream*. Even so, it is reasonable to assume that one order would have come to predominate in each pattern. After all, for *earlier wake* and *wake earlier* to be entirely interchangeable would risk violating the expectation of synonymy avoidance. Consistent ordering within cliché patterns could therefore have arisen independently of syntax. (Or you may prefer to say 'independently of any other aspect of syntax'. For our purposes the choice between those two formulations makes no difference, except that, since syntax is a branch of grammar, choosing the second would require us to qualify the claim that protolanguage is grammar-free.)

The relevance of cliché patterns to the synonymy dilemma is this. The decision that the child's brain makes about whether it is dealing with one

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[6] In *Syntactic Structures* (1957: 16), Chomsky makes the point that, in the sentence frame *I saw a fragile ___* , the probability of hearing *whale* is as low as that of hearing *of*; nevertheless, *I saw a fragile whale* (where a noun fills the gap) is grammatically acceptable whereas *I saw a fragile of* (where a preposition fills the gap) is not. Chomsky therefore concludes that probability is irrelevant to those aspects of syntax that concern him. But this sort of argument has no weight in relation to protolanguage, because protolanguage has no word-class distinctions such as between nouns and prepositions.

phonological shape or more than one (/flut/, /flyt/, and /flyʔ/, say) will be influenced by the incidence of cliché patterns in the contexts where the shapes concerned appear. The more that certain cliché patterns predominate, the harder it will be for the child's brain to associate what it hears with some 'basic' phonological representation that appears outside those patterns. If a child hears many collocations belonging to the pattern 'COUNTABLE [(ə)na]' meaning 'several COUNTABLES', yet few instances of [ina] meaning 'several' outside such collocations, its brain is unlikely to attribute the [u/y] alternation (and similar alternations involving other vowels) to the assimilatory influence of an underlying /i/ in [(ə)na]. Suppose further that (as is entirely possible) [ina] 'several' drops out of use entirely, leaving [(ə)na] restricted to the cliché pattern 'COUNTABLE [(ə)na]'. The [u/y] alternation has now lost all shadow of phonological motivation. The speaker's brain is thus left with the problem of how to reconcile its synonymy aversion with the existence of at least two distinct forms, [flut] and [flyt], that seem to mean the same thing, as well as analogous pairs of forms among all countable vocabulary items with 'umlautable' vowels.

The story that I have told about the effect of the cliché pattern on /flut/ and /ina/ focused on the varying shape of the selective item (/flut/) rather than the unselective one (/ina/). But it is easy to concoct phonologically plausible stories where it is the unselective item that acquires distinct but seemingly synonymous shapes. Consider a situation where the unselective item meaning 'earlier' originates as /iranu/, and that it appears in a cliché pattern 'ACTION-OR-PROCESS /iranu/'. Some of the ACTION-OR-PROCESS items with which /iranu/ can be collocated have an odd number of syllables and others have an even number of syllables. Now suppose that certain phonological processes begin to operate: alternate syllables are stressed, unstressed vowels are elided, and [rn] coalesces to [ɳ] (an apicodorsal or retroflex nasal). Lo and behold, the original shape /iranu/ diverges to [iɳu] in some contexts and [ran] in others, thus:

(15)		CVCiranu	CVCVCiranu
	Alternating stress	'CVCi'ranu	'CVCV'Cira'nu
	Vowel elision	CVCran	CVCCirnu
	[rn] coalescence	—	CVCCiɳu

Even if [ran] continues to occur only after a single consonant and [iɳu] after a cluster of two consonants (the residue of a vowel elision), it will still be scarcely feasible for the brain on the basis of this evidence to reconstruct a single underlying phonological representation for the two shapes that both mean 'earlier'.

There is no getting away from it, then: if protolinguistic speech was as fluent as modern speech is, synonymy dilemmas would inevitably have arisen. There are two obvious but relatively uninteresting ways of resolving any such dilemma. There are also two less obvious ways with potentially far-reaching implications for the brain organization of (proto)language. I will discuss all these in the next section.

3.4 Isolated synonymies versus systematic synonymy patterns

3.4.1 *Two obvious ways of resolving synonymy dilemmas*

The first way to resolve a synonymy dilemma is to discard all but one of the synonyms. Conceivably, for example, the brain could have dealt with our first hypothetical illustration by discarding all shapes for 'stream' except [flut], so as to convert ['flytna] and ['flyʔna] 'several streams' to ['flutna]. (After all, with the disappearance of the original /i/ in /ina/, there would no longer be any phonological assimilatory influence favouring [y] over [u].) Likewise, in our second hypothetical example, either of the two expressions for 'earlier' ([ran] or [iɳu]) could have been generalized to all contexts, displacing the other entirely. This mode of resolution parallels many of those recorded or reconstructable morphological changes that are typically attributed by historical linguists to 'levelling' or 'analogical extension'.

The second way to resolve a synonymy dilemma is to treat the collocation in which the dilemma manifests itself as no longer a collocation but rather a single vocabulary item. After all, if ['flyʔna] is not analysed as a sequence of [flyʔ] and [na], the question of potential synonymy between [flyʔ] and [flut] does not arise. This may be an attractive option for the brain to choose if the collocation appears in circumstances sufficiently restricted so that a new element of meaning (an element not derivable from [flyʔ] or [na] by itself) can readily be discerned in it. For example, if ['flyʔna] comes to be used only in application to a particular locality where several streams flow, the new element of meaning is akin to what distinguishes the phrase *the yellow river* from the proper name *the Yellow River*. I am expressing myself circuitously here in order to avoid saying 'if ['flyʔna] becomes a proper name'. This is in order to avoid having to discuss whether it is appropriate to ascribe proper names to protolanguage; after all, Hurford (2007) has argued that proper names did not emerge until a late stage in language evolution. The important point is that, as soon as ['flyʔna] can plausibly be analysed by the brain as no longer consisting of two items, a way out of the synonymy dilemma opens up.

Both these solutions may well have been applied often in protolanguage. Both have parallels in morphological changes that are historically recorded or

reconstructable. The first solution is parallelled by instances where the effect of phonological change is undone or blocked in order to preserve morphological uniformity. For example, in Greek (as was pointed out in Chapter 2), original /s/ was generally lost between vowels, yet an intervocalic /s/ was preserved or restored in Attic Greek when it was the morphological marker of aorist tense, as in *e-ly:-s-a* 'I untied', *e-time:-s-a* 'I honoured'. The second solution is parallelled in numerous placenames: for example, *lind* (or *lynd*) once existed as a synonym for *lime (tree)* (it is still preserved in the archaic or dialectal form *linden-tree*), while *hurst* was once a synonym for *wood* in the sense 'area of trees';[7] but no synonymy embarrassment affects *Lyndhurst*, institutionalized as a placename that modern English speakers' brains no longer analyse as a compound.

It is clear that, if those were the only techniques ever used to remedy synonymy dilemmas in protolanguage, nothing that happened in protolanguage could explain the development of morphology as it is today, and in particular the readiness with which the brain handles allomorphy. But there are situations where those techniques would not have been suitable, as the next section explains.

3.4.2 *Systematic synonymy patterns and how they might evolve*

I said just now that the brain could conceivably have dealt with our first hypothetical illustration of synonymy by discarding all shapes for 'stream' except [flut]. Let me emphasize 'conceivably'. Some readers may already be saying to themselves that that outcome, though perhaps conceivable, is unlikely. After all, the [flut/flyt] alternation, if it was due to the assimilatory effect of /i/ in /ina/ 'several', would not have been isolated. Many other countable items could have figured in the cliché pattern 'COUNTABLE /ina/'. Let us assume furthermore a version of protolanguage with not just one back vowel but three, /u o a/.[8] Then any vocabulary item denoting a countable object and containing one of these three vowels would also have appeared in a shape containing [y], [ø], or [æ] respectively. If we were dealing with a contemporary language, we would not hesitate to describe this situation by saying that vowel fronting (or umlaut) has come to be exploited systematically as a marker of plurality. In relation to protolanguage, such a way of talking

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[7] Or perhaps a near-synonym rather than a perfect synonym. The Oxford English Dictionary Online gives the primary meaning of *hurst* as 'a grove of trees; a copse; a wood; a wooded eminence'.

[8] This version of protolanguage can be thought of as having a five-vowel system /i e a o u/ that is extremely widespread in contemporary languages. I depart from strict IPA usage here to the extent of using [a] to represent a low central or back vowel, and [æ] to represent its fronted counterpart.

would be anachronistic: there is not yet any grammatical feature 'plural' for anything to be a marker of. All the same, even in protolanguage, the wholesale jettisoning of the umlauted alternants in the interests of avoiding synonymy would have been phonologically disruptive. The discontinuity would have been uncomfortably noticeable—too noticeable, most likely, for speakers' brains to take it in their stride.[9] So it is reasonable to assume that, in seeking the least disruptive way of resolving the synonymy dilemma, the protolinguistic brain would have explored the possibility of innovating not overtly, in pronunciation, but rather covertly, or cognitively, in terms of what the apparent synonyms meant.

What kinds of cliché pattern would lend themselves to cognitive rather than phonological treatment? In attempting to answer that question, one is hampered in two ways. Firstly, as is obvious, any suggestions about what protolanguage was like at a pre-syntactic stage of language evolution are bound to be based on highly indirect evidence. Secondly, and more subtly, to answer this question involves looking for system and order in a variety of language where a huge proportion of the kind of system and order that we take for granted in modern language is lacking. The distinction between unselective and selective vocabulary items is fuzzy, not clear-cut, and vocabulary items are (ex hypothesi) not yet assignable to syntactic categories. Therefore one cannot make distinctions among cliché patterns on a par with (say) the distinctions between nominal and clausal expressions or between active and passive sentences.

Having said all that, one can nevertheless identify some cliché patterns that would occupy a relatively extreme position, in the sense that potential synonymies affecting them (or, rather, affecting vocabulary items in collocations that conform to them) would lend themselves most naturally to cognitive rather than articulatory remedy. These are patterns for which an articulatory rather than a cognitive remedy would involve excessively numerous and therefore salient replacements of one phonological shape by another, in contexts where such replacements would no longer have a phonological motivation.

Let me clarify this with illustrations. Imagine a pair of cliché patterns 'ACTION-OR-PROCESS earlier' and 'ACTION-OR-PROCESS later'. (The English words 'earlier' and 'later' are placeholders for unselective protolanguage vocabulary items with these meanings.) An ACTION-OR-PROCESS item that appears regularly in collocation with 'earlier' is likely also to appear regularly

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[9] The actual Greek reinstatement or retention of [s] between vowels, as a marker of tense, would not have been nearly so salient, because it would not have had any similar effect on the phonological inventory.

in collocation with 'later'. We thus arrive at a more abstract level of organization: we step up from the level of the individual cliché pattern to the level of what I propose to call a **cliché pattern menu**. A cliché pattern menu is a set of cliché patterns related through substantial or complete overlap in the selective items that appear in them.[10] Thus we have already begun discussing a cliché pattern menu with two alternatives:

(16) ACTION-OR-PROCESS earlier
ACTION-OR-PROCESS later

I use 'menu' rather than (say) 'system' because 'system' is already an overused word in linguistics; also, to use the term 'system' would make it sound as if I wished to impute to protolanguage an implausible degree of orderliness—certainly a greater degree of orderliness than my argument requires. For similar reasons, I steer clear of the term 'construction'. A cliché pattern menu is something like a syntactic construction, but it would be wrong to use that label because syntax involves a kind of hierarchical structuring that protolanguage (as I envisage it) lacks.

Let us now introduce the convention of using subscript numerals to indicate differences in shape that were once phonologically motivated but can no longer be described in purely phonological terms. Examples would be the difference between [flut] and [flyt] and the difference between [ran] and [iṇu] discussed in section 3.3.3. For collocations within the scope of the cliché pattern menus at (16), there are many ways in which such differences in shape could be distributed. Two contrasting possibilities are shown in (17) and (18):

(17) $sleep_1$ $wake_1$ eat_1 die_1
$sleep_2$ later $wake_2$ later eat_2 later die_2 later
$sleep_3$ earlier $wake_3$ earlier eat_3 earlier die_3 earlier

(18) sleep   wake   eat   die
sleep $later_1$ wake $later_2$ eat $later_3$ die $later_4$
sleep $earlier_1$ wake $earlier_2$ eat $earlier_3$ die $earlier_4$

In (17) it is the selective items that exhibit the differences, whereas in (18) it is the unselective items. Of course, there is no reason why differences should not appear in both. However, the exposition will be clearer if I focus on situations where the variation is restricted to only one element in the collocation: the selective item or the unselective item respectively.

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[10] If the term 'menu' reminds readers of the pull-down menus that appear at the top of a computer screen, that is no bad thing. A computer menu resembles a cliché pattern menu in that it encompasses a set of choices.

There is one important factor that I have so far said nothing about. That is the degree of phonological difference between the different shapes. It so happened that, in our earlier discussion, the differences between the shapes of the selective item /flut/ were relatively small ([u] versus [y] and [t] versus [ʔ]). On the other hand, the differences that emerged between the shapes of the unselective item /iranu/ were considerable ([ran] versus [iɳu]). It is not by accident that I contrived things that way. Unselective items are likely to be used more often than selective ones. Furthermore, it is reasonable to assume that, in protolanguage just as in modern language, items that are used more often will tend to be shorter than ones that are used less often. Even if not short to begin with, a frequently used item is under heavy pressure to become shorter through various kinds of phonological erosion. (Think of British English *thank you* eroded to [kʰʲʊʔ] and Spanish *Vuestra Merced* 'Your Grace' eroded to *Usted* [us'te(ð)] 'you'.) But the shorter an item is, the more likely it is that its various shapes will diverge in such a way as to lose their original resemblance entirely. A short item will have less phonological ballast, so to speak, to protect the resemblance between its shapes. So it is fair to regard [ran] and [iɳu] as having become distinct but synonymous items. By contrast, [flut] and [flyt], though no longer relatable by means of purely phonological processes, are still recognizably forms of the same item.

Admittedly, through an appropriate selection of phonological processes, it would not be hard to contrive the opposite situation: that is, an instantiation of (18) such that the four different shapes of the unselective item *earlier* resemble each other closely (likewise the four shapes of *later*), and by contrast an instantiation of (17) such that the three different shapes of the selective item *sleep* (likewise the three shapes of *wake*, *eat*, and *die*) become as different as [ran] is from [iɳu]. This illustrates again the unavoidable fuzziness of any generalizations about likely effects of phonological processes in protolanguage. But what matters is that the early human brain was confronted with two different situations in which two or more items had (or risked having) the same meaning:

- **distinct items**, usually but not always historical descendants of what had once been an unselective item with a single phonological shape;
- **distinct forms of the same item**, usually but not always historical descendants of what had once been a selective item with a single phonological shape.

We have already encountered one cognitively unwelcome novelty that phonology has engendered, namely apparent synonymy. Here now is a second novelty, not necessarily adding to the cognitive embarrassment, but still a

challenge to the brain: the fact that some of the distinct but synonymous vocabulary items resemble each other enough to be classified as forms of the same item.

As we have already noted, one way of remedying this unwelcome proliferation of items (or of forms of one item) would have been to jettison all but one of them in each case. For example, in (17), perhaps the forms $sleep_2$ and $sleep_3$ could be jettisoned, along with the corresponding forms of *wake*, *eat*, and *die*; and perhaps in (18) $earlier_1$, $earlier_3$, and $earlier_4$ could be jettisoned, leaving only $earlier_2$, and similarly with the synonyms meaning 'later'. But that would have involved not isolated substitutions on a par with the replacement of *memory stick* by *thumbdrive*, but simultaneous and phonologically unmotivated discontinuities affecting a considerable number of cliché patterns. Such a remedy would have forced its way into speakers' conscious awareness. Yet it is reasonable to suppose that, in implementing linguistic changes, speakers' brains would have preferred, then as now, to work surreptitiously. (Recall that, until the rise of sociolinguistics in the last half-century, even linguists regarded changes in language as unobservable until they were completed. That view is taken by, for example, Charles F. Hockett in a well-known textbook: 'No one has yet observed sound change: we have only been able to detect it via its consequences' (1958: 439).)

Oddly enough, a flaw in my presentation of this synonymy dilemma lends to the 'jettison-all-but-one' remedy that I have just criticized a kind of undeserved plausibility. I have talked as if, in the development of patterns such as those at (17) and (18), two chronological stages can be distinguished: first, phonological developments generated potential synonyms, and only after that did the synonymy-avoiding brain wake up to the anomaly (so to speak). But of course this is wrong. Even as the new potential synonyms were appearing, speakers' and hearers' brains would have been responding to the challenge of finding ways to differentiate them. So what form would this response have taken?

The answer to this question will, we hope, shed light on how morphology operates in contemporary human language. That means that, in order to avoid circular reasoning, our answer must not be influenced by things we already know about morphology. But it is legitimate for our answer to be influenced by what we know about vocabulary and how it is organized, and about related aspects of how the brain categorizes our experiences. After all, even though protolanguage is syntaxless, it does have a vocabulary and a phonology. Moreover, on the basis of what we know about synonymy avoidance by apes and even by the dog Rico, we are entitled to assume that, while vocabularies have may well have become larger in modern languages than in

protolanguage, the ways in which vocabularies are organized and in which our experience is categorized have not changed all that much.

3.5 The way ahead

The plan of the rest of this book is as follows. First, in Chapter 4, we will look at two dimensions of linguistic structure, the so-called 'paradigmatic' and 'syntagmatic' dimensions, and especially at a certain paradigmatic mode of large-scale vocabulary organization that shows up in a number of contemporary languages. This may at first seem remote from the issue of how morphology originated. But I will argue in Chapter 5 that this way of organizing multiple vocabularies, if it was invoked in early human protolanguage, would have resolved to the brain's satisfaction some of the apparent synonymies between distinct items. As for distinct forms of the same item, relationships in the paradigmatic dimension were relevant to them too, as I will argue in Chapter 6.

Up to the end of Chapter 6, we will be concerned almost exclusively with phenomena that, in modern terms, will be classified as inflectional rather than derivational. But in Chapter 7 I will suggest ways in which the evolutionary perspective may help to solve a puzzle posed earlier: why so many morphologically complex words are stored as 'lexical items', even though the two senses of 'lexical' discussed in Chapter 2 are logically distinct. In Chapter 8, I will comment on two topics that I have said nothing about, despite their prominence in recent morphological theorizing: syncretism and binary morphosyntactic features. In Chapter 9 I will offer a summing-up in the light of what I said in Chapter 1 about what counts as a good abductive argument.

4

Modes of synonymy avoidance

We will begin by looking at two kinds of prima facie synonymy, as exemplified in *rancid* versus *addled* and in English *cat* versus French *chat*. These two kinds of synonymy highlight the two dimensions of linguistic structure classically distinguished by Saussure (1973) as the syntagmatic and the associative (later renamed paradigmatic) dimensions. This is necessary preparation for considering how a brain that is equipped to cope with synonymy (or apparent synonymy) between protolanguage vocabulary items might handle synonymy dilemmas like those illustrated in Chapter 3: [flut] versus [flyt] and [ran] versus [iɲu].

4.1 Syntagmatic synonymy avoidance

The **syntagmatic** dimension is that of succession in a temporal sequence. In the English phrase *rancid butter*, *rancid* is syntagmatically related to *butter* in three ways. First, the adjective *rancid* is in construction with the following noun *butter*, modifying it, so that the whole forms a noun phrase. Secondly, the adjective contributes to the meaning of the whole phrase: 'inedible through being kept too long'. But thirdly, the adjective *rancid* is highly selective in the sense that I introduced in the previous chapter. Most foods cease to be edible if they are kept too long. However, only very few foods can be described as 'rancid'; in fact, butter and oil are the only examples that come immediately to mind.

It is not that a convenient term such as *rancid* is unavailable for other foods. Far from it; I have already mentioned *addled*, *sour*, *rotten*, and *stale* as equivalent to *rancid*. What distinguishes these words is not their denotation but their possibilities of collocation: the only food that can be 'addled' is eggs, the only food that can be 'stale' is bread and other baked goods such as cakes, the only food that can be 'sour' (in the sense that we are concerned with here) is milk and (in some contexts) cream. To differentiate these words that would

otherwise risk being synonymous, the brain applies syntagmatic restrictions of a certain kind, namely restrictions on collocation.

Two things to note are the randomness of these collocation restrictions and the subtlety of them. There is no obvious reason why butter should share a term for 'inedible' with oil rather than with another dairy product, milk; nevertheless, this is the pattern that English speakers' brains arrive at. And *sour* means 'inedible' in application to cream only when used predicatively, not attributively. If someone tells me 'That cream is sour', I will avoid eating it, but if someone tells me 'That is sour cream', I will be happy to mix it with chopped chives and spread it on a bagel. This is because *sour cream* has an idiomatic sense (denoting a particular dairy product) that takes precedence over the compositional interpretation of the phrase. So far as synonymy avoidance is concerned, however, this complication makes no difference: *sour* is distinguished from *rancid* and the rest just as effectively in the context of the idiom *sour cream* as it is through collocation restrictions.

How random can collocation restrictions be? My next illustration seems at first sight bizarre, but is nevertheless genuine. It involves a noun represented by two distinct phonological forms, which we can call shape A and shape B. Their distribution can be stated as follows:

(1) Shape A The noun is singular, or is immediately preceded by certain numerals including those that mean 'four', 'five', 'seven', 'eight', 'nine', 'seventeen', 'eighteen', 'nineteen' (but not 'fourteen' or 'fifteen'), 'twenty', 'thirty', 'forty', 'fifty', . . .

   Shape B Elsewhere (i.e. the noun is plural but not immediately preceded by any of the numerals listed above).

Such a distribution looks like something that might have been thought up by Charles Darwin's grandson Charles, who amused himself when young by inventing a language that had none but irregular verbs (Raverat 1952: 66). However, it is exactly the distribution displayed by the two shapes [œf] and [ø] meaning 'egg(s)' in colloquial French (Swiggers 1985).

Some readers who know French may be puzzled. Surely (they will ask) the French noun meaning 'egg', in common with all French count nouns, has a plural form *oeufs* that occurs in all plural contexts, irrespective of the choice of any preceding numeral, and consistently distinct from the singular form *oeuf*? But that is true only of the spelling. The *-s* at the end of *oeufs*, like most final orthographic *-s* in French, is not pronounced. Hence, so far as pronunciation is concerned, most French nouns are identical in the singular and the plural.

Plurality in the noun phrase (or determiner phrase) is usually manifested only in the determiner.[1] But some nouns do have distinct plural forms, including many of those that end in [al] in the singular, such as *cheval* [ʃəval] 'horse', plural *chevaux* [ʃəvo]. However, the two forms of *cheval* do not manifest the same peculiar distribution as the two forms of *oeuf(s)*: the plural form [ʃəvo] shows up not just in *six chevaux* 'six horses' but also in *sept chevaux* 'seven horses', as one would expect. So what is going on with [œf] and [ø] meaning 'egg(s)'?

The answer turns out to have to do with whether the previous word ends in [z] or not. The form [ø], which is restricted to plural contexts, begins with a vowel. Therefore determiners such as *les* [le(z)] 'the', *des* [de(z)] 'some', and *nos* [no(z)] 'our', when they precede [ø], will appear in their pre-vocalic shapes [lez], [dez], and [noz], not their pre-consonantal shapes [le], [de], and [no]. And such contexts account for nearly all the plural occurrences of 'eggs'. Therefore [ø] 'eggs' will nearly always be heard in a phonological context following [z]. So what French speakers' brains have done is take just one small extra step: to make the [z] context mandatory for the occurrence of [ø] in preference to its rival form [œf]. The form [ø] will therefore appear after only those numerals that end in [z] before vowels (*deux* [døz] 'two', *trois* [tʁwaz] 'three', *six* [siz] 'six', *dix* [diz], *onze* [ɔ̃z] 'eleven', *douze* [duz] 'twelve', *treize* [tʁɛz] 'thirteen', and *quatorze* [katɔʁz] 'fourteen'), not after other numerals such as *quatre* [kat(ʁ)] 'four', [sɛt] 'seven', or *huit* [ɥi(t)] 'eight'.

What this illustrates, alongside the *rancid-addled* example, is the brain's resourcefulness and flexibility in seeking out syntagmatic remedies for synonymy risk. Remedying factors may be arbitrarily lexical (*butter* versus *milk*) or morphosyntactic (plural versus singular) or phonological (preceding [z] versus no preceding [z]). Linguists are in the habit of thinking of linguistic contrasts as serving communicative or cognitive functions. It is notable therefore that, when faced with a conflict between representing a meaningful contrast (singular versus plural number) and observing a syntagmatic phonological constraint with no extralinguistic function whatever (and no assimilatory motivation either!), the brain can at least sometimes grant precedence to the non-functional constraint.

Table 4.1 anatomizes a further example involving five items that risk synonymy (they all mean 'my'), and that are distributed according to a complex pattern. As with [œf] and [ø], this distribution of items involves

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[1] In this respect, French closely resembles New Zealand Maori. In Maori, too, few nouns have distinct plural forms, but plurality is consistently represented in the determiner: /te/ 'the (singular)' versus /ŋaː/ 'the (plural)', /teːnei/ 'this' versus /eːnei/ 'these', and so on.

Table 4.1. Five words meaning 'my', and their distribution

Item A   The noun phrase is plural and the word immediately following 'my' either
    (a) begins with a consonant or
    (b) is itself the head noun and, although beginning with a vowel, belongs to one of the arbitrary subclasses mentioned at (d) and (f) below.

Item B   The noun phrase is plural and the word immediately following 'my' begins with a vowel, except as specified in (b) above.

Item C   The noun phrase is singular, the head noun belongs to Class I and the word immediately following 'my' either
    (c) begins with a consonant or
    (d) is itself the head noun and, although beginning with a vowel, belongs to an arbitrary subclass that selects item C.

Item D   The noun phrase is singular, the head noun belongs to Class II and the word immediately following 'my' either
    (e) begins with a consonant or
    (f) is itself the head noun and, although beginning with a vowel, belongs to an arbitrary subclass that selects item D.

Item E   The noun phrase is singular and the word immediately following 'my' begins with a vowel, except as specified in (d) and (f) above.

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number (singular versus plural) and a phonological factor (this time involving not the preceding item but the following one: whether it begins with a consonant or a vowel). However, the distribution also involves a classification of nouns into a Class I and a Class II, and a further subclassification of the vowel-initial members of each of these classes, in that a minority of these behave as if they began with not a vowel but a consonant. To make matters worse, membership of Classes I and II is largely arbitrarily from a semantic point of view, and membership of the minority vowel-initial subclasses is entirely arbitrary. The variety of intersecting factors that the brain juggles with here is impressive.

The five items that behave this way are, again, in French. 'Class I' in Table 4.1 represents masculine nouns; 'Class II' represents feminines. The minority subclasses contain those nouns traditionally described as beginning with an 'aspirated *h*' ('*h* aspiré'), although in fact no [h] is pronounced.[2] Putting flesh on the bones of Table 4.1, we have:

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[2] There have been recent attempts to mitigate the exceptional character of the '*h* aspiré' words by treating them as beginning with a syllable onset that is empty or is filled by a defective consonant. However, none of these attempts accounts for all the facts successfully. Rather, according to Côté (2008), '*h* aspiré' words really do begin with a vowel, but are also (from a French point of view) unusual in placing a high priority on coincidence between syntactic and phonological boundaries at their left edge.

(2)   Item A: *mes* [me], as in
  (a) *mes frères* [me fʁɛʁ] 'my brothers', *mes filles* [me fij] 'my daughters'
  (b) *mes héros* [me eʁo] 'my heroes', *mes harpes* [me aʁp] 'my harps'

  Item B: *mes* [mez], as in *mes assiettes* [mez asjɛt] 'my plates', *mes héroïnes* [mez eʁoin] 'my heroines', *mes anciens héros* [mez ɑ̃sjɛ̃ eʁo] 'my former heroes'

  Item C: *mon* [mɔ̃], as in
  (c) *mon frère* [mɔ̃ fʁɛʁ] 'my brother'
  (d) *mon héros* [mɔ̃ eʁo] 'my hero'

  Item D: *ma* [ma], as in
  (e) *ma fille* [ma fij] 'my daughter', *ma petite héroïne* [ma ptit eʁoin] 'my little heroine'
  (f) *ma harpe* [ma aʁp] 'my harp'

  Item E: *mon* [mɔn], as in *mon assiette* [mɔn asjɛt] 'my plate', *mon héroïne* [mɔn eʁoin] 'my heroine', *mon ancien héros* [mɔn ɑ̃sjɛ̃ eʁo] 'my former hero'

Traditional accounts of French possessive determiners emphasize number and gender as the principal factors determining the choice between the forms. This reflects history: the five forms of MON 'my' (what I called just now the five 'items' whose distribution has to be determined) are indeed all descended from Latin ancestors which differed according to number and gender (and also case, which has disappeared in French). But, as can be seen, gender has receded in importance: it is a conditioning factor only in relation to items C and D, whereas a syntagmatic factor (phonological characteristics of the immediately following word) is relevant to all five items.[3]

  To some readers, the way in which I have presented these well-known facts may seem perverse. Surely (one is tempted to say) the differences between the various forms of the French 1st-person singular possessive determiner MON (and the exactly parallel forms of the 2nd and 3rd singular determiners, TON and SON) fulfil a morphosyntactic function: they signal number and gender. Indeed, both these categories are often signalled nowhere in the noun phrase except on the determiner. But that is to privilege one particular way of looking at the facts. It is equally true to say that number and gender are two of the factors to which the brain has recourse in determining how to contrive

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[3] Perlmutter (1998) similarly emphasizes the importance of phonology in the choice between allomorphs for a range of French items, going well beyond the possessive pronoun MON.

contrasts between the five French forms that mean 'my' (and 'your' and 'her/ his'), but they are not the only two factors; what's more, in contemporary French, more prominent than gender is a syntagmatic factor with no morphosyntactic function at all.

Speakers of English will be able to confirm for themselves that syntagmatic phonological factors govern the distribution of four extremely common and otherwise synonymous forms: those of the indefinite article *a(n)*. The distribution is as follows:

(3) *an* [æn]   stressed, before a vowel, as in: *I said he worked in* AN *office* ['æn ɒfɪs], *I didn't say it was* THAT *office*.

a [ei]   stressed, before a consonant, as in: *I said she was a member of* A *union* ['ei junjən], *I didn't say it was* THAT *union*.

an [ən]   unstressed, before a vowel, as in *an apple*

a [ə]   unstressed, before a consonant, as in *a hospital*

The example supplied for *a* [ei] underlines the (for linguists) elementary point that the word *union* counts as beginning in a consonant because it begins with the glide [j], just as *hospital* begins in a consonant because it begins with [h]. There is nevertheless an issue about the status of [h]. A century ago it was not unusual for educated speakers to use [ən] rather than [ə] before a few words beginning with [h], including *hotel* and *historic*. This was a way of showing off their knowledge that *hotel* is 'really' a French word, so that the *h* 'ought to be' unpronounced, and that *historic* is derived from Ancient Greek, where [h] did not behave as a consonant and, in Byzantine scribal practice, was written not with a normal letter but with a diacritic mark (the so-called 'rough breathing') over the first vowel in a word. Nowadays, such displays of erudition are rare. But the fact that they were once possible confirms the brain's capacity to accommodate an arbitrary set of words (one could call them the 'erudite *h*' set) as a syntagmatic factor in determining the distribution of shapes for the indefinite article, just as French speakers' brains accommodate the arbitrary 'aspirated *h*' set.

4.2 Paradigmatic synonymy avoidance

The **paradigmatic** dimension is that of mutual substitutability within a linguistic context. In the sentence *Alice adores asparagus*, the noun *asparagus* is syntagmatically related to the verb *adores*, in that it follows it and is the direct object of it. By contrast, *asparagus* is paradigmatically related to other

nouns such as *broccoli* and *cauliflower*, which can be substituted for it so as to produce new sentences that are syntactically and semantically well-formed. It is customary (and helpful) to think of the two dimensions as being situated at right angles to one another. In this section we will consider various contemporary and recent linguistic situations in which the paradigmatic dimension contributes to synonymy avoidance.

4.2.1 *Individual multilingualism*

Does the existence of *cat* [kʰæt] and *chat* [ʃa] as distinct words meaning 'cat' violate the no-synonymy expectation? One's immediate reaction is to say no. The fact that one is English and the other is French means that they do not compete with one another. Even in a community where a substantial number of people are bilingual in English and French and use both languages on a regular basis (parts of Canada, say), it will always be clear which language a person is talking at any one time, so even there no synonymy dilemma arises.

What about children who are brought up bilingual, and who may encounter both [kʰæt] and [ʃa] for 'cat' before they know that there are two distinct languages, English and French? If they have an in-built cognitive distaste for synonymy, how will they manage? Their reaction tends to confirm the in-built distaste. In many families where the parents decide to bring up a child bilingual, a conscious decision is made that each parent will talk to the child in only one language. This allows the child to differentiate [kʰæt] and [ʃa] (for example) in a fashion that is just as effective as labelling them 'English' and 'French'; one of them belongs to 'Mummy's words' and the other to 'Daddy's words'. But it seems that children strongly prefer such differentiations to be clear-cut. If Daddy uses some of Mummy's words, or vice versa, the child is likely to become indignant. Later on, when the child learns about the French and English languages and their international status, the labels 'English' and 'French' will suffice to keep [kʰæt] and [ʃa] apart, and the child tolerates parental language-switching; but at every stage the brain needs some differentiating factor to latch on to.[4]

Strict differentiation, whether between English and French or between 'Mummy's words' and 'Daddy's words', can be described in purely linguistic terms by appeal to the paradigmatic dimension. The word *cat* is paradigmatically related to *dog* but not to the French *chien* 'dog', inasmuch as only *dog*, not *chien*, could plausibly be substituted for *cat* to yield a well-formed sentence. Each of the words *cat* and *chat*, despite the fact that they designate the same creatures in the non-linguistic world, points towards a distinct menu

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[4] For a survey of studies of childhood bilingualism, see Romaine (1995).

of linguistic alternatives: *dog, rabbit, mouse*…on the one hand and *chien, lapin, souris*…on the other. One hardly wants to say that *chien, lapin, souris,*… are part of the 'meaning' of *chat*, alongside 'cat'; nevertheless, the paradigmatic relationship between *chat* and *chien, lapin, souris,*…is sufficient to satisfy the brain's cognitive requirement that *chat* and *cat* should not be perfect synonyms.

One can perfectly well imagine a world in which a child exposed to a bilingual upbringing combines the resources of vocabulary offered to them for a kind of rainbow effect, so that (4) and (5) are equally acceptable, along with any other conceivable mixture of appropriate English and French vocabulary:

(4)   *J'ai demandé un* cat but *Maman m'a* given a *lapin*!

(5)   I asked for a *chat mais* Mummy has *donné* me *un* rabbit!

No doubt this child will have to get used to the fact that, outside the family, not all the words she uses are understood; but, at home, macaronic exuberance flourishes. In such a world, children brought up this way could well be envied for their rich expressive resources. In adulthood, perhaps, such individuals would enjoy a special advantage as creative writers. The nuances achievable in Spanish-Portuguese poetry, for example, might be especially admired, while translators struggle to replicate the unsettlingly jagged sound effects of those rare works composed in Mandarin-Zulu…But such a world is not our world. It seems reasonable to suppose that that sort of multilingual promiscuity, with *rabbit* and *lapin* as freely interchangeable as the bishop and the thimble in the chess game alluded to in Chapter 3, would be unachievable by humans whose brains inherit a synonymy-avoidance expectation. A bilingual child who learns both *rabbit* and *lapin* must differentiate them somehow; and it seems that she differentiates them paradigmatically, by treating the two menus within her repertoire as distinct.[5]

4.2.2 *Community multilingualism: The Vaupés case*

The kind of bilingualism that we have been discussing so far is not obligatory. That is, we have not so far considered any speech community in which to know more than one language is a requirement for any individual to be considered to have normal linguistic competence. In Europe and North

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[5] Some combining of languages within utterances and discourses does indeed take place; 'code-switching' is a recognized phenomenon (Romaine 1995). But to switch between codes is not the same as to mix vocabulary items from two codes freely. The very fact that code-switching is a vigorous research topic within sociolinguistics implies that it is subject to limits and conditions. It never seems to involve the sort of mish-mash that I illustrated at (4) and (5).

America there may indeed be communities in which multilingualism is usual (many Latino communities in the USA, for example), but this is for social and economic reasons rather than strictly linguistic ones.

A community where multilingualism is obligatory exists among a thinly scattered population (numbering perhaps 10,000 people in the 1960s) living in longhouses, each occupied by about four to eight nuclear families, on various tributaries of the Amazon in the Vaupés region of Colombia and neighbouring parts of Brazil (Sorensen 1967; Jackson 1974). These Vaupés people speak some twenty distinct languages, some of them genetically related, some not. But despite their linguistic diversity, they share a culture, one of whose central principles of kinship is that all people with the same 'father-language' are classified as brothers and sisters. Therefore one cannot select a husband or wife from among people with the same father-language, because that would count as incest. A man chooses his wife from a longhouse with a different father-language and brings her to live with him in his own longhouse, where their children will be born. A child's father-language, the language of his father's longhouse, is thus necessarily different from his mother's father-language, and indeed different from the father-language of all the adult women in the longhouse.

In this situation, multilingualism is inevitably the norm. What is interesting is the form that it takes. None of the numerous languages has more prestige than the others, although one language, Tukano, is known by nearly everybody and so serves to ensure that any two Vaupés individuals will almost certainly have at least one shared language. There are no ritual or ceremonial purposes for which one language is preferred. There is no tradition of rivalry, much less hostility, between speakers with distinct father-languages. Within a longhouse whose language is Tuyuka (for example), it is Tuyuka that is used by males and when speaking to males, but women with the same father-language may speak it to each other, and children learn not only their father-language but also their mother's language and the languages spoken by other women in the longhouse. In a conversation involving people from more than one longhouse, individuals start out speaking their own father-language but may then change to the language of the host longhouse, or to whatever language is most convenient (such as Tukano).

In some respects, then, the Vaupés situation seems remarkably free and easy. On the other hand, the identification of each person with his or her father-language is strict, and this identification is the first thing one learns about any new acquaintance. And there is a strong expectation that, whatever language one is speaking, one should speak it correctly. When individuals

learn a language other than their father-language or their mother's language, they listen for a long time before they venture to speak. As Sorensen puts it (1967: 678):

In the course of time an individual is exposed to at least two or three languages that are neither his father's nor his mother's language. He comes to understand them and, perhaps, to speak them. I observed that as an individual goes through adolescence, he actively and almost suddenly learns to speak these additional languages to which he has been exposed, and his linguistic repertoire is elaborated. In adulthood he may acquire more languages; as he approaches old age, field observation indicates, he will go on to perfect his knowledge of all the languages at his disposal. . . . [But there is] no development of cross-linguistic puns. There is no stylistic device of switching from one language to another or of interspersing one's conversation with quotes from another language.

So the free-and-easiness has limits. For a Vaupés person, when she is grasping for a word that she has momentarily forgotten, it is not acceptable to substitute a synonym from another language in her repertoire, even if this other language is known by the people she is talking to. That is not to say that such rules are never broken; after all, no one is perfect. But Jackson's comment on such lapses is revealing (1974: 62–3):

I observed instances where women were scolded for allowing words from other languages to creep into conversations which were being held in Bará. Other Indians would comment that such women were not setting a good example for their children, who should learn to speak their father's and mother's languages correctly. Occasionally such language mixing would be overtly criticized because of my presence, with remarks to the effect that I would shame the longhouse if I learned to speak Bará with Tuyuka words.

What we observe in all this is the phenomenon of 'Daddy's words' and 'Mummy's words' writ large. From one point of view, all Vaupés people have at their command at least three synonymous or near-synonymous terms for every concept. But the cognitive dilemma thereby generated is resolved by strict attention to the paradigmatic dimension. The Vaupés culture is unusual inasmuch as the requirement of linguistic exogamy is unusual. One can easily imagine this requirement breaking down through the development in each longhouse of a unique local blend of vocabulary items. But the fact that the requirement has not broken down (at least, not for internal reasons, as opposed to external pressures) is a tribute to the ease with which human brains can find a paradigmatic solution for a synonymy dilemma, even if that solution requires each individual to acquire three, four, or more vocabularies rather than just one.

4.2.3 *Multivocabulism: Two Australian cases*

As I have just said, every Vaupés Indian acquires three or more vocabularies, in virtue of learning three or more languages. But situations exist in which speakers of just one language have to master two or more vocabularies or partial vocabularies within it. They are not multilingual, inasmuch as their language has only one grammar. I therefore propose the new terms **multivocabular** and **multivocabulism** for their situation. Two Australian instances are supplied by the North Queensland languages Dyirbal (Dixon 1971, 1972) and Guugu Yimidhirr (Haviland 1979a, 1979b).

In Dyirbal there are (or used to be) two vocabularies: Guwal, used in ordinary circumstances, and Dyalŋuy, used in the presence of certain taboo relatives including, for a man, his mother-in-law, and for a woman, her father-in-law.[6] The traditional structure of Dyirbal society allows a person's taboo relatives (including potential mothers-in-law or fathers-in-law) to be determined from birth, so both vocabularies can be learned natively in childhood. The two vocabularies are entirely separate; that is, in the five open word-classes identified by Dixon (noun, verb, adjective, adverbial and time qualifier), there is no word that is used in both Guwal and Dyalŋuy.

Dyirbal speakers can supply a Dyalŋuy equivalent for any Guwal word. However, the relationship between them is not one-to-one. Dyalŋuy has only about a quarter as many words as Guwal has, and there are numerous many-to-one correspondences, such as the following (Dixon 1971: 437):

(6)

Guwal	Dyalŋuy
nudin 'cut deeply, sever'	dʸalŋgan 'cut'
gunban 'cut less deeply, cut a piece out'	
baygun 'vigorously shake or wave, bash something on something else'	bubaman 'shake, wave or bash'
dyindan 'gently wave or bash, e.g. blaze bark, rain falling gently'	
banʸin 'split a soft or rotting log by embedding a tomahawk in the log then bashing the log against a tree'	

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[6] Dyalŋuy had fallen out of regular use by the time Dixon did his fieldwork in the 1960s, Guwal being then spoken even in the presence of taboo relatives. However, older speakers were ready to show off their knowledge of Dyalŋuy when asked, and I will use the present tense in describing the Guwal-Dyalŋuy relationship.

Dixon draws attention to the window that Dyalŋuy opens into the semantic structure of Dyirbal, in that certain semantic contrasts within Guwal are neutralized in Dyalŋuy while others are not. I would like to draw attention to a different aspect, however. Many-to-one correspondences of the kind illustrated at (6) are consistent with having more than one vocabulary, provided that some overlap between vocabularies is allowed. A conceivable situation of this kind is presented at (7), which exploits the same individual words as (6) but illustrates a hypothetical Pseudo-Dyirbal with not two vocabularies but four: Guwal, Dyalŋuy, and two 'Intermediate' vocabularies:

(7)

Guwal	Intermediate I	Intermediate II	Dyalŋgan
nudin	nudin	nudin	dʸalŋgan
gunban	dʸalŋgan	dʸalŋgan	
baygun	baygun	bubaman	bubaman
dʸindan	dʸindan		
banʸin	banʸin	banʸin	

In Pseudo-Dyirbal society, let us assume, there are three degrees of taboo status. In the presence of fully taboo relatives, Dyalŋuy is used, but in the presence of less strictly taboo people one or other of the Intermediate vocabularies are used. The Intermediate I vocabulary is smaller in size than Guwal but larger than Intermediate II, which in turn is larger than Dyalŋuy. Crucially, neither Intermediate I not Intermediate II has any words peculiar to it. In (7), every Intermediate word is the same as either its Guwal or its Dyalŋuy counterpart.

Could a multivocabular situation analogous to Pseudo-Dyirbal actually exist? From an anthropological point of view, one is inclined to answer: why not? Many societies recognize more than just two significant divisions within them, and if a special vocabulary can be used with a special class of relatives (as in actual Dyirbal), surely other special vocabularies could be used with other classes of relatives. As for the memory load imposed by rich multivocabulism, it would be no greater than that imposed by the rich multilingualism of the Vaupés—especially, perhaps, if the further vocabularies of Pseudo-Dyirbal did not require the memorization of any further words but simply exploited the combined word-store of the two vocabularies at the extremes of the taboo spectrum.

It is precisely this last characteristic of Pseudo-Dyirbal that should give us pause, however. Recall the situation of English and French bilingual child, and

of a multilingual member of the Vaupés community. For the bilingual child, *chat* and *cat* mean the same thing, in some sense; yet they are differentiated in that each is paradigmatically associated with precisely one menu of alternative vocabulary items, distinct from the other menu. For the Vaupés person, similarly, each word in a set of prima facie synonyms belongs to precisely one language, and carelessly to use a word from one language while speaking another invites reproof. The role of the paradigmatic dimension in differentiating potential synonyms is therefore clear-cut. But in Pseudo-Dyirbal things are not clear-cut. If one hears *baygun* meaning 'shake vigorously', one knows that the speaker is not in the presence of a relative so taboo as to require the substitute *bubaman*; however, one cannot tell if the vocabulary in use is Guwal or Intermediate I.

It is true that, even in Pseudo-Dyirbal, *baygun* and *bubaman* do not risk perfect synonymy, inasmuch as *bubaman* is a semantically more general Dyalŋuy term corresponding to three Guwal terms *baygun*, *dʸindan*, and *banʸin*. Nevertheless, a new feature of Pseudo-Dyirbal, not encountered in the Vaupés languages or in actual Dyirbal (or in Anglo-French bilingualism) is the fuzziness or blurring of the boundaries between vocabularies that is evident in (7).

Let us look at another Australian case that will put this budding hypothesis to the test, in that it may seem superficially to resemble Pseudo-Dyirbal. Guugu Yimidhirr was traditionally spoken in an area measuring about 50 by 100 kilometres north-west of the site of modern Cooktown in the Cape York Peninsula in northern Queensland.[7] Guugu Yimidhirr society resembles Dyirbal society in being divided into sections ('moieties'), this division being the basis for strict constraints on interpersonal behaviour. Taboo relatives are called 'Dhabul'. These include a man's mother-in-law, who he is not allowed to speak to at all, and certain male in-laws, who he is allowed to speak to, but only in a slow and respectful manner, and using a special 'brother-in-law' vocabulary (Haviland 1979a, 1979b).[8]

Thus far, the Guugu Yimidhirr situation sounds virtually identical to that of Dyirbal, with brother-in-law vocabulary corresponding to Dyalŋuy. A further parallel is that one brother-in-law word may correspond to a set of semantically related ordinary words. An important difference, however, is

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[7] The English word *kangaroo* is a borrowing from the Guugu Yimidhirr word *gangurru*, which Captain Cook and his companions presumably heard when they encountered the Guugu Yimidhirr people in 1770.

[8] I deliberately do not say what happens in situations involving a woman and her father-in-law. Haviland explains that he was unable to gather information about any special vocabulary used by women.

that ordinary vocabulary and brother-in-law vocabulary in Guugu Yimidhirr are not completely distinct. The ordinary word *mayi* 'food' has a brother-in-law counterpart *gudhubay*, but *budhuurr* 'zamia-nut' has no counterpart. Even so, *budhuurr* can be used in the hearing of Dhabul relatives, provided that the appropriate respectful manner of speaking is observed. This is typical of names of plant and animal species.

The permission granted for some words to be used in both ordinary and Dhabul contexts shows that the clear-cut vocabulary distinction illustrated at (6) for Dyirbal does not apply universally in Guugu Yimidhirr. And there is yet further apparent fuzziness between vocabularies, as shown at (8) (where the 'unrestricted' category refers to the sort of speech that a man could have with his wife, or in the special uninhibited relationship between a grandfather and grandson):[9]

(8)

	Dhabul		*non-Dhabul*		*unrestricted*
	opposite sex: no speech	same sex: respectful speech	opposite sex: polite speech	same sex: everyday speech	joking or vulgar language
'zamia-nut'		budhuurr			
'food'		gudhubay	mayi		
'axe'		gadiil-baga	guliirra	warrbi	
'penis'					gulun

Does this not illustrate just the sort of vocabulary blurring whose avoidance on the brain's part might (I suggested) render Pseudo-Dyirbal not just a non-existent language but an impossible language?

Let us consider first 'zamia-nut' and 'food'. It is not that two vocabularies are blurred here, so as to yield a hybrid intermediate vocabulary. Rather, it is that certain words, particularly belonging to what one might call the technical vocabulary of Guugu Yimidhirr, simply lie outside the domain of the distinction between ordinary and brother-in-law language. In both Dyirbal and Guugu Yimidhirr, phonology and grammar lie outside this domain, which

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[9] (8) is adapted from Haviland's Table 4.15 (1979b: 227).

is why we talk of not distinct languages but distinct vocabularies within one language.[10] In Guugu Yimidhirr, the domain is restricted somewhat further, so as to exclude certain types of vocabulary. But, within this restricted domain, the distribution of *mayi* and *gudhubay* for 'food' observes the principle of clear-cut distinction between vocabularies.

We turn now to the words for 'axe'. We have focused so far on the linguistic effects of the distinction between Dhabul and non-Dhabul relatives. But we have already noticed that sex plays a role too, in that male and female Dhabul relatives are treated differently. And sex differences have linguistic effects in non-Dhabul contexts too. A man must be careful in how he speaks to certain female blood relatives. As Haviland puts it (1979b: 225–6):

[A] man was expected to monitor his behavior with his elder sisters and, to some extent with his mother. . . . [A]lthough one used everyday vocabulary, it was important to prune from one's speech with such people all 'bad words', that is, words with vulgar overtones. . . . For example, a man should not say *warrbi* 'axe' to his sister because to her it might suggest 'penis'. He should not say *nambal* 'stone' because she might interpret instead 'testicles'. He should not say *warrigan* 'hole' because it suggests 'vagina'. And so on. . . . Instead of saying *warrbi*, a man might use the more polite word *guliirra*, which also means 'axe'. Or in modern times he could simply use the English word 'axe'. . . . Neither word would offend his sister, although neither would be sufficiently polite for speaking to his father-in-law or brother-in-law. With them he would use the brother-in-law word *gadiil-baga*, said to be the 'deepest' or most polite word for 'axe'.

As for the word *gulun*, which means 'penis' and nothing else, it is so rude that it can be used only with other men with whom one is on the most intimate terms, including (according to Guugu Yimidhirr convention) one's father's father or son's son.

What the table at (8) illustrates, then, is not a single dimension of avoidance or taboo status. Rather, it illustrates the linguistic manifestation of two superimposed dimensions: a two-term dimension of avoidance involving relationship by marriage (Dhabul versus non-Dhabul) and a three-term dimension of politeness (restricted to non-Dhabul contexts) involving sexual acts and associated body parts. At the polite end, in the presence of certain female blood relatives, words with a metaphorical sexual connotation such as

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[10] Dixon (1971: 436, 437) does at one point refer to Guwal and Dyalŋuy as 'two distinct languages' that are at the disposal of 'every user of Dyirbal'. But he also says that 'Dyalŋuy has identical phonology and almost exactly the same grammar as Guwal', the grammatical differences being only 'differences of degree' (for example, Dyalŋuy makes more use of verbalization and nominalization processes than Guwal does).

warrbi 'axe' must be replaced by a term free of that connotation. At the rude end, there are some words that cannot be used even in the presence of other non-Dhabul men, except for that subset of them with whom sexually tinged joking behaviour is permitted. On that basis, we are entitled to ignore the 'axe' and 'penis' rows in (8) if our interest is solely in brother-in-law language; and, as soon as we do so, the last three columns in (8) merge into one, so that the apparent blurring of boundaries between vocabularies disappears.

4.2.4 *'High', 'middle', 'low', honorific, and belittling vocabularies in Javanese*

Probably every student of sociolinguistics, when introduced to the phenomenon of diglossia, learns that, in Javanese speech situations, vocabulary choice is governed in complex ways by the relative social standing of the interlocutors (Geertz 1960). Relevant also is the degree of respect shown by them to each other and to third parties mentioned in the conversation. Poedjosoedarmo (1968: 59) distinguishes no fewer than nine levels of politeness in Javanese, with three basic levels divided into three sublevels, as follows:

A. Krômô: 1. Muḍô-krômô
           2. Kramantôrô
           3. Wreḍô-krômô

B. Madyo: 4. Madyô-krômô
           5. Madyantôrô
           6. Madyô-ngoko

C. Ngoko: 7. Bôsô-antyô
           8. Antyô-bôsô
           9. Ngoko-lugu

Does this then mean that Javanese has nine distinct vocabularies, whether complete or partial, analogous to Dyirbal's two? Unsurprisingly, the answer is no. Consequently, if one were to illustrate in tabular fashion Poedjosoedarmo's nine sublevels on the lines of the Guugu Yimidhirr table at (8), many words would appear in more than one of the nine columns. In fact, few 'ordinary' words have more than two 'polite' or 'respectful' counterparts. Javanese would thus seem to present the severest challenge yet to the hypothesis that each vocabulary within a multivocabular or multilingual situation must be clearly distinct from the others.

The challenge largely melts away, however, when one teases apart the three independent dimensions that govern vocabulary choice in Javanese. This daunting task has been undertaken by Errington (1988). Let us consider first

the factors relating to the social status of the interlocutors before considering the honorific and belittling terms.

What children learn first (the most basic vocabulary) is 'low' or Ngoko. For open-class items such as nouns and verbs there is a single 'high' or Krama alternate for many (not all) Ngoko words.[11] The 'high' alternates are used when talking to people whose high social status one wishes to acknowledge; such a person, in replying, may use 'low' vocabulary, but in some circumstances there is reciprocal 'high' usage. For closed-class items, however, such as deictic elements, grammatical formatives, and pronouns, there is a three-way contrast between 'low', 'middle', and 'high'. Javanese thus has two distinct domains in which more than one vocabulary operates: the open-class domain, with two vocabularies, and the closed-class domain, with three. In principle, the two domains operate independently. I empha-size 'in principle', however, because the social meanings associated with the 'low' and 'high' vocabulary in each domain preclude the combination within one sentence of a 'high' closed-class item with a 'low' open-class one, or vice versa. (In much the same way, the meanings of the phonological features [high] and [low] preclude any vowel from being [+high, +low].) In practice, there is room for manoeuvre only in contexts where the vocabu-lary used for closed-class items is 'middle' (Madya); Madya items can be combined with either Ngoko or Krama items from the open-class vocabu-lary, so as to express respectively a lower or higher degree of middle-level deference.

Within the closed-class vocabulary, second-person pronouns occupy a special position, having more than three alternatives. That is not surprising when one considers that in many languages, including most European ones, there is a contrast between polite and familiar pronouns for 'you'. One could if one wished regard second-person pronouns as constituting a distinct vocabu-lary on their own, independent of the general closed-class vocabulary as well as the open-class vocabulary, and combinable with both of them freely, subject to any incompatibilities of the 'high'–'low' type.

The third vocabulary distinction (not counting words for 'you') involves honorific (Inggil) and belittling (Andhap) vocabulary. As an example, con-sider Javanese verbs roughly translatable as 'take'. A Ngoko verb *njupuk* contrasts with a Krama equivalent *mendhet*. But there is also an Inggil word *mundhut*, compatible with both Krama and Ngoko vocabulary elsewhere in the sentence, whose use marks the speaker's respect or deference towards the

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[11] 'Krama' is equivalent in Errington's transliteration to Poedjosoedarmo's 'krômô'.

person doing the taking, whether that person is the addressee or not. As Errington puts it (1988: 99):

Honorific terms differ from ngoko, madya, and krama terms structurally because they can be used in any address style, semantically because their membership is (crudely put) restricted to the domain of persons, and pragmatically because their social significative function as markers of deference is keyed to the identity of objects of linguistic reference [that is, to who one is talking about rather than who one is talking to].

Again, the Inggil-Andhap dimension is in principle independent of both the closed-class Krama-Madya-Ngoko dimension and the open-class Krama-Ngoko dimension, even though pragmatic considerations rule out certain logically possible combinations. (For example, even when speaking in Ngoko to a social inferior, it would be arrogant to express respect for oneself by using an Inggil verb to describe one's own actions.)

In view of all this, the range of alternative formulations that Javanese makes available for any one propositional content is formidable. It would be easy to construct a table on the lines of (8) in which many words are shared between more than one formulation, in complexly overlapping ways. But Errington's analysis shows that this variability does not involve vocabulary blurring. What it shows rather is that one dimension (such as Inggil-Andhap) may sometimes take priority over another (such as Krama-Ngoko) in respect of a domain of vocabulary to which it applies. In this respect, paradigmatic synonymy-avoidance is no different from syntagmatic: we have already seen, for example, how phonological context may take precedence over gender in respect of the choice between the words that mean 'my' in French.

4.2.5 *A common characteristic: Vocabular clarity*

This concludes a sample survey of multivocabular situations. No doubt many more such situations exist, whose behaviour may or may not be consistent with what I will be proposing in Chapter 5 with regard to inflectional morphology. Nevertheless, it is remarkable that the four that we have looked at, from three widely separated and culturally unconnected regions (South America, Java, and Australia), share with bilingualism at the individual level a characteristic that I will call **vocabular clarity**.

Recall that, for an English-French bilingual child, the synonymy of *cat* and *chat* is potentially a problem, but a problem that she resolves through assigning them to two distinct vocabularies. As it happens, these are the vocabularies of two distinct languages. Her brain may at first distinguish them as

'Mummy's words' and 'Daddy's words' rather than as 'English' and 'French'; but that does not matter. The crucial point is that the two vocabularies are not muddled together. The use of the word *chat* is a signal to expect, in the same immediate context, *lapin* rather than *rabbit* and *chien* rather than *dog*. Bilinguals make a binary choice each time they speak; they do not take the opportunity to display a continuum of linguistic possibilities from (for example) an English extreme to a French extreme.

The same sort of clear-cut distinction emerges in relation to the institutionalized multilingualism of the Vaupés people and the Guwal and Dyalŋuy vocabularies of Dyirbal. In Guugu Yimidhirr and Javanese the situation is different in that not all 'ordinary' words have a counterpart in the 'special' vocabulary. Some words lie outside the contrast between specifically Dhabul and non-Dhabul vocabulary in Guugu Yimidhirr, and some open-class words in Javanese are neutral in that they can be used alongside both Krama and Ngoko words (though the traditional terminology tends to obscure this, in that 'Ngoko' is used both for words that contrast with a Krama counterpart and words that are neutral between the two vocabularies). Also, in Guugu Yimidhirr and Javanese the picture is complicated by orthogonal vocabulary distinctions, such as, in Guugu Yimidhirr, words usable and not usable by a man in a woman's hearing and, in Javanese, honorific and demeaning words, and a three-way Krama-Madya-Ngoko distinction for closed-class words. Some combinations from orthogonal vocabularies are for social and pragmatic reasons excluded. Nevertheless, a pattern that is perfectly conceivable, yet is not observed, is a vocabulary continuum. One does not use more or fewer Krama words in order to express gradations of deference. Rather, in a context where an open-class Krama word occurs, we can be confident of finding no Ngoko words for which an open-class Krama counterpart is available. Vocabulary choice, though complex, is subject to the fundamental requirement that it is clear which vocabulary one is using at any one time.

In the social contexts where multivocabulism occurs, it is easy to see why vocabulary choice should be so clear-cut. It has social functions whose fulfilment is central to how individuals interact in the communities concerned. What's more, the complexity of social relationships and interactions manifested in non-human primate communities shows that this kind of social function could well have been important already in prehistoric communities whose language possessed as yet no grammar. Indeed, it would be bizarre to suppose that our protolanguage-using ancestors could not have been as status-conscious as (for example) vervet monkeys, as described by Cheney and Seyfarth (1990).

In this book, however, I approach vocabular clarity from the point of view of not a social anthropologist but a linguist interested in language evolution. While vocabular clarity serves important social functions, it may well be coopted by the brain to serve other functions too. I will be suggesting in Chapter 5 that it was coopted to assist with the task of mitigating synonymy in morphology.

5

The ancestors of affixes

In Chapter 4 we looked at certain contemporary languages, examining kinds of synonymy-avoidance technique that operate in non-morphological contexts. In this chapter we begin the task of exploring how such techniques may shed light on the origin of morphology. There is an important assumption here: that synonymy-avoidance techniques used by human brains today to distinguish pairs such as English *addled* and *rancid*, or Guugu Yimidhirr *gudhubay* and *mayi* 'food', would have been available to our ancestors' brains at a stage of language evolution when there was a vocabulary (or vocabularies) though perhaps as yet no syntax. But, in view of the evidence that synonymy avoidance has roots in primate cognition and even in the cognition of other mammals, that is not a risky assumption. Quite the reverse: in the light of evidence such as is presented in Chapter 3, it would be eccentric to reject it.

Here again are examples (18) and (17) from Chapter 3, renumbered as (1) and (2), illustrating situations where synonymyavoidance is jeopardized:

(1) sleep   wake   eat   die
     sleep later$_1$ wake later$_2$ eat later$_3$ die later$_4$
     sleep earlier$_1$ wake earlier$_2$ eat earlier$_3$ die earlier$_4$

(2) sleep$_1$ wake$_1$ eat$_1$ die$_1$
     sleep$_2$ later   wake$_2$ later   eat$_2$ later   die$_2$ later
     sleep$_3$ earlier   wake$_3$ earlier   eat$_3$ earlier   die$_3$ earlier

These represent schematically two ways in which phonological factors might alter the shapes of vocabulary items within cliché patterns. In (1) it is the unselective item in each pattern that is affected by the selective item that it collocates with; in (2) it is the selective item that is affected by the unselective item. In section 5.1 we will look more closely at the implications of there being two patterns, not one.

As I said in Chapter 3, effects may well be mutual, if the phonological conditions are appropriate. But we need to learn to walk before we can run. That is, it is sensible to try to work out the brain's likely response to simpler patterns first, and

in this book that as far as we will go. In section 5.2 we will consider the implications of the pattern at (1), before turning in Chapter 6 to the pattern at (2).

5.1 Preliminaries: 'Distinct items' and 'distinct forms of the same item'

In Chapter 3 I said that potential synonyms arising from phonological developments would fall into two categories:

- **distinct items**, usually but not always historical descendants of what had once been an unselective item with a single phonological shape;
- **distinct forms of the same item**, usually but not always historical descendants of what had once been a selective item with a single phonological shape.

As hypothetical examples of 'distinct items', I cited [ran] and [iŋu] meaning 'earlier'. Although these originated from the single vocabulary item [iranu], the phonological changes that I posited would in due course have made it impossible for speakers' brains to interpret them as forms of the same item. And I suggested that, within cliché patterns, it would typically be unselective vocabulary items rather than selective ones that would suffer this fate. Unselective items, being more readily combinable, would typically be more frequent in usage. They would therefore typically be shorter and so more susceptible to the kind of phonological erosion that would destroy resemblances between their various shapes. By contrast, as hypothetical examples of 'distinct forms of the same item', I cited [flut] and [flyt], which share the relatively selective meaning 'stream'. Even when the phonological trigger for the [u~y] alternation had disappeared, the resemblance between these two shapes would be so close as to require the brain to accommodate a new phenomenon: apparent sameness of meaning combined with small-scale differences in shape.

I should however explain what lies behind those words 'usually but not always'. The implication is that, in a minority of cases:

(a)   synonymous 'distinct items' may not always be traceable wholly to one unselective item with a single phonological shape, and

(b)   'distinct forms of the same item' may not always be traceable wholly to one selective item with a single phonological shape.

I will give illustrations of each possibility. Each, I suggest, sheds light on how the brain handles a particular aspect of morphology: (a) affixal versus non-affixal (or non-concatenative) morphology, and (b) the phenomenon of 'thematic' or stem-forming extensions, such as 'theme vowels'.

To illustrate (a) I will posit a hypothetical but plausible set of phonological changes analogous to those which in Chapter 3 yielded [ran] and [iɲu]. This time, however, the focus is on not an unselective item but on the selective items with which various unselective items are collocated. Let us assume a variety of protolanguage in which there are vocabulary items *nehat* 'sleep', *musap* 'wake', *tolak* 'eat', *paran* 'die', *pi* 'later', and *un* 'earlier'. (I have assumed that the second syllable of the selective items all happen to contain the vowel [a] for a reason that will appear in due course.) Inserting these vocabulary items at the appropriate places in the cliché patterns at (1), we arrive at:

(3) nehat   musap   tolak   paran
     nehat pi musap pi   tolak pi   paran pi
     nehat un musap un   tolak un   paran un

Let us assume too that selective items (the first items in these collocations) are stressed on the final syllable and also normally receive stronger stress than unselective items, so that (for example) /nehat pi/ is realized as [ne'hatpi]. Now consider the effects of the following phonological changes, applying in the order presented:

(4) a. Before a pause, plosives are deleted.
     b. Nasals assimilate in place of articulation to a following obstruent.
     c. Obstruents between vowels are voiced.
     d. An unstressed vowel in the final syllable of a cliché is elided and any consonant cluster thereby created is simplified through the loss of all but the first consonant.

The effects on the forms at (3) will be as set out in (5)–(8):

(5)   ne'hat   ne'hat pi   ne'hat un
     a. ne'ha   —   —
     c. —   —   ne'hadun
     d. —   ne'hat   ne'had

(6)   mu'sap   mu'sap pi   mu'sap un
     a. mu'sa   —   —
     c. —   —   mu'sabun
     d. —   mu'sap   mu'sab

(7)   to'lak   to'lak pi   to'lak un
     a. to'la   —   —
     c. —   —   to'laɡun
     d. —   to'lak   to'laɡ

(8)   pa'ran   pa'ran pi   pa'ran un
    b. —   pa'rampi   —
    d. —   pa'ram   pa'ran

By this means, the set of forms at (3) is transformed as follows:

(9)   'sleep'   'wake'   'eat'   'die'
                   neha   musa   tola   paran
     'later'   neha t   musa p   tola k   param
     'earlier'   neha d   musa b   tola g   paran

If one compares (9) with (3), 'transformed' seems an appropriate word. Whereas there was originally only one item meaning 'later', namely [pi], there now appear to be three, namely [t], [p], and [k]—an unpalatable pattern of synonymy. What's more, the item meaning 'die' is collocated with none of these but instead displays a change in the final nasal. A similar situation applies to 'earlier', except that what had been the collocation meaning 'die earlier' is no longer a collocation at all. Nor can speakers' brains attribute the choice between [t], [p], and [k] or between [d], [b], and [g] to (for example) some phonological influence of a neighbouring vowel, because the same vowel [a] appears alongside all of them.

Later I will suggest how the brain might be expected to reconcile such a pattern with its dislike of synonymy. For present purposes, what matters is that we now have what appear to be six vocabulary items, [t], [p], [k], [d], [b], and [g], none of which preserves any segments from the original items meaning 'later' ([pi]) and 'earlier' ([un]). (The [p] of [musa p] 'wake later' may look as if it preserves the [p] of [pi] 'later', but in fact is descended from the original final [p] of [musap] 'wake'.) The old unselective items have disappeared, but before doing so they have affected their collocational partners in such a way that new items meaning 'earlier' or 'later' (new 'affixes', in modern terms) have been extruded. Lest this should seem too outlandish a process, I would point out that it has parallels in modern languages. The contrast between the neatly perspicuous array of forms at (1) and the seeming randomness at (3) recalls a notorious instance where affixes are partly composed of material extruded from earlier stems is supplied by Maori (Hale 1973: 414-18). In Maori, passive suffixes have acquired what used to be a stem-final consonant:[1]

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[1] De Lacy (2004: 501) argues for 'a basically phonological approach' to the Maori passive alternation. However, although he shows that the choice of suffix is subject to interesting prosodic constraints, he

(10) *Active*   Passive
      'drink'   [inu] (*earlier* *[inum])   [inu-mia]
      'catch'   [hopu] (*earlier* *[hopuk])   [hopu-kia]
      'embrace'   [aɸi] (*earlier* *[aɸit])   [aɸi-tia]
      'carry'   [mau] (*earlier* *[maur])   [mau-ria]
      'point out'   [tohu] (*earlier* *[tohuŋ]   [tohu-ŋia]

Example (9) thus provides a hypothetical protolinguistic instance of minority situation (a). As an illustration of situation (b), consider the following, where once again we supply hypothetical protolinguistic counterparts for the English glosses at (1):

(11)   tuma   lati   nose   roku
       tuma pi   lati pi   nose pi   roku pi
       tuma un   lati un   nose un   roku un

It will be seen that we retain here *pi* 'later' and *un* 'earlier' from the vocabulary used at (3), but introduce new vocabulary items *tuma* 'sleep', *lati* 'wake', *nose* 'eat', and *roku* 'die'. Now consider the effects of the following phonological changes:

(12)   a. Before a pause, vowels are deleted.
       b. When two vowels appear in sequence, the first is deleted.

It is not hard to work out that the effect of these changes on the forms at (11) is as follows:

(13)   tum   lat   nos   rok
       tum ap   lat ip   nos ep   rok up
       tum un   lat un   nos un   rok un

In (13), as in (9), I have used a space to indicate what is, on the face of it, the most obvious boundary between the selective vocabulary items and the unselective ones. There are now in these examples not one but four forms meaning 'later', namely [ap], [ip], [ep], and [up]. It will be seen that what was previously the final vowel of each selective item has now leaked across, so to speak, so as to become part of the unselective items.

Are these then four distinct items? To put it more concretely: are these items as distinct as [ran] and [iŋu], the descendants of earlier [iranu] in the example discussed in Chapter 3? The answer has to be no. All of them share

does not respond to Hale's arguments in favour of analysing the consonants in the *-Cia* passives as belonging to the suffix, not the stem. The contrast between earlier and later stages of Maori recalls a contrast between Finnish and the closely related language Estonian, outlined by Comrie (1989: 50–1).

the final [p]; they differ only in the preceding vowel. The pattern at (13) in fact reminds one of the sort of pattern familiar to students of Romance languages, as in the Italian present indicative verb forms at (14):

(14)		'speak'	'fear'	'depart'
	1 Sg ('I')	parl o	tem o	part o
	2 Pl ('you')	parl ate	tem ete	part ite
	1 Pl ('we')	parl iamo	tem iamo	part iamo

Standard descriptions of this pattern in Romance philology would treat the 2 Pl vocabulary items (or, in modern terms, suffixes) as basically the same, differing however in their theme vowels ([a], [e], and [i]); these theme vowels however do not show up in the 1 Sg and 1 Pl forms. In generative phonology since the late 1960s, there has been a strong bias to towards treating suffixes such as [ate], [ete], and [ite] as not merely 'basically' the same but entirely the same: the vowels [a], [e], and [i] belong underlyingly to the stems, not the suffixes, and their absence in the 1 Sg and 1 Pl forms (so that we have [parlo], not *[parlao], for example) is due to phonological processes that obscure superficially a fundamental uniformity.

The wisdom or otherwise of this sort of approach has been debated exhaustively in the literature on phonological theory. Fortunately, we need not take a position on the matter. For present purposes, it is enough to acknowledge that at (13) we have an illustration of a situation where distinct forms of the same item are a composite of material from two sources: they are traceable not to a single selective item (like our hypothetical [flut] and [flyt]) but rather to a combination of an unselective item with a variety of selective items. What's more, this illustration is plausible in that similar patterns can be found in contemporary languages.

What implications does this have for how the brain handles apparent synonymies of this kind? What takes precedence: the fact that [flut~flyt] and [ap~ip~ep~up] resemble one another in being both 'distinct forms of the same item', or the fact that they differ in that [flut~flyt] are forms of a selective item whereas [ap~ip~ep~up] are forms of an unselective one within a cliché pattern menu? The answer turns out to be the latter, I will suggest. The reason for this answer emerges when we consider the application of multivocabulism to cliché pattern menus in the next section.

5.2 Multivocabulism among unselective items: The origin of inflection classes

Our task is to consider how the brain would reconcile the kind of pattern illustrated in (1) with its inbuilt dislike of synonymy. Example (1) represents a

pattern in which there are four forms of both the item glossed 'later' and the item glossed 'earlier'. Thus, each of the selective items glossed 'sleep', 'wake', 'eat', and 'die' is represented as being collocated with a distinct vocabulary item meaning 'later' (or perhaps, as illustrated in (13), a distinct form of the same vocabulary item). But this proliferation of items (or forms) is not inevitable. It is hardly likely, in any version of protolanguage, that every selective item in a cliché pattern would as a result of phonological processes be collocated with a unique version of every unselective item. It could be, for example, that, while there are four items that mean 'later', representable schematically as $later_1$, $later_2$, $later_3$, and $later_4$, there are only two items that mean 'earlier', namely $earlier_1$ and $earlier_2$. Furthermore, it could be that $earlier_1$ and $earlier_2$ are distributed as in (15), where $earlier_1$ collocates with any selective item that goes with either $later_1$ or $later_2$, and $earlier_2$ collocates with any item that goes with either $later_3$ or $later_4$:

(15) sleep   wake   eat   die
      sleep $later_1$ wake $later_2$ eat $later_3$ die $later_4$
      sleep $earlier_1$ wake $earlier_1$ eat $earlier_2$ die $earlier_2$

This may seem an obvious and trivial point to make. But it has big consequences as soon as we consider one option for the protolinguistic brain in making sense of (1) or (15): the option of positing multiple vocabularies of unselective items meaning 'earlier' and 'later'. These consequences will emerge as we recall that the various examples of multivocabulism described in Chapter 4 all conformed to a principle of vocabular clarity: it was always clear which vocabulary was in use at any one time.

In relation to (1) and (15), this hints at an intriguing possibility. In (1), the choice of the item $earlier_2$ for 'earlier' identifies precisely which is the appropriate item for 'later' in the same context, namely $later_2$. But in (15), the same does not hold. The choice of $earlier_2$ is compatible with either $later_3$ or $later_4$. So (1), as an exemplification of the cliché pattern menu at (16) in Chapter 3 ('ACTION-OR-PROCESS earlier' and 'ACTION-OR-PROCESS later'), can be described in terms of multiple vocabularies on Javanese or Dyirbal lines. There are four vocabularies (albeit minimal ones), consisting of the four sets of unselective items {$later_1$, $earlier_1$}, {$later_2$, $earlier_2$}, {$later_3$, $earlier_3$}, and {$later_4$, $earlier_4$} and (what is important) these vocabularies satisfy the requirement of vocabular clarity. On the other hand, (15) cannot be so described. The choice of $earlier_2$ does not identify a vocabulary, because there is no straightforward paradigmatic relationship between $earlier_2$ and precisely one vocabulary item for 'later'. Nor can one say that $earlier_2$ is outside the domain of contrasting vocabularies altogether, as (for example) names of plant and animal species are in Guugu Yimidhirr; for that analysis cannot account for the alternative

form *earlier₁* that also breaches the requirement of vocabular clarity in that it is collocated both with an item *sleep* that 'takes' *later₁* and with an item *wake* that 'takes' *later₂*.

In contemporary terms, one way to describe the contrast in the unselective items that collocate with *sleep*, *wake*, *eat*, and *die* is by saying that they belong to distinct inflection classes. This is how one describes, for example, the differences in behaviour illustrated at (14) for contemporary Italian *parlare* 'speak', *temere* 'fear' and *partire* 'depart'. So a hypothesis concerning inflection classes in contemporary languages now suggests itself. Let us suppose that the way in which protolinguistic brains coped with patterns such as at (1) and (15), when they arose through phonological processes, was by trying to allocate the distinct unselective items to distinct vocabularies that satisfy the requirement of vocabular clarity. If so, then it is reasonable to explain in the same way the brain's readiness to accommodate elaborate inflection-class patterns in contemporary languages. The sets of inflectional affixes for (let's say) nouns of inflection classes 1, 2, 3, and 4 in some contemporary language are to be thought of as vocabularies 1, 2, 3, and 4 in a multivocabular set-up whose domain is the inflectionally relevant properties of nouns.

To supply neurophysiological evidence bearing on this hypothesis, in our present state of knowledge, is probably not possible. Positive neurophysiological evidence would be (for example) evidence that the way in which the brain handles inflection-class distinctions resembles closely the way in which the brain handles the multiple vocabularies of Javanese or the multiple languages of Vaupés peoples. However, even without neurophysiological evidence, it should be easy to show that this hypothesis is false, if it is indeed false. All that is necessary is to show that inflection-class systems habitually resemble (15) rather than (1): that is, that they habitually violate the requirement that a particular noun's choice of a particular affix to express one meaning (let's say, /am/ for the accusative singular) is a reliable predictor of the affixes that will be chosen by the same noun to express other meanings (including, let's say, /aːrum/ for the genitive plural). So the hypothesis is readily falsifiable. Is it in fact falsified? If not, then that lack of negative evidence constitutes evidence in favour of the hypothesis, in terms of the sort of abductive reasoning defended in Chapter 1: two apparently independent phenomena, namely (i) multivocabulism and the requirement of vocabular clarity under which it operates, and (ii) the exuberant proliferation of inflection classes in some languages, turn out to be related in that they can be attributed to a single cognitive impetus. This impetus is the brain's search for ways to reconcile, on the one hand, certain phonologically induced changes and, on the other hand, its aversion to synonymy.

We need to investigate, therefore, whether the hypothesis is falsified or not. Is it really the case that affixal inflection respects vocabular clarity in the form of 'inflection-class clarity'? A firm answer to that question would require examination of inflection-class systems in a large representative sample of languages. That is work for the future. But a look at a few highly inflected languages (with an admitted Indo-European bias) suggests that the answer is an appropriately qualified yes. We will look at some case studies, all of which tend to support this view while at the same time clarifying both how affixes are related to non-affixal inflection and what it means to be 'outside the domain of contrasting vocabularies' (as I put it earlier).[2]

In this investigation, the drawback of arbitrariness in the sample of languages examined is mitigated by the fact that the sort of disconfirming behaviour that we are looking for is in no way outlandish; it would not be discouraged by any obvious independent factors and, if it exists, it should be easy to spot. And the drawback of Indo-European bias is mitigated by the fact that, although in broad terms the existence of inflection classes is an inherited Indo-European characteristic, formal characteristics of the distribution of particular affixes between inflection classes is not (and indeed, as we shall see in Chapter 6, even closely related languages can differ strikingly in how they impose order when synonymy threatens). By 'an appropriately qualified yes', I mean 'yes, subject to acknowledgement that other factors may sometimes override what one would predict on the basis of paradigmatically determined vocabulary membership, just as, for example, in Javanese the use of honorific and belittling vocabulary (Inggil and Andhap) may override the word choice that one would predict purely on the basis of the Krama-Ngoko distinction'.

5.2.1 *Case study 1: Hungarian verbs and the role of phonological context*[3]

Hungarian, although not Indo-European, resembles many Indo-European languages in that its verbs carry suffixes that signal tense, mood (indicative, imperative, and conditional), number, and person. These suffixes also signal definiteness, that is whether the object of a transitive verb is definite or not;

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[2] Readers familiar with recent literature on the theory of inflectional morphology will recognize here a new presentation of the kind of evidence adduced to support the No Blur Principle by Carstairs-McCarthy (1994). The cognitive explanation that I now suggest for these facts is somewhat different, however. In Chapter 9, when summing up the book's argument, I will consider in more detail some putative counterevidence to the No Blur Principle that has been adduced from Icelandic.

[3] For information about Hungarian I have drawn on Bánhidi et al. (1965), Sauvageot (1951), and Lotz (1939). Hungarian forms are given in normal Hungarian spelling. The stress is always on the first syllable. Acute accents mark long vowels; double letters indicate long consonants. The letters have roughly their IPA values except that *s, sz, a, e, ö, ü* represent [ʃ], [s], [ɒ], [ɛ], [ø], [y] respectively.

for convenience I will use the traditional but not very apt labels 'objective' (meaning 'with a definite object') and 'subjective' (meaning 'with an indefinite object or no object'). A pervasive feature of Hungarian is vowel harmony: most suffixes have two shapes, with a front vowel and a back vowel respectively, and some front-vowel suffixes have a further shape with a front rounded vowel *ö* instead of *e*. Historically this is due to phonological assimilation to the vowel of the stem, and it is still the case that all front-vowel and most back-vowel suffixes are collocated with preceding stems whose last vowel is front or back respectively. However, I will ignore this fact for the time being. The reason for doing so will be explained in due course. For now, it is enough to say that to neglect front-back harmony complicates, rather than simplifies, the task of reconciling Hungarian verb inflection with vocabular clarity, so it will be all the more interesting if this reconciliation can even so be achieved.

What we will be considering is not the full range of person-number endings for Hungarian verbs, but only those that present the greatest diversity and therefore the greatest cognitive challenge for synonymy-avoiding brains. At (16) are present the subjective person-number suffixes, in all three moods, but in the present tense only, while at (17) are the relevant forms of the verb *olvasni* 'to read', which illustrate one actual combination of person-number suffixes out of the many combinations that are imaginable:

			Indicative	Imperative	Conditional
(16)					
	Sg	1	ok, ek/ök, om, em/öm	ak, ek, am, em	ék, ám, ém
		2	(a)sz, (e/ö)sz, ol, el/öl	Ø, ál, él	ál, él
		3	Ø, ik[4]	on, en/ön, ék	a, e, ék
	Pl	1	unk, ünk	unk, ünk	ánk, énk
		2	(o)tok, (e/ö)tek	atok, etek	átok, étek
		3	(a)nak, (e)nek	anak, enek	ának, ének

			Indicative	Imperative	Conditional
(17)					
	Sg	1	olvas-ok	olvass-ak	olvas-n-ék
		2	olvas-ol	olvass, olvass-ál	olvas-n-ál
		3	olvas	olvass-on	olvas-n-a
	Pl	1	olvas-unk	olvass-unk	olvas-n-ánk
		2	olvas-tok	olvass-atok	olvas-n-átok
		3	olvas-nak	olvass-anak	olvas-n-ának

The existence of two 2nd singular imperative forms, *olvass* and *olvassál*, seems straight away to challenge synonymy avoidance. But it is a challenge that is

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[4] 'Ø' represents the absence of a suffix, i.e. a bare stem form.

easily rebutted. The two forms are not synonymous in that the suffixless *olvass* is more brusque, while *olvassál* is more polite. That is true of all verbs. Therefore for present purposes we can ignore this distinction.

Vowels that are enclosed in parentheses in (16) are dependent on the phonological context: they are present if the preceding stem ends in two consonants or a long vowel followed by *t*. Slashes also indicate phonologically conditioning: some (not all) instances of *e* are replaced by *ö* after a stem whose last vowel is front rounded *ö* or *ü*. We may be inclined to regard each of the suffixes concerned as having a single underlying representation from which the various surface shapes are derived by phonological rules, as is traditional in generative phonology. Even if we do not, however, the apparent synonymous *nek* and *enek* for the 3rd person plural (for example) are clearly differentiated by syntagmatic factors, rather like the synonymy of French *mon* [mɔn] and *ma* 'my' that was discussed in Chapter 4. Therefore in the present context, where it is a paradigmatic factor that we are interested in (specifically, the possible role of multivocabulism), we are entitled to disregard the difference between *nek* and *enek*. That applies to all such syntagmatically distinguished suffixal groups in (16). From now on, therefore, I will use just the first member of each group to stand for all of them.

The imperative stem of *olvasni* is *olvass*- ['olvɒʃʃ], distinguished by its long final consonant from the usual stem *olvas*- ['olvɒʃ]. Other verbs exhibit other imperative stem patterns, often involving a glide *j*. However, our focus here being on the person-number suffixes, those stem variations do not matter.

Let us now look at how the inflectional resources at (16) are actually exploited. At first sight, the outlook seems bleak for vocabular clarity. As shown in Table 5.1, we seem to need to distinguish six inflection classes, even though no cell in the paradigm is filled by more than four affixes. Highlighted in bold are those suffixes which belong to only one inflection class and which therefore identify it, in the way in which the use of a Krama word in Javanese clearly identifies the open-class vocabulary currently in use as Krama. They are few: the Sg 1 suffixes of classes E and F. However, a very different picture emerges if we look only at the plural suffixes. For each plural cell in the paradigm there are only two suffixes. Furthermore, they are distributed in the most economical way possible: there are only two ways in which plural affixes are combined paradigmatically, as in (18):

(18)			ABE	CDF
Indic	Pl	1	unk	ünk
		2	otok	etek
		3	anak	enek

(18) (continued)   ABE   CDF
      Imper   Pl 1   unk   ünk
                     2   atok etek
                     3   anak enek
      Condit   Pl 1   ánk   énk
                     2   átok étek
                     3   ának ének

From the pattern of vocabular confusion at Table 5.1, where almost every affix appears in more than one inflection class, we lurch towards perfect vocabular clarity! How could the brain's distaste for synonymy have engineered so disparate an outcome?

   Javanese and the other multivocabular languages we have examined suggest one possible answer. We may need to think in terms of more than one vocabulary domain. One of the principal vocabulary distinctions in Javanese is between the domain of open-class items, in which there are two vocabularies

Table 5.1. Inflection classes of Hungarian verbs (first approximation)

			A	B	C	D	E	F
Indic	Sg	1	ok	ok	ek	ek	**om**	**em**
		2	*asz*	*ol*	*esz*	*el*	ol	el
		3	Ø	Ø	Ø	Ø	ik	ik
	Pl	1	unk	unk	ünk	ünk	unk	ünk
		2	otok	otok	etek	etek	otok	etek
		3	anak	anak	enek	enek	anak	enek
Imper	Sg	1	ak	ak	ek	ek	**am**	**em**
		2	Ø	Ø	Ø	Ø	Ø	Ø
		3	on	on	en	en	ék	ék
	Pl	1	unk	unk	ünk	ünk	unk	ünk
		2	atok	atok	etek	etek	atok	etek
		3	anak	anak	enek	enek	anak	enek
Condit	Sg	1	ék	ék	ék	ék	**ám**	**ém**
		2	ál	ál	él	él	ál	él
		3	a	a	e	e	ék	ék
	Pl	1	ánk	ánk	énk	énk	ánk	énk
		2	átok	átok	étek	étek	átok	étek
		3	ának	ának	ének	ének	ának	ének

Examples: A: ír 'write'   D: vesz 'take'
                   B: olvas 'read'   E: játsz 'play'
                   C: szeret 'love'   F: esz 'eat'

(Krama and Ngoko), and the domain of closed-class items, in which there are three vocabularies (Krama, Madya, and Ngoko). Similarly, it could be that Hungarian speakers' brains make sense of the affixal proliferation at (16) by (among other things) distinguishing a domain of plural persons from a domain of singular persons. Within the plural domain, at least, vocabular clarity is achieved.

What are we to make of the singular domain, however? Part of the answer lies in noticing that the only difference between classes A and B in Table 5.1 resides in the italicized 2nd singular indicative forms, and the same applies to classes C and D. It turns out that within these four classes (unlike classes E and F) the distribution of the four suffixes *asz, ol, esz,* and *el* is determined by the last consonant of the preceding stem: *ol* and *el* appear only after coronal strident consonants *s* [ʃ], *sz* [s], and *z* [z], while *asz* and *esz* respectively (or, more often, their vowelless counterpart *sz*) appear elsewhere. That is why we find *olvas-ol* 'you are reading' in (17), by contrast with (for example) *ír-sz* 'your are writing'. A kind of phonological dissimilation is at work. And it is surely not a coincidence that these suffixes *ol* and *el* are the same as those that appear in classes E and F. The distribution of these four rival suffixes thus resembles that of some of the French words meaning 'my': it is determined generally by the affixal vocabulary that the accompanying stem prefers, but this preference can be overridden by characteristics of the stem-final consonant.

I have just referred to 'the affixal vocabulary that the accompanying stem prefers', but I have yet to show whether in the singular we are entitled to talk of 'vocabularies' at all. The table at (19) illustrates the effect of combining the four classes A, B, C, and D into two, on the basis of the predictability of their differences in the indicative 2nd singular:

(19)				AB	CD	E	F
	Indic	Sg	1	**ok**	**ek**	**om**	**em**
			2	**asz/ol**	**esz/el**	**ol**	**el**
			3	Ø	Ø	ik	ik
	Imper	Sg	1	**ak**	**ek**	**am**	**em**
			2	Ø	Ø	Ø	Ø
			3	**on**	**en**	ék	ék
	Condit	Sg	1	ék	ék	**ám**	**ém**
			2	ál	él	ál	él
			3	**a**	**e**	ék	ék

What we are hoping to find is a solid array of forms in bold. Yet in (19), although boldface is better represented than in (18), there are still twelve cells

(out of 36) that are filled by affixes that do not identify unequivocally which vocabulary they belong to.[5] So vocabular clarity is still not achieved.

At this point, many readers are likely to be puzzled as to why I have not adopted the apparently obvious course of combining together those pairs of inflection classes that differ only in respect of front-back vowel harmony. In doing so, we would be saying that (for example) *ok* in class AB and *ek* in class CD do not count as belonging to different vocabularies because the choice between them is determined syntagmatically. (Likewise, *addled* and *rancid* belong to the same vocabulary; it's just that they are used to modify different nouns.) This would reduce the four vocabularies at (19) to two and the two vocabularies at (18) to one, thus:

(20)

			ABCD	EF
Indic	Sg	1	**ok/ek**	**om/em**
		2	**asz/ol/esz/el**	**ol/el**
		3	**∅**	**ik**
Imper	Sg	1	**ak/ek**	**am/em**
		2	**∅**	**∅**
		3	**on/en**	**ék**
Condit	Sg	1	**ék**	**ám/ém**
		2	**ál/él**	**ál/él**
		3	**a/e**	**ék**
Indic	Pl	1	unk/ünk	
		2	otok/etek	
		3	anak/enek	
Imper	Pl	1	unk/ünk	
		2	atok/etek	
		3	anak/enek	
Condit	Pl	1	ánk/énk	
		2	átok/étek	
		3	ának/ének	

It is still the case that *ál/él* does not identify its vocabulary in the conditional 2nd singular. But it does not need to. Bear in mind that the cognitive motivation for vocabular clarity, according to my argument, is to differentiate competing items that would otherwise appear to be synonymous. Where there is no competition, the need disappears. In this way, *ál/él* resembles in Javanese those Ngoko words that have no Krama counterpart.

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[5] The cells that are filled by zeros are not of concern (at least, not in terms of our present inquiry), because where only where there is an affix can a risk arise of affixal synonymy.

A second apparently strong reason for combining back-front pairs in this way is their obvious phonological resemblance: their consonants are always the same as each other. Surely it is ridiculous to suggest that the brain ignores such a salient fact! If we assign members of back-front pairs to different vocabularies, as in (18) and (19), are we not suggesting that (for example) indicative 1st plural *unk* and *ünk* might just as well have been *unk* and *öp* or *unk* and *eve*, from the point of view of the brain's handling of them?

One may be tempted to accept these arguments. If one does so, the task of reconciling Hungarian present-tense subjective person-number inflection with vocabular clarity seems complete. But there are reasons not to do so, and these reasons may shed light on a strong historical trend in Hungarian person-number inflection: the disappearance of most of the special inflectional characteristics of the '*ik*-verbs' (those that belong to classes E and F in (19)).[6]

First let us deal with the argument from phonological resemblance. When we look at multivocabular situations such as in Javanese and in Australian languages, we find that, although corresponding words from different vocabularies often look quite different, that is by no means always the case. For example, the Javanese words for 'grass' are *suket* in Ngoko but *rumput* in Krama, but for 'sunset' the two words are *surup* and *serap* (Poedjosoedarmo 1969). The same pattern of resemblance is exhibited by *gugu* and *gega* 'to follow advice', *buruh* and *berah* 'to labour', and so on; and there are other such patterns too, as illustrated in Table 5.2. We may perhaps be tempted to describe each of the pairs in Table 5.2 (a)–(c) as constituting only one vocabulary item, whose Ngoko and Krama variants are related by a phonological process. But this style of analysis looks less attractive for Table 5.2 (d)–(k). Even though it is reasonable to suppose that Javanese speakers' brains notice the resemblances listed in Table 5.2, and that this assists the learning of the items concerned, it does not follow that the proper way to capture this resemblance in linguistic theory is to posit a single underlying phonological representation.[7] More importantly, to treat *gugu* and *gega* (for example) as underlyingly the same would require us to ignore what is central to their

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[6] On the original reflexive function of the *ik*-verbs, their loss of this function, their demise in most dialects (except for the *ik* suffix itself) and their revival in nineteenth-century literary Hungarian, see Simonyi (1907: 354–60).

[7] Poedjosoedarmo says that less well educated people often make the mistake of producing a non-existent Krama form according to one of the templates at Table 5.2, even when the word in question is outside the domain of Ngoko-Krama contrast. On the issue of whether the pairs of items at Table 5.2 share phonological representations, this evidence is equivocal, perhaps. But when I discuss data from Polish in Chapter 6, I will have more to say about relationships that look quintessentially phonological but aren't.

Table 5.2. Some phonological patterns in Javanese Krama

Ngoko	Krama	
	a. Vowel melody *CeCa(C)*	
gugu	gega	'follow advice'
lunggoh	lengah	'sit down'
	b. Vowel melody *CiCa(C)*	
susah	sisah	'sad'
kuna	kina	'ancient'
	c. Replacement of final vowel by *i*	
tuna	tuni	'to suffer a loss'
negara	negari	'country'
	d. Replacement of final syllable by *nton*	
pari	panton	'rice (in the field)'
lemari	lemanton	'cupboard'
	e. Replacement of final syllable by *djeng*	
madju	madjeng	'forward'
kayu	kadjeng	'wood'
	f. Replacement of final syllable by *wan*	
ala	awan	'bad
kalah	kawan	'to lose'
	g. Replacement of final syllable by *bet*	
mlebu	mlebet	'to enter'
sambong	sambet	'to connect'
	h. Replacement of final vowel by *i*	
tuna	tuni	'to suffer a loss'
negara	negari	'country'
	j. Replacement of final syllable by *nten*	
kira	kinten	'to think'
dina	dinten	'day'
	k. Replacement of final rhyme by *as*	
ganti	gantas	'to change'
wadi	wadas	'secret'
dandan	dandas	'to dress'

Sources: Poedjosoedarmo 1968: 64–6; 1969; Uhlenbeck 1950: 286–93.

Note: In accordance with Errington's practice, I write *a* where Poedjosoedarmo writes ô, representing a rounded allophone of /a/ that occurs finally or (in some circumstances) in the last two syllables. I have reversed Poedjosoedarmo's order of presentation, in order to place at the start those pairs whose relationship looks as if it could most plausibly be accounted for in purely phonological terms.

social function: the very fact that they are different items, one belonging to the ordinary vocabulary, the other to the vocabulary of deference.

What is the upshot as regards the Hungarian data at (19)? It is that, even though classes AB and CD may be phonologically similar, the hypothesis that inflection-class organization involves multiple vocabularies counts against combining AB and CD on the basis of vowel harmony, as in (20). And the same applies to classes E and F.

This anti-combination view derives support when we look at Hungarian person-number inflection more widely. We have been concentrating on those parts of the verbal paradigm which are inflectionally most diverse: in fact, the parts where *ik*-verbs behave differently from other verbs. If we bring into consideration present indicative objective forms, we find greater differences between corresponding 'back-vowel' and 'front-vowel' forms—differences that go beyond mere *o/e*, *a/e*, and *u/ü* alternation, as shown in (21):

(21) Present indicative objective:

		AB	CD	
Sg	1	om	em	
	2	od	ed	
	3	ja	i	(*not* 'je')
Pl	1	juk	jük	
	2	játok	itek	(*not* 'jétek' *or* 'ítek')
	3	ják	ik	(*not* 'jék' *or* 'ík')

What do we make of the 3rd singular and 2nd and 3rd plural forms? One is tempted to say that they each have a single underlying representation, respectively /jV$_i$/, /jV$_i$tV$_{mid}$k/, and /jV$_i$k/; the only question is the identity of V$_i$. If that is correct, then we can combine AB and CD here just as in (20). But what can V$_i$ be? Its 'front' manifestation must fuse with the glide /j/, so as to yield [i]. This suggests that it is a short vowel. But then it is hard to account for the fact that, in the 2nd and 3rd plural, its 'back' manifestation is long. Besides, there is nothing phonotactically odd about the non-occurring front-vowel versions in parentheses, and some of them actually occur. For example, *je* occurs as a 3rd person singular possessive suffix on some nouns, such as in *kert-je* 'his or her garden', and *jétek* contrasts with *itek* as imperative to indicative (for example, in *kéritek* 'you are asking for it' and *kérjétek* 'ask for it!'). Granted, with ingenuity and a sufficiently accommodating theory of phonology, a synchronic phonological solution to this problem can no doubt be contrived. But, under an analysis whereby the AB and CD forms at (21) belong to distinct affixal vocabularies, no such such ingenious contrivance is necessary.

Considerations involving phonological resemblance thus turn out to speak less strongly for a class-combining analysis than one might expect. Other considerations too support a multivocabular analysis. The first has to do with the distribution of the back-front competitors at (20) and (21). Back-vowel verb stems are consistently followed by back-vowel suffixes, but not all front-vowel verb stems are followed by front-vowel suffixes. There are some front-vowel verbs, mostly monosyllabic and all containing *i* or *í*, which take back-vowel suffixes.[8] These include some common verbs, both in class AB (e.g. *ír-ok*, not **ír-ek*, 'I write') and in class E (e.g. *isz-om*, not **isz-em*, 'I drink'). There is a clear historical reason for this state of affairs. These verbs (and likewise some nouns) contained at one time a high back unrounded vowel [ɯ], after which back-vowel suffixes were entirely expected, but this vowel was later fronted to [i]. Synchronically, there are two traditional methods within generative phonology to handle this kind of situation. In Hungarian, they involve: (i) treating the vowel /ɯ/ as still underlyingly present in these stems, thus engendering back-vowel harmony, before being merged 'on the surface' with /i/; (ii) attributing to these anomalous verbs a lexical 'exception feature' which requires them to trigger back-vowel harmony. But method (i) has drawbacks that were noted as long ago as the 1960s by Kiparsky (1968), and method (ii) merely describes the unusual behaviour of these verbs without either explaining it or exploiting it to explain other things.

The non-merger of E and F, in particular, may be exploitable to help explain a fact about these two classes: they are obsolescent. The array of inflectional resources presented at (16) is that of a conservative or literary variety of Hungarian. In colloquial Hungarian, verbs traditionally belonging to classes E and F are generally inflected like verbs of classes AB and CD respectively, except for retaining the *ik* suffix of the 3rd singular indicative. Why should that be, if the two-class analysis at (20) is correct? After all, in (20), perfect vocabular clarity is achieved. On the other hand, if the four-class analysis at (19) more closely reflects how Hungarian speakers' brains organize the facts, it could be that the very lack of vocabular clarity (highlighted in (19) by the affixes in plain type) has been a factor in the demise of the so-called '*ik*-verbs'. Consider what happens if we amend (19) so as to reflect modern colloquial Hungarian. We will delete all the suffixes in E and F except for the one that occurs most commonly, namely the 3rd singular subjective present suffix *ik* itself. The result is shown in (22):

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[8] Lotz (1939: 40) lists thirty such verbs.

(22)				ABE	CDF
	Indic	Sg	1	**ok**	**ek**
			2	**asz/ol**	**esz/el**
			3	Ø/ik	Ø/ik
	Imper	Sg	1	**ak**	**ek**
			2	Ø	Ø
			3	**on**	**en**
	Condit	Sg	1	ék	ék
			2	**ál**	**él**
			3	**a**	**e**

We now have just two singular vocabularies, exactly in line with the two plural ones at (18). The appearance of conditional 1st singular *ék* in both vocabulary ABE and vocabulary CDF means that it does not identify a vocabulary; however, this is not problematic, because it has no rival that it needs to distinguish itself from. As for *ik*, it does have what looks like a rival, namely the bare or suffixless stem; but because the lack of a suffix is not a suffix, no issue of differentiating *ik* from a potentially synonymous vocabulary item arises. The brain does indeed need to store information about which verbs use the suffix *ik* and which do not; but that is a task to which the analysis of inflection classes suggested here—an analysis in terms of multiple vocabularies of suffixes—need not be expected to contribute.[9]

The historical developments that have led to the pattern at (22) have a further consequence. It is no longer necessary to distinguish a singular person-number domain, with four vocabularies, from a plural person-number domain that has only two. To what extent that is a cognitive improvement depends on the cost to the brain of distinguishing vocabulary domains. The evidence of distinct vocabulary domains in Guugu Yimidhirr and Javanese (for example, the distinct domains of Ngoko-Krama, Ngoko-Madya-Krama, and Inggil-Andhap) suggests that this possibility should be available in inflectional morphology too. But, if we assume that the brain prefers not to recognize distinct domains unless it is unavoidable, then the domain uniformity that emerges at (22) will count as another cognitive improvement.

I am not arguing that the analysis presented at (19) and (22) (for conservative and colloquial varieties respectively) is 'right' while that presented at

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[9] I am aware that some contemporary approaches to morphology make generous use of phonologically empty affixes, particularly Distributed Morphology in the tradition of Halle and Marantz (1993). But that sort of analysis, by treating much non-affixal morphology as if it were affixal, prevents one from making sense of the differences between affixal and non-affixal morphology that the evolutionary account offered in this book predicts. Quite apart from that, Stump's critique of the zero-affix technique is persuasive (1998: 41).

(20) is 'wrong'. It is conceivable that, in its search for ways to make sense of the affixal abundance at (16), the synonymy-avoiding brain endeavours to exploit a paradigmatic factor (multiple vocabularies) and a syntagmatic factor (vowel harmony) at the same time. Each of them works pretty well, but there are still aspects of Hungarian verbal person-number inflection that remain as cognitive irritants: a bit of vocabular unclarity at (19), and non-harmonic suffixes on some front-vowel verbs. Yet marginal awkwardness such as this should not count as an embarrassment if one believes, like most historical linguists, that there are grammar-internal stimuli for linguistic change as well as external (social) stimuli.

5.2.2 *Case study 2: Latin nouns and the role of gender*

Many readers will have recognized Latin as the source of two of my earlier examples: /am/ as an accusative singular suffix and /a:rum/ as a genitive plural one. They belong to what is traditionally known as the 'first declension' of Latin nouns. At Table 5.3 is a table illustrating all the suffixes used to indicate number and case in Latin nominal inflection, with a first attempt at showing their distribution according to inflection class. (I will discuss presently the implications of the parentheses and the slashes in the table.)

Here, as in Hungarian at Table 5.1, we have a pattern in which few suffixes are peculiar to one inflection class. Those few are indicated in bold. Thus, from the point of view of vocabular clarity, Latin nouns look as bad as Hungarian verbs did at first sight. But I will argue that, as with Hungarian, first impressions are misleading.

First, let us consider the relevance of gender to Table 5.3. Our concern with vocabular clarity (or the lack of it) in inflection-class systems requires us to view gender in an unfamiliar light. Discussions of gender usually emphasize its semantic basis (perhaps obscured by various kinds of meaning shift) or its syntactic role in linking 'controllers' (nouns) with 'targets' (adjectives, determiners, or verbs) (Craig 1986; Carstairs 1988b; Corbett 1991). Our emphasis here will instead be on its role as a syntagmatic synonymy-avoidance device. I am not suggesting that synonymy avoidance is the primary function of gender. That would be as strange as to suggest that the primary function of the two words *eggs* and *butter* is to differentiate syntagmatically the otherwise synonymous words *addled* and *rancid*. Rather, I am pointing out that gender is a resource that the brain can exploit in its search for something to differentiate otherwise synonymous affixes. In particular, gender may sometimes resolve synonymy problems that a multivocabular analysis leaves dangling. Just as when we were seeking to account for how the Hungarian 2nd

Table 5.3. Affixal inflection of Latin nouns

Class	A	B	C	D	E	F	G	H	J	K
Sg Nom	**a**	us	Ø	**um**	(i)s/Ø	e:s	Ø	us	**u:**	e:s
Sg Voc	**a**	**e**	Ø	**um**	(i)s/Ø	e:s	Ø	us	**u:**	e:s
Sg Acc	**am**	um	um	um	em/**im**	em	Ø	um	**u:**	em
Sg Gen	**ae**	i:	i:	i:	is	is	is	u:s	u:s	**e:i:**
Sg Dat	**ae**	o:	o:	o:	i:	i:	i:	**ui:**	**u:**	**e:i:**
Sg Abl	**a:**	o:	o:	o:	e/i:	e	i:	u:	**u:**	**e:**
Pl Nom	**ae**	i:	i:	a	e:s	e:s	(i)a	**u:s**	ua	e:s
Pl Acc	**a:s**	o:s	o:s	a	i:s/e:s	i:s/e:s	(i)a	**u:s**	ua	e:s
Pl Gen	**a:rum**	o:rum	o:rum	o:rum	(i)um	um	(i)um	uum	uum	**e:rum**
Pl Dat	i:s	i:s	i:s	i:s	ibus	ibus	ibus	ibus	ibus	**e:bus**
Pl Abl	i:s	i:s	i:s	i:s	ibus	ibus	ibus	ibus	ibus	**e:bus**

Note: In the plural, the vocative is never distinguished from the nominative, so the 'plural vocative' combination is omitted. Even in the singular, only class B has a distinct vocative form.

singular suffixes *(a)sz* and *ol* are distributed, the syntagmatic dimension may come to the rescue of the paradigmatic.

Latin has three genders: masculine, feminine, and neuter. They are potentially relevant to the problem posed by Table 5.3 inasmuch as many of the ten classes A–K have clear gender allegiances, as show in (23):

(23) A   mostly feminine, a few masculine (denoting humans)
      B   mostly masculine, some feminine
      C   masculine only
      D   neuter only
      E   masculine, feminine
      F   feminine (except for masculine *verre:s* 'boar')
      G   neuter only
      H   mostly masculine, some feminine
      J   neuter only
      K   feminine only

We have yet to explore the extent to which the vocabular unclarity at Table 5.3 correlates with gender differences. But, if it does so to a significant extent, then the objectionable synonymy that that unclarity gives rise to will be mitigated and perhaps dissolved altogether. To see why, let us take a step back and recall the kind of multivocabulism that we encountered in Chapter 4.

There is an important difference between how multiple vocabularies work in an English-French bilingual child's upbringing (say) and how they work in Hungarian verbal person-number inflection (say). In the bilingual family situation, when faced with two apparently synonymous items such as *chien* and *dog*, the child's brain will not find that (for example) *chien* is used when the preceding word ends in a vowel and *dog* when the preceding word ends in a consonant. The very suggestion sounds ludicrous. Bilingualism just does not work like that. To differentiate *chien* and *dog*, there is no choice but to maintain vocabular clarity: *chien* identifies unequivocally the language being spoken as French, and *dog* similarly identifies the language as English.[10] But that does not mean that the child's brain would ignore a differentiation between *chien* and *dog* in terms of their phonological context, if it existed. Among Hungarian person-number suffixes, by contrast, that kind of differentiation does indeed sometimes happen, as we saw in the previous section: in the colloquial language, the only factor that differentiates *ol* and *(a)sz*, both of

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[10] There is a difference in this respect between *chien* as used normally and '*chien*' as quoted. When '*chien*' is quoted, in can certainly appear in a non-French context, e.g. *The French for 'dog' is 'chien'.*

Table 5.4. Some Latin noun inflection classes sorted by gender

	CD		EG		HJ	
	masc	neut	non-neut	neut	non-neut	neut
Sg Nom	Ø	um	(i)s/Ø	Ø	us	u:
Sg Voc	Ø	um	(i)s/Ø	Ø	us	u:
Sg Acc	um	um	em/im	Ø	um	u:
Sg Gen	i:		is		u:s	
Sg Dat	o:		i:		ui:	u:
Sg Abl	o:		e/i:	i:	u:	
Pl Nom	i:	a	e:s	(i)a	u:s	ua
Pl Acc	o:s	a	i:s/e:s	(i)a	u:s	ua
Pl Gen	o:rum		(i)um		uum	
Pl Dat	i:s		ibus		ibus	
Pl Abl	i:s		ibus		ibus	

which mean '2nd singular indicative', is that *ol* appears after coronal strident sounds (sibilants) and *(a)sz* appears everywhere else.

The pairs or sets of apparently synonymous Latin affixes at Table 5.3 (encompassing all those affixes that do not identify the inflection class that they belong to) are, just in virtue of being affixes, more akin to *ol* and *(a)sz* than to *chien* and *dog*. There is no reason why a Latin-speaking child's brain should not look for a syntagmatic factor to differentiate them. As for what sort of syntagmatic factor—we have, again, no good reason to suppose that the brain's resourcefulness is particularly inhibited. We already know that the brain can invoke lexical collocation (*addled eggs* versus *rancid butter*) as well as the sort of phonological factor that is relevant in Hungarian. And once syntax had appeared on the scene, more possibilities opened up. Perhaps the differentiating factor could involve an actual or potential 'target' item in an appropriate syntactic relationship to a 'controller'. In other words, the differentiating factor could be gender. It is important to notice that, in saying this, one is not saying that syntactic phenomena such as gender are either necessary or sufficient in order for different patterns of nominal case-number marking to proliferate in the fashion illustrated at Table 5.3. One is saying merely that syntactic phenomena, of which gender is one, are among the several devices that the brain can exploit in its quest for differentiation between apparent synonyms.

Is there, then, any way of enhancing vocabular clarity in Table 5.3 by recourse to gender? This amounts to asking whether the ten classes at Table 5.3 lend themselves to being sorted into groups on the basis of affixal similarity, such

that the affixal differences correlate neatly with gender differences. The beginning of an answer is supplied by the table at Table 5.4, in which information from (23) concerning gender is exploited to combine some of the classes at Table 5.3. I have refrained from using boldface here to indicate affixes that are peculiar to one inflection class. To do so would have been potentially misleading, because data from classes A, B, F, and K are absent. Nevertheless, it seems clear that vocabular clarity is more closely approached in Table 5.4 than in Table 5.3. Let us use the term 'macroclass' for a set of two or more inflection classes that are combined in the way that CD, EG, and HJ are.[11] In fact, in the genitive, dative, and ablative singular and the nominative, accusative, and genitive plural, all the three macroclasses and (where appropriate) the gender-related component classes within them seem satisfactorily distinct. But we still need to account for:

(a) the so far neglected classes A, B, F, and K;
(b) the nominative singular forms;
(c) the slashes and parentheses in macroclass EG;
(d) the dative and ablative plural forms;
(e) class A in relation to classes B and CD.

These topics turn out to overlap in ways that account for the seemingly haphazard order in which I have listed them. We will need to pay attention to two oversimplifications in the way I have presented the data so far: I have said nothing about the relative size of the classes, nor about those aspects of Latin nominal inflection that are particularly variable or fluid at the stage in the language's history to which the data at Table 5.3 applies (roughly 200 BCE to 50 CE).

Consider first classes A, F, and K, all of which are wholly or mainly feminine. Of these, A is large and productive, while F and K are both very small. In terms of variation, A and K overlap in that some members of K can also be inflected as in A, such as *ma:te:rie:s* 'timber', which has a variant *ma:te:ria*. There is also vacillation between F and K, both of which have *e:s* in the nominative singular: for example, *ple:be:s* 'people', originally in F, tends to shift into K by acquiring forms such as ablative singular *ple:be:*, or else into E by acquiring a nominative singular *ple:bs* (Ernout 1953). But this vacillation is no embarrassment, for present purposes. Indeed, it is just what we should

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[11] 'Macroclass' is used here in essentially the same sense as in Carstairs (1987), although its use there was not associated with the idea that affixal inflection classes might constitute distinct vocabularies on the lines of Krama and Ngoko in Javanese.

expect, if Latin speakers' brains baulk at the lapse in vocabulary clarity by the nominative singular suffix *e:s*. Two ways to solve this problem are to get rid of the *e:s* suffix altogether, or to inflect uniformly all nouns that have it. Both techniques are toyed with, but without a stable outcome.

Notice that the problem of nominative singular *e:s* would disappear if the nominative singular did not display a suffix at all—that is, if all nominative singular forms were analysed as a bare stem, even if different in shape from the stem that is found in other cells of the paradigm. In that case, for example, the nominative singular of the class F noun *fa:me:s* 'hunger' would not be analysed as a stem *fa:m* plus a suffix *e:s* but rather as *fa:me:s* with no suffix, a different stem from that which appears in the genitive singular *fa:mis* (*fa:m* plus suffix *is*). Admittedly, that sounds perverse. But there is ample motivation elsewhere in Latin nominal inflection for regarding nominative singular forms as special. In class C, the nominative singular is suffixless, at least superficially: examples are *ager* 'field' (genitive singular *agri:*), *puer* 'boy' (*pueri:*), *magister* 'master' (*magistri:*), *vir* 'man (adult male)' (*viri:*). All these examples end in *r* and most in *er*, so one might well consider combining class C with class B (which has nominative singular *us*) on the basis of phonological conditioning, analogously to Hungarian *ol/asz*, thus: *us* is absent after *er*. However, this will not quite work, because of class B words such as *numerus* 'number' and *umerus* 'shoulder'. And there are numerous words in class E which, at least superficially, have no nominative singular suffix. Some examples, with the genitive singular to illustrate the non-nominative stem, are given in (24):

(24) consul   consul-is   'consul'   ordo: ordin-is   'order'
      sa:l   sal-is   'salt'   la:tro: la:tro:n-is 'robber'
      so:l   so:l-is   'sun'   caro: carn-is   'meat'
      labor   labo:r-is   'work'   cinis   ciner-is   'ash'
      venter   ventr-is   'stomach'   fu:r   fu:r-is   'thief'

As can be seen, many of these also have a nominative singular stem which appears to differ from the stem in other inflectional contexts. That also applies to a number of nouns that carry the class E suffix *s*, and to a number of neuters which, belonging to class G, are suffixless in the nominative singular. Examples are in (25) and (26) respectively:

(25) fron-s   front-is 'forehead' mi:le-s mi:lit-is 'soldier'
      fron-s   frond-is 'leaf'   sege-s   seget-is   'corn-crop'
      nix [nik-s]   niv-is   'snow'   obse-s   obsid-is   'hostage'
      grex [ɡrek-s] gregis   'flock'   lapi-s   lapid-is   'stone'

(26) caput   capit-is   'head'   cor   cord-is   'heart'
      lac   lact-is   'milk'   iter   itiner-is   'journey'
      tempus   tempor-is   'time'   iecur   iecinor-is   'liver'

In talking about nominative stems that 'appear to differ' or that differ 'at least superficially', from stems used in other forms, I have implied that underlyingly the stems may really be the same. Certainly, in many undergraduate courses in phonology, students encounter pairs of nominative forms such as *frons* 'forehead' and *frons* 'leaf' and are invited to conclude that their underlying stems are respectively /front/ and /frond/, the surface identity being merely a by-product of a phonological process of simplifying final plosive-sibilant clusters. Likewise, they are invited to note that Latin does not allow the cluster [rd] in word-final position and to conclude accordingly that the underlying form of the nominative *cor* 'heart' is /kord/. And maybe the brains of Latin speakers carried out similar analyses. But it is less easy to account in this way for the contrasting behaviour of *ordo:*, *la:tro:*, and *caro:* at (24) or the pattern of alternation between [e] and [i] in the second column at (25). And faced with the peculiar stem behaviour of *iter* and *iecur* at (26), synchronic phonology is bound to admit defeat.[12]

The upshot of all this is that there is a fair amount of evidence that might lead Latin learners' brains to treat nominative singular forms of classes E and F in a special way: specifically, to treat them as suffixless even when they look as if they have a suffix *s* or *is*. Seen thus, the problem of accounting for the distribution of *s* and *is* in class E (for example, why does *urbs* 'city' differ in just this minimal respect from *orbis* 'globe'?) disappears. As I have already said, no issue of vocabular clarity arises in relation to an affix that does not exist.

Did Latin speakers' brains perhaps apply more widely the analysis of nominative singular forms as suffixless? We have already noted that class C (*puer* 'boy' etc.) has no overt suffix. The nominative singular of class B seems to have the suffix *us*; but if this *us* (despite appearances) is treated as part of the stem, then B can be merged with C so as to be part of a BCD macroclass. That also has the advantage that class H becomes the only class in which *us* in the nominative singular counts as a suffix, so it is now reconciled with

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[12] Historically, these nouns are less mysterious. Old Indo-European nominal stem-forming suffixes include *-r, which acquired a new function as a plural marker in German nouns such as *Kinder* (singular *Kind*) 'children', and *-n, which occurs in oblique forms of Russian *ímja* 'name' and *vrémja* 'time' (genitives *ímeni*, *vrémeni*). The Latin words meaning 'journey' and 'liver' happen to have inherited both suffixes.

vocabular clarity. But it would perhaps be expecting too much of the brain to extend the nominatives-are-special analysis even to nouns where there is a clear candidate for a suffix (namely *us*) and where there is nothing odd about the stem to which the *us* is attached. If we assume that, as usual, the brain looks for vocabular clarity on the part of this *us* suffix, we will predict that the existence of the two distinct inflection classes B and H should be problematic. There should be a tendency for one of the classes to become obsolete or for nouns to shift between them. And this prediction is broadly correct. Class B is large; class H is small, and precarious from the start. The most common class H word is *domus* 'house, home', but it shows up also with class B suffixes in all cases except the dative-ablative plural; and no less than twenty class H words appear with the class B genitive singular suffix *i:* rather than the 'proper' class H suffix *u:s* (Ernout 1953: 66). So what may at first seem an embarrassment for the approach we are exploring, namely the fact that two classes share the nominative singular suffix *us*, turns out to be an advantage, in that our approach predicts consequent instability of the kind that we observe.

Similar instability underlies the slashes and parentheses in macroclass EG at Table 5.4. Historically this macroclass arises from a merger of consonant-stem and *i*-stem nouns that was still in train during the classical Latin period. As one might guess, the accusative singular suffix *im* was inherited from original *i*-stem nouns, but is replaced by *em* (from the consonant-stem inheritance) nearly everywhere in the classical period. It survives, at least optionally, in a few words belonging mainly to particular semantic categories: (a) bodily states and body parts (*sitis* 'thirst', *tussis* 'cough', *cutis* 'skin'); (b) some agricultural and nautical terms (*buris* 'plough-beam', *puppis* 'stern', *restis* 'rope'; (c) some names of rivers (*Albis* 'Elbe', *Li:ris* 'Garigliano', *Tiberis* 'Tiber') (Carstairs 1985). This is a disparate bunch. What are we to make of it? The agricultural and nautical terms could be borrowings from local dialects, whose use in the urban dialect survives to connote non-urban strangeness, much as an English-speaking city-dweller, describing a sailing trip, may self-consciously say *I saw an island to starboard* rather than *I saw an island on the right*. But the river names are strange in another respect too: they are masculine, whereas all other nouns with accusative singular *im* are feminine. This illustrates a characteristic of synonymy avoidance that should by now be somewhat familiar. What the brain latches on to as a distinguishing factor between potentially synonymous forms (in this instance, *im* and *em*) need not be something that is either useful for the organization of experience or high in everyday communicative priorities. Pretty much anything will do, it seems, provided that it is consistently exploited.

Most of the other alternatives given for macroclass EG are distributed on a basis that is similarly pointless from the point of view of experience and communication. One or other alternative is favoured on the basis of either gender (masculine versus feminine) or phonological context. The details are complex (Carstairs 1984), and I will not go into them here. Particular interest does however attach to the *i:s/e:s* competition in the accusative plural. As one would expect, *e:s* is for original *i*-stem nouns the newer suffix, displacing *i:s*. Consequently *i:s*, so long as it survived, had an old-fashioned feel. As Nyman (1988: 504) puts it, there was 'a social re-interpretation of the [*i:s*] variant . . . In coin legends, the formula *ob civis servatos* ['for the safety of the citizens'] was supplanted by *ob cives servatos* from Tiberius' period on, until the older, "republicanizing" usage *ob civis servatos* was re-introduced after Nero's fall. This is a linguistic repercussion of changed social attitudes: an available ending variant was made use of to carry a social function.'

Our analysis so far has not resolved the problem of the dative-ablative plural suffixes *i:s* and *ibus*.[13] Even with B, C, and D merged into one macroclass and E, F, and G merged into another, *i:s* is still shared by A and BCD while *ibus* is shared by EFG and HJ. Neither suffix displays vocabular clarity, therefore. But that is not the end of the story, because there is some evidence in favour of merging A with BCD so as to form an even larger macroclass. This may seem odd if one looks at Table 5.3; nearly all class A's affixes are in bold, indicating that they are shared by no other class. Yet there is a relationship between the large, productive classes A and B, in that some names for humans have two forms, one in class A and one in B, denoting females and males respectively: *serva* and *servus* 'slave', *patrona* and *patronus* 'patron', and so on. A much stronger relationship is demonstrated by adjectives: one adjective will conform to class A, class B (or C), and class D when agreeing with feminine, masculine, and neuter nouns respectively. Thus the gender biases in nouns of classes A and B correspond to rigid gender associations in adjectives. And it is true that nouns with the 'wrong' gender for their class are not random sets but semantically characterizable ones. Thus masculines in class A denote human males (such as *agricola* 'farmer', *nauta* 'sailor'), while the few feminines in class B mostly denote tree species (such as *fa:gus* 'beech').

Let us take it then that to combine A with BCD on the basis of gender is legitimate, the predicted gender being overridden in rare instances by

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[13] In all Latin nouns, adjectives and pronouns, the dative and ablative plural forms are identical. I will comment on this further in Chapter 8.

semantic factors. This means that *i:s* is confined to one macroclass and thus acquires vocabular clarity. The rival suffix *e:bus* is similarly confined to class K. But what of *ibus*? It is still shared by two macroclasses: EFG and HJ. Would this have constituted a genuine cognitive irritant to the Latin brain? Or was the HJ class too precarious to matter, for reasons already mentioned? Or is there yet a third possibility: that *ibus* could get away with failing to identify any inflection class because both its rivals, *i:s* and *e:bus*, were successfully differentiated from it anyway by their macroclass allegiances? It is not necessary that there should be a single 'right' answer to here. Conceivably, different Latin speakers' brains could have adopted different solutions, all compatible with a multivocabular view of inflection classes—barring a few loose ends whose status as loose ends is confirmed by the vacillation or change affecting them. But the third possibility is one that we will explore more fully in connection with German.

5.2.3 *Case study 3: German noun inflection and the role of 'elsewhere'*

German noun inflection differs from both the Hungarian and the Latin data that we have looked at in an important respect. Hungarian and Latin have at their disposal an impressive inventory of suffixes. However, in German, the eight case-number combinations that are inflectionally relevant (there are four cases and two numbers) are expressed through a relatively small inventory: just *e*, *en*, *er*, *es*, *ens*, and *s*.[14] In many of the cells the bare stem of the noun is used. There is also stem change (umlaut) in the plural of some nouns.

Not surprisingly, there is a long tradition of exploring the morphosyntactic implications of homonymies within the German paradigms: to the extent that these homonymies are systematic, what do they reveal about relationships between cases? Early and recent representatives of this tradition are Bierwisch (1967) and Alexiadou and Müller (2008: 125–9). But such issues will not concern us here. This is not because homonymies within paradigms are unimportant (although I have some cautionary words in Chapter 8 on how revealing they are really likely to be). Rather, it is because in the context of this chapter what is important is the number of distinct inflection classes, not the relationship between forms within a single inflection class. And the proliferation of inflection classes in German seems likely to pose as serious a challenge as any to the approach being explored here,

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[14] Orthographic *e* here represents a central schwa vowel. In the suffix spelt *es* the schwa is usually optional, but because the [əs] pronunciation of it is available, this suffix must be distinguished from obligatorily schwa-less *s* [s].

Table 5.5. Inflection classes of German nouns

	A	B	C	D	E
Gen Sg	es	es	Ø	es	es
Nom Pl	e	Ø	e	**er**	**s**
Example	Tag	Ziegel	Hand	Mann	Uhu
	'day'	'brick'	'hand'	'man'	'owl'

	F	G	H	J	K
Gen Sg	Ø	en	Ø	es	**ens**
Nom Pl	s	en	en	en	en
Example	Mutti	Bär	Rose	Dorn	Name
	'mummy'	'bear'	'rose'	'thorn'	'name'

based on synonymy avoidance. The more classes there are, the harder it will be for a limited repertoire of affixes to achieve vocabular clarity, it would seem—or, at least, the more the brain will have to rely on differentiating factors of other kinds.

Not all the eight cells in the nominal paradigm are equally important in distinguishing inflection classes. In fact, all the inflectional behaviour of any German noun is predictable on the basis of its genitive singular and nominative plural forms (the nominative singular being always suffixless). So in Table 5.5 I indicate how these two cells are represented in each of the ten inflection classes that must at first sight be recognized as distinct.[15] As indicated in boldface, the only suffixes that display vocabular clarity here are nominative plural *er* in class D and *s* in class E, and genitive singular *ens* in class K. This does not look promising.

As in Latin, however, there is a strong link between some prima facie classes and gender. Classes C, F, and H contain only feminine nouns; the other classes contain no feminine nouns. Inspection will reveal that classes C, F, and H are suffixless in the genitive singular. In fact, all German feminine nouns are invariant and suffixless throughout the singular. On this basis, each of C, F, and H can be combined into a macroclass with a non-feminine counterpart on the basis of a shared nominative plural suffix, thus:

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[15] My discussion here draws heavily on in Carstairs-McCarthy (1994). The main differences are that: (a) I no longer seek to relate inflection-class distinctions directly to synonymy avoidance via a No Blur Principle, preferring rather to invoke multivocabulism and the constraints on it noted in Chapter 4; (b) I take account now of the analysis of *en* suggested by Carstairs-McCarthy (2008).

(27)

	AC	EF	GH
Gen Sg	es~Ø	es~Ø	en~Ø
Nom Pl	e	s	en

It turns out also that the suffixless plural form found in class B is phonologically determined: when the stem ends in schwa plus a sonorant consonant (/l, r, m, n/), the suffix *e* is absent. (In noun plurals, though not in adjective plurals, German avoids dactylic rhythms with a succession of two weak syllables at the end of the word.) Thus B can be combined with AC. Putting this information together, we can reduce the number of distinct classes and macroclasses to six, as in (28):

(28)

	ABC	D	EF	GH	J	K
Gen Sg	es~Ø	es	es~Ø	**en~Ø**	es	**ens**
Nom Pl	**e~Ø**	**er**	s	en	en	en

By this means, two additional suffixes achieve vocabular clarity: *e* in the nominative plural of ABC and *en* in the genitive singular of GH. But we still have *es* spread over no less than four classes in the singular, and *en* spread over three classes in the plural. Nor can it be said that any of these classes is vanishingly small, or negligible for any other reason. So it still looks as if a multivocabular approach to inflection-class behaviour is hard to reconcile with the proliferation of inflection classes for nouns in German. On the part of the two affixes that I have just mentioned we seem to observe vocabular blurring of a kind that does not occur with the neatly distinguished vocabularies of Javanese, Dyirbal, and Guugu Yimidhirr, nor in the linguistic usage of the multilingual Vaupés people.

Before we draw this pessimistic conclusion, however, it is worth recalling the cognitive function of multivocabulism, and then examining more closely a crucial difference between uncontroversial multivocabular situations and the inflection-class phenomena towards which I am advocating a multivocabular approach. In saying 'cognitive function', I am explicitly not referring to any social function. It is clear that Guwal and Dyalŋuy fulfil a social function among traditional Dyirbal speakers: the contrast between them underpins the kinship system that is central in Dyirbal culture. But that is not what is important here. As I pointed out in Chapter 4, there would be scope to use just the existing linguistic resources of Guwal and Dyalŋuy to distinguish more than just two groups within society (and likewise to use the existing linguistic resources of open-class Krama and Ngoko in Javanese to indicate more than two levels of respect). There could be a series of intermediate vocabulary steps between extreme Guwal and extreme Dyalŋuy, and

likewise between extreme Ngoko and extreme Krama, with each Dyalŋuy item extending to more or fewer intermediate stages in the direction of Guwal, and likewise for Krama and Ngoko. But this would mean that potentially synonymous items would no longer be differentiated in a clear fashion, by belonging to just one vocabulary.

Notice that the idea of having a graduated scale of respect or of avoidance status, rather than a two-way contrast, is by no means excluded on social or cultural grounds. Both Javanese and Guugu Yimidhirr speakers manage to express finer distinctions through their linguistic usage. But crucially they do so not through intermediate combinations of Krama-Ngoko or Dhabul-Nondhabul vocabulary; rather, they call upon independent vocabulary contrasts in different domains that intersect with the Krama-Ngoko and Dhabul-Nondhabul distinctions. In Javanese there is indeed a three-way contrast between Krama, Madya, and Ngoko terms within the domain of closed-class items—but even here, the Madya vocabulary is clearly distinct, not a mere selection of some Krama and some Ngoko items. So the brain appears to have a distaste for a kind of stepwise gradation that is perfectly imaginable and would be economical of vocabulary resources. Such stepwise gradation would fail to differentiate the vocabularies neatly, and that failure could well be seen by the brain as requiring toleration of cognitively repugnant synonymy.

With inflection classes, however, no potential issue of gradation arises. It is not that (say) Latin nouns of class A are at the terminus of a continuum with class K as the other terminus. And inflection classes do not fulfil social or cultural functions, such as showing respect for one's spouse's relatives, or affirming social class differences. So it is only that cognitive function of multivocabulism, rather than any social functions, that will be relevant in any application of it to inflection-class behaviour.

In the light of this, let us look again at the German behaviour as represented at (28). At (29) I list each of the affixes concerned along with its grammatical meaning (in terms of case and number) and, where appropriate, a differentiating factor in the form of an indication of the inflectional (macro)class (or 'vocabulary') to which it unequivocally belongs:

(29) en   Gen Sg   GH   e   Nom Pl   ABC
      ens Gen Sg   K   er   Nom Pl   D
      es   Gen Sg   s   Nom Pl   EF
                            en   Nom Pl

The suffix *es* in the genitive singular and *en* in the nominative plural have no differentiating factor because neither belongs to just a single inflectional class.

But does this matter, from the point of view of synonymy avoidance? It so happens that no other genitive singular suffix apart from *es* lacks a differentiating factor, and likewise no other nominative plural suffix apart from *en*. Synonymy among both the genitive singular and the nominative plural suffixes is therefore avoided, even though one suffix in each case violates vocabular clarity. Crucially, there is only one that does so. If a second genitive singular suffix apart from *es* were shared by more than one inflection class (or macroclass), then it would not be satisfactorily differentiated from *es*, and synonymy would not be avoided; and the same applies to nominative plural *en*. Carstairs-McCarthy (1994) calls such affixes 'class-defaults', and argues that only one class-default is permitted within any set of rival, or potentially synonymous, affixes.

A similar loophole does not seem to be available in the multivocabular situations of Javanese, Dyirbal, and Guugu Yimidhirr; there are no precisely analogous 'vocabulary-default' words. But this is due (I suggest) to the fact those distinct vocabularies are freighted with such heavy social meaning. A word may lie outside the domain in which multiple vocabularies apply (as many words do in Javanese, and as names of animal and plant species do in Guugu Yimidhirr, for example); but, apart from that, every word must be associated with a particular vocabulary unequivocally.[16] 'Mother-in-law language' and inflection class organization both comply with the no-synonymy expectation, I suggest, and the parallels between them are close. But the fact that the parallels are not exact is hardly surprising, given their very different niches in linguistic ecology.

Readers have, I hope, been persuaded that the diffusion of *en* over several classes in the nominative plural is not a problem, because it is the class-default plural suffix. There is another possible analysis for *en*, however, in terms of which it would be equally unproblematic. Let us suppose that *en* is a meaningless all-purpose inflectional suffix for German nouns and adjectives, used everywhere except where the bare stem or a more specific suffix is stipulated. If so, then the question of whether *en* in the nominative plural is synonymous with *e* or *er* or *s* does not arise. Because *en* has no meaning at all, it runs no risk of having the same meaning as anything else. This may

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[16] I would expect the situation to be different in communities that are not just multivocabular but multilingual, like the Vaupés people. Let us suppose that, in two of the languages in use among the Vaupés, the word for some concept happens to be the same. That is not unlikely, since some of their languages are genetically related. Does that cause cognitive discomfort? I suspect not, because within almost any utterance the languages would be clearly identifiable in other ways, grammatical and phonological as well as lexical.

sound bizarre. However, the idea of *en* as a meaningless suffix helps to tie together some otherwise random facts about the so-called 'weak' and 'mixed' declensions of nouns (classes GH and J at (28)) and the 'weak' declension of adjectives (Carstairs-McCarthy 2008). So this possibility suggests another way of looking at the apparent vocabular unclarity in the nominative plural suffixes—though not, admittedly, in the genitive singular ones.

A loose end remains: the role of stem vowel alternation. I mentioned earlier that some German nouns display umlaut in the plural: that is, a stem vowel *a*, *o*, *u*, or *au* changes to *ä* ([ɛ]), *ö* ([ø] or [œ]), *ü* ([y] or [ʏ]) or *äu* ([ɔʏ]). What happens if one counts a suffix accompanied by umlaut and the same suffix without umlaut as distinct for the purposes of allocating suffixes to inflection classes? I will not answer that question directly, but will instead give reasons in section 5.2.4 and in Chapter 6 for handling non-affixal inflection (such as umlaut) separately from affixal.[17] As for German, the connection between plural umlaut and plural inflection is equivocal. The distribution of umlauted plurals is as follows:

(a) all nouns in class D that have umlautable vowels (e.g. *Gott* 'god', plural *Götter*; *Wald* 'forest', plural *Wälder*);

(b) all feminine nouns in class ABC without exception (e.g. *Nacht* 'night', plural *Nächte*; *Kuh* 'cow', plural *Kühe*; *Maus* 'mouse', plural *Mäuse*);

(c) many non-feminine nouns in class ABC, including perhaps the majority of masculines in this class (e.g. *Gast* 'guest', plural *Gäste*, versus *Tag* 'day', plural *Tage*; *Fluss* 'river', plural *Flüsse* versus *Hund* 'dog', plural *Hunde*).

Thus, one cannot straightforwardly predict umlaut on the basis of suffix choice or vice versa. There are nouns in class D that do not have umlaut (for example *Bild* 'picture', plural *Bilder*), and there are many nouns that have umlaut but are not in class D. The most one can say confidently is that if a noun is feminine and umlauted it will be in class ABC—or, conversely, that if a noun is feminine and in class ABC it will be umlauted. It is as if, among feminines, affix choice and umlaut support each other. One cannot distinguish confidently between the signifier and what is signified. But let us suppose that, in its search for ways to avoid perfect synonymy, the brain is easily satisfied: any difference between potential

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[17] For some earlier discussion of the general point at issue here, see Carstairs (1988a), and for German nouns in particular, see Carstairs-McCarthy (2001b).

synonyms will do, provided the difference is sufficiently clear-cut. In that case, a universally clear distinction between signifier and signified is hardly to be expected. Rather, the differentiation can sometimes be mutual. The dog may chase its tail, as it were. If we expect language to be designed neatly for some identifiable purpose, that may seem surprising; but I have already in Chapter 1 cited evidence that evolution often produces untidy outcomes.

5.2.4 Case study 4: Italian verbs and the irrelevance of stem alternation

I said earlier that 'distinct forms of the same item' were likely to arise in protolanguage most often when a selective item appeared in clichés alongside a range of different unselective items with different phonological effects on it. In contemporary terms, these distinct forms would be like distinct stem alternants belonging to the same root, such as *sing*, *sang*, and *sung*. By contrast, unselective items, even when their divergent forms continued to resemble one another significantly, would lend themselves rather to treatment as 'distinct items' belonging to different vocabularies. After all, corresponding items in Javanese Krama and Ngoko, even when (as often) they resemble one another, still count as distinct for vocabular purposes.

If this is correct, then we will expect to find that affixal inflection-class systems (involving distinct vocabularies) can operate relatively independently of any patterns of alternation in the stems to which the affixes attach. According to the account being offered here, the brain handles the two phenomena differently. That is not to say that affixation never pays attention to stem alternation, or vice versa. We have already seen that, in affix choice, the paradigmatic factor of vocabulary membership can be overridden by a syntagmatic factor such as the phonological shape of a neighbouring item (as with Hungarian *ol* and *asz*); and what I have just said about the distribution of *e* and *er* as plural suffixes in German nouns suggests that another kind of syntagmatic factor may be a particular stem alternant. However, we should expect to find at least as often that the link between stem alternation and affix choice is weak or non-existent. I will illustrate that with some data from Italian.

Some common Italian verbs resemble English BE in that their present indicative forms show large-scale irregularity: for example, SAPERE 'know' and FARE 'do' as well as ESSERE 'be' itself. Let us put those aside and concentrate on verbs that are more or less regular. Among them, there are four patterns of stem alternation in the present indicative, indicated

schematically at (30) (where hatching or the lack of it differentiates stem alternants, and shading indicates stress on the stem):

(30)

		Pattern I	Pattern II	Pattern III	Pattern IV
Sg	1				
	2				
	3				
Pl	1				
	2				
	3				

Much the most common is pattern I, where the stem is unchanged throughout, whether stressed or unstressed. In pattern II, however, different stem alternants occur according to whether the stem is stressed or not. In pattern III, it is only the 1st person singular and the 3rd person plural that have a special alternant. Pattern IV, a combination of patterns II and III, involves three alternants: the 1st person singular and the 3rd person plural have a special alternant, while the other two stem-stressed forms have a different alternant. Here are illustrations of the four patterns (in ordinary Italian spelling, except that an acute accent is added to indicate stress):

(31)

		Pattern I: PARTIRE 'depart'	Pattern II: UDIRE 'hear'	Pattern III: SALIRE 'go up'	Pattern IV: MORIRE 'die'
Sg	1	párt o	ód o	sálg o	muói o
	2	párt i	ód i	sál i	muór i
	3	párt e	ód e	sál e	muór e
Pl	1	part iámo	ud iámo	sal iámo	mor iámo
	2	part íte	ud íte	sal íte	mor íte
	3	párt ono	ód ono	sálg ono	muói ono

Some of what I have labelled 'different alternants' in (31) are, admittedly, very similar to one another, especially *muor* ['mwɔːr] and *mor* [mor] for

MORIRE. An obvious conjecture is that they are really the same alternant, with diphthongization or the lack of it being a predictable phonological consequence of stress. But this will not work. The forms *ódo* ['ɔːdo], *ódi* and so on in the example for pattern II show stress without diphthongization; and a minimal pair such as *nuotiámo* 'we swim' versus *notiámo* 'we note' shows that the diphthong *uo* [wo] need not be stressed.[18]

The question now is: how, if at all, are these four alternation patterns associated with the three inflection classes that are relevant to affixal person-number inflection in Italian? The present indicative person-number suffixes for the three classes are given at (32):

(32)

		Class A: PARLARE 'speak'	Class B: TEMERE 'fear'	Class C: PARTIRE 'depart'
Sg	1	párl o	tém o	párt o
	2	párl i	tém i	párt i
	3	párl a	tém e	párt e
Pl	1	parl iámo	tem iámo	part iámo
	2	parl áte	tem éte	part íte
	3	párl ano	tém ono	párt ono

(There is widespread violation of vocabular clarity here; only 3rd person *a* and *ano* and 2nd person plural *ate*, *ete*, and *ite* identify their inflection class. But synonymy is still avoided, because every other affix complies with the expectation in section 5.2.3 that it should be the class-default for its paradigmatic cell.)

On the basis of (32) it is easy to check that all the verbs used to illustrate the four stem-alternation patterns at (31) belong to class C. Therefore knowing that a verb belongs to class C for the purposes of affixal inflection tells us nothing certain about its non-affixal inflectional behaviour. What about the other inflection classes? The full range of possible combinations of pattern and class is indicated at (33), together with verbs that exemplify those combinations that exist:[19]

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[18] The difference between the relatively long open vowel [ɔː] in *muoro* ['mwɔːro] 'I die' and the relatively short close vowel [o] in *moriamo* [moˈriaːmo] 'we die' and *nuotiamo* [nwoˈtiaːmo] 'we swim' is indeed a function of stress, but that does not affect the point being made here about diphthongization.

[19] The facts about Italian verbs that I rely on here can be confirmed in any reference grammar, such as Grandgent and Wilkins (1915).

(33)		Class A	Class B	Class C
	Pattern I	PARLARE 'speak' and many others	TEMERE 'fear', and others	PARTIRE 'depart' and several others
	Pattern II	SONARE 'play (music)' and a few others	SEDERE 'sit'	UDIRE 'hear'
	Pattern III	none	PORRE 'put' and several others	SALIRE 'go up' and a few others
	Pattern IV	none	TENERE 'hold'	MORIRE 'die', USCIRE 'go out', VENIRE 'come', and many verbs with stem extension -*isc*-

What is striking in (33) is that all but two of the twelve possibilities are instantiated. They are certainly not instantiated in equal profusion; for example, SEDERE, TENERE, and UDIRE are perhaps the only occupants of their respective boxes in the table. But this is to a large extent a by-product of the fact that only two pattern-class combinations are productive: the combination of Class A and Pattern I and, to a lesser extent, that of Class C and Pattern IV, inasmuch as new verbs are formed with the stem extension -*isc*- (about which more will be said in Chapter 6).

Italian thus illustrates little connection between how verbs are classified affixally and how their stems alternate in the present tense. At one level, this is a disappointment; in any inquiry one hopes to discover connectedness rather than randomness. On the other hand, this lack of a connection is just what we expect if, as proposed here, affixal inflection is organized by the brain in essentially the same way as multiple vocabularies are, with essentially the same constraints, whereas non-affixal inflection (such as stem alternation) is organized in some other way. But what is this other way? That question is the business of Chapter 6.

6

The ancestors of stem alternants

In the previous chapter we were concerned with the modern counterparts of unselective items in protolinguistic cliché patterns. But I illustrated how not only unselective but selective items could, through phonological processes, acquire more than one phonological shape. Thus with selective items too, phonological processes risked creating synonymies that our ancestors' brains would have found unpalatable. It is time now for me to suggest how the brain responded to that risk.

This will naturally involve looking at how stem alternants behave in contemporary languages. So far as the protolinguistic starting point is concerned, no precise analogy to multivocabulism is available as the basis for predictions. However, we already have some grounds for supposing that, in its search for differentiating factors, the brain is happy even with factors that serve no discernible purpose in mentally representing or communicating experience. (I have invoked the English words *addled* and *rancid* as stock illustrations.) So we should not be surprised if some stem alternations seem similarly pointless. Provided that the alternants are differentiated in an orderly and reliable way, the brain will be satisfied.

6.1 Differentiation by semantic or syntactic function

As I said in Chapter 1, there is a strong tradition, articulated forcefully by Pinker and Bloom (1990), according to which language is designed by natural selection primarily for communication. From that point of view, the most obvious way for a distinct stem alternant to distinguish itself from other distinct but similar alternants with which it competes is through having a clear-cut semantic or syntactic function. For example, in English the noun lexeme WIFE is represented by two stem alternants, *wife* [waif] and *wive-* [waiv]. They are differentiated syntactically, however, in that *wive-* appears in all and only syntactically plural contexts. The same applies to LIFE, PATH, HOUSE and a number of other common monosyllabic nouns whose singular form ends in a voiceless fricative. Similarly, with German noun lexemes that

have both an umlauted and a non-umlauted stem form, the umlauted one signals 'plural'.

A reasonable hypothesis to explore would be the hypothesis that, when language change has obscured the original phonological cause or basis of a stem alternation, either that alternation must disappear (one of the alternants supplanting the others) or else it must promptly acquire a new motivation of a syntactic or semantic nature. That has been one of the leading ideas of Natural Morphology (Dressler 1977, 1985; Dressler et al. 1987). For example, Dressler (1977: 13–14; 1985: 168–76) argues that the palatalization of velar consonants before front vowels in Italian ([k, g] → [tʃ, dʒ]), once automatic but now phonologically opaque, has acquired in nouns a clear-cut syntactic motivation: it signals plural number, as in *porco* ['pɔrko] 'pig', plural *porci* ['pɔrtʃi], *psicologo* 'psychologist', plural *psicologi* [psi'kɔlodʒi]. This is not to say that a noun whose stem ends in [k] or [g] is bound to have an alternant ending in [tʃ] or [dʒ] respectively; merely that, if such an alternant exists, it will be used precisely in the plural. In this respect, Italian velar palatalization resembles fricative voicing in English. There are plenty of fricative-final nouns which lack a voiced-fricative alternant (for example, FIFE, CLOTH, FACE); it is just that, if a noun has such an alternant, its association with the plural is secure. (There is indeed a plural noun *clothes*, with a voiced [ð]; but in modern English it is not the plural of CLOTH.)

Because this kind of differentiation prevents synonymy in a straightforward way, it complies with the brain's cognitive expectations neatly. In the context of this book, there is nothing more to say about it, therefore. Unfortunately for the Natural Morphologists' expectation, however, there are innumerable instances of stem alternation that do not behave so conveniently. We have already seen instances in section 5.2.4, where we discussed the relationship (or lack of it) between affixal inflection and stem alternation in the present indicative of Italian verbs. Verbs complying with Pattern III and Pattern IV have a special alternant in the 1st singular present indicative—but not only there, because the same alternant shows up in the 3rd plural. So, if this special alternant has a syntactic or semantic function, it is not clear-cut. There are plenty of proposals to analyse certain combinations of grammatical persons as constituting natural classes: for example, with binary features [±1st] and [±2nd], we will predict (accurately or not) that the second and third persons constitute a natural class through sharing the value [−1st], that the inclusive 1st person plural (found in many Austronesian languages, for example) is naturally specifiable as [+1st, +2nd], and so on. This teasing apart of inflectional properties into binary features has been commonplace for over four decades. Yet no straightforward combination of features and values, whether

binary or non-binary, identifies precisely the 1st person singular and the 3rd person plural.

It follows that, if the special stem alternant found in precisely those two cells in Patterns III and IV is to be distinguished satisfactorily from the other present indicative alternant (or alternants), it cannot be in terms of syntax or semantics. How it is in fact distinguished will be discussed in section 6.3. But before we turn to that, it will be helpful to get out of the way in section 6.2 a different kind of distinguishing factor that is also exemplified in the Italian data of section 5.2.4.

6.2 Differentiation by syntagmatic phonological factors, and a note on suppletion

Pattern II in section 5.2.4 is a pattern of stem distribution whose description requires reference only to phonology. And, since Pattern IV is a combination of Patterns II and III, it follows that phonology makes sense of at least part of Pattern IV. Pattern II assigns one stem alternant to forms in which the stem is stressed, and another alternant to forms where the suffix is stressed. I call this a syntagmatic factor because, in most Italian verb forms, whether it is the stem or the suffix that is stressed depends on the suffix: some suffixes are always stressed, some never.[1]

In the present indicative, the suffixes that are always stressed are those of the 1st plural (always *iámo*) and the 2nd plural (*áte*, *éte* or *íte*, according to inflection class). Therefore in the present indicative of the verb UDIRE, used to illustrate Pattern II at (31) in Chapter 5, it is precisely in the 1st and 2nd plural that the unstressed alternant *ud* [ud] appears.

I have already made the point that, despite the phonological similarity between the alternants *ud* and *ód*, and and still more between *son* and *suón* from SONARE 'to play' and between *sed* and *siéd* from SEDERE 'to sit', one cannot unhesitatingly posit a single underlying phonological representation for both surface shapes. The reason I gave was that the vowels or diphthongs that are stressed in these forms can appear unstressed elsewhere, and vice versa, and I cited the minimal pair NOTARE 'to note' and NUOTARE 'to swim'. As regards the *e*∼*ie* alternation, relevantly contrasting examples are VIETARE

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[1] I say 'in most forms', not 'in all forms', because there are some verbs of Class B where in the past definite tense the stem too plays a role in determining the position of the stress: for example, the past definite stems *conóbb-* 'knew', *prés-* 'took', and *rúpp-* 'broke' (corresponding to the normal stems *conosc-*, *prend-*, and *romp-*) are always stressed (*conóbbi* 'I knew' etc.), and consequently cannot be used with the stressed 1st and 2nd plural suffixes of the past definite, -*émmo* and -*éste*: hence *conoscémmo* 'we knew', not *conóbbémmo*.

'to forbid' with *ie* throughout and CHETARE 'to quieten' with *e* [e] through-
out. Admittedly, LEVARE 'to lift' has a stem alternant ['lɛv] when stressed, as
in *lévo* 'I lift', and another alternant [lev] when unstressed, as in *leviámo* 'we
lift'. But this alternation between open [ɛ] and close [e] is genuinely phono-
logical: the seven-vowel system, with contrasting open and close mid vowels,
that Italian manifests in stressed syllables (as in *levo* ['lɛːvo] 'I lift' versus
cheto ['keːto] 'I quieten'), condenses into a five-vowel system in unstressed
syllables.[2]

In support of multiple underlying phonological representations here,
I could have added that the alternation *ud∼od* is unique in Italian. This
pair is thus suppletive, so far as the vowel is concerned.[3] What would add
yet further weight to my argument is the discovery of a pair of stem alternants
that bear no phonological resemblance to one another at all (that are, in other
words, grossly suppletive) but whose distribution is still determined purely on
the basis of whether they are accompanied by a stressed or an unstressed
suffix. And one such example can be found, as illustrated in (1), where I cite
subjunctive as well as indicative forms:

(1)　　　ANDARE 'to go'

		indicative	subjunctive
Sg	1	vád o	vád a
	2	vái	vád a
	3	vá	vád a
Pl	1	and iámo	and iámo
	2	and áte	and iáte
	3	vánno	vád ano

I have not indicated any boundary between stem and suffix in the indicative
2nd and 3rd persons singular and the 3rd plural, because determining such a
boundary is problematic. (I will return to this point.) But in front of
the stressed suffixes in the 1st and 2nd plural there is a clear boundary, and
the stem alternant here (*and*) is quite different from anything we observe
in the other four present-tense forms. In fact, the alternant *and* shows up
consistently wherever an unstressed stem alternant is required, which happens

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[2] The vowel length indicated in these two forms is also phonologically predictable: stressed vowels
are long in open syllables.

[3] Non-linguist readers may not have come across the term 'suppletive'. Suppletion is the phenom-
enon illustrated by English *go* versus *went* and *bad* versus *worse*. These pairs of word forms are clearly
related inflectionally in the same way as *wait* is to *waited* and *slow* is to *slower,* and innumerable other
paired examples; however, they are unusual in that their stems are quite different.

to be everywhere outside the present tense (e.g. *andái* 'I went', *andávo* 'I was going', *andrò* 'I will go').

The verb ANDARE thus supplies a clear instance of what I have called 'phonologically conditioned suppletion' (Carstairs 1988c, 1990; see also Mascaró 2007). What is going on here is essentially the same as with *asz* and *ol* as suffixes competing in Hungarian to express the 2nd person singular. The relationship between *asz* and *ol* is not usually classified as suppletive, however, any more than that between *i:* and *is* as rival genitive singular suffixes in Latin, or that between *er* and *e* as rival nominative plural suffixes in German. The only reason for this traditional distinction in usage seems to be that the complete phonological dissimilarity between *asz* and *ol* as rival affixes is usual—similar examples abound—whereas the dissimilarity between *vad* and *and* as rival stem alternants is unusual, and it is only the unusual phenomenon for which a special label has been thought necessary.

Are we then to say that *vad* and *and* in (1) are also 'different forms of the same item', despite their complete lack of any phonological resemblance? At first sight, the safe answer would seem to be no. Recall that, according to my protolanguage-based account, 'different forms of the same item' became something that the brain had to learn to cope with as soon as phonological processes began to yield items that were phonologically similar and apparently synonymous, yet whose phonological relationship was no longer transparent. Surely it is implausible to suppose that whatever mechanisms the brain devised to handle such similar-but-different items could also be applied to relationships between items that are entirely different (one may think).

That argument becomes less convincing, however, when one notices that the way in which suppletive alternants function within paradigms is exactly the same as the way in which non-suppletive alternants do. It therefore seems reasonable to suppose that, although phonological similarity between 'forms of the same item' is what the brain expects, and is indeed the protolinguistic basis for this pattern of linguistic and cognitive organization, this pattern has become sufficiently robust and firmly established that phonological similarity is no longer something that the brain insists on. If so, the brain can store *vad* and *and* as different stems for ANDARE in just the same way as it stores *muoi, muor*, and *mor* as different stems for MORIRE.[4]

We have thus arrived at what may seem like the converse of our position with regard to similar-but-different affixes in Hungarian in section 5.2.1.

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[4] In section 6.3.5 I will describe a situation in Polish where certain nouns are generally expected to have more than one phonological shape, so that the few that do not must be marked lexically as exceptional.

I argued there that the similarity between back-vowel and front-vowel affixes with the same meaning was for various reasons not a sufficient basis to deny their distinctness (and thus to deny the brain's need to do something about their apparent synonymy). I am arguing here that the radical phonological difference between *vad* and *and* is not a sufficient reason to deny their status as distinct forms of the same item (in contemporary terms, stem forms of the lexeme ANDARE). But the two situations are not exactly converse. Distinct forms are still distinct, whether they are 'forms of the same item' or not; and, if they are apparently synonymous, this situation still has to be remedied. In the case of *vad* and *and*, it is a syntagmatic phonological factor that comes to the rescue; but in principle we will expect that any kind of differentiating factor that may apply to non-suppletive alternants may apply also to suppletive ones.

I said that I would return to the issue of how to handle the forms *vái*, *vá*, and *vánno* in (1). This can be taken care of briskly. What is unusual about these three forms is that they have no clear stem–affix boundary. But from the point of view of synonymy avoidance, this is an advantage, not a drawback. They are distinct forms each of which has a clear morphosyntactic function: 2nd singular, 3rd singular, and 3rd plural respectively. Therefore the question of differentiating their stems does not arise. In respect of their lack of morphosyntactic ambiguity, these whole word forms resemble the stems discussed in section 6.1.

There is a paradoxical moral here. Synonymy risks involving stem alternants do not arise if either there is only one stem alternant, or if the stem is fused with the suffix in such a way as to yield a unique form in which no stem alternant is discernible. The only patterns that are cognitively risky are those where there is more than one alternant and where stem–affix boundaries are clear. Yet patterns like this are extremely common in languages worldwide. The next section will seek to account for this paradox.

6.3 Differentiation by paradigmatic predictability

6.3.1 *Case study 5: Italian verbs: Stem alternants with uniform distribution*

At (30) in Chapter 5, I illustrated the four stem-alternation patterns (labelled I–IV) found in the present indicative of Italian verbs. At (1) in this chapter I illustrated ANDARE, a verb that conforms to pattern II except in the three fused forms *vái*, *vá*, and *vánno*. I went beyond Chapter 5, however, in supplying forms for the present subjunctive as well as the indicative. It remains to show how verbs of patterns III and IV behave in the subjunctive. I give a complete schematic representation in Table 6.1, and some illustrations

Table 6.1. Stem alternation patterns in Italian verbs

		Pattern I	Pattern II	Pattern III	Pattern IV
Present Indicative					
Sg	1	▨ gray	▥ diagonal	▮ vertical	▮ vertical
	2	▨ gray	▥ diagonal	▨ gray	▥ diagonal
	3	▨ gray	▥ diagonal	▨ gray	▥ diagonal
Pl	1				
	2				
	3	▨ gray	▥ diagonal	▮ vertical	▮ vertical
Present Subjunctive					
Sg	1	▨ gray	▥ diagonal	▮ vertical	▮ vertical
	2	▨ gray	▥ diagonal	▮ vertical	▮ vertical
	3	▨ gray	▥ diagonal	▮ vertical	▮ vertical
Pl	1				
	2				
	3	▨ gray	▥ diagonal	▮ vertical	▮ vertical

in Table 6.2. What this shows is that, in patterns III and IV, the special stem alternant of the 1st singular and the 3rd plural indicative occupies all the four subjunctive cells in which the stem is stressed, trumping (so to speak) the ordinary stressed-stem alternant (if there is one). But what is the basis for the distribution of the special alternant? What differentiates it from the other alternants for synonymy-avoidance purposes?

A sceptical reader may be inclined to answer: 'Nothing—but that does not matter. What we observe in Table 6.2 is just a set of by-products of phonological change. They have no general theoretical interest.' This amounts to claiming that stem distributions such as these are purely peripheral facts, in the sense of Chomsky's 'core-periphery' distinction (1986: 147): accidental

Table 6.2. Examples of some Italian verb stem alternation patterns

		Pattern I: PARTIRE 'depart'	Pattern II: UDIRE 'hear'	Pattern III: SALIRE 'go up'	Pattern IV: MORIRE 'die'
Present Indicative					
Sg	1	párt o	ód o	sálg o	muói o
	2	párt i	ód i	sál i	muór i
	3	párt e	ód e	sál e	muór e
Pl	1	part iámo	ud iámo	sal iámo	mor iámo
	2	part íte	ud íte	sal íte	mor íte
	3	párt ono	ód ono	sálg ono	muói ono
Present Subjunctive					
Sg		párt a	ód a	sálg a	muói a
		párt a	ód a	sálg a	muói a
		párt a	ód a	sálg a	muói a
Pl		part iámo	ud iámo	sal iámo	mor iámo
		part iáte	ud iáte	sal iáte	mor iáte
		párt ano	ód ano	sálg ano	muói ano

left-overs of change, learned by speakers purely on the basis of positive evidence in their linguistic environment, and subject to no interesting general linguistic constraints.

It is certainly true that phonological changes provide a historical explanation for some of the contrasting stem shapes in modern Italian. The Late Latin ancestors of 1st singular indicative *salgo* and *muoio* in Table 6.2 were *salio* and *morio*, pronounced something like ['saljo] and ['mɔrjo], and the modern shapes are understandable as incorporating reflexes of the glide [j]. (In *salgo*, [j] has been strengthened to a velar obstruent.) However, phonological factors of this kind do not account for all instances of patterns III and IV. Let us consider the Latin verb FUGIO 'flee' in its present indicative and subjunctive forms (in normal orthography):

(2)   Present   Present
           Indicative   Subjunctive
  Sg 1 fugio   fugiam
      2 fugis   fugias
      3 fugit   fugiat

  Pl 1 fugimus   fugiamus
      2 fugitis   fugiatis
      3 fugiunt   fugiant

I have not indicated a stem–affix boundary here, because it is not entirely clear-cut: does the *i* belong to the stem or the suffix? There is even a tradition in generative phonology and morphology, dating back to Lieber (1980), of treating the *i* here as an underlying stem-final glide /j/ that is vocalized between two consonants. But the merits of that view do not matter here. The important point is that the velar consonant /g/ is followed in all these forms by a high front non-consonantal sound of the kind that regularly triggered palatalization, so as to yield eventually Italian [ddʒ]. So the predicted Italian outcome seems obvious: the Italian counterparts of all twelve forms in (2) should contain just one stem alternant, namely [fuddʒ]. But this prediction is incorrect. The actual Italian forms are given at (3):

(3)

		Present Indicative		Present Subjunctive	
Sg	1	fuggo	['fuggo]	fugga	['fugga]
	2	fuggi	['fuddʒi]	fugga	['fugga]
	3	fugge	['fuddʒe]	fugga	['fugga]
Pl	1	fuggiamo	[fud'dʒaːmo]	fuggiamo	[fud'dʒaːmo]
	2	fuggite	[fud'dʒiːte]	fuggiate	[fud'dʒaːte]
	3	fuggono	['fuggono]	fuggano	['fuggano]

So FUGGIRE, the modern descendant of FUGIO, has somehow acquired a stem alternant [fugg] for which there is no historical phonological motivation, and distributes it alongside the 'regular' [fuddʒ] precisely according to pattern III!

This is not an isolated case. Maiden (1992, 2005) gives numerous examples in Italian and other Romance languages of a kind of productive irregularity in verb stems—as he puts it, an 'apparent iconoclastic impulse to destroy invariance and complicate allomorphy' (1992: 301). But the fact that phenomenon is productive shows that the brain finds it relatively easy to handle. So what does the brain latch on to?

Let us use the label '1st-singular-plus' for the special alternant which, in patterns III and IV, appears in the present indicative 1st singular and elsewhere. A common factor links the 1st-singular-plus form in all modern Italian verbs: it appears in precisely the same present-tense forms, namely the indicative 1st singular and 3rd plural, and the present subjunctive singular and 3rd plural. This uniformity of distribution is striking. Even if these verbs are counted as irregular because they do not conform to the single-stem pattern I, their irregularity is of a remarkably regular kind. What's more, this regularity of distribution has nothing to do with the actual shapes of the alternants in a particular verb. Two of the 1st-singular-plus alternants so far noted do end with [g] (*salg, fugg*) and the same is true of *teng, veng*, and *pong*

Table 6.3. Three Italian verbs, illustrating stem alternations

		Pattern III: CRESCERE 'grow'	Pattern IV: USCIRE 'go out'	Pattern IV: FINIRE 'finish'
Present Indicative				
Sg	1	crésc o ['kresko]	ésc o ['ɛsko]	fin ísc o [fi'nisko]
	2	crésc i ['kreʃʃi]	ésc i ['ɛʃʃi]	fin ísc i [fi'niʃʃi]
	3	crésc e ['kreʃʃe]	ésc e ['ɛʃʃe]	fin ísc e [fi'niʃʃe]
Pl	1	cresc iámo [kreʃ'ʃaːmo]	usc iámo [uʃ'ʃaːmo]	fin iámo [fin'jaːmo]
	2	cresc éte [kreʃ'ʃeːte]	usc íte [uʃ'ʃiːte]	fin íte [fin'iːte]
	3	crésc ono ['kreskono]	ésc ono [ɛs'kono]	fin ísc ono [fi'niskono]
Present Subjunctive				
Sg	all	crésc a ['kreska]	ésc a ['ɛska]	fin ísc a [fi'niska]
Pl	1	cresc iámo [kreʃ'ʃaːmo]	usc iámo [uʃ'ʃaːmo]	fin iámo [fin'jaːmo]
	2	cresc iáte [kreʃ'ʃaːte]	usc iáte [uʃ'ʃaːte]	fin iáte [fin'jaːte]
	3	crésc ano ['kreskano]	ésc ano ['ɛskano]	fin ísc ano [fi'niskano]

corresponding to the verbs TENERE, VENIRE, and PORRE that we noted at (33) in Chapter 5; but their non-1st-singular counterparts display a variety of different shapes. Moreover, there is no [ɡ] in the threesome presented in Table 6.3, whose similarities and differences deserve comment.

The verb CRESCERE is descended directly from Latin CRESCERE, which originally had a uniform stem [kresk]. The Italian alternant [kreʃʃ] is in historical phonological terms the outcome of palatalization, just like [fudd ʒ]. However, the alternation pattern exhibited in Table 6.3 cannot be a purely phonological development, because, in the subjunctive 1st and 2nd plural, phonological change alone would have yielded *[kres'kaːmo] and *[kres'kaːte] from original Latin *crescamus* and *crescatis*. What has happened is that, in Italian, CRESCERE has slipped into compliance with the productively 'irregular' pattern III. The verb USCIRE observes pattern IV, in that outside the 1st-singular-plus forms (with stem *esc* [ɛsk]) it has not one stem alternant but

two: *esc* [ɛʃʃ] when stressed, and the semi-suppletive *usc* [uʃʃ] when unstressed. And FINIRE—representative of a large and moderately productive group, including a number of verbs formed from adjectives such as INGIAL-LIRE 'turn yellow', ARROSSIRE 'blush'—belongs to pattern IV too, but in a novel way: the root *fin* is never stressed, so, in forms where the stem is required to bear the stress, a stem extension *isc* is added (phonetically [isk] in 1st-singular-plus contexts and [iʃʃ] elsewhere). The sole function of *isc*, one may say, is to protect the root from having to carry the stress. But, again, this is not a matter of respecting any independently motivated characteristic of the root but rather of slipping into compliance with an independently existing stem-alternation pattern. The roots *usci-*, *fin-*, *giall-*, and *ross-* are all perfectly capable of bearing stress outside the context of verbal inflection, as in the nouns *úscio* 'doorway' and *fíne* 'end', and the adjectives *giállo* 'yellow' and *rósso* 'red'.[5]

One may be tempted to guess that [isk] and [iʃʃ] preserve even in contemporary Italian the same underlying representation (presumably /isk/), diverging on the basis of the phonological context: [iʃʃ] before front vowels [i] and [e], [isk] before back vowels [a] and [o]. But although this reflects pretty well the historical origin of these alternants, it cannot be made to work without awkward stipulations, and it obscures a generalization. The stipulations would be needed because the /sk/ cluster in many words such as DISCO 'disc', FRESCA 'fresh', and PESCARE 'to fish' would have to be protected from palatalization in forms such as *dischi* ['diski] 'discs', *fresche* ['freske] 'fresh (feminine plural)', and *peschi* ['peski] '(you) are fishing'. (Indeed, it was pointed out in section 6.1 that the palatalization in a few noun plural forms such *porci* ['pɔrtʃi] 'pigs', corresponding to singular *porco* ['pɔrko], is now phonologically opaque, and the two stems are differentiated on the basis of number.) And the generalization that this analysis obscures is the exact parallel in distribution between [isk]~[iʃʃ] on the one hand and, on the other, [mwɔi]~[mwɔr] from MORIRE and [vɛŋg]~[vjɛn] from VENIRE, for example—even though neither of these latter distributions can be attributed to a synchronic process of palatalization before [i] and [e]. So it really is morphology that is in operation here, not phonology.[6]

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[5] The acute accents here draw attention to the position of the stress. They are not part of normal Italian spelling.

[6] Vogel (1993) argues for a treatment of Italian verbs in which a considerably larger role is attributed to phonology in accounting for inflectional stem alternations. She argues that to posit two or more distinct stems for whole classes of verbs, rather than just individual suppletive verbs such as ANDARE,

It is high time now to tackle the serious question that these Italian data pose for synonymy avoidance. How does the brain handle the 1st-singular-plus stem alternants that crop up in patterns III and IV? They are always stressed, yet, because they compete with another alternant that can (or must) also be stressed, the syntagmatic factor or stress cannot be the basis of their differentiation. What is there for the brain to latch on to, then?

I suggest that what the brain latches on to is the distributional uniformity that I have already drawn attention to. It is not that (for example) some 1st-singular-plus stems are used in the 3rd plural subjunctive while others are not, some are used in the singular subjunctive while others are not, and so on, in a seemingly unpredictable fashion. Rather, once an Italian speaker's brain has decided that a particular alternant of a particular verb belongs in the 1st-singular-plus category, all questions about the distribution of that alternant within that verb's paradigm are answered. Crudely, one can say that the extra 'meaning' of [mwɔi] 'die' and [vɛŋg] 'come', for example— the factor that differentiates them from [mwɔr] and [vjɛn]—is their own reliably predictable distribution. I say 'crudely' because I do not seriously suggest that the paradigmatic cells in which [mwɔi] appears are part of what a semanticist would recognize as the meaning of MORIRE. But using the word 'meaning' like this is a way of emphasizing that semantic factors are not the only kind of factor that can serve to differentiate potential synonyms. We have already encountered syntagmatic factors such as collocational restrictions and phonological context, and one paradigmatic factor, namely multivocabulism. Here is another sort of paradigmatic factor relating to stem alternation.[7]

I said just now: 'once an Italian speaker's brain has decided that a particular alternant of a particular verb belongs in the 1st-singular-plus category...'. But how does the brain make such decisions? If that question is interpreted as being about the stages of Italian children's inflectional mastery and the kinds of mistake they make during it, I cannot answer it. On the other hand, if it is interpreted as being about the logical structure of the decisions involved, it is not hard to answer. The structure of the decision tree is like this:

'would result in . . . the loss of the ability to generalize across lexical items' (1993: 225). But I will suggest later, in relation to Polish, that this kind of argument is weak. Also, Vogel does not consider the parallels in stem distribution (but not stem shape!) exhibited by, for example, USCIRE, MORIRE, VENIRE, and FINIRE, and consequently does not notice the generalizations whose capture depends on treating all the stems concerned as distinct for morphological purposes.
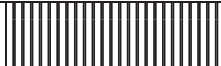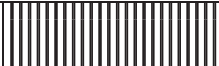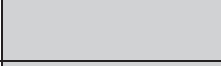
[7] Maiden (2005) likewise emphasizes the 'coherence' of stem-alternation patterns, not just in Italian but in many Romance languages and dialects. An innovation that affects a particular stem alternant is overwhelmingly likely to affect at the same time all the forms with that alternant.

(4)   If a verb in the present tense (indicative and subjunctive) has:

one stem alternant, then it belongs to	pattern I;
three stem alternants, ...	pattern IV;
two stem alternants, then	
if only one is stressable, ...	pattern II;
if both are stressable, ...	pattern III.

As regards verbs with two stem alternants, notice how the syntagmatic factor of stress cooperates with the paradigmatic factor of stem diversity in such a way as to allocate each verb unambiguously to one of the available distribution patterns. Thus not much positive evidence is needed in order for the child's brain to sort out the stem behaviour of any verb. In particular, no evidence is needed about which alternant is used in any specific cell in the paradigm.

This analysis implies a claim that many varieties of Italian that might conceivably exist not only do not but could not exist. Here is one such variety. In this particular pseudo-Italian there are four stem-distribution patterns, just as in actual Italian. Patterns II and IV are absent, however. Instead we have pattern I (with only one stem form) and, alongside pattern III, two new patterns: one just like pattern III except in the 3rd plural subjunctive (let's call it pattern IIIa), and a further pattern just like pattern III except in the singular subjunctive (let's call it pattern IIIb). This state of affairs is illustrated at (5):

(5)

		Pattern III	Pattern IIIa	Pattern IIIb
Present Indicative				
Sg	1	▨	▨	▨
	2	░	░	░
	3	░	░	░
Pl	1			
	2			
	3	▨	▨	▨

(5) (*Continued*)		Pattern III	Pattern IIIa	Pattern IIIb
Present Subjunctive				
Sg	1	▓▓▓	▓▓▓	░░░
	2	▓▓▓	▓▓▓	░░░
	3	▓▓▓	▓▓▓	░░░
Pl	1			
	2			
	3	▓▓▓	░░░	▓▓▓

In this language, if a child learns that (for example) a verb uses a different stem in the 1st singular indicative from the 3rd singular indicative, her brain can decide confidently that that verb does not belong to pattern I, but it cannot decide which of the three patterns III, IIIa, and IIIb it does belong to. I am bound to predict, then, that, if phonological change risks engendering a set of patterns such as in (5), the brain must intervene and enforce deviations from what on a purely phonological basis one would be led to expect. These deviations will be of the kind traditionally lumped together as 'analogical'. However, a prediction based on the need for uniform distribution of stem alternants will be more precise than traditional explanations in terms of 'proportional analogy' or 'levelling'.

   Can a claim as strong and precise as this really be correct? Certainly, if it is false, that fact should be easy to demonstrate. I will in fact argue in section 6.3.3 and subsequently that synonymy avoidance by paradigmatic predictability may not always require paradigm uniformity, even though the kinds of non-uniform distribution that are permitted would still exclude (5). I will develop this argument by reference to Russian, German, and Polish. First, however, in section 6.3.2 I will introduce stem-alternation data from Dhaasanac that reinforce suggestions that I have made on the basis of Italian.

6.3.2 *Case study 6: Dhaasanac: Another instance of uniform distribution*

Baerman et al. (2005), in their survey of systematic inflectional homonymy across a variety of languages, do not discuss the Italian stem alternations that

we have just been looking at. However, they describe a situation in the Cushitic language Dhaasanac (citing Tosco (2001)) which is remarkably similar. In four Dhaasanac tenses there are two verb stem alternants, labelled 'A' and 'B', which are distributed among persons and numbers as in (6):

(6)

	Singular	Plural
1st inclusive[8]	—	A
1st	A	B
2nd	B	B
3rd feminine	B	A
3rd masculine	A	A

However, there is no consistent phonological relationship between A stems and B stems. This is illustrated in (7) (Baerman et al. (2005: 106), drawn from Tosco (2001: 123–206)):

(7)

	stem A	stem B
'fall down (perfective)'	leeði	leeti
'die (perfective)'	kufi	kuyyi
'migrate (imperfective)'	ɡuurma	ɡuuranna
'cough (perfective)'	ʔuufumi	ʔuufeeni
'walk (perfective)'	seð	sieti
'kill (perfective)'	yes	ces

Baerman et al. (2005: 169) call this an 'unnatural result' of phonological and morphological change. By 'unnatural' what they mean is: '[The] combination of values [for stem B] cannot plausibly be argued to have any basis in the structure of person or number'. This is certainly correct. Neither stem A nor stem B correlates neatly with any syntactic function or meaning, in the way that (say), in German nouns, umlauted and non-umlauted stems correlate with the plural-singular contrast. However, to call the Dhaasanac state of affairs 'unnatural' presupposes that a neat correlation with syntax or semantics is the only 'natural' technique available to the brain for differentiating potentially synonymous forms of the same item. It is true that syntax and semantics are grounded outside the morphological system, so differentiation by reference to them is less likely to be obscured or disrupted by phonological changes than is differentiation by reference to the paradigmatic factor of distributional uniformity. It is also true that some syntactic and semantic contrasts (such as 'singular' versus 'plural', 'past' versus 'present', 'animate' versus 'inanimate')

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[8] '1st inclusive plural' means 'I and you', as opposed to '1st plural', which means 'I and s/he or they (not you)'.

are exploited morphologically over and over again in languages around the world, whereas stem distribution patterns such as in Table 6.1 and example (6) are inherently language-particular. But that does not mean that distributional uniformity is 'unnatural' as a morphological phenomenon.

I said just now that certain contrasts are exploited morphologically in many languages. It would have been more usual to use the word 'expressed' rather than 'exploited' here. But I chose the latter word deliberately. If the approach to morphological evolution presented here is on the right lines, then the relationship between morphological form and content is a more equal one than it is usually thought to be. It is not enough to say that formal contrasts (such as between stem alternants) may serve to express syntactic or semantic distinctions. One must supplement this by saying that syntactic or semantic distinctions may serve to differentiate any two or more items (or forms of the same item) that risk being otherwise perfectly synonymous. Morphological and extramorphological aspects of a language often support each other in this way, and grammatical arrangements of this kind are likely to be relatively robust and long-lasting. But sometimes morphology has to draw upon its own resources (so to speak). Multivocabulism is one such resource; distributional uniformity (I suggest) is another.

For Baerman and his colleagues, who are interested in 'the syntax-morphology interface' (as the subtitle of their book implies), what is centrally important about the stems A and B in Dhaasanac is how they are linked up with the appropriate syntactic features. They achieve this link-up within their Network Morphology framework through stipulating that stem B is associated with 2nd-person forms, 3rd singular feminine forms, and exclusive 1st plural forms, while stem A is the default stem, used elsewhere. In their notation, this is expressed as follows (2005: 185):

```
(8)  VERB:
     <index>==_A
     <index2nd>==_B
     <index3rdsgf>==_B
     <index1st_exclpl>==_B
```

The labels 'A' and 'B' are stems 'indexes', telling us where contrasting stem alternants appear in the paradigm but saying nothing directly about their shape. As Baerman et al. put it (2005: 186): 'by treating the inventory of forms available as separate from the associated morphosyntax, it is possible to capture the morphological systematicity of the Dhaasanac verb.'

In the light of the previous section on Italian, however, there is room to wonder whether this approach captures that morphological systematicity

entirely. Just as we considered a variety of pseudo-Italian, let us now consider a pseudo-Dhaasanac. In this language, the A and B stems are distributed in pattern I as in actual Dhaasanac, but there are also some verbs that conform to a pattern II. There is no basis for associating either of the alternants in pattern II with A or B, so let us give them new labels (or indexes), C and D, as in (9):

(9)

		Pattern I	Pattern II
Singular	1st	A	C
	2nd	B	C
	3rd fem	B	D
	3rd masc	A	D
Plural	1st incl	A	C
	1st	B	C
	2nd	B	D
	3rd fem	A	D
	3rd masc	A	C

I have contrived pattern II so that the D stem occupies cells that are one notch lower down (so to speak) than those occupied by the B cells. The distribution of the C and D stems is still 'unnatural' in syntactic terms, but its unnaturalness is different from that of pattern I.

Now, how could such a pseudo-Dhaasanac be handled in Baerman's[9] Network Morphology? The answer is: quite easily. Let us assume that the pattern I verbs and the pattern II verbs are arbitrary classes, whose membership is not predictable on any extramorphological grounds (syntactic, semantic, or morphological). A precedent for describing such arbitrary classes is supplied by Baerman's treatment of Russian nominal inflection, where four nominal inflection classes are represented in their notation as 'N_I', 'N_II', 'N_III', and 'N_IV' (2005: 205). Therefore in order to represent pseudo-Dhaasanac in Baerman's style, all that is necessary is to amend (8) so as to distinguish two classes of verbs, 'V_I' and 'V_II', as in (10):

```
(10)   VERB:
         V_I:
             <index>==_A
             <index2nd>==_B
             <index3rdsgf>==_B
             <index1st_exclpl>==_B
```

---

[9] I will henceforth use 'Baerman' as shorthand for 'Baerman et al. (2005)'.

```
V_II:
     <index>==_C
     <index3rdsg>==_D
     <index2ndpl>==_D
     <index3rdplf>==_D
```

(10) is more complex than (8), reflecting the fact that pseudo-Dhaasanac is a more complex language than actual Dhaasanac. But there is nothing in Baerman's Network Morphology framework that would rule pseudo-Dhaasanac out as a possible language.

Let us suppose, however, that, when stem alternations lack syntactic or semantic correlates (so as to be, in Baerman's terms, 'unnatural'), the way in which the alternants are differentiated for synonymy-avoidance purposes is through distributional uniformity. Then the situation illustrated at (9) shows that pseudo-Dhaasanac is not a possible language, because distributional uniformity is violated—unless, that is, pattern II has some non-paradigmatic motivation: for example, if stem C appears only before vowels and stem D before consonants. But if such motivation is lacking, then the brain of a Dhaasanac-learning child, just on the basis of knowing that a verb has two stem alternants, will not be able to decide which of patterns I or II to assign that verb to.

The number of stem-distribution patterns that are mathematically available for any verb with two stem alternants depends on the number of cells in the paradigm; but, even with the small fragments of paradigms that we have been looking at in Dhaasanac or Italian, the total is large. So a requirement for perfect uniformity of stem distribution is empirically highly restrictive, in that (unless non-paradigmatic factors interfere) it excludes all but one pattern for every verb. It is striking, then, that in two unrelated and geographically remote languages (Italian and Dhaasanac) this requirement is satisfied. Clearly these languages have reached their current situation by different historical routes. But their convergence is less surprising if uniformity of stem distribution is a by-product of the brain's insistence that potential synonyms should be differentiated.

6.3.3 *Case study 7: Nesting in Russian nominal stress patterns*

Russian nouns inflect suffixally for two numbers (singular and plural), and are traditionally described as inflecting also for six cases (nominative, accusative, genitive, dative, instrumental, and prepositional). There are thus in principle twelve cells in nominal inflectional paradigms. However, this over-simplifies the situation somewhat. Firstly, in the plural, the accusative form of

any noun is always the same as either its nominative (if the noun is inanimate) or its genitive (if it is animate), and the same is true for most nouns in the singular too. Secondly, a minority of nouns divide the range of functions performed by the genitive case between two suffixes, a normal genitive and a 'second genitive', while another minority (partially overlapping with the first) distinguish a normal prepositional form and a 'second prepositional'. This 'second prepositional' will become relevant in section 6.3.5, where we discuss Polish.

There are several inflection classes, reflecting the availability of several rival inflectional realizations for most case-number combinations. Thus, the issue arises whether or not Russian displays vocabular clarity in its inflection-class organization. In this section, however, our focus is not on affixal inflection but on stress patterns. In Russian, stress is subject to complex lexical and morphological conditions. What's more, the stress behaviour of Russian nouns presents at first sight a serious challenge to the suggestion in section 6.3.2 that, unless other differentiating factors come into play, the way in which stem alternants avoid synonymy is through distributional uniformity.

Of the suffixes, only two have any inherent relationship with stress: the 'second prepositional' suffix *-ú* and the relatively infrequent plural suffix *-á*, both of which are always stressed.[10] Therefore nearly all the variability in stress placement must be attributed to the noun stem. In Table 6.4, based on Forsyth (1963: 20), the position of the stress, whether early or late, is indicated by grey shading. 'Nom/Gen' indicates patterns in which the accusative singular is the same as either the nominative or the genitive, according to animacy, and thus has no stress pattern of its own. What I have called 'late stress' nearly always means stress on the suffix rather than the stem, but there are a few nouns in which 'late stress' is on the stem too, such as the pattern-D nouns ОZERO 'lake' (nominative singular *ózero* [ˈɔzʲɨrə], nominative plural *ozëra* [aˈzʲɔrə]) and DEREVO 'tree' (nominative singular *dérevo* [ˈdʲerʲɨvə], nominative plural *derév′ja* [dʲɨˈrʲevʲjə]). This reinforces the correctness of treating stress variation within the paradigm as a matter of stem distribution. However, for simplicity's sake, I will speak from now on as if 'late stress' equates with stress on the suffix.

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[10] There are also a few feminine nouns where it depends on the preposition whether the prepositional suffix *-i* is stressed or not; I have not counted this stressed *-i* as a distinct suffix. The inherently stressed nominative plural suffix *-á*, limited to masculine nouns, must be distinguished from the neuter nominative plural suffix *-a*, which has no inherent stress.

Table 6.4. Russian nominal stress patterns

		A: stul 'chair'	B: stat´ja 'article'	C: stol 'table'	D: dom 'house'
Sg	Nom				
	Acc	Nom/Gen		Nom/Gen	Nom/Gen
	Other				
Pl	Nom				
	Other				

		E: gost´ 'guest'	F: okno 'window'	G: duša 'soul'	H: guba 'lip'
Sg	Nom				
	Acc	Nom/Gen	Nom/Gen		
	Other				
Pl	Nom				
	Other				

		J: ruka 'arm'	K: kon´ 'horse'
Sg	Nom		
	Acc		Nom/Gen
	Other		
Pl	Nom		
	Other		

On the face of it, then, Russian nouns are divided among ten distinct stress-distribution patterns. But on closer inspection this apparent haphazardness melts away. First of all, if a noun is consistent in having either 'early' or 'late' stress in all twelve paradigmatic cells, there is no stem alternation to account for. This takes care of patterns A and B. It also takes care of pattern C. This pattern is limited to nouns that have no suffix in the nominative singular, so

the apparent difference in stress placement between this form and the others is illusory; where there is no suffix, even 'late' stress is confined to the stem.[11]

Patterns D and F are also unproblematic inasmuch as the stress contrast has a clear syntactic correlate. The Russian child's brain hears two stress patterns for these nouns, but learns to distinguish them on the basis of number. If singular forms are stressed 'early', then plural forms are stressed 'late', and vice versa.

That leaves us with E, G, H, J, and K. But inspection shows that each of these is identical with one of the patterns already discussed except in the nominative plural or the accusative singular or both. Pattern E resembles pattern D except in having early stress in the nominative plural, and pattern H resembles pattern B likewise. What's more, K can be combined with H for the same reason that C was combined with A, in that K has a suffixless nominative singular on which the contrast between late and early stress is neutralized. Finally, pattern G resembles pattern F except in having early stress in the accusative singular, and pattern J resembles pattern B except in having early stress in both the accusative singular and nominative plural. However, the impression that the nominative singular and accusative plural can vary independently is misleading; for in the two patterns where early stress in the accusative contrasts with late stress elsewhere in the singular (namely G and J) there is also early stress in the nominative plural—though only in J does this early stress contrast with the rest of the plural.

We represented at (4) the decisions that determine which stem alternation pattern an Italian verb belongs to in the present tense. A similar representation for Russian is at (11)–(13):

(11)   If a noun has alternating stress in the singular, then it is stressed early in the accusative, late in the other cases.

(12)   If a noun has alternating stress in the plural, then it is stressed early in the nominative, late in the other cases.

(13)   If a noun has alternating stress other than as specified in (11) or (12), then it is stressed early in the singular and late in the plural, or vice versa, except where this would conflict with (11) or (12).

This accounts for nearly all of what we have observed, on the assumption that suffixless forms are ignored for the purpose of determining whether stress

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[11]   There are also numerous nouns with no suffix in the genitive plural (that is, whose genitive plural form consists of the bare stem). Superficially, therefore, these nouns have 'early' stress in the genitive plural, wherever the stress lies elsewhere in the plural. So if genitive plural forms had been indicated separately in Table 6.4, this would have yielded apparently yet more patterns. But all the new patterns would have been combinable with existing ones, once the neutralization of 'early' and 'late' in suffixless forms was taken into account.

alternates or not. To apply (11)–(13), the Russian child's brain must notice only whether a stress alternation exists, and whether it exists within forms that agree in number. As with Italian and Dhaasanac, the alternation patterns are uniform in the sense that, in order to decide how the alternants are distributed, the brain does not have to know in advance which alternant is used in any individual cell. On the other hand, the need to look separately at singular and plural forms reflects the fact that Russian has not one distribution pattern (as in Dhaasanac) nor two potentially overlapping ones (as in Italian) but three.

But there is more to say. We have taken care of nearly everything in Table 6.4 but not everything. Nothing at (11)–(13) rules out an eleventh stress pattern as at (14):

(14)

		*L: (no examples)	
Sg	Nom		
	Acc		
	Other		
Pl	Nom		
	Other		

The non-existence of pattern *L reflects the fact that the accusative singular and the nominative plural do not vary independently. Rather, early stress in the accusative singular entails early stress in the nominative plural (however the other plural forms may behave). So the analysis offered at (11)–(13), though it may seem to differentiate the early and late stressed alternants adequately for synonymy-avoidance purposes, misses something.

Let us therefore focus on the relationship between these two cells, ignoring for the moment the straightforwardly number-related stress alternation. It will be helpful to contrast Russian with Dhaasanac. I will present the relevant

information in pared-down diagrams (15) (where three boxes are numbered for ease of identification later) and (16):

(15)

Russian			
Two-Early		One-Early	
Elsewhere	▨		▨
Sg Acc	1		▨
Pl Nom	2	3	
Elsewhere	▨	▨	

(16)

Dhaasanac
Elsewhere — A
2nd, 1st pl, — B
3rd fem sg
Elsewhere — A

For Dhaasanac there is only one column, reflecting the uniformity of the distribution of the A and B stem alternants. In Russian there are two columns which I have labelled 'Two-Early' and 'One-Early', reflecting the two possibilities for those nouns that make special arrangements for certain cells. (Indeed, I could have added a third column, standing for all those nouns, in patterns A, B, C, D, and F, that make no such special arrangements.) But notice that the early-stress box in the One-Early column is a subset of the early-stress boxes in the Two-Early column. There is thus an implicational link between the accusative singular and nominative plural cells:

(17)   Early stress in accusative singular ⊃ early stress in nominative plural.

Equally in Dhaasanac one could describe the relationship between any two of the B-stem cells in terms of an implication, for example:

(18)   Special stem in 3 fem sg ⊃ special stem in 2 sg.

(I call the B stem 'special' in line with Baerman's analysis of the A stem as the basic or 'elsewhere' stem.) Now, there is an important difference between Dhaasanac and Russian—important but (I will suggest) not fundamental—, namely that in Dhaasanac this implication is mutual:

(19)   Special stem in 2 sg ⊃ special stem in 3 fem sg.

Indeed, it is easy to see that with a uniform pattern of distribution of stem alternants, such as Dhaasanac has, all implications between cells occupied by a special stem alternant are reversible in this fashion. Uniformity, in the sense that currently concerns us, is equivalent to universal mutual implication.

But what if universal mutual implication, though desirable from the brain's point of view, is not essential? Recall again that, in terms of the proposal being advanced here, what is important for the brain is that distinct stem alternants such as those labelled 'A' and 'B' in Dhaasanac should not be synonymous. Two possible ways of differentiating them (namely in terms of syntactic function and syntagmatic context) do not work for Dhaasanac; only the paradigmatic dimension is available. But on the paradigmatic dimension Dhaasanac succeeds brilliantly. Each of the alternants A and B signals unambiguously its own distribution. There is no way in which the paradigmatic dimension could perform the differentiation task better. But less brilliant success may nevertheless be adequate. In Russian (I suggest) the paradigmatic dimension performs the task adequately, even if less brilliantly, with the help of the one-way implication at (17). Here is how. (Bear in mind that we are considering the pared-down Russian at (15), ignoring patterns in which the stress alternation signals number. To take account of those patterns at this point would merely complicate the exposition.)

In order to identify which nouns comply with each of the contrasting patterns at (15), it is not sufficient for a child to know that a Russian noun has two stress alternants. In this respect, Russian nouns contrast with Italian verbs, as indicated at (4). The child needs to know, in respect of some nouns, at least one cell in which some particular alternant occurs. However, in virtue of the implication at (17), she does not need information about more than one cell. In respect of nouns that follow the Two-Early pattern, all she needs to know is that they have early stress in the accusative singular; the early stress in the nominative plural then follows in virtue of the implication at (17). As for nouns that follow the One-Early pattern, she needs to know that they have early stress in the nominative plural. So what needs to be specified in the child's lexicon in order for her to allocate nouns to the two stress-alternation patterns at (15) can be summed up at (20):

(20)   Lexical specification:
        Two-Early pattern   early stress in accusative singular
        One-Early pattern   early stress in nominative plural

Now we are in a position to specify the factors that differentiate an early-stressed stem from a late-stressed one in the Two-Early and One-Early

patterns. The need to identify the accusative singular as early-stressed in the Two-Early pattern means that a more complex question arises in respect of Russian than in respect of Dhaasanac. In Dhaasanac the answer to the question 'What differentiates the B stem from the A stem?' is straightforward: 'The B stem signals its own uniform distribution'. But in Russian the lexical specification of early stress in the accusative singular in particular highlights the early-stressed stem there as opposed to elsewhere. So what is signalled by early stress in the accusative singular—that is, in the box numbered '1' at (15)? The answer cannot be 'accusative singular' itself, because early stress is found in at least one other form of the lexeme too, namely the nominative plural. Thus, in terms of the representative nouns at patterns G and J in Table 6.4, what signals 'accusative singular' is not the stressed stem alternant *dúš* 'soul' or *rúk* 'hand' by itself; rather, it is either the stem plus the suffix (*dúš u, rúk u*) or, one might argue, the suffix alone, since for these nouns the suffix *u* signals no other case-number combination. Early stress in patterns G and J thus contrasts in a crucial fashion with umlaut in German noun inflection, where we are indeed entitled to say that an umlauted stem form signals 'plural' because it is used in all and only plural forms.

What comes to the rescue in Russian, I suggest, is precisely the implication at (17), stipulating that if there is early stress in the accusative singular, it will be found in the nominative plural also. This yields the following differentiating factors for early stress in respect of the numbered boxes in the table at (15):

(21) Pattern   Box   Differentiating factor
     One-Early   3   'early stress in nominative plural' as lexically
                           specified
     Two-Early   1   'early stress in nominative plural' by virtue of (17)
     Two-Early   2   implied by (17) on the basis of box 1

Notice that, even though nouns of the Two-Early pattern are lexically specified as having early stress in the accusative singular, 'early stress in accusative singular' does not serve as the factor differentiating the stressed stem alternant in box 1 (the accusative singular) from the unstressed alternant. This is for the reason just given: in Two-Early nouns, not only the accusative singular but also the nominative plural has early stress. Rather, because any noun with early accusative stress will, in virtue of (17), also have early stress in the nominative plural, what differentiates the stressed stem alternant in box 1 is precisely that implication. The early stress in box 2 thus fits into the pattern not as a signifier (for 'nominative singular', for example) but rather as something signified. In the One-Early pattern, on the other

hand, the lexically specified 'early stress in nominative plural' can indeed serve as the differentiating factor in box 3 because, in that pattern, early stress is found nowhere else.[12]

The differentiating factor for box 2 deserves a little more comment. Here, early stress contributes to synonymy avoidance not by having a unique 'meaning' but rather by being what is 'meant' by early stress somewhere else. This may sound weird. Can morphological 'meanings' really be so functionless in terms of anything outside morphology? But by now the reader should be not too surprised if the answer is yes. After all, the uniform stem-distribution patterns that we have observed in Dhaasanac and Italian constitute their own 'meanings': they are what is signified by themselves as signifiers. And we noted in section 5.2.3 that the implication holding between umlaut and plural suffixation for some nouns in German was similarly reversible. The only aspect that is new about box 2 in Russian's Two-Early pattern is that it has no signifier role alongside its role as something signified. But none of this should seem surprising when one recalls that (according to my argument) the capacity for morphology arose not as a direct communicative or cognitive adaptation but as the indirect consequence of the brain's dislike of synonymy. There is nothing to stop morphology from fulfilling what is usually seen as its function, namely to encode extramorphological information. But there is also nothing to stop the brain from organizing morphological phenomena in a fashion that fulfils no such function—nothing, that is, except the limit on the brain's capacity to memorize contrasts, which is apparently so generous as to be effectively no limit.

In earlier sections I argued that my proposed analyses made clear empirical predictions about imaginable kinds of pseudo-Italian and pseudo-Dhaasanac which, if I am right, ought not to exist. Two questions now arise in connection with Russian. Firstly, does my analysis carry any implications about imaginable but non-existent kinds of pseudo-Russian? Secondly (and perhaps more ominously), in relaxing for Russian the requirement of distributional uniformity, have I opened loopholes through which pseudo-Italian and pseudo-Dhaasanac might sneak in? Have I, in fact, undermined my previous claims? I will answer the first of these questions now and the second in section 6.3.3.1.

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[12] An example is *gubá* 'lip' at H in Table 6.4. 'Nowhere else' must be qualified in the sense that the full Table 6.4, as opposed to the pared-down version at (15), illustrates in E (*gost'*) the fact that some One-Early nouns in Russian have early stress throughout the singular also. But it is reasonable to suppose that Russian children's brains learn to distinguish between stress that is number-related and stress that is not.

Consider a hypothetical pair of distribution patterns for 'special' stress that are not much different from the Russian ones but in which neither is nested within the other, such as in (22) (modelled on (15)):

(22)

Pseudo-Russian 1				
	Acc-Early		Dat-Early	
Elsewhere				
Sg Acc	1			
Sg Dat			2	
Pl Nom	3		4	
Elsewhere				

Lexical specifications, modelled on (20), will be as in (23):

(23)   Lexical specification:
    Acc-Early pattern   early stress in accusative singular
    Dat-Early pattern   early stress in dative singular

Instead of the one implication at (17), two implications apply in pseudo-Russian 1:

(24) a. Early stress in accusative singular ⊃ early stress in nominative plural.
      b. Early stress in dative singular ⊃ early stress in nominative plural.

So far, so good. But now consider what the would-be differentiating factors must be, or, in other words, what the counterpart of (21) will look like:

(25) Pattern   Box   Potential differentiating factor (where needed)
      Acc-Early   1   'early stress in nominative plural' (by (24))
      Dat-Early   2   'early stress in nominative plural' (by (24))
      Acc-Early   3   implied by (24) on the basis of box 1
      Dat-Early   4   implied by (24) on the basis of box 2

Contrary to appearances, perhaps, there is nothing wrong here with boxes 3 and 4. Their early stress contributes to synonymy avoidance by being not a signifier but rather a thing signified, just like the early stress in box 2 at (15). (That is the reason for the words 'where needed' in (25).) But there is trouble with boxes 1 and 2. Their early stress is lexically specified as in (23); but (just as in actual Russian's Two-Early pattern) the cells mentioned in their lexical specifications cannot serve as differentiating factors because, in these Accusative-Early and Dative-Early patterns, early stress is not limited to the accusative and the dative respectively. So recourse must be had to the implicational 'meanings' of early stress in these cells, as presented in (24). But these 'meanings' (that is, the implications for stress elsewhere in the paradigm) are identical. And, being identical, they do not satisfactorily avoid the pitfall of synonymy.

My prediction is bound to be, then, that if there were a risk that a pattern structured as in pseudo-Russian 1 might come into being, perhaps as a by-product of phonological change, the brain would not tolerate it. Some alteration would be needed in order to make it cognitively acceptable. A plausible alteration would be one that created a nested pattern of distributions for the early-stressed alternant. A way of doing that would be to extend early stress to the dative singular cell in the Accusative-Early pattern, as illustrated schematically at box 2 in (26):

(26)

Pseudo-Russian 2					
Three-Early		Two-Early		One-Early	
Elsewhere					
cell A	1				
cell B	2		3		
cell C	4		5		6
Elsewhere					

Notice that superficially pseudo-Russian 2 seems more complicated than pseudo-Russian 1. I have arranged for the distributions of early stress to be nested, as in actual Russian, but I have also added a One-Early pattern, so that pseudo-Russian 2 has three patterns for early stress, not two. Yet pseudo-Russian 2 is consistent with the synonymy-avoidance requirement, as I will proceed to demonstrate.

Firstly, pseudo-Russian 2 conforms to a two-stage implication, as in (27), rather than one single-stage one, as in (17) for actual Russian, or two single-stage ones, as in (24) for pseudo-Russian 1:

(27) Early stress in cell A ⊃ early stress in cell B ⊃ early stress in cell C.

This two-stage implication is a more complex example of what in Natural Morphology is called an implicational paradigm structure condition (Wurzel 1984). (I will have more to say about this notion later.) To a noun with early stress in cell B but not in cell A, only the second stage applies.

Secondly, by virtue of (27), lexical specifications for items belonging to each of the patterns will be as in (28):[13]

(28)		Lexical specification:
	Three-Early pattern	early stress in cell A
	Two-Early pattern	early stress in cell B
	One-Early pattern	early stress in cell C

Finally, the differentiating factors will be as in (29):

(29)	Pattern:	Box:	Differentiating factor (where needed):
	Three-Early	1	'early stress in cell B' (by (27))
	Two-Early	3	'early stress in cell C' (by (27))
	One-Early	6	'early stress in cell C' as lexically specified
	Three-Early	2	implied by (27) on the basis of box 1
	Three-Early	4	implied by (27) on the basis of box 1
	Two-Early	5	implied by (27) on the basis of box 3

Again, it does not matter if boxes that fit into the pattern as things signified rather than as signifiers fulfil the same function. That takes care of boxes 2, 4, and 5. And 'early stress in cell C' as lexically specified in box 6 can serve as a differentiating factor because, in One-Early nouns, no other box has early stress. But now, crucially, in contrast to (25), there is no 'X' such that two or

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[13] I assume that, rather as in actual Russian, there is a fourth, more numerous, pattern (what one might call Zero-Early) in which no cells display individual early-stress behaviour. Nouns conforming to this pattern will need no lexical specification.

more boxes contain lexically unspecified 'early stress in cell X'. Therefore there are no two or more boxes that turn out to be, in the relevant sense, synonymous. Instead, boxes 1 and 3 are differentiated inasmuch as the implication of box 1 relates to cell B while that of box 3 relates to cell C. Pseudo-Russian 2 is thus predicted to be a possible language, unlike pseudo-Russian 1, despite being superficially more complex.

Such a prediction has the desirable property of being highly vulnerable, disconfirmed by any language where stem-distribution patterns are not nested (and where no other factors, such as semantic or phonological ones, differentiate them). But in sections 6.3.4 and 6.3.5 I will present evidence from German and Polish tending to confirm that the kind of nesting relationship that we have observed in actual Russian, as well as pseudo-Russian 2, is not accidental.

In respect of predictiveness, it is worth comparing what I say here with a comprehensive analysis of Russian stress in terms of Network Morphology by Brown et al. (1996). They say (1996: 79): 'An analysis grounded within the network morphology framework makes predictions that vary in the degree to which they might hold or be violated.' This already indicates a difference: I envisage the kind of cognitive constraint that pseudo-Russian 1 violates as relatively rigid, not of the kind that a language can comply with to a greater or lesser degree. In any case, it turns out that the kind of prediction that Brown and his colleagues are interested in concerns what sort of stress behaviour may or may not be observed in Russian itself—whether a word with a particular stress-alternation pattern would or would not be a possible Russian word. They are not concerned (at least, not explicitly) with whether imaginable languages that deviate from Russian in specific ways could or could not occur.

Brown and his colleagues also discuss an issue about which I have said little: the relationship of Russian stress-distribution patterns to affixal inflection. They claim that the relationship is relatively close. In the four declension classes that they recognize, the various stress patterns are by no means equally represented. This is not merely because the 'early stress in accusative singular' patterns are restricted to the one declension class that has a dedicated accusative singular form, distinct from both the nominative and the genitive. So, for our present purposes, it may look as if I have neglected a potential source of differentiation for the stress alternants, namely the particular inflectional affixes that they can or cannot accompany. But my neglect has been deliberate. Even though there are some stress patterns that never appear in certain declensions, nevertheless each declension is compatible with at least three of the eight stress patterns that Brown et al. recognize, and one of them is compatible with seven of the eight. So the degree to which stress could be said to signal affixal inflection is severely limited. Rather, Russian seems

consistent with the expectation discussed in section 5.2.4 in relation to Italian verbs: the expectation that affixal inflection classes, with their evolutionary origin in protolinguistic multivocabulism, should behave relatively independently of stem alternation, with its separate origin.[14]

6.3.3.1 *Pseudo-Italian and pseudo-Dhaasanac reconsidered*

In the previous section I argued that distributional uniformity of the kind observed in Italian and Dhaasanac was not necessary in order for distinct stem alternants to be differentiated through their paradigmatic relationship. But, I asked: in relaxing for Russian the requirement of distributional uniformity, have I opened loopholes through which pseudo-Italian and pseudo-Dhaasanac might sneak in? Have I, in fact, undermined my previous claims?

Let us first consider pseudo-Dhaasanac as presented at (9). For a reason that will become clear, I will present the singular and plural separately at (30) and (31), identifying the special alternants in the B and D boxes through shading:

(30)

		Pattern I	Pattern II
Singular	1st	A	C
	2nd	B	C
	3rd fem	B	D
	3rd masc	A	D

(31)

		Pattern I	Pattern II
Plural	1st incl	A	C
	1st	B	C
	2nd	B	D
	3rd fem	A	D
	3rd masc	A	C

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[14] The independence of stress and affixal inflection in Russian nouns is emphasized also by Stump (2005: 284–6).

The reason for presenting the data in this way is to draw attention to the parallels with pseudo-Russian 1 at (22). In that pseudo-Russian, there is no nesting; instead, in two alternation patterns (Accusative-Early and Dative-Early), early stress in distinct boxes (accusative singular and dative singular) implies the same thing, namely early stress in the nominative plural. Early stress in those two boxes thus creates an objectionable synonymy. In (30) and (31) the same pattern is reproduced. In (30) the 'special' alternants in the 3rd feminine are predictable on the basis of the appearance of the same 'special' alternant in the 2nd or the 3rd masculine, but at the cost of failing to differentiate these 'special' alternants from each other. And in the plural at (31) the same geometry of cell relationships is reproduced. It is clear, too, that pseudo-Dhaasanac is not 'saved' by recombining (30) and (31). The absence of a nesting relationship between the patterns—or of uniformity, which is mutual nesting—renders them irreconcilable with the brain's no-synonymy requirement.

Now let us turn to pseudo-Italian as presented in (5). Here, pattern III, called by Maiden (2005) the 'U pattern', which is actually found in Italian, is set alongside two rival patterns, IIIa and IIIb. The difference between the patterns is confined to the present subjunctive, as shown at (32):[15]

(32)		Pattern III	Pattern IIIa	Pattern IIIb
Present Subjunctive				
Sg	1	▨	▨	▩
	2	▨	▨	▩
	3	▨	▨	▩
Pl	1			
	2			
	3	▨	▩	▨

[15] Recall that the shaded boxes indicate stress on the stem. Where the special alternant occurs the stem is always stressed, but stress on the stem is compatible with the normal alternant too.

On the basis of a lexically specified special stem alternant in the subjunctive 3rd plural, a child's brain can reliably conclude that the verb in question displays U-pattern behaviour in the indicative (that is, the special stem in the 1st singular and the 3rd plural):

(33) Special stem in subjunctive 3rd plural ⊃ U-pattern in indicative.

However, it cannot determine whether the special stem alternant appears also in the subjunctive singular (as in pattern IIIa) or not (as in pattern IIIb). Perhaps, then, the special stem alternant needs to be lexically specified wherever it appears in the subjunctive singular too. The three patterns at (32) are thus distinguished from each other lexically: IIIa has one specification, IIIb has another, and III has both. The trouble now is that the implication at (33) is now joined by another:

(34) Special stem in subjunctive singular ⊃ U-pattern in indicative

So, even if the U-pattern forms in the indicative (the special stems in the 1st singular and the 3rd plural) slot into place as things signified rather as signifiers, we are still left with two cells in the paradigm (the subjunctive singular and 3rd plural) where special stems 'mean the same thing' by virtue of (33) and (34).

This is a positive outcome. What we have confirmed is that, even though Russian is not compatible with the stringent uniform-distribution requirement that we originally hypothesized on the basis of Italian and Dhaasanac, the particular way in which that requirement needs to be relaxed in order to accommodate Russian by no means opens the floodgates to every imaginable possibility. Rather, Italian and Dhaasanac satisfy the same nesting requirement as Russian does, but in a more condensed fashion: all conceivable nestings are compressed concertina-wise into a single pattern.[16]

Which way of satisfying the nesting requirement is most commonly encountered (assuming that the nesting requirement is indeed genuine)? Is there any consistent tendency over time for uniform patterns like that of Dhaasanac to unfold into multiple nested patterns, as in Russian? Conversely, does the compression of nested patterns into uniformity render paradigmatic differentiation of stem alternants more robust and more likely to endure? The widespread occurrence and resilience of 'irregular' or 'unnatural' stem-distribution patterns in Romance, noted by Maiden, may suggest the latter. In that case, to call such patterns 'irregular' (Maiden 1992) or 'unnatural' (Baerman et al. 2005) no longer

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[16] Stump (2006), discussing inflection-class mixture in general (not just stem-alternation patterns), suggests that it is constrained by what he calls the Privileged Category Restriction. However, in respect of stem patterns, this Restriction does not seem to impose the nesting requirement that is argued for here.

seems quite appropriate. It is a hangover, perhaps, from a view of morphology as a device for fulfilling functions that to human language-users seem obvious, rather than as a device contrived to reconcile the effects of phonological processes with a deep-rooted cognitive requirement of the primate brain.

6.3.4 *Case Study 8: German verbs and implicational paradigm structure conditions*

Pseudo-Russian 2, as discussed in section 6.3.3, displays a nesting of stem-alternation patterns that conforms to the two-stage implication stated at (27). I described this as an example of an implicational **paradigm structure condition** (PSC) (Wurzel 1984). Wurzel suggests that PSCs constitute an important element in how inflection classes are organized. Because of the way in which PSCs link information about particular characteristics of inflection classes, they make possible a language in which there is a wide yet stable diversity of such classes, with membership in any given class (insofar as it is not predictable on independent grounds, such as gender or phonological shape) being specifiable in a simple fashion. Each noun needs only a lexical indication of the point at which it boards the implicational train, so to speak. Thus, in pseudo-Russian 2, nouns that conform to the Three-Early pattern in (26) need to be specified only as having early stress in cell A. The fact that they also have early stress in cells B and C is then derivable from the PSC at (27).

Wurzel does not of course discuss my invention, pseudo-Russian. He illustrates complex multi-stage PSCs in an actual language by applying them to Latin nouns, thus (1984: 120):

(35)   Acc.Sg./im/ ⊃ Abl.Sg./i:/ ⊃ Acc.Pl./i:s/ ⊃ Gen.Pl./ium/

(36)   Gen.Pl./um/ ⊃ Acc.Pl./e:s/ ⊃ Abl.Sg./e/ ⊃ Acc.Sg./em/

But a conflict looms here. We have already discussed Latin nouns in section 5.2.2, in the context of arguing that Latin's apparently elaborate system of inflection classes is compatible with vocabular clarity. This is in line with my proposal that the brain, confronted with apparent synonymy between inflectional affixes, can draw upon its tolerance for multiple but distinct vocabularies, as in Javanese and elsewhere. Yet I will show directly that multi-stage PSCs can handle neatly a hypothetical inflection-class system in which vocabular clarity is massively violated. So, in accepting a PSC such as (27) as a way of representing the nested relationship between the stem alternation patterns for pseudo-Russian 2, have I undermined my own argument in respect of affixal inflection classes? The answer turns out to be no, for reasons that fit in well with the story told here about affixal and non-affixal inflection.

Consider first a hypothetical system of six inflection classes such as the one represented schematically in (37). The morphosyntactic properties (of case, person, number, or whatever) associated with the five cells in the paradigm do not matter, because we are concerned with formal properties of the system rather than with what its elements mean; consequently these five cells are identified just by numbers. Similarly, the six inflection classes are labelled with upper-case letters, and lower-case letters stand for ten distinct affixes:[17]

(37)

	A	B	C	D	E	F
1	**a**	*p*	*p*	*p*	*p*	*p*
2	b	b	q	q	q	q
3	c	c	c	r	r	r
4	d	d	d	d	s	s
5	*e*	*e*	*e*	*e*	*e*	**t**

If what I said about German in section 5.2.3 holds for this language, then every affix will either display vocabular clarity (that is, it will be restricted to one inflection class), or else be a class-default for its cell (that is, it will be the only affix for its cell that is not restricted to one class). In (37), affixes restricted to just one class are in bold, and class-defaults are in italics. But these affixes are found only in cells 1 and 5. It is clear, then, that this language is not like German, because in cells 2, 3, and 4 there is vocabular blurring. Six of the ten affixes fail to behave in the way in which my vocabulary-based account leads us to expect, but instead appear in 'synonymous' pairs: *b* with *q*, *c* with *r*, and *d* with *s*.

Consider now how the system at (37) might be described in terms of Wurzel-style PSCs. Let us assume that class A is the largest and the one towards which new words gravitate. Therefore words belonging to class A (it is reasonable to assume) need no special lexical specification. Their membership in it can be attributed, in Wurzel's terms, to a single-stage PSC as at (38):

(38)   Word $\supset$ {1a, 2b, 3c, 4d, 5e}

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[17] The argument in this paragraph recapitulates an argument presented elsewhere: see Carstairs-McCarthy 1991: 239–41, 1994: 755–6. Carstairs(-McCarthy) (1984, 1991) argues that the situation in the Latin 'third declension' (corresponding to classes E, F and G at Table 5.3) is more complex and fluid than Wurzel's PSCs (35) and (36) make it seem.

Words that belong to the other five classes need some lexical specification. However, their specification is a simple matter when one notices that the inflection classes are linked—in fact, nested—in a fashion that can be captured by a four-stage PSC as at (39):

(39)   5t ⊃ 4s ⊃ 3r ⊃ 2q ⊃ 1p

That is, these five inflection classes can be arranged neatly in a series from the most 'irregular' (class F) to the one which departs least from the regular pattern (class B). The only lexical specifications needed to assign words to each class are as in (40):

(40)   Class B   1p   Class E   4s
        Class C   2q   Class F   5t
        Class D   3r

All the rest of the inflectional behaviour of any word is derivable from the multi-stage PSC at (39) or, when that fails, the 'regular' PSC at (38). Thus, by means of precisely the mechanism that I invoked to handle the Two-Early and One-Early stress patterns of Russian nouns, it turns out to be an easy matter to describe an affixal inflection pattern that misbehaves seriously in terms of my multivocabular approach to inflection classes.

   The solution to this paradox lies in the word 'affixal'. Stress alternation in actual Russian nouns does not involve affixes, whereas the hypothetical inflection-class system that we have just been looking at involves nothing else. Let us suppose, then, that paradigm structure conditions are part of the brain's toolkit for dealing with selective items but not unselective ones. Recall that, in protolanguage, unselective vocabulary items, being relatively frequent and therefore tending to be relatively short, could easily diverge through phonological change into quite dissimilar shapes. Paradoxically, perhaps, for this sort of radical divergence the hominid brain had an analytical tool ready to hand: it could sort the unselective items (ancestors of modern inflectional affixes) into distinct vocabularies, respecting vocabular clarity. But with selective vocabulary items (the ancestors of major-class lexemes), phonological developments would more typically have confronted the brain with a new cognitive puzzle: a set of forms that were the same yet different—recognizably similar phonologically and with the same basic meaning, but appearing in different contexts. To handle this phenomenon, vocabular clarity was no help. But synonymy could still be avoided, provided that some distinct role for each of the different forms could be concocted. That role would often be of a kind that most morphologists would regard as 'natural', such as expressing some syntactic or semantic content ('plural number', 'past tense',

'feminine gender'). But the role could also be 'unnatural', whether as a signifier or as a thing signified, either syntagmatically (such as 'alternant to be used before vowels') or paradigmatically (such as 'alternant to be used in cell 3 if a lexeme uses the same alternant in cell 2').

As support for this suggestion, what we would like to find is more evidence of this last-mentioned kind of paradigmatic relationship: an actual set of nested stem-distribution patterns involving a multi-stage paradigm structure condition as elaborate as Wurzel's would-be Latin ones at (35) and (36). For this purpose, we can conveniently draw upon an analysis of German verbs by Andreas Bittner (1985, 1996).

To understand the gist of Bittner's argument, it is sufficient to know that the inflection of verbs in German is on the same lines as in English, but more elaborate. In English, most verbs are inflected according to a regular pattern, as in (41e), but there are also many common verbs that display different affixal inflection (or none at all), usually accompanied by stem alternation, as in (41a–d):

(41) a. take   b. sing   c. hit   d. bring   e. bake
        took   sang   hit   brought   baked
        taken   sung   hit   brought   baked

In German, Bittner distinguishes seven classes, exemplified in (42) with a subset of each verb's forms chosen mainly to illustrate stem-alternation patterns:

(42) | | a. | b. | c. | d. |
|---|---|---|---|---|
| *infinitive* | helfen 'help' | fahren 'go' | rufen 'call' | denken 'think' |
| *imperative 2nd sg* | hilf! | fahre! | rufe! | denke! |
| *present 3rd sg indic* | hilft | fährt | ruft | denkt |
| *preterite 3rd sg indic* | half | fuhr | rief | dachte |
| *preterite 3rd sg subjunc* | hülfe | führe | riefe | dächte |
| *perfective participle* | geholfen | gefahren | gerufen | gedacht |

	e.	f.	g.
infinitive	schinden 'skin'	mahlen 'paint'	loben 'praise'
imperative 2nd sg	schinde!	mahle!	lobe!
present 3rd sg indic	schindet	mahlt	lobt
preterite 3rd sg indic	schindete	mahlte	lobte
preterite 3rd sg subjunc	schünde	mahlte	lobte
perfective participle	geschunden	gemahlen	gelobt

Here, (42g) is the regular or so-called 'weak' pattern, corresponding to (and cognate with) English (41e). (42a–c) are so-called 'strong' verbs, including some very common verbs; they contrast with the weak verbs in having more than one stem alternant. (42e) and (42f), each with very few members, represent verbs that are mainly weak but which retain a few strong characteristics. (42d) contains hybrid verbs, 'weak' in terms of suffixation but nevertheless displaying more than one stem alternant.

Bittner argues that, despite this inflectional diversity, nearly all the inflectional diversity illustrated in (42) can be described neatly with the help of a five-stage PSC, as follows (where 'special' means 'distinct from the infinitive'):

(43)   1. special stem alternant in imperative 2nd singular
     ⊃   2. special stem alternant in present indicative 2nd and 3rd singular
     ⊃   3. no suffix in indicative preterite 1st and 3rd singular
     ⊃   4. special stem alternant in preterite indicative
     ⊃   5. possibility of an umlauted vowel in preterite subjunctive
     ⊃   6. 'strong' perfective participle with suffix -*en*, usually with special stem alternant

This PSC does not specify the particular vowel to be found in individual 'special' stems, but it provides most of the necessary information about where they will occur within the paradigm of each verb. Moreover, says Bittner, it is usually necessary to specify for each verb only the point (from 1 to 6) at which it joins the PSC. Weak verbs like LOBEN at (42g) do not join it at all; they inflect according to a default pattern, complied with by all verbs unless otherwise specified. At the other extreme, some strong verbs such as HELFEN at (42a) join at point 1, so as to be subject to all five stages of the PSC. A verb such as MAHLEN joins at point 6, so as to be inflected 'weakly' everywhere except in the perfective participle. And so on.

At this point, however, I need to clarify what lies behind the qualifications 'nearly all' and 'usually' in the previous paragraph. Bittner admits that the PSC at (43) does not quite work for the hybrid verbs exemplified by DENKEN at (42d). (Other common verbs of this type are BRENNEN 'burn', KENNEN 'know', and BRINGEN 'bring'.) These display a special stem vowel in the preterite indicative and umlaut in the preterite subjunctive. On the other hand, they have a suffix (-*te*) in the preterite 1st and 3rd singular. This suggests that they join at point 4 in the PSC. Yet they are not subject to the final stage of the PSC, in that in the perfective participle their suffix is 'weak' -*t*, not 'strong' -*en*. Therefore in Bittner's analysis they need a double lexical specification: one to allow them to join the PSC at point 4, and one to override the last stage of it.

This awkwardness disappears, however, once we recognize that the only aspect of point 6 that is troublesome for the DENKEN verbs is the mention of the suffix *-en*. What happens if we remove from (43) all mention of affixation, so that it deals with stem alternations only? The result is as in (44):

(44)   1. special stem alternant in imperative 2nd singular
    ⊃ 2. special stem alternant in indicative present 2nd and 3rd singular
    ⊃ 4. special stem alternant in preterite indicative
    ⊃ 5. possibility of an umlauted vowel in preterite subjunctive
    ⊃ 6. 'strong' perfective participle, usually with special stem alternant

The effect is to amend point 6 and to remove point 3 entirely. But notice now that the DENKEN verbs at (42d) no longer need any extra lexical specification. They get on board the PSC (so to speak) at point 4 and stay through to the end, complying with points 5 and 6 in that the stems *däch-* and *dach-* (for example) are both different from the infinitive stem *denk-*.[18]

The significance of this outcome should not be underestimated. Bittner assumed a framework of morphological analysis in which there is no expectation that paradigm structure conditions should be especially relevant to stem alternations rather than to affixation. That is, in his framework of analysis there was nothing wrong with the multi-stage affixal PSCs at (35) and (36) that Wurzel posited—erroneously, I have suggested—for Latin. So it is striking that his proposed multi-stage PSC for German verbs works best precisely if it is restricted to stem alternation, just like the single-stage PSC at (17) that links the Two-Early and One-Early stress patterns in Russian nouns.

It is fair to object that my tidying-up of Bittner's five-stage PSC comes at a cost. The version at (44), unlike the version at (43), does not predict that in verbs characterized by point 6 the perfective participle suffix is *-en*. Yet this outcome has helpful aspects. For the purpose of verbs in the DENKEN class, this is just the outcome we want; their perfective participle suffix is after all not *-en* but *-t*. Indeed, these verbs illustrate the possibility (unsurprising in our framework) that affix choice may fail to correlate exactly with stem alternation. There is no denying, however, that all the verbs in the 'strong' verb classes (42a, b, c, e, f) do have the *-en* suffix. Could this perhaps be a

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[18] The several verbs containing *-ann-* in their preterite stem, such as *brannte* from BRENNEN 'burn', have an umlauted preterite subjunctive stem (written *brenn-* in this case) which looks like the infinitive stem. But this is due to the accident that [bʁɛn], the umlauted version of [bʁan], happens to be homophonous with the infinitive stem and is conventionally spelt in the same way (rather than '*bränn-*').

matter of syntagmatic differentiation: *-en* appears rather than *-t* when there is a special stem alternant in the perfective participle? The answer is no, for two reasons: first, as we have just seen, a special stem alternant can co-occur with the suffix *-t*, as in *gedacht* from DENKEN, and secondly because the suffix *-en* can occur with a stem alternant that is the same as the one found in the infinitive, as in *gefahren* from FAHREN and *gerufen* from RUFEN (the illustrative verbs at (42b) and (42c)).

There is still more to be said, however, illustrating both the dilemmas and the opportunities opened up by our evolution-oriented approach to morphology. If *-en* cannot be differentiated from *-t* syntagmatically in terms of the stems it attaches to, the obvious alternative is to look for a paradigmatic differentiation in terms of distinct vocabularies. But the 'vocabulary' that *-en* would need to belong to is exiguous in the extreme. Indeed, it would have no other members at all. This is because, so far as forms other than the perfective participle are concerned, strong verbs display no affixes of their own. In any given cell, a strong verb either displays the same affix as a weak verb, or it displays no affix at all (as in the strong preterites *half* 'helped' and *fuhr* 'went' by contrast with the weak preterite *lob-te* 'praised'). So is the brain willing to recognize an affixal 'vocabulary' with only one member? It will hardly be surprising if the answer turns out to be no. Perhaps, then, what we need to say about the perfective participle elements *-holfen*, *-fahren*, *-rufen*, *-schunden*, and *-mahlen* at (42) is that each of them consists of not a stem and a suffix (*-holf-en*, *-fahr-en*, and so on) but rather of a stem alone. In that case, RUFEN (for example) has not two stems, *ruf-* and *rief(-)*, but three: *ruf-*, *rief(-)*, and *rufen*.

At first sight, to deny the affixal status of something so clearly affix-like as the *-en* of the perfective participle in German verbs looks bizarre. It would seem to undermine the claim that affixal and non-affixal inflection play by different rules because they have different evolutionary origins. But if an affix is not paradigmatically related to any other affixes with which it may constitute a 'vocabulary', then perhaps the brain has no choice but to analyse it as not an affix at all but part of a stem. Our prediction will be, then, that the brain will treat an apparent affix as part of a stem precisely when, if it were an affix, there would be no satisfactory way of differentiating it from a rival. In this instance, if *-en* is not treated as part of the stem in German strong perfectived participles, it risks lacking adequate differentiation from its rival *-t*.[19]

I have by no means said all that needs to be said about German verbal inflection classes. For example, I have said nothing about the actual shapes of

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[19] Wunderlich and Fabri (1995) also question the affixal status of perfective participle *-en*, for independent reasons.

the various stem alternants that many German verbs display (Wunderlich and Fabri 1995: 255–60; Bittner 1996: 187–94). But that is not a crucial omission because, as we have already seen in relation to Dhaasanac and Italian, similarity between lexemes with regard to stem distribution does not presuppose that the lexemes' stem alternants should exhibit similar phonological relationships. What I have concentrated on is illustrating again one way in which (I suggest) a synonymy-averse brain may impose order on different forms of the same item—namely, by relating the forms to one another, as signifiers and things signified, by means of a paradigm structure condition that is more elaborate than the Russian one at (17), but still respects synonymy avoidance.

6.3.5 *Case study 9: Stem and affix interactions in Polish*[20]

In contrasting Russian with Dhaasanac and Italian, I said that the tightest kind of implicational relationship between stem alternants is bidirectional, yielding complete uniformity of distribution, but unidirectional implications may nevertheless be sufficient for the brain's purposes. German verbs are similar to Russian nouns in that the relevant implications are unidirectional. The same applies in a fragment of Polish morphology that we are about to discuss. However, in Polish, stem alternation is linked with affix distribution so as to build a synonymy-avoidance structure that appears at least as robust as what we observed in Italian.

A nice consequence of the analysis that I will propose is that it explains the strange-seeming confidence with which Polish speakers choose between competing vocative suffixes for nouns denoting inanimate objects. This is an aspect of nominal morphology on which a child's linguistic environment will normally supply almost no evidence, the practical usefulness of such vocatives being close to zero. But, after all, according to the position defended in this book, there is no requirement that the ways in which morphology differentiates potential synonyms should yield a communicatively or cognitively useful outcome.

The competing vocative suffixes for masculine nouns are -*e* and -*u*, as shown in a provisional presentation of distinct classes for the singular inflection of masculine nouns in Table 6.5, based on Teslar (1957).[21] Phonetic symbols helpfully draw attention to certain stem alternations in the vocative or locative. I called this 'a provisional presentation'. That is just as well, because if Table 6.5 were the last word on Polish masculine nouns, then the hypothesis

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Table 6.5. Inflection of Polish masculine nouns (singular only)

	Class 1	Class 2	Class 3	Class 4
Nom	profesor	Polak	pies	pan
	'professor'	'Pole'	'dog'	'mister'
Gen	profesor a	Polak a	ps a	pan a
Dat	profesor owi	Polak owi	ps u	pan u
Instr	profesor em	Polaki em	ps em	pan em
Loc	profeso[ʒ] e	Polak u	p[ɕ] e	pan u
Voc	profeso[ʒ] e	Polak u	p[ɕ] e	pa[ɲ] e

	Class 5	Class 6	Class 7
Nom	kupie[ts]	dw[u]r	kraj
	'merchant'	'manor'	'country'
Gen	kup[ts] a	dwor u	kraj u
Dat	kup[ts] owi	dwor owi	kraj owi
Instr	kup[ts] em	dwor em	kraj em
Loc	kup[ts] u	dwo[ʒ] e	kraj u
Voc	kup[tʃ] e	dwo[ʒ] e	kraj u

that inflection-class organization obeys constraints on multivocabulism would be severely dented. The nominative has no suffix and the instrumental has consistently the suffix *-em*, but the other four cases are each represented by two suffixes, distributed among the seven inflection classes in such a way that none of them counts as a class-default (as defined in Chapter 5). Vocabular clarity is nowhere to be seen, apparently.

Associated with this problem is that of the stem alternations in the locative and vocative. The [ɕ] and [ɲ] that we observe in the vocative forms *p[ɕ]e* and *pa[ɲ]e* (orthographically *psie* and *panie*) look like palatalized counterparts of [s] and [n] respectively. Historically speaking they are indeed products of a phonological process of palatalization before non-low front vowels including the mid-front vowel [e]. So are we dealing with genuinely distinct stem alternants here, or does a synchronic phonological process of palatalization allow us to derive the surface forms from a single underlying phonological representation?

declension class, are not considered here. The plural is omitted because the problematic characteristics of these nouns are concentrated in the singular. Besides, there is strictly speaking no masculine plural gender in Polish; rather, there is a virile gender (for nouns denoting human males) and a non-virile gender (for all other nouns). The accusative is omitted because, as in Russian, it is always the same as the genitive (for animates) or the nominative (for inanimates).

The spelling *Polakiem* (not **Polakem*) indicates a pronunciation [pɔˈlacɛm], not **[pɔˈlakɛm]. This reflects the fact that underlying /ke/ always surfaces phonetically as [cɛ] except inside a few unassimilated loanwords (Gussmann 2007: 56, 65). Consequently the stem alternation between [pɔlak] and [pɔlac] (and analogous alternations involving stem-final velar plosives) will be treated as superficial and ignored.

Much hangs on the answer to this question. Let us suppose that *pan* and *pa*[ɲ], along with their counterparts in classes 1, 5, and 6 at Table 6.5, really are distinct stem alternants on the lines of [fuɡɡ] and [fuddʒ] from Italian FUGGIRE 'flee', [yes] and [ces] meaning 'kill (perfective)' in Dhaasanac, and so on. Then it is possible that the vocabular unclarity at Table 6.5 may be reduced or eliminated, inasmuch as the locative and vocative suffixes *-e* and *-u* may be differentiable by reference to the stem alternants that they accompany rather than the inflectional 'vocabularies' that they belong to. That is, rather than *-e* triggering palatalization in the stem, it may be the stem alternant that triggers the selection of *-e* rather than *-u*. But the case for an analysis on those lines depends on the strength of the rival case for a phonological analysis requiring only one underlying stem shape for each noun. And analyses of this second kind have had plenty of defenders. As Halle and Marantz (2008: 66) put it: 'Traditionally this alternation in stem form for the cases expressed by *-e* has been explained by referring to the rule of Polish phonology that spreads the feature [−back] from *-e* to the immediately preceding consonant'. So, if an analysis of this kind can be made to work, then an alternative that will 'save' the multivocabular approach will at best be just one of two alternatives of roughly equal plausibility. Thus it behoves us to consider carefully the possibility that is less welcome from our point of view.

As I have said, [ɕ] and [ɲ] look like palatalized counterparts of [s] and [n] respectively, being [+high] and [−back]. But the [ʒ] and [tʃ] that we observe in *profeso*[ʒ]*e* and *kup*[tʃ]*e* (orthographically *profesorze* and *kupcze*) are not quite so obviously analysable as palatalized counterparts of [r] and [ts]. It seems even less natural to regard [l] as a phonologically palatalized counterpart of [w]; yet stems ending in [l] replace stems ending in [w] in 'palatalizing' contexts, as with *generale*, the locative form of *genera*[w] 'general'. This reflects the fact that, as all describers of Polish acknowledge, 'palatalization' as a synchronic phenomenon is exceedingly complex. However, let us assume that our theory of phonology can accommodate these complexities. Then four conditions, if they are satisfied, will incline us towards accepting a synchronic phonological analysis of the locative-vocative stem alternants in Table 6.5:

(45) a. Other suffixes beginning with *-e*, not just the vocative and locative *-e*, trigger the same palatalization.
      b. Locative-vocative *-e* (and its rival *-u*) are not choosy about the phonological shape of the stems to which they attach. The only difference between them is that *-e* triggers palatalization, if that is phonologically possible, while *-u* does not.

    c. Any locative or vocative suffix *-e* that appears with nouns other than
the masculines at Table 6.5 has the same palatalizing effect.

    d. Palatalization is productive, occurring in new coinings and loan-
words as well as in established and native vocabulary.

But, as I shall show, only (45d) is satisfied; (45a–c) are not.

Condition (a) is not satisfied because of the instrumental forms at Table
6.5. The instrumental suffix *-em* does not trigger the same palatalization as the
locative-vocative *-e*—indeed, it triggers no palatalization at all except in
Polakiem (discussed in n. 21). This leads the Polish phonologist Edmund
Gussmann to conclude (2007: 114): '... the vowel [ɛ] found in the desinence
-em leaves the preceding consonant intact, while an identical vowel in the
desinence *-e* appears to palatalize it. Unless we are prepared to uphold the
patently absurd statement that it is the presence of the final nasal that inhibits
the palatalization of the stem-final consonant, or something equally implaus-
ible, we must conclude that the frontness of the vowel and palatalization of
the consonant are separate issues.'

Condition (b) is not satisfied, because the two rival suffixes *-e* and *-u* are
indeed choosy about the phonological shape of the stems to which they
attach. In particular, *-e* is never attached to a stem ending in a velar nor to
a stem that is already 'palatalized' (Gussmann 2007: 110–11). Because of this
second restriction, stems such as *ko*[ɲ] 'horse' and *li*[çtç] 'leaf' choose *-u*.
This is particularly puzzling. Why should 'palatalized' stems be unfriendly
towards a palatalizing suffix? Granted, palatalization would have no visible
effect on such stems—but in generative phonology there has never been
any objection to the so-called 'vacuous application' of phonological
processes.

Condition (c) is not satisfied, because *-e* appears as a locative suffix also on
nouns (nearly all feminine) with nominative singular *-a*, yet with different
palatalizing effects. Consider (46) (based on Gussmann 2007: 106–7):

(46)   *nominative singular*   locative singular
        Polk a 'Polish woman'   Pol[ts] e
        wst[ɛɲ]g a 'ribbon'   wst[ɛn]dz e

As we have just noted, the locative suffix *-e* is never attached to masculine
nouns ending in velars, but with nouns in the *-a* class it is subject to no such
restriction—provided we assume that the [ts] and [dz] in the locative
examples at (46) are genuinely due to a phonological palatalization process
affecting underlying stems terminating in /k/ and /g/ respectively. Yet this
assumption leads to a paradox. From a phonological point of view, it makes

no sense for a particular set of nouns (namely, most masculines) to be averse to one manifestation of phonological palatalization but not to others.

In the light of all this, it does not seem surprising that Gussmann should conclude (2007: 102): 'Palatalization is not a live phonological regularity because there is no systematic connection between the appearance of palatalized consonants and the environment.' On the other hand, the view that most palatalization in Polish is synchronically a morphological rather than a phonological phenomenon—that is, that palatalized and plain stem alternants have distinct underlying phonological representations—seems at first sight hard to reconcile with the fact that condition (45d) is satisfied: at least some kinds palatalization are fully productive. Even foreign names such as *Carter*, *Nixon*, and *Ford* have palatalized alternants before the locative -*e* suffix, so as to yield *Carte*[ʒ]*e*, *Nixo*[ɲ]*e*, and *For*[dʑ]*e* (contrasting, as expected, with absence of palatalization in the instrumental forms *Carterem*, *Nixonem*, and *Fordem*) (Rubach 1984: 64). Also, for example, English *chat* in its Internet sense, borrowed into Polish as *czat* [tʃat], has a locative singular form [tʃatɕɛ] (orthographically *czacie*), not *[tʃatɛ] (Gussmann 2007: 78).

Except as regards condition (d), then, the case for synchronic phonological palatalization as the source for the 'palatalized' locative and vocative stems in Table 6.5 is weak. So does the morphological alternative (recognizing the stems as phonologically distinct) merely avoid the drawbacks of the phonological approach, or does it have positive explanatory merits? I will argue for the latter.

Let us suppose that, in contemporary Polish, the historical process of palatalization has undergone a reanalysis: it is not that the suffix -*e* in the locative and vocative triggers changes in the stem, but that stem alternants of a particular kind trigger the choice of -*e*. Of what particular kind, then? It cannot be palatal alternants, for a reason that we have just noted: stems that consistently end in a palatal or 'palatalized' consonant choose -*u*. But that word 'consistently' supplies a clue. Inspection of Table 6.5 reveals that a locative or vocative form has -*e* precisely when the stem it is attached to is different from the other stem alternant (or alternants). And this pattern is consistent for all masculines falling within the ambit of Table 6.5. The noun POLAK does not choose -*e* because it does not have a distinct stem alternant such as *Pola[ts] to which -*e* might attach. The noun PAN has -*u* in the locative and -*e* in the vocative because in the vocative (but not the locative) this noun has a special stem alternant *pa*[ɲ]. The noun PIES has -*e* in both the locative and vocative because it has a special stem alternant *p*[ɕ] in both those cases. And so on.

This observation casts new light on the problem of how -*e* and -*u* avoid synonymy. They do so not through membership in distinct affixal vocabularies, but through choosing distinct stem alternants, as in (47):

(47)   -e   'locative/vocative, special stem alternant'
        -u   'locative/vocative, elsewhere'

Thus the fact that two nouns differ in respect of their vocative or locative suffixes is not by itself a reason for assigning them to distinct inflection classes. The seven classes at Table 6.5 need to be re-examined. The outcome is as in (48):

(48)   1, 2, 5   3, 4   6, 7
        Nom   Ø   Ø   Ø
        Gen   a   a   u
        Dat   owi   u   owi
        Instr   em   em   em
        Loc   e∼u   e∼u   e∼u
        Voc   e∼u   e∼u   e∼u

Seven classes are reduced to three. The effect is that the apparent vocabular blurring so evident in Table 6.5 disappears entirely. In the genitive, -*u* identifies its class while -*a* is the class-default. In the dative, -*u* again identifies its class while -*owi* is the class-default. The nominative is realized by no affix at all, the instrumental by just one affix, and the locative and vocative by a pair of affixes that divide their labour on a syntagmatic rather than a paradigmatic basis. So far as the affixal inflection of these Polish nouns is concerned, no better outcome could be hoped for.

I am suggesting, then, that what triggers -*e* is just the fact that the stem alternant is different from the other singular alternant(s) in SOME way, rather than that it is different in a particular way. If that is correct, we will not be surprised to find instances where the special locative-or-vocative alternant differs from the majority alternant in other ways than merely having a 'palatalized' final consonant. And that is in fact the case. Here are some instances:

(49)   *nominative*   locative
        a. obiad 'dinner, nom.'   obie[dʑ]e
        b. [ɕ]wiat 'world, nom.'   [ɕ]wie[tɕ]e
        c. ko[ɕtɕ]o[w]a 'church, gen.'   ko[ɕtɕ]ele
        d. k[ɕɔn]dz 'priest, nom.'   k[ɕɛ̃ʒ]e

The alternations in (49c, d) are practically unique, but those in (49a, b), involving *a* and *e*, have a number of parallels where the consonant preceding *a*

is palatalized. All the same, there are exceptions, such as [ɕ]*lad* 'trace', whose locative is [ɕ]*la*[dʑ]*e*, not *[ɕ]*le*[dʑ]*e (Gussmann 2007: 253-4, 257). We will also expect differences in affixal inflection on the part of any doublets akin to *brothers* and *brethren* in English, that is, any nouns which have different meanings correlated with stem choice. One such pair is ʟɪ[ɕtɕ] 'leaf' and ʟɪsᴛ 'letter'. Historically these were once the same noun, but they have diverged morphologically as well as semantically: the former now has only one stem, *li*[ɕtɕ], while the latter has two, palatalized *li*[ɕtɕ] and plain *list*. As we would predict, the locative of ʟɪ[ɕtɕ] is *li*[ɕtɕ]*u* while that of ʟɪsᴛ is *li*[ɕtɕ]*e*. This divergence is inexplicable in a purely phonological account of these stem alternations.

I have still to explain what differentiates the stem alternants themselves, if phonological palatalization does not account for them. At least part of the answer is supplied at (47), where I glossed *-e* as 'locative/vocative, special stem alternant'. Recall that differentiation can be achieved not only through what a form signifies (as, for example, umlauted stems in German nouns, as counterparts of plain stems, signify 'plural'), but also through a form's role as something signified (as early stress in the nominative plural, within Russian's Two-Early stress pattern, is what is signified by early stress in the accusative singular). To ensure that (for example) the alternant *pan* is not synonymous with *pa*[ɲ], nor *profesor* with *profeso*[ʒ], it is sufficient that *pa*[ɲ] and *profeso*[ʒ] mean respectively not just 'mister' and 'professor' but 'mister, alternant signified by *-e*' and 'professor, alternant signified by *-e*'.

Admittedly, this situation reminds one of a dog chasing its tail. It is the stem alternant that imposes the choice of *-e* (I am suggesting), yet what differentiates this stem alternant from others is precisely the fact that *-e* implies its presence. This is a bit like the German situation described in section 5.2.3: *-e* as a plural suffix on feminine nouns signifies an umlauted stem, while the umlauted stem signifies not only (as usual) 'plural' but also the presence of the suffix *-e* rather than the more usual feminine plural suffix *-en*. The circularity would be of concern, perhaps, if we had good reason to think that morphology was designed for the efficient and economical coding of information; but I emphasize once again that, if morphology arose as a conglomeration of synonymy-avoidance devices, the circularity is no surprise.

That is not all there is to say about 'palatalized' stem alternants, however. Inspection of Table 6.5 reveals a one-sided relationship between the locative and the vocative. A noun can have a special stem alternant in the vocative only, as ᴘᴀɴ and ᴋᴜᴘɪᴇᴄ do, but no noun has a special stem alternant in the

locative only. This should put readers in mind of stress distribution in
Russian: a noun can have non-number-related early stress in the nominative
plural only or in both the nominative plural and the accusative singular, but
not in the accusative singular only. That situation was represented by means
of an implication, which (it was suggested) is part of the repertoire of devices
employed by Russian speakers' brains to differentiate early-stressed and late-
stressed alternants in those nouns that have them. A parallel implication can
be posited for Polish:

(50)   Special stem in locative ⊃ special stem in vocative

At (51) is a table showing which inflection classes in Table 6.5 display each of
the special-stem possibilities. This illustrates again the relative independence
of affixal and non-affixal inflection that our evolutionary account has led us
to expect—while at the same time illustrating that this independence can be
overridden when syntagmatic factors need to be exploited for differentiation,
as with *e* and *u*:

(51)

	Polish masculine singular nouns		
	Two-Special: classes 1, 3, 6	One-Special: classes 4, 5	No-Special: classes 2, 7
Nom[22]			
Gen, Dat, Instr			
Loc	1		
Voc	2	3	

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[22] Inspection of Table 6.5 shows that some Polish masculine nouns have special stem forms in
the nominative singular too. But some of these may be predictable phonologically, like the alternant
Pola[c] that appears instead of *Polak* before the instrumental suffix *-em*; for example, *pies* replaces
vowelless *ps* 'dog' in the suffixless nominative singular. In any case, the fact that these stem forms are
peculiar to one cell means that they are differentiable in terms of their grammatical content; for
example, *dw*[u]*r* 'manor, nominative' contrasts with *dwor* 'manor' elsewhere.

Again, as with Russian at (21), we can identify the factors differentiating the special stem as follows:

(52) Pattern   Box   Paradigmatic differentiating factor
      One-Special   3   'special stem in vocative' by default or as lexically
                           specified[23]
      Two-Special   1   'special stem in vocative' (by (50))
      Two-Special   2   implied by (50) on the basis of box 1

The right-hand column in (52) is labelled 'Paradigmatic differentiating factor', whereas the corresponding column in (21) is labelled only 'Differentiating factor'. The reason is that in Polish, unlike Russian, special or 'palatalized' stem alternants are differentiated syntagmatically too, as being part of what *-e* signifies but *-u* does not. Polish thus exhibits a kind of belt-and-braces arrangement: the special stem is doubly insured against synonymy with the plain stem, through both a paradigmatic implication at (50) and a syntagmatic link with the suffix *-e* at (47).

I mentioned at the beginning of this section the puzzling confidence with which Polish grammar books and reference works announce a vocative form even for nouns denoting inanimate objects. It is (50) that solves the puzzle. This implication serves the brain's need to differentiate special stems from plain stems; as a by-product, however, it supplies speakers of Polish with a degree of confidence in choosing between rival vocative suffixes that, given the inflectional diversity at Table 6.5, seems at first sight baffling.

The words 'by default' in (52) bring us at last to the only condition, of the four set out in (45), that seems to favour a synchronic phonological account of stem alternation: its productivity. Gussmann (2007: 110–11) describes the situation by saying that stems ending in velars and 'palatalized' consonants choose *-u*, whereas stems ending in labials and coronals choose *-e*. However, one can just as well say that stems ending in labials and coronals have a special alternant alongside a plain one. In discussing (49) we noted that special alternants do not have to be related to their plain counterparts in predictable ways. In practice, however, most of them are—which is why Polish speakers have no difficulty in determining the shape of the expected special alternant for foreign names and loanwords such as *Nixon*, *Carter*, *Ford*, and *czat* 'internet chat'. But the existence of two alternants for these nouns, conforming to a consistent phonological pattern, need not be seen as any more problematic than the existence of two forms, singular and plural, for count nouns in English. The relationship between the plural form *cats* and the

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[23] The implication of the words 'by default' will be explained shortly.

singular form *cat* is perfectly regular, yet no one suggests on that account that they are related by some synchronic phonological process. Equally, there is no reason to assume that the perfectly regular relationship in Polish between *Nixon* and *Nixo*[ɲ] is due to a synchronic phonological process. The two cases appear different, perhaps, because *cats* has a suffix, with an obvious meaning and syntactic relevance. But everyone agrees that not all morphology involves suffixation, so the lack of a suffix on *Nixo*[ɲ] tells us nothing about whether we are dealing with phonology or morphology. And the lack of any obvious meaning or syntactic relevance in the 'palatalization' of *Nixo*[ɲ] does not mean that it is entirely functionless. To understand its function requires only (I suggest) an understanding of morphology's role as a toolkit for synonymy avoidance.[24]

The default expectation, then, is that stems ending in labials and coronals will have two stem alternants: a plain one and a special one, to be used in the locative. In virtue of (50), this will be used in the vocative too. But the term 'default' implies the possibility that it may be overridden. There are indeed some nouns in which it is overridden: frequently occurring nouns, as one would expect, inasmuch as their unpredictable behaviour must be learned by positive evidence. The nouns SYN 'son' and DOM 'house', despite ending in a coronal and a labial respectively, have only one stem form; consequently, as we would predict, their locative and vocative forms are *synu* and *domu*. The nouns PAN and KUPIEC (and perhaps one or two others, though this pattern seems to be obsolescent) have two alternants, but do not use the minority alternant in the locative; they must be lexically specified as using it in the vocative only. Again, this irregular behaviour requires positive evidence in order to be learnable, so it is hardly surprising that these nouns denote human beings and are thus nouns whose vocative case form children are likely to encounter.[25]

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[24] In having regularly two stem alternants, Polish masculine nouns that end in labials and coronals resemble intriguingly most verbs in the Swiss Rumantsch language Surmiran (Anderson 2008). In Surmiran, as in Polish, the two alternants usually resemble one another in systematic ways, but in some words (again, as in Polish) they differ unpredictably. What determines the choice between the alternants in Surmiran, however, is not affixal allomorphy but rather the position of the stress. Surmiran is thus Italian-like in respect of how the alternants are distributed, but Polish-like inasmuch as the sort of lexeme that is traditionally seen as phonologically well-behaved—namely, lexemes with only one phonological representation—are unusual.

[25] I mentioned earlier Halle and Marantz's (2008) remark about the traditional reliance on phonology to explain Polish 'palatalization' synchronically. They themselves favour a phonological account, and take issue with Cameron-Faulkner and Carstairs-McCarthy (2000) on a number of points. In contrast to us, Halle and Marantz encode inflection class membership through combinations of binary features, a tactic that will be discussed in Chapter 8. For the present it suffices to point

I will offer a final comment on the history of the -*u* suffix. Originally this was not a suffix but a part of the stem, in nouns of the so-called *u*-stem type that are traceable to Indo-European. The Latin inflection class represented as H at Table 5.3 preserves this pattern, although precariously; many of its members defected to the so-called *o*-stem nouns (column B at Table 5.3), and it has left no trace in Romance languages. In Russian the -*u* suffix remained as a solitary remnant of the old pattern, just as in Polish, competing with -*e*. However, the way in which Russian chose to differentiate the two rivals could hardly be more different. What Russian did is something that to many linguists may seem more 'natural': it used the formal contrast to express a semantic contrast (although with a few lexemes only), so that for example *v sadú* 'in the garden' (denoting a physical location) has a different suffix from *o sáde* 'concerning the garden' (denoting something more abstract). This is a nice example of what Lass (1990) calls the 'exaptive' treatment of linguistic 'junk': a formal difference that has lost its original motivation is put to a new use.

Polish puts the *u*∼*e* contrast to a new use also. What is so striking about the Polish solution, however, is how entirely intramorphological this new use is. Nothing about the *u*∼*e* distribution or the distribution of palatalized and plain stem alternants expresses anything that could not have been expressed in a pseudo-Polish where every masculine noun has a single stem alternant and there is only one locative-vocative suffix. Yet writers on Polish report no tendency for Polish to change in that direction. The connection between the rival suffixes and the rival stems (for nouns that have them) seems remarkably tight. It is evidently readily learnable. But if morphological learnability is tied to synonymy avoidance as much as to syntactic or semantic content, that is not so surprising as it would otherwise be.

6.4 Summing up: The importance of non-affixal morphology

I noted in Chapter 2 that John J. McCarthy (1981) introduced to the linguistic world the term 'nonconcatenative morphology'. This reminded us neatly that

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out two obstacles to accepting the Halle-Marantz account. First, Halle and Marantz fail to observe (or at least fail to comment on) the fact that all masculine nouns with a single stem throughout the singular (even a palatalized one) select the locative-vocative -*u* suffix. In their analysis, this appears to be a pure coincidence. Secondly, they regard the implicational relationship between vocative and locative forms as a depending not on the stem (as at (50)) but rather on the suffix: 'speakers that hear a form with -*u* in the vocative can predict . . . that the noun will also take -*u* in the locative' (2008: 70). This seems to imply that, for example, a child learns the locative of ʟɪ[ɕtɕ] 'leaf' as *li*[ɕtɕ]*u* rather than *li*[ɕtɕ]*e* on the basis of hearing a vocative form *li*[ɕtɕ]*u* 'O leaf!', and likewise for other inanimate masculine nouns with locative -*u*. That is hardly plausible. These two obstacles, along with the one discussed in Chapter 8, are individually formidable; taken together, they seem insurmountable.

we are used to thinking of ordinary common-or-garden morphology as involving the linear concatenation of roots and affixes. What's more, it is among the affixes that we expect the action to take place; the root or stem, by contrast, is expected to be relatively inert. Even when stems vary in shape, this is often analysed as a mere by-product of accompanying affixal behaviour. Indeed, it is possible to rig one's description so that changes in stems are always by-products of affixation, if (for example) one analyses *gave* as consisting of the root *give* plus a zero past-tense suffix that triggers vowel change.

In this chapter, by contrast, it is roots or stems that have been to the fore, as descendants of selective items in protolanguage. I have argued that the way they behave (in particular, the way stems vary in shape) is both more interesting and more narrowly constrained than is usually thought. Inevitably, the range of evidence that we have looked at is only a tiny fragment of the evidence that is potentially relevant. Whether the kinds of constraint that I have suggested will be confirmed by further evidence remains to be seen. But there are hopeful signs. I will mention a couple of them briefly. There is a pattern of verbal stem alternation in Amharic that seems likewise to lend itself to analysis in terms of a multi-stage paradigm structure condition (Trommer 2008). (In Trommer's own analysis, feature-geometric links play the part of Wurzel-style implications). And nouns in Estonian comply with the No Blur Principle (and hence with vocabular clarity) in respect of their affixal inflection, even while exhibiting a profusion of stem shapes (James Blevins 2004, 2005). So do stem-distribution patterns in Estonian conform to paradigm structure conditions that are unmarred by synonymy? This remains to be explored, but there is room for optimism if, as Blevins puts it (2004: 89), 'a single leading entry determines the full paradigm of most open-class nouns'. As for the question of the relationship between affixal and non-affixal inflection, Blevins sidesteps it by concentrating on inflected word forms as wholes. However, that sidestep may prove unhelpful if (as I suspect) the question turns out to have an interesting answer, compatible with the approach developed here. So I hope that others may decide that this area of inquiry deserves more attention than it has traditionally received.

The discussion in Chapters 5 and 6 has implications for a longstanding bone of contention in morphological theory, particularly as regards inflection: which is more central, the paradigmatic relationship between (say) plural *girls* and its singular counterpart *girl*, or the syntagmatic relationship between the root *girl* and the plural affix *-s*? The paradigmatic relationship is to the fore in so-called 'Word-and-Paradigm' approaches to morphology, whose advocates (e.g. Anderson 1992; Blevins 2006) point out that it is often difficult to match up morphological functions neatly with affixes. (For

example, in *children* 'child.PLURAL', where does the stem cease and the affix begin?) The syntagmatic relationship is to the fore in what Hockett (1954) christened 'Item-and-Arrangement' approaches, whose advocates (e.g. Lieber 1992) argue that affixes differ from stems only in requiring a stem for support. It emerges now that both approaches are partly right. Affixes do indeed deserve to be recognized as more than just parts of inflected word forms, inasmuch as they can be sorted into distinct 'vocabularies' corresponding to inflection classes. But the paradigmatic relationship between inflected forms of the same lexeme is indeed central to understanding one way in which distinct stem alternants avoid synonymy.

I will suggest in the next chapter reasons for thinking that the study of not only inflectional but also derivational morphology may benefit from our evolutionary perspective. Derivation too has puzzling features, more of them than are immediately evident; but these too may turn out to be less puzzling when their possible protolinguistic ancestry is considered.

7

Derivation, compounding, and lexical storage

7.1 Two gaps in coverage

In Chapter 3, when discussing protolanguage, I introduced the idea of a cliché pattern. For example, items denoting an action or process (it would be anachronistic to say 'verbs') could often have been collocated with an item meaning 'earlier' (it would be anachronistic to say 'a past-tense marker' or even 'an adverb indicating past time'). I suggested that two or more cliché patterns might be related as alternatives within a menu of cliché patterns: for example, 'ACTION-OR-PROCESS earlier' and 'ACTION-OR-PROCESS later' might constitute such a menu. Chapters 5 and 6 have been devoted to exploring how, within cliché-pattern menus, distinct items or distinct forms of the same item may avoid synonymy through syntagmatic and paradigmatic devices similar to devices that distinguish apparently synonymous words in languages today. And I have argued that, seeing things in this light, we can make sense of certain otherwise puzzling aspects of how morphology works, as well as of the very existence of morphology in contradistinction to syntax.

Nevertheless, there have been yawning gaps in my coverage. Firstly, those 'certain otherwise puzzling aspects' have been limited to areas of morphology that would traditionally be regarded as inflectional rather than derivational. So what about derivation—not to mention compounding, which also conventionally belongs to morphology rather than syntax? Secondly, we noted in section 2.6 that there is in principle no reason why a complex item with an internal structure that is 'lexical' in the sense of being morphological rather than syntactic should also be 'lexical' in the sense of requiring to be listed, or vice versa. Why, then, is there in practice so much overlap between the two classes of items—enough overlap to camouflage the traditional ambiguity?

The aim of this chapter is to suggest answers to these questions. A possible answer to both turns out to flow naturally from the perspective on morphological evolution developed in previous chapters. This is another consideration in its favour.

7.2 Clichés outside cliché patterns

When I introduced the notion 'cliché pattern' I passed rapidly on to the notion 'cliché-pattern menu', even while acknowledging that not all cliché patterns might cluster together in menus. Consider, for example, a subset of ACTION-OR-PROCESS items, namely ACTION items. In Chapter 3 I give examples involving four ACTION-OR-PROCESS items, namely *sleep*, *wake*, *eat*, and *die*. Of these, only *eat* can plausibly be said to denote an action involving an agent and something acted upon. Thus *eat stuff* might be a plausible protolanguage collocation to express 'stuff to eat, i.e. food', whereas *sleep stuff*, *wake stuff*, and *die stuff*, while well-formed as protolanguage expressions, seem less likely to occur, even if with ingenuity one may be able to think up appropriate contexts for them. In this respect they resemble the collocations *stream brother* and *wake pond* that I cited in Chapter 3: it is difficult to envisage for them plausible occasions of use.

I say 'seem less likely to occur', not 'could never occur'. This is in recognition of the intrinsic fluidity of protolanguage. It is conceivable, for example, that within some protolinguistic community *die stuff* might become the conventional way of referring to anything mortally dangerous, such as a treacherous bog or a cave with a gradually collapsing roof. Likewise, given an ACTION item *hunt*, the collocation *hunt stuff* might conventionally designate game suitable for hunting. The item *hunt* would also lend itself to use in an expression such as *hunt person* meaning 'hunter'.

The expression *hunt person* may seem to bring us within reach again of a possible cliché pattern. Alongside *hunt person*, it is easy to envisage *speak person*, *sing person*, or *tree person* becoming institutionalized to refer to people who are recognized as good at speaking, good at singing, and knowledgeable about trees (or perhaps good at climbing them). But could there be cliché patterns that these expressions exemplify—cliché patterns of the form 'X *stuff*' or 'Y *person*'? This amounts to asking about semantic characteristics of items that would be plausible fillers for the 'X' and 'Y' positions, and their relationship to the unselective elements *stuff* and *person*. Do likely fillers for X and Y fall into semantically homogeneous categories similar to ACTION-OR-PROCESS or ACTION, and is their semantic relationship to *stuff* and *person* consistent enough to justify our talking of a pattern?

This is a tricky question to answer. The answer that I am about to give is tentative. In saying this, I am aware that some indignant readers will at once think: 'A tentative answer about a hypothetical variety of protolanguage? That goes beyond the bounds of worthwhile speculation, even within a field of

inquiry so intrinsically speculative as language evolution!' But, paradoxically, that very reaction goes a considerable way towards establishing the point that I want to make. This is because such a reaction was much less likely to be provoked by the cliché patterns that I introduced in Chapter 3: 'ACTION-OR-PROCESS *earlier*' and 'ACTION-OR-PROCESS *later*'. The protolanguage items *earlier* and *later* are unselective (that is, they display juxtapository productivity), in that it is easy to imagine them figuring in many straightforwardly interpretable collocations. What's more, the classes of items with which *earlier* and *later* readily collocate are the same. Thus the cliché patterns that they belong to are part of the same cliché-pattern menu. And I suggested in Chapter 5 that multiple vocabularies within the domain of a single cliché-pattern menu could provide a cognitive device for differentiating potential synonyms. But if for *stuff* and *person* we cannot discern a clear route towards their participation in cliché-pattern menus, then any synonyms for *stuff* and *person* that may develop (through phonological processes, for example) will not be 'saved' through a multivocabular analysis. Furthermore, if the more selective items that collocate with *stuff* and *person* are also reshaped through phonological processes, the fact that their new shapes are not ranged alongside other shapes in a single cliché-pattern menu will make it less easy for the brain to recognize them as different forms of the same item. The brain, seeking as usual a remedy for apparent synonymy, will have to find some other way forward.

After that apologetic preamble, I now offer my tentative answers. I mentioned *eat stuff* and *hunt stuff* as plausible collocations to express 'food' and 'game for hunting'. On that basis, one might guess that they exemplify a cliché pattern 'ACTION *stuff*'. In that case, *stuff* should be readily combinable with other ACTION vocabulary items of protolanguage, such as (let's assume) *put-down*, *kick*, *drop*, and *run*.[1] Recall however the requirement that in a cliché pattern the meaning relationship between the selective and unselective elements should be consistent. A consistent meaning relationship is indeed exhibited by *hunt earlier*, *put-down earlier*, *kick earlier*, and so on. But what plausible consistent meaning relationship exists for *put-down stuff*, *kick stuff*, *drop stuff*, and *run stuff*? The plausible interpretation 'food' for *eat stuff* derives from the interest that all humans have in what they eat, and their conventional notions (differing from one culture to another) of what counts as food. But we are not similarly interested in things that we put down, kick, or drop; and *run stuff* presents further problems, since *run* denotes an action

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[1] The hyphen in *put-down* is meant to indicate that it stands for a single unanalysable vocabulary item in protolanguage.

that typically does not affect any object. So, although *eat stuff* and *hunt stuff* might acquire conventional meanings (they might become clichés, in fact), they would do so in relative isolation, not conforming to any semantically consistent cliché pattern.

As for *person*, we have so far considered *hunt person*, *speak person*, *sing person*, and *tree person* as collocations illustrating something like the consistent interpretability exhibited in *eat earlier* and *die earlier*. We may be tempted to see most of these *X person* collocations as exemplifying a cliché pattern 'ACTION *person*'. (I say 'most of these' because *tree* clearly does not belong in the ACTION category.) Such a cliché pattern may seem more plausible than 'ACTION *stuff*' for the following kind of reason. As we have just noted, it is difficult to visualize an occasion on which *drop stuff* might be used with an interpretation parallel to *eat stuff* and *hunt stuff*. On the other hand, it is less hard to visualize an interpretation of *drop person* that would be more or less parallel to that of *sing person*. If a *sing person* is someone who sings, then a *drop person* will be someone who drops things. But recall that I glossed *sing person* earlier as 'someone who is good at singing'; and that is not quite the same as 'someone who sings'. On the other hand, it is not likely that there would be much occasion among protolanguage speakers, any more than among modern humans, to use an institutionalized expression with the literal meaning 'someone who is good at dropping things'. (I may say to you: 'Oh, you can rely on Fred to break your vase, he is good at dropping things'; but you will know that I wish to be understood ironically.)

What this illustrates is that an institutionalized cliché pattern 'ACTION *person*', even if it is more likely than 'ACTION *stuff*', is liable to be semantically fluid in a way that 'ACTION *earlier*' and 'ACTION *later*' are not. It is not that the vocabulary item *person* is selective with respect to the action terms that it can be collocated with. It is rather that the circumstances of language use in any conceivable human society, where people's skills and habits are admired or despised and applauded or condemned, will make it hard for collocations of the form 'ACTION *person*' to maintain the sort of bland semantic uniformity that a cliché pattern requires. What's worse: this semantic fluidity spawns new synonymy risks. A person who is good at singing is admired; a person who is prone to dropping things is blamed or pitied. Therefore the collocation *sing person*, once it is institutionalized, will in practice mean the same thing as *sing expert*, while *drop person* will in practice mean the same as *drop clumsy*. (We must of course pay no attention to the fact that, in English, *person* is a noun, *clumsy* is an adjective and *expert* can be either. Their protolanguage counterparts do not differ in word class because 'word class' is a syntactic notion, not yet applicable.) How can such synonymy be avoided?

At this point it is worth stepping back to contrast the situation of *sing expert* and *sing person*, as potential synonyms, with that of some potential synonyms that we encountered in Chapter 3. The three potential synonyms *sleep$_1$*, *sleep$_2$*, and *sleep$_3$* at (17) in Chapter 3, being embedded in a cliché-pattern menu, would have been interpretable by the brain as distinct forms of the same item, and differentiated either semantically, syntagmatically or paradigmatically, in one of the ways discussed in Chapter 6. The four potential synonyms *earlier$_1$*, *earlier$_2$*, *earlier$_3$*, and *earlier$_4$* at (18) in Chapter 3, being similarly embedded, could be differentiated as belonging to distinct vocabularies, as outlined in Chapter 5. But neither remedy is available to mitigate the synonymy risk presented by *sing expert* and *sing person*. Even though *expert* and *person* are clearly not synonymous, the availability of not one but two collocations as plausible protolanguage expressions for 'person who is good at singing' is an embarrassment.

What choices does the brain have left? In respect of potential synonyms such as *sleep$_1$* and *sleep$_2$* and likewise *earlier$_1$* and *earlier$_2$*, one possible remedy was always to jettison all but one of the alternatives. That is a brute-force solution that, thanks to the brain's cognitive resourcefulness, is often avoidable—and is remarkably often avoided. It is this resourcefulness that (according to my argument) brought morphology into existence. Nevertheless, where cliché-pattern menus do not come to the rescue, the brute-force solution may be the only one available. Consequences of this will be discussed in the next section.

7.3 Individually memorized collocations

Let us imagine a protolanguage speech community in which *sing expert* is the collocation habitually used for 'person who is good at singing'. Brute-force synonymy avoidance banishes *sing person* as an alternative in this community, notwithstanding that *sing* and *person* crop up freely as vocabulary items in other contexts. These two items may still be collocated with each other, even, but in that case they must mean something different: for example, 'It's a person who is singing (not a bird or a gibbon)'. Synonymy avoidance thus leads to synonymy blocking, a notion familiar to morphologists since work by Aronoff (1976).[2] Two far-reaching consequences follow.

Firstly, the fact that *sing expert* is the conventional term for 'person who is good at singing', memorized as such by all members of a speech community,

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[2] Blocking is closely related to the 'Panini Principle', 'Elsewhere Condition', or 'Proper Inclusion Precedence Principle' proposed to regulate the application of phonological and morphological rules by Kiparsky (1973), Koutsoudas et al. (1974), and Anderson (1986).

tells us nothing about what will be the conventional term for 'person who is good at hunting'. That is because a cliché pattern 'ACTION *expert*' will have at least as much difficulty getting established as 'ACTION *person*' has. It will have more difficulty, in fact, because *expert* is collocationally more selective than *person* is; for example, people who are often sick or who habitually tell lies might in protolanguage be called *sick person* or *lie person*, but they would hardly be called *sick expert* or *lie expert*, except ironically. (It would be presumptuous to assume that a capacity for irony did not develop in humans until after the capacity for syntax.) So a community that has institutionalized *sing expert* may nevertheless institutionalize *hunt person* rather than *hunt expert* as the expression to be used for 'person who is good at hunting'. When collocations are institutionalized in the absence of a cliché pattern, the speech community makes each choice independently. This leads to arbitrary divergences in the coding of what is essentially the same concept (such as 'someone who is good at . . .').

In earlier chapters we have already encountered plenty of instances where interaction between articulatory developments and cognitive pressures yields seemingly arbitrary outcomes. We saw this, for example, in the account given in Chapter 5 of how distinct inflection classes arise. But the kind of arbitrariness illustrated by *sing expert* versus *hunt person* is new. Superficially, *expert* contrasts with *person* here in just the way that *earlier$_1$* contrasts with *earlier$_2$* in *sleep earlier$_1$* and *wake earlier$_2$* at (18) in Chapter 3. But the contrast between the institutionalized forms *sleep earlier$_1$* and *wake earlier$_2$* is not due to their being institutionalized separately, in isolation from other collocations which share with them either the first or the second element. Rather (according to the argument presented in Chapter 5) what was institutionalized in respect of *sleep* and *wake* was their respective collocation with a whole distinct vocabulary of items: *earlier$_1$*, *later$_1$*, and so on versus *earlier$_2$*, *later$_2$*, and so on. By contrast, the institutionalized collocations *sing expert* and *hunt person* depend for their maintenance purely on storage in the memory as individual items. In being stored in this way, they resemble any simple vocabulary item such as *sing* or *person* or *sleep* or *cave*. Yet, unlike simple vocabulary items, their relationship with their meanings is not arbitrary, at least at the outset. Semantically, both *sing expert* and *hunt person* are motivated. So long as the shapes of their two component items are not distorted into unrecognizability by phonological processes, they are easily interpretable by any protolanguage speaker who has not encountered them before—or rather, at least as easily interpretable as any protolanguage expression is, given the absence of any syntax to narrow down the range of potential meanings.

I said just now that the meaning of these institutionalized collocations would be motivated 'at least at the outset'. Certainly, phonological change of the kind that produced [ran] and [iɲu] from an earlier uniform shape [iranu], as in our hypothetical example in Chapter 3, might so distort one or both the components as to destroy this motivation, much as phonological changes in English *cupboard* have destroyed the original motivation that is still evident in the spelling. But there are other factors that would weaken motivation, as I will explain in the course of exploring how we might expect the just-described protolinguistic phenomena to be reflected in contemporary languages.

I have argued that the brain's distaste for perfect synonymy would have enforced a choice between collocations such as *sing expert* and *sing person*, and likewise between *hunt expert* and *hunt person*; however, because of the fuzziness of selectivity, these choices would have had to be made independently, without the guidance of clear-cut cliché patterns, let alone of cliché-pattern menus. If that is correct, then we will expect to find similar behaviour in some complex items in contemporary languages: that is, a similar paradoxical combination of motivatedness and randomness.

Consider now some verbs and related nouns in contemporary English, as presented at Table 7.1. All the nouns listed there end in one of the suffixes -*ment* or -*tion*. Nearly all these nouns denote the process or result of Xing, where X stands for a verb identical or similar in shape to the noun's stem. The stems of the nouns in the rightmost column differ in shape from the corresponding verb, but (except for *enunciation*) they do so in just one of two ways, illustrated in sections (a) and (b) respectively of Table 7.1. To that extent, all these nouns are motivated. Nevertheless, none of these nouns has both a meaning and a form so predictable that it does not need to be listed in a dictionary. One fact at least must be listed about each noun: the very fact that it exists, instead of or alongside a rival noun with the other suffix. This is so even if the meaning of the noun is exactly what one expects. For example, even though *demolition* is semantically transparent ('process or result of demolishing'), it must be memorized as a complex item, because of the arbitrary fact that corresponding to the verb *demolish* there happens to be, in current use, no noun *demolishment*.

In its need to be memorized, *demolition* is exactly like *sing expert* in my hypothetical variety of protolanguage. Indeed, the pattern at Table 7.1 seems to combine motivatedness and randomness in just the way one would expect if it is the modern counterpart of a protolinguistic situation that was as I have described it. Explaining why so many word-formation processes are less than fully 'productive' (one of the perennial problems of morphological

Table 7.1. Some English verbs and corresponding nouns

(a) announce		announcement	Annunciation
denounce			denunciation
	enunciate		enunciation
pronounce		pronouncement	pronunciation
renounce			renunciation
(b) abolish		(abolishment)	abolition
accomplish		accomplishment	
admonish		admonishment	admonition
astonish		astonishment	
banish		banishment	
demolish			demolition
embellish		embellishment	
establish		establishment	
impoverish		impoverishment	
nourish		nourishment	
			premonition
punish		punishment	
replenish		replenishment	

theory) is, in this light, much less of a problem. Their lack of productivity is the natural outcome of institutionalization enforced by synonymy avoidance, in circumstances where no robust cliché pattern is on hand to encourage consistency.

Four of the examples at Table 7.1 are of special interest. Alongside the four verbs *announce*, *pronounce*, *abolish*, and *admonish* two nouns are listed, not one. How can that be, if the brain eschews synonymy? But on closer examination the problem disappears. Either the two forms do not belong to the same variety of English, or else they are not synonymous. Consider first *abolishment* and *abolition*. I put parentheses around *abolishment* because it is not in my own active vocabulary (that is, the vocabulary that I spontaneously draw on when speaking). I hear other people say *abolishment* (especially, perhaps, younger people), and I am not puzzled about its meaning; but it seems best to regard me and them as speaking different varieties of English, in one of which *abolition* blocks *abolishment* while in the other the blocking is reversed. Now consider *admonishment* and *admonition*. Both of these are in my active vocabulary, but they mean different things: admonishment is the activity of admonishing, while an admonition is what the admonisher delivers. That may not be the case for all English speakers: some may use only *admonishment*, some only *admonition*. But I would predict that any

English speaker who uses both words will feel them to differ in meaning somehow, even if not in the same way as I do. Between *pronouncement* and *pronunciation* there is a more clearly institutionalized difference in meaning, corresponding to different meanings of the verb *pronounce*, as in *The judge pronounced her verdict* and *He pronounced that Russian word wrong*. And as regards *announcement* and *Annunciation*, the capital letter betrays the fact that the latter is used only as the proper name for a unique event (in full: *the Annunciation of the Blessed Virgin Mary*), that is, according to Christian belief, the announcement to Mary by the angel Gabriel that she would give birth to Jesus. So the data at Table 7.1 comply with the synonymy-avoidance expectation after all.

What about the majority of the nouns at Table 7.1—those that lack a competitor? Since they do not need to be differentiated from any competitor, one might expect all their meanings to be entirely transparent, like that of *demolition*. But recall that even *demolition*, despite its transparency, needs to be stored in the memory as an individual complex item, distinct from the verb *demolish* and the suffix *-ion* even though related to both. Stored in this way, *demolition* achieves a kind of independence that is not available to individual collocations such as protolanguage *sleep earlier₁* and *wake earlier₂* or modern English *slept* and *woken* (in which the stems are collocated with different suffixes, [t] and [ən]). A complex item stored without the support of a cliché-pattern menu (or, in more modern terms, not anchored in a robust gap-free paradigm of related word-forms) is free to drift semantically so as to acquire a meaning not predictable from the meanings of its parts. As it happens, *demolition* has not taken advantage of this opportunity; but other nouns in Table 7.1 have done so to a greater or lesser degree, particularly *accomplishment* and *establishment*. (One can say *Playing the piano is one of her many accomplishments* but not **She has accomplished (playing) the piano*. And even though one can say *King's College Cambridge is a famous educational establishment*, it sounds odd to identify its founder by saying *King's College Cambridge is an establishment of King Henry VI*.) There is even one noun in Table 7.1 that is so independent that it gets along fine without any verb to support it (so to speak), namely *premonition*.

The upshot of all this that the articulatory and cognitive characteristics of protolanguage-using humans yield expectations not only about collocations that conform to cliché-pattern menus but also about ones that don't. Moreover, these expectations are consistent with the gappiness and semantic quirks of derivational morphology. But there are still issues about the contrast between 'derivation' and 'inflection' that need addressing. We will turn to them in the next section.

7.4 The link between derivation and individual memorization

Just now, as examples of complex word-forms that are memorized as part of a paradigm rather than as individual items, I cited *slept* and *woken*. As examples of complex word-forms that are memorized as individual items, I cited *demolition*, *pronouncement*, and so on. The former are traditionally classified as belonging to the inflectional morphology of English, the latter to derivation. But it has often seemed strangely difficult to find a solid synchronic basis for the distinction between inflection and derivation. Could it be, then, that the distinction is to be explained by reference to whether an item is memorized individually rather than, for example, as one of a set of items that are related through shared membership in a paradigm? The answer is yes and no.

One reason for saying 'no' as well as 'yes' is that many complex word-forms that we classify as 'inflected' rather than 'derived' must be memorized. Our brains must retain the information that the past tense of SLEEP is *slept*, not **sleeped*. Yet more obviously, *went* as the past form of GO and *children* as the plural of CHILD do not reflect any productive processes. A second reason is that not all products of derivational morphology are memorized and stored in the way that monomorphemic words must be and in the way that (I suggested) *sing expert* and *hunt person* would have had to be in my version of protolanguage. Clearly, on the occasion of its first use, a derived word cannot be already stored in the memory of both speaker and hearer. Also, if it so happens that some derivational process has no rivals that would risk creating products synonymous with its own, the synonymy-avoidance imperative for memory storage will be absent. Earlier, I said that *demolition* would have to be stored because of the need to block the potential synonym **demolishment*. But an adverb formed with the suffix *-ly* escapes that kind of need because there is no rival adverb-forming suffix in English. The option is always available for a *-ly* adverb to be memorized in isolation and to develop an unpredictable meaning, as *terribly* has done (so that, for example, *She's terribly nice* does not imply that her niceness inspires terror). Yet the vast majority of *-ly* have not taken that course. It is easy to compose a list of adverbs that one is not conscious of ever having uttered, heard, or read, but which clearly do not need to be listed in any dictionary: *reversibly, felicitously, loathsomely, cluelessly* are examples that come to mind as I write. All four of these have probably been used before by someone, but that is irrelevant; the point is that my confidence in their well-formedness and their meaning does not depend on that previous use. Likewise, a few pages ago I said: 'an admonition is what the admonisher delivers'. I am sure I have never before used the word *admonisher*, but I am

Table 7.2. More English verb-noun correspondences

(a)	confer	conference		conferment		
	defer	deference		deferment	deferral	
	infer	inference				
	prefer	preference		preferment		
	refer	reference			referral	
	transfer	transference	tránsfer		(transferral)	
(b)	admit	admittance				admission
	commit			commitment	committal	commission
						intermission
	permit		pérmit			permission
	remit	remittance	rémit		remittal	remission
	submit					submission
	transmit	transmittance			transmittal	transmission

equally sure that it is a well-formed word and that every reader will understand what I mean by it.

Yet there is a curious ambivalence in the kind of morphology that is not anchored in paradigms—an ambivalence that may be a hang-over from the synonymy-avoidance pressures that enforced individual storage of some complex items in protolanguage. This is what allows us to say 'yes' as well as 'no'. The products of derivational morphology may not always need to be stored, but the brain seems to want to store them anyway. This shows up in a peculiar divergence between semantic predictability and formal productivity. Consider the verbs and the corresponding nouns at Table 7.2. We see here a gappy distribution of forms similar to Table 7.1. Here, too, we find clear but idiosyncratic meaning differences between many of the rival nouns. For example, if one defers to someone else's opinion one shows deference, whereas a man who wants to defer his conscription into the army seeks a deferment. An accused person is committed for trial at committal proceedings, whereas an enthusiast who commits himself to a cause shows commitment. A remit is a resolution submitted for debate at a political conference, whereas a remittance is a payment of the kind that the black sheep of the family might receive on condition of staying out of sight.

What is new in Table 7.2, however, is that one noun-forming process is completely productive with each type of verb. For those with the root -*fer* there is always a noun in -*ference*, and for those with root -*mit* there is always a noun in -*mission*. If the new verbs *demit* and *interfer* come into use (for example), one can guarantee that the new nouns *demission* and *intérference* (not to be confused with the already existing noun *interférence*!) will

automatically appear at the same time. Surely, then (one is inclined to think), any entirely idiosyncratic meanings displayed by nouns in Table 7.2 (meanings quite unconnected to any contemporary meanings of the corresponding verb) will be found only with less than fully productive suffixes. Even if, for reasons of synonymy avoidance, a noun in *-ference* or *-mission* does not have meanings corresponding to ALL the meanings of its parent verb, surely it will not have a meaning that corresponds to NO meaning of the verb. But this expectation turns out to be incorrect. Even though *commission* is formed by a process that is fully productive with *-mit* verbs, two of its meanings are 'payment to a salesman as a reward for making a sale' and 'military office above the rank of sergeant'; yet neither of these has anything to do with any contemporary meaning of *commit*. Similar idiosyncratic meanings of productively formed nouns are 'pause in development (of a cancer)' for *remission* and 'gearbox of a car' for *transmission*. Also, a meeting may count as a conference even if no conferring takes place there, but only the delivery of academic papers or speeches.

This uncoupling of semantic and formal productivity, insofar as it has been noticed, has been an embarrassment for morphological theorists. But it falls into place naturally if one sees derivational morphology as preserving from protolanguage the expectation that institutionalized collocations, if not supported by tight cliché patterns, should be memorized as wholes in order to be able to block potential synonymous rivals. As soon as the new nouns *demission* and *intérference* come into existence, the brain will uncouple them for storage purposes from the new verbs *demit* and *interfer*, and they are free to go their own way semantically.

Why does not the same institutionalization and blocking operate in syntax, then? The answer is that it does. The sentence *What time is it?* has an internal structure that is syntactic, not morphological; even so, it blocks alternative ways of asking the time, such as *What hour is it?* or *How late is it?*[3] The pervasiveness of idiomatic or semi-idiomatic phrases in normal usage has been emphasized by Andrew Pawley and Frances Hodgetts Syder, who talk of 'lexicalized sentence stems' (Pawley and Syder 1983). Nevertheless, our brains do not seem to expect a sentence to be idiosyncratic in the way that they expect a complex word to be. That is not surprising if the penchant for idiosyncrasy in derivational morphology relates to its origin at a protolanguage stage when the brain could not yet categorize a stable, recurring collocation of two vocabulary items as some kind of syntactic unit. After all,

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[3] The strangeness of *How late is it?* is a fact about English only, of course. Its literal counterpart in German (*Wie spät ist es?*) happens to be one of the usual ways of asking the time.

syntactic units may not yet have come into existence. Unless a stable collocation could be slotted into a suitable cliché-pattern menu, the only way in which the brain could categorize it was as an individual vocabulary item—the same kind of item as its two components, and thus with the same expectation of storage in the memory and the same expectation that it would not have exactly the same meaning as any other item.[4]

This suggests a new perspective on the development of syntax. One of syntax's effects was to liberate the brain from the straitjacket of lexicalization. It provided the brain with new ways of imposing structure on strings of individually meaningful items. But the old way still remains available. Thus, structure that is 'lexical' in the sense of being morphological rather than syntactic retains its evolutionary link with 'lexicalization' through memorization. That is why (I suggest) it has been so easy for linguists to muddle the two logically distinct senses of 'lexical'. One can easily imagine the world being otherwise, so that this confusion would be less tempting. The circumstances that make it tempting are an indirect evolutionary outcome of the synonymy-avoidance expectation that human babies share with chimpanzees and the border collie Rico.

7.5 A puzzle partly solved: Phrases inside compound words[5]

We have been concerned in this chapter with the issue of how the proto-linguistic brain would have handled institutionalized collocations that could not be assigned to cliché-pattern menus, such as *sing expert* and *hunt person*. I have suggested that synonymy avoidance would have encouraged the brain to store them individually as wholes, even if their meaning was transparent so as to render memorization (on the face of it) superfluous. And the same outcome would have been encouraged by the fact that, if a collocation was not analysable as a different form of some already stored vocabulary item (like *sleep later₂* and *sleep earlier₂*), the only way in which the brain could categorize such a collocation was as a new vocabulary item. We have thus seen how a synonymy-avoiding brain, interacting with phonology, yielded necessities of storage that gave rise to a particular sort of grammar independent of syntax.

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[4] This may help to explain Aronoff's (2007) observations about words or 'lexemes' (rather than morphemes) being the basic items of lexical storage. As he puts it (2007: 828): '[w]e expect that [even] newly coined words will have idiosyncratic meanings that are understandable only in the extralinguistic context of their creation'.

[5] This section covers similar ground to Carstairs-McCarthy (2005b), though the conclusions reached here are somewhat different.

If this is how morphology originated, a new upside-down possibility suggests itself. Could it be that any lexically stored expression is available to participate in morphology, so as to become a part of a larger morphological unit? A good case can be made for answering 'yes'. If so, then we have yet further confirmation for the hypothesis about morphological evolution presented here. The 'yes' answer is supported by certain English facts involving both compounding and derivation.

Can a syntactic phrase appear inside a word? Here is an example where such an opportunity presents itself. Someone who paints portraits can be designated by the compound noun *pórtrait artist*. We call this a compound noun rather than a phrase because of the stress on the first element *portrait* (just as in *bláckboard* and *cóathanger*) and because the first element cannot carry inflectional affixes (there is no term *portraits artist* available to designate someone who paints many portraits). But what about someone who specializes in painting large portraits? It would make sense, surely, to replace *portrait* with the phrase *large portrait*, and create the expression *large portrait artist*, understanding the structure of this collocation to be [[large$_A$ portrait$_N$]$_{N'}$ artist$_N$]$_N$.[6] But that is not possible. The expression *large portrait artist* can only mean a large person who paints portraits; it is a phrase, not a compound, and has the structure [large$_A$ [portrait$_N$ artist$_N$]$_N$]$_{N'}$. Similarly, a particularly well-designed furniture shop is a particularly well-designed shop that sells furniture, not a shop that sells particularly well-designed furniture.

These examples suggest that phrases cannot appear inside compound words. This restriction seems inherently plausible, inasmuch as one thinks of words as being smaller than phrases in general, even though many individual words (for example, *uncommunicativeness*) are longer than some individual phrases (for example, *Go away!*). It also fits in with both the more general fact that morphology and syntax are distinct branches of grammar (if the arguments in Chapter 2 are valid), and the more specific claim that syntactic rules and processes are blind to the internal structure of words (Lapointe 1981; Selkirk 1982). Details of this so-called **Lexicalist Hypothesis** and the motivation for it need not concern us here, however; we are concerned not with whether syntactic operations can 'see inside' words but rather with whether parts of words can themselves have a syntactic structure.

I mentioned in Chapter 2 what Emonds (2002) called the Domain Size Restriction, according to which no phrase can appear inside a word. The same idea has also been labelled the **No Phrase Constraint** (Botha 1981: 18).[7] But

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[6] The label N′ ('N-bar') stands for a phrase consisting of a noun and any preceding modifiers, such as an adjective, but not including determiners.

[7] This is a whimsical term, in that it appears to constitute a violation of itself.

Lieber (1992) argues that the No Phrase Constraint is incorrect, and uses this as support for her view that morphology is really just syntax below the level of the word (section 2.3). Certainly, the No Phrase Constraint seems to need qualification. If it were correct, then (for example) *American history teacher* would be unambiguous: it could mean only 'American teacher of history'. Because *American history* is a phrase (just like *brief history* or *fascinating history*), it could not be substituted for the noun *history* in the compound *history teacher*. Therefore *American history teacher* could not mean 'teacher of American history'. But in fact it has both meanings. This seems to point towards two distinct grammatical structures:

(1)   [American$_A$ [hístory$_N$ teacher$_N$]$_N$]$_{N'}$ 'American teacher of history'
(2)   [[American$_A$ hístory$_N$]$_{N'}$ teacher$_N$]$_N$ 'teacher of American history'

Yet, as we have just noted, not just any phrase can appear inside a compound:

(3)   *[[large$_A$ pórtrait$_N$]$_{N'}$ painter$_N$]$_N$ 'painter of large portraits'
(4)   *[[glorious$_A$ hístory$_N$]$_{N'}$ teacher$_N$]$_N$ 'teacher of glorious history'
(5)   *[[dull hístory$_N$]$_{N'}$ teacher$_N$]$_N$ 'teacher of dull history'
       (*contrast* [dull$_A$ [hístory$_N$ teacher$_N$]$_N$]$_{N'}$ 'dull teacher of history')

So what differentiates the acceptable examples from the unacceptable ones?

One thing to note is that *American history* is a cliché, denoting an institutionalized specialism within history as an academic discipline. By contrast, *glorious history* and *dull history* are not clichés—or, at least, are not clichés to the same extent. In this respect, they resemble *large portrait* and *well-designed furniture*. Further examples confirm that phrases can appear inside compounds, but only if they are institutionalized. Consider the following threesome:

(6)   [[defective compónent] problem]
(7)   ?[[expensive compónent] problem]
(8)   *[[Norwegian compónent] problem]

The expression *defective component problem* is naturally interpreted as meaning 'problem with defective components'; it would be perverse to read it as meaning 'defective problem with components'. On the other hand, *Norwegian component problem* is most naturally interpreted as meaning 'problem in Norway with components', implying a phrasal bracketing [Norwegian$_A$ [compónent$_N$ problem$_N$]$_N$]$_{N'}$. It cannot readily mean 'problem with components made in Norway'. This fits in with the fact that, on 7 October 2008, the word-string *defective component* scored 'about 80,300' Google hits while

expensive component scored 'about 68,900' and *Norwegian component* only 'about 1,120'. That is what one expects if no speaker's memory stores *Norwegian component* as a single unit but some speakers' memories do store *expensive component*, while more still store *defective component*, thanks to familiarity with the institutionalized jargon of technical manuals and the manufacturer's guarantees ('Any defective component will be replaced without charge...').

Two similar sets of examples are (9)–(11) and (12)–(14):

(9)   [[broken gláss] injuries]

(10)   ?[[broken pláte] injuries]

(11)   ??[[broken wíng] injuries]

(12)   [[capital cíties] lesson]

(13)   ?[[British cíties] lesson]

(14)   ??[[dangerous cíties] lesson

The phrase *broken glass* is a cliché, whereas *broken plate* and *broken wing* are not. Notice that it is not that *broken pláte injuries* and *broken wíng injuries* are uninterpretable, nor that occasions for their use are unimaginable. It is easy to visualize 'broken plate injuries' occurring in the dining room of a ferry during a rough crossing of the Cook Straight (the turbulent stretch of water between the North and South Islands of New Zealand). 'Broken wing injuries' could arise at a microlite aircraft show if a mishap causes pieces of an aircraft to land among spectators. Similarly, *capital cities* is a cliché (the name of a possible topic for a primary school geography lesson) whereas *British cities* and *dangerous cities* are not, even though one can easily visualize a lesson on these topics. (Senior executives who travel the world extensively, for example, may benefit from instruction about those cities where they need to be particularly careful about their own safety.) What makes the compounds (9) and (12) more acceptable than the others is not that one cannot visualize circumstances where the others might be useful, but that the phrase on the left in (9) and (12) is institutionalized.[8]

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[8] Frazier (1990), discussing ANN collocations such as *fast food delivery*, likewise emphasizes the importance of whether or not the first two items (in this instance, *fast food*) constitute a cliché; however, as Bayer (1990) points out, she is wrong in thinking that, just because *fast food* is a cliché, it must be a compound. Wiese (1996) argues that when a phrase appears as the first element of a compound it is being quoted, so its internal structure becomes opaque. But that seems to entail incorrectly that the quoted question in *He asked 'Where am I?'*, even though it is not a cliché, should be able freely to function as the non-head in a compound, as in *his 'Where am I?' question.

Carstairs-McCarthy (2005b) explores but rejects a different possibility for analysing examples such as *American history teacher*: perhaps, even when it means 'teacher of American history', its structure is [*American* [*history teacher*]]. This is in line with a proposal by Spencer (1988) about how to handle

So far we have considered the circumstances under which a phrase may constitute the first element in a compound word. What about the second element? Can a cliché phrase appear in this position? Examples might be (15)–(17):

(15)   *[window [broken glass]]   'broken glass from windows'

(16)   *[port [capital cities]]   'capital cities which are ports'

(17)   *[aircraft [defective components]] 'defective components of aircraft'

All of these are pragmatically easy to interpret, but they all seem quite unacceptable, however one manipulates the stress pattern. So is it just a brute fact that cliché phrases are permitted (in English) as the first element in a compound, but not as the second element?

One would hope to provide a better answer than that. And the way in which compounds mesh with syntax suggests one. Jackendoff (2002) does not argue that morphology in general is pre-syntactic in origin, but he does make this claim in respect of compounding: he suggests that it is precisely the way in which complex structures were built up out of minimal vocabulary items in protolanguage.⁹ That view is broadly compatible with the argument presented here. Yet, to fit into modern-style language, this kind of protolinguistic combination has to negotiate a compromise with syntax: the complex structures formed by means of it must be assignable to some open word-class (noun, verb, or adjective) in order to engage with the syntax of the sentences where they appear. And in order for that engagement to happen smoothly, the syntax must be able to 'read' an initial or final constituent of the compound as a noun, verb, or adjective. In English, where compounds are head-final, it is the final constituent which is 'read' in this way. But although in the compound *window glass* the final element *glass* can be 'read' as a noun, that is not possible in the putative compound *window broken glass*. This seems likely to be because the final element *broken glass* is not a noun: it is a phrase headed by a noun.

What I have offered here is by no means a full discussion of the problem of phrases inside compounds. For example, I have concentrated entirely on

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'bracketing paradoxes', or what Stump (1991) calls 'morphosemantic mismatches'. For some examples that Spencer discusses, such as *nuclear physicist*, this style of analysis is attractive. But it will not work for examples such as *a days-of-the-week lesson*: the bracketing [*a* [*days* [*of* [*the* [*week lesson*]]]]] must be wrong (quite apart from the fact that it does not correspond to the meaning) because *a days* is syntactically ill-formed.

⁹ Sadock (1998) likewise argues, for independent reasons, that compounding deserves to be recognized as a branch of grammar distinct from both syntax and the rest of morphology.

compound nouns. But what about the possibility of phrases inside compound adjectives or compound verbs? What does my account of morphological evolution predict about the acceptability or unacceptability of *clear-sky-blue* alongside *sky-blue*, or *electric-oven-bake* alongside *oven-bake*? Could it be that only nominal compounding, not compounding in general, is a hang-over from protolanguage? If so, why? Is it that nouns are in some sense more direct descendants of protolinguistic 'vocabulary items' than verbs or adjectives are? But I hope that, even with these questions unanswered, our evolutionary perspective may shed some light on the puzzlingly slippery No Phrase Constraint.

8

Morphological homonymy and morphological meanings

This chapter discusses briefly two topics that have been prominent in recent morphological theorizing:

(a) morphological homonymy (especially inflectional identity or syncretism), and
(b) the nature of morphological meanings or functions, especially the supposed advantages of analysing these meanings in terms of binary features.

The approach to morphological evolution that I have been arguing for has little to contribute to these discussions. A possible reaction on the reader's part may be to see this as a deficiency in my approach. In response, I can fairly claim that an account of why grammar has evolved so as to distinguish syntax and morphology need not be expected to explain every aspect of how morphology works. Many aspects of morphology may be as they are for reasons quite independent of what triggered its existence. But a cheekier response is also possible: perhaps the apparent irrelevance of morphological evolution to topics (a) and (b) shows that their importance has been over-rated. I will offer here some considerations in favour of that somewhat cheeky stance.

8.1 Homonymy

As an illustration of homonymy, consider the following forms of Latin nouns:

(1)   HOSTIS 'enemy'   PUELLA 'girl'
    Singular Dative   hosti: puellae
              Ablative hoste   puella:
    Plural   Dative   hostibus   puelli:s
              Ablative hostibus   puelli:s

Dative and ablative cases must be distinguished in Latin because in most nouns they have distinct forms in the singular. But the dative and ablative plural forms of any noun are always the same, as illustrated in (1), though different nouns use different suffixes (*ibus* or *i:s*). This suffixal homonymy is a pervasive fact of Latin affecting not only nouns but also adjectives and pronouns, and the difference in shape of *ibus* and *i:s* shows that it cannot be due just to a random phonological convergence. So it would be perverse to deny that dative-ablative plural syncretism is a systematic feature of Latin morphology. And innumerable syncretisms that seem equally systematic can be observed elsewhere in Latin and many other languages.

How important is syncretism, then, in relation to morphology as a whole? The titles of two recent books on morphological theory, *Inflectional Identity* (Bachrach and Nevins 2008) and *The Syntax-Morphology Interface: A Study of Syncretism* (Baerman, Brown, and Corbett 2005), draw particular attention to it. Indeed, the title of the latter even seems to hint that syncretism is all there is to the syntax-morphology interface, though this is surely not what the authors intend. But what does our evolutionary perspective lead us to think?

Homonymy is in a sense the obverse of synonymy: not two different forms with the same meaning, but rather the same form expressing two different meanings (such as *lie* 'prevaricate' and *lie* 'be horizontal', or *beer* 'alcoholic drink' and *bier* 'platform for a coffin'). We have noted evidence for a deep-rooted cognitive distaste for perfect synonymy. One might expect there to be a similar distaste for homonymy. After all, the disadvantages of having the same phonological shape express two quite distinct meanings within one language seem evident.[1] What's more, synonymy may even in some circumstances be beneficial; if the same content is expressed in more than one way, that content may stand a better chance of being perceived accurately over a 'noisy' communication channel. By contrast, it is hard to envisage any comparable benefit from homonymy. So it is perhaps not surprising to find one morphological theorist, Gereon Müller, espousing energetically the following heuristic assumption:

(2) **Syncretism Principle**: Identity of form implies identity of function (within a certain domain, and unless there is evidence to the contrary) (Müller 2004: 197; 2005: 236; 2007: 4; Alexiadou and Müller 2008: 103).

I have suggested that much of what happens in morphology makes better sense if we see it as a by-product of the synonymy-avoidance assumption that guided the acquisition of protolinguistic vocabulary items and still guides the

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[1] For fuller discussion of this point, see Wasow et al. (2005).

Table 8.1. Illustrations of the propensity to homonymy in French

		Italian		*French*		
(a)	finge	['findʒe]	feint	[fɛ̃]		'pretends'
	fame	['faːme]	faim	[fɛ̃]		'hunger'
	fino	['fiːno]	fin	[fɛ̃]		'fine'
	fine	['fiːne]	fin	[fɛ̃]		'end'
(b)	cento	['tʃɛnto]	cent	[sɑ̃]		'hundred'
	sente	['sɛnte]	sent	[sɑ̃]		'feels'
	senza	['sɛntsa]	sans	[sɑ̃]		'without'
(c)	alto	['alto]	haut	[o]		'high'
	al	[al]	au	[o]		'to the' (masculine singular)
	acqua	['akkwa]	eau	[o]		'water'
	ossi	['ɔssi]	os	[o]		'bones'

Note: Contrast French [o] 'bones' with [ɔs] 'bone (singular)', also spelled *os*. This noun is thus one of the few in French whose singular and plural pronunciations differ.

acquisition of words. Could the Syncretism Principle then be the by-product of some principle that applies to words, too? If that is correct, then we would expect to find something like (2) applying to words, not only to inflected word-forms. Thus, to classify *beer* and *bier* as distinct words would require evidence to counter the assumption that they are really one and the same word with a single but complex meaning ('alcoholic drink OR platform for coffin'). Homonymy should be unusual, not commonplace.

The puzzling fact is, however, that, at least in some languages, homonymy is indeed commonplace. There seems to be no general cognitively based presumption against homonymy among words, comparable to the presumption against synonymy. Consider the Italian and French pairs of cognates in Table 8.1. Purely phonological developments have rendered homonymous in French various sets of words which in Italian, with its more conservative phonology, remain distinct.[2] A similar picture emerges if one compares, for example, Cantonese (with less homonymy) and Mandarin (with more, owing to phonological changes affecting especially syllable-final consonants) (Norman 1988). The brains of French and Mandarin speakers have evidently felt no need to combat this trend. Yet this does not mean that French speakers now ascribe

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[2] Because of the distinct spellings, most of these French examples are technically homophones, not homographs. What matters for linguistic purposes is homophony alone. But distinct spellings of words pronounced alike ('heterography', one could call it) may supply a clue that the homophones in question really do lack 'identity of function' for native speakers.

an identical function to [fɛ̃], [sɑ̃], and [o] in all their occurrences—a function as a single polysemous item disambiguated by context. New 'identities of function' do indeed arise from time to time; for example, the English word *ear*, meaning both 'organ of hearing' and 'seedhead of cereal plant', is probably perceived by most English speakers as a single word, the second meaning being a metaphorical application of the first (as if seedheads were shaped like ears), even though historically it is descended from two distinct words, as is shown by their German cognates (*Ohr* in the first sense, *Ähre* in the second) (Bloomfield 1933: 436). But such convergence is unusual, relying on accidental similarities in both meaning and shape. The important point is that, though homonymy may be more or less common in different languages, no general bias against it is rooted in the language faculty.

That being so, there is (at least on the basis of the evidence that we have been exploring) no reason to suppose that there should be a general bias against accidental homonymy between distinct inflectional affixes. This is in itself a reason to be sceptical about Müller's Syncretism Principle. There is a further reason too. Just in virtue of being relatively short, affixes stand a greater risk of losing their phonological distinctness than stems do. So we should not be surprised if purely accidental homonymy among inflectional affixes is widespread—much more common than among stems. A particular identity of form may indeed be sometimes a clue to grammatical structure, as is surely the case with *puelli:s* and *hostibus* at (1); but, when an inflectional homonymy is being considered, it seems wise to assume that it is accidental until there is positive evidence for an identity of function.

At the start of the last paragraph, I was careful to qualify what I said with the words 'at least on the basis of the evidence we have been exploring'. Other evidence may turn out to support something like the Syncretism Principle in relation to affixes (though not, clearly, in relation to stems). As of now, my complaint is simply that homonymy puzzles have come to loom too large in morphological theorizing, overshadowing allomorphy puzzles. On a smaller scale, this resembles the way in which syntax and phonology have come to loom too large in linguistic theorizing generally, overshadowing morphology. A better balance of research effort is needed. As to why the imbalance has arisen within morphology, I will suggest some reasons in the next section.

8.2 The decomposition of morphological meanings

In setting out tables of inflected forms in Latin, Italian, Hungarian, and other languages in earlier chapters, I used traditional labels such as 'genitive singular' and '1st person plural'. In fact, the information was laid out much as it

would be in a traditional pedagogical grammar. I paid no attention to possible ways in which Latin's six cases (nominative, vocative, accusative, genitive, dative, and ablative) might be grouped or organized on the basis of shared elements of meaning, nor to the corresponding questions regarding Italian and Hungarian verb forms. That is excusable, in that an account of the evolution of morphology is not required to suggest an answer to every question of morphological theory. But it is worth considering, even if briefly, what sort of answer one might expect to emerge on the basis of our hypothesis that morphology arose, independently of syntax, from cliché collocations of vocabulary items.

On the basis of this hypothesis, we might look for meaning relationships between (say) case suffixes or person-number suffixes that would be similar in character to meaning relationships between words—relationships of the kind studied in lexical semantics. But saying that does not take us as far as one might hope. Lexical semantics, like morphology, has been something of a Cinderella. For more than a century, it has taken a back seat in linguistic theory to syntax and phonology. It has attracted more attention from philosophers, psychologists, and cognitive scientists. What, then, have workers in those disciplines contributed?

The second half of the twentieth century saw two linked innovations in theorizing about word meanings. One was the proposal that categories are intrinsically 'fuzzy' or based on 'family resemblances' (Wittgenstein 1963: 31–4; Rosch and Lloyd 1978). On this view, what distinguishes the meanings of *mug* and *cup* (for example) may not be determinable on the basis of checklists of defining features; we should not be surprised to find intermediate objects on whose classification fluent native speakers of English can legitimately disagree (Labov 1973). The second innovation, exploited by grammaticalization theorists (Chapter 2), has been the championing of metaphor and metonymy as relevant not merely in the study of literature but also in the scientific study of meaning and meaning change (Lakoff 1987; Lakoff and Johnson 2003).

As an example of the way in which categories can be stretched in contrasting directions through metaphor and metonymy, consider the two meanings 'party hat made of coloured paper' and 'governmental authority'. At first sight these have nothing in common. Yet the word *crown* applies to both. A prototypical crown is a piece of ornamental headgear worn by a monarch as a symbol of his or her authority. By metaphorical extension, it can mean any kind of unusual headgear that vaguely resembles a royal crown and is worn on special occasions. By metonymic extension, it can

refer to the authority exercised by the government that the monarch nominally appoints.

The older 'checklist' approach to semantic analysis is classically represented in linguistics by Katz and Fodor (1963). They analyse the meaning of the word *bachelor* in terms of the 'semantic markers' (Human), (Animal), (Male), and (Young), which are seen as resembling closely syntactic markers such as Noun and Verb, along with the 'distinguishers' [who has never married], [young knight serving under the standard of another knight], [young fur seal when without a mate during the breeding time], and [who has the first or lowest academic degree]. The contrast between this and the 'family resemblance' approach is most evident in the distinguishers. It is not an accident, surely, that *bachelor* applies to young knights and young fur seals, rather than old ones, given that a human bachelor is prototypically young. (Most old men are not bachelors any more, but all old men were young bachelors once.) Yet in Katz and Fodor's approach, similarities between distinguishers are not captured.

Within morphology, as Müller (2005: 241) points out, the analysis of inflectional meanings has been influenced enormously by two pieces of pioneering work on nominal case systems that reflect the checklist approach: Jakobson (1936) on Russian and Bierwisch (1967) on German. Both Jakobson and Bierwisch decomposed traditional cases such as 'nominative' and 'genitive' into combinations of more abstract binary features such as [±direct] and [±oblique]. This emphasis on binarity reflects the tradition already well established in phonology.[3] Yet, since the 1960s, the binary requirement for phonological features has been relaxed in that many-valued and single-valued features have made an appearance in 'feature geometry' (Clements and Hume 1995). There have been non-binary approaches to morphological features too (e.g. Chomsky 1970; Zwicky 1985). So which of the two fits morphological (particularly inflectional) meanings better: the checklist approach or the family-resemblance approach?

To do justice to this question would require another whole book. I will offer here just one illustration of the uncertainties surrounding it. In Latin, three

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[3] Featural analyses became prominent in the work of Prague School phonologists in the 1930s. The two most influential of these were Roman Jakobson, a proponent of binary features, and Nikolai Trubetzkoy, who recognized a greater variety of feature types, including singulary and multi-valued ones. Jakobson reached the USA and exercised great influence over the development of generative phonology, but Trubetzkoy died in 1938. It is tempting to speculate about how differently linguistic theory might have developed if Trubetzkoy too had succeeded in escaping across the Atlantic.

suffixes compete to express '1st person singular active' in verb forms: *i:*, *m*, and *o:*. Their distribution is as follows:[4]

(3)   *Subjunctive*: m
        Indicative:
            Past: m
            Future: m *in some inflection classes,* o: *in others*
            Present:
                Perfective: i:
                Imperfective: o:

How should these suffixes be analysed? I will outline first a checklist approach and then a family-resemblance one.

The future tense, with both *m* and *o:*, looks particularly problematic. But the problem turns out to be neatly circumscribable. Those future forms that have *m* are identical with the corresponding present subjunctive forms, so this is an instance of systematic homonymy; in some verbs, the task of realizing the future indicative is 'referred' to the present subjunctive in the 1st person singular context.[5] In relation to the indicative future, we are thus entitled to ignore the words '*m in some inflection classes*', because the relevant forms, so far as their shape is concerned, are not future indicative forms after all.

Let us turn now to the suffix *i:*. This, being limited to the indicative present perfective, looks neatly analysable in checklist terms as [1st, singular, indicative, present, perfective]. (I ignore here one meaning that *i:* has in common with *m* and *o:*, namely [active] as opposed to [passive].) But then what are we to make of *m* and *o:*? Perhaps *o:* is [1st, singular, indicative, non-past] while *m*, the most widely distributed of the three suffixes, is simply [1st, singular]. This analysis assigns each of the suffixes to its correct contexts, provided we assume that the *m* is not used in contexts where the more precisely defined *o:* will also fit, and *o:* in turn is not used in the one context where the even more precisely defined *i:* will fit. (Giving priority to the most precisely fitting among the available alternatives complies with the Elsewhere Condition or Panini Principle mentioned in n. 2 of Chapter 7.)

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[4] For a fuller discussion of these Latin issues, see Carstairs-McCarthy 1998b and 2001a. A parallel Hungarian situation is discussed in Carstairs-McCarthy 1998a: 296-9. I assume that Latin had two aspects (perfective and imperfective) but only three tenses (present, past, and future). So what are traditionally called 'perfect' tense forms (*ama:ui:* 'I have loved' etc.) should be analysed as perfective present, 'future perfect' as perfective future, 'pluperfect' as perfective past, and 'imperfect' as imperfective past. All combinations of aspect, tense, and mood are possible except that, in the subjunctive mood, only present and past tenses are distinguished, not future.

[5] For discussion of 'rules of referral' in relation to syncretism, see Stump (2001) and Baerman et al. (2005). The same phenomenon is called 'take-over' by Carstairs (1987).

There are two reasons to be unsatisfied with this checklist analysis, however. One reason relates to the other indicative present perfective forms, alongside *i:*. I illustrate them in (4):

(4)

		i:-forms	*o:*-forms	*m*-forms
Singular	1	i:	o:	m
	2	isti:	Vs	Vs
	3	it	Vt	Vt
Plural	1	imus	Vmus	Vmus
	2	istis	Vtis	Vtis
	3	e:re, erunt, e:runt[6]	Vnt	Vnt

What is represented as 'V' in the *o:*-form and *m*-form columns is a vowel that varies according to mood, tense, and inflection class. Setting that vowel aside, it is clear that the two columns are identical except in the 1st person singular. The *i:*-forms, by contrast, differ from the others not only in the 1st person singular but also in the 2nd singular and plural and the 3rd plural. So readers who have absorbed the message of Chapter 5 will, I hope, be saying to themselves: 'Ah! Perhaps what we are dealing with in the *i:*-form column is not an accidental coincidence whereby four suffixes *i:*, *isti:*, *istis*, and *e:re* all happen to signal [indicative, present, perfective], but rather a special suffixal vocabulary, limited not to a particular verbal inflection class but rather to the indicative present perfective domain in all verbs! It is this special vocabulary, not the individual items within it, that signals [indicative, present, perfective].' Yet the checklist approach does not encourage us to look sideways, so to speak, at how other person-number combinations are expressed in the same context of mood, tense, and aspect.

The second apparent drawback of the checklist analysis is the function that it requires us to ascribe to *o:*. If two affixes differ in that one has a more precise meaning and the other a less precise one, linguists traditionally expect the extra precision to involve specification of relatively 'marked' content (such as subjunctive or past) rather than relatively 'unmarked' content (such as indicative or present). Yet with *o:* and *m*, as we have analysed them, the markedness relationship is the other way round; the extra specification of *o:* involves the unmarked features [indicative] and [non-past] (where 'non-past' stands for 'present or future'). I called this only an apparent drawback because there is reason to be sceptical about the supposed link between precision and 'markedness'; for example, the Caucasian languages Adyghe and Kabardian

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[6] Of the three suffixes here, *e:re* is the oldest. The other two are blends of the old suffix with the more general 3rd-person plural suffix *Vnt*.

have a special prefix which appears precisely in present indicative verb forms (Peter Arkadiev, personal communication).[7]

We are still left with a dilemma, however. Which is really preferable: to treat *m* as the 'elsewhere' suffix, or instead to analyse *m* in terms of a disjunction of binary features, [subjunctive OR past]? Are such disjunctions compatible with the spirit of Jakobson and Bierwisch? Or should we introduce a new feature that one might call 'proximate', with [+proximate] being equivalent to [−subjunctive, −past], so that, among 1st singular contexts, *m* occurs in precisely those that are [−proximate]? Yet if new binary features can be posited freely in this way, is there any conceivable pattern of inflectional behaviour that could not be analysed in terms of such features? Unfortunately, in the binary checklist approach, questions of this nature lurk constantly in the background.

Does a family-resemblance approach perform any better, then? As far as the first drawback goes, perhaps not. This approach to lexical meaning does not supply any obvious precedent for analysing contrasting sets of affixes such as those at (4) in terms of competing vocabularies. With regard to the second drawback, however, this approach fares better. Metaphor and metonymy yield lexical meanings with either-or characteristics, such as 'elaborate headgear OR relating to government authority'. The crucial point is that, to a prototypical crown, both characteristics apply. Taking our cue from this example, we can happily acknowledge the possibility that *m* is more precisely defined than *o:*, rather than the other way round. A prototypical instance of a 1st singular verb form with *m* would be one that is both past and subjunctive (just as a prototypical crown is elaborate headgear worn only by a monarch), yet *m* is used under 'fuzzier' conditions too: past but not subjunctive, and subjunctive but not past. In that case *m* can be glossed as [1st, singular, EITHER subjunctive OR past], mimicking our definition of *crown*, while *o:* means simply [1st, singular].[8]

My purpose has not been to adjudicate between checklist and family-resemblance approaches to morphological meaning. Very likely, a proper understanding of how morphology works will require contributions from

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[7] Haspelmath (2006) offers a number of reasons to be sceptical about the usefulness of the notion 'markedness'.

[8] Noyer (2005: 286, 305) describes a somewhat similar situation in Old Russian nouns, citing it as counterevidence to Carstairs-McCarthy's (1994) No Blur Principle (mentioned in Chapter 5, and see later in Chapter 9): the instrumental plural suffix was generally *mi*, but in nouns that were either neuter or belonged to the 'o-stem' class it was *y*. This points towards a family-resemblance style of analysis, whereby neuter *o*-stem nouns were the prototypical nouns with instrumental plural *y*, but not the only ones. If this is correct, then the distribution of *y* and *mi* no longer constitutes straightforward counterevidence to the NBP.

both. But I have illustrated some of the issues that arise in comparing them, in the light of which it seems fair to say that the family-resemblance approach has been unfairly neglected.[9] In the next section I will demonstrate a drawback of that style in relation to some data that we considered in Chapter 6: the inflection of masculine nouns in Polish.

8.3 Drawbacks of binary features in describing inflection classes

In Chapter 6 there was one aspect of Polish masculine noun inflection (as presented at Table 6.5) that we did not comment on. The four cases for which more than one suffix is available are the genitive, dative, locative, and vocative. In all these cases, one of the two suffixes available is -*u*. According to Halle and Marantz (2008), this is not a coincidence. They analyse -*u* as the 'elsewhere' suffix, used in any cell where, for some reason, the suffix specific to that cell is unavailable. Thus, for example, the genitive case is realized by -*u* in inflection classes 6 and 7 because (they say) nouns in these inflection classes are characterized lexically as [−Genitive], preventing the normal genitive suffix -*a* from being used with them; the dative case is realized by -*u* in classes 3 and 4 because nouns in these classes are characterized lexically as [−Dative], preventing the normal dative suffix -*owi* from being used with them; and so on. In the terminology of Distributed Morphology, these nouns exhibit morphosyntactic 'impoverishment'.

Two questions arise. First, is this analysis plausible? Second, what implications does it have for the maximum number of inflection classes that could exist in a hypothetical language that has at its disposal the inflectional resources of Polish? I will suggest that the analysis is not particularly plausible and, even if it were correct, it would leave unaddressed the issue of constraints on inflection-class organization.

An immediate problem with the Halle-Marantz analysis of -*u* is that -*u* does not appear in the nominative. If -*u* is supposed to appear whenever a case-specific affix does not, it ought to appear in the nominative in all inflection classes, because there is no dedicated nominative suffix. Halle and Marantz's suggestion (2008: 68) is that there is indeed a dedicated nominative suffix, but it is phonologically null. This idea suffers from the usual drawbacks of morphological zeros pointed out by Stump (1998: 39–41). In particular, it is

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[9] The shortage of input to morphological theory from family-resemblance approaches to meaning is illustrated by the relative neglect of morphology in *Fuzzy Grammar: A Reader* (Aarts et al. 2004) and *The Cognitive Linguistics Reader* (Evans et al. 2007).

inherently untestable. Let us suppose that someone suggests that a null suffix is indeed present in the nominative in classes 1–3 and 5–7, so as to block the addition of *-u*, but in class 4 there is no suffix at all, not even a null one, so the fact that the nominative form is *pan* rather than **panu* is a genuine anomaly. This suggestion has no attractions; unfortunately, however, there are no Polish data that Halle and Marantz can produce to show that it is wrong.

A second problem arises from what I said in section 8.1 about accidental homonymy in inflection. Polish has only five vowels /i, e, a, o, u/ (and perhaps /ɨ/, if this is analysed as underlyingly distinct from /i/). Therefore, for a suffix consisting of only a vowel, there are only five (or perhaps six) shapes available. The data at Table 6.5 show that masculine singular nouns use three of these shapes, namely /e/, /a/, and /u/; further data would reveal that they use also /i/ and /ɨ/ in the plural. Thus only /o/ 'goes to waste' in masculines. It would not be surprising, then, if some affixal allomorphs consisting of just a single vowel are identical to allomorphs of other affixes merely by accident. The identity of *-e* and *-u* in the locative and vocative, linked to stem alternation, is evidently systematic, and it is possible that, beyond that, part at least of the distribution of *-u* by itself in Table 6.5 is systematic too. But Halle and Marantz need more evidence, such as similar homonymies in nouns of other genders, in order to demonstrate this.

Let us suppose, however, that Halle and Marantz are right, and that *-u* is really the same suffix everywhere in Table 6.5. Then, in order to distinguish the inflection classes, one can assign to each class an appropriate set of lexical impoverishment features, as follows:

(5)   Class   Set of impoverishment features
　　　1　　　none
　　　2　　　[–Loc, –Voc]
　　　3　　　[–Dat]
　　　4　　　[–Dat, –Loc]
　　　5　　　[–Loc]
　　　6　　　[–Gen]
　　　7　　　[–Gen, –Loc, –Voc]

This analysis 'works' in the sense that it correctly yields the facts of Polish. But underlying it is a pessimistic assumption about inflection-class organization: the assumption that vocabular clarity has no application to inflection classes, contrary to what was argued for in Chapters 5 and 6. To see this, consider how many inflection classes could in principle be distinguished by various

combinations of the four impoverishment features invoked at (5). The total turns out to be sixteen, as shown at A-R in (6):[10]

(6)

Feature combination	Polish class?	Feature combination	Polish class?
A: none	1	J: [–D, –G, –L, –V]	no
B: [–D]	3	K: [–G]	6
C: [–D, –G]	no	L: [–G, –L]	no
D: [–D, –L]	4	M: [–G, –V]	no
E: [–D, –V]	no	N: [–G, –L, –V]	7
F: [–D, –G, –L]	no	P: [–L]	5
G: [–D, –G, –V]	no	Q: [–L, –V]	2
H: [–D, –L, –V]	no	R: [–V]	no

Only seven of the sixteen possibilities are actualized in Polish. Yet nothing in Halle and Marantz's Distributed Morphology framework predicts this. From their point of view, eight or nine or even more of the sixteen options could perfectly well exist in a hypothetical variety of Polish that violates no constraints on how inflectional systems can be structured. In actual Polish, combination C ([–D, –G]) (for example) does not occur, which allows Halle and Marantz to state the generalization 'No noun impoverishes both Gen and Dat' (2008: 70); but there is no reason why this generalization should hold in all varieties of Polish.

Combination C would yield a class resembling 3 at Table 6.5 except in having -*u* instead of -*a* in the genitive. Let us call this 'class 3a'. If inflection classes are constrained by vocabular clarity, however, we will predict that a dialect of Polish with the extra class 3a could not exist—or, at least, would be a worrying anomaly warranting close examination. Let me explain why.

In Chapter 6 we worked out that, once stem alternation and its relationship with -*e* and -*u* were taken into account, the seven apparently massively blurred inflection classes at Table 6.5 collapse to only three:

(7)

	1, 2, 5	3, 4	6, 7
Nom	Ø	Ø	Ø
Gen	a	a	u
Dat	owi	u	owi
Instr	em	em	em
Loc	e~u	e~u	e~u
Voc	e~u	e~u	e~u

[10] '[–Dat]' is abbreviated as '[–D]', and so on.

But now let us add the new class 3a into the mix:

(8)

	1, 2, 5	3, 4	3a	6, 7
Nom	Ø	Ø	Ø	Ø
Gen	a	a	u	u
Dat	owi	u	u	owi
Instr	em	em	em	em
Loc	e~u	e~u	e~u	e~u
Voc	e~u	e~u	e~u	e~u

The one respect in which class 3a differs from class 3 is enough to destroy vocabular clarity in both the genitive and the dative. Of the two affixes in each of these cells, neither is a peculiar to one inflection class. Even if we treat -*u* as a global default affix, as Halle and Marantz suggest, that still leaves the genitive -*a* and the dative -*owi* in not one but two classes. Our approach thus makes a satisfyingly strong prediction: this hypothetical variety of Polish ought not to exist. By contrast, no such prediction follows from Halle and Marantz's approach. And it is not just class 3a that is problematic for us. Many other hypothetical choices of feature combinations A-R would yield inflection-class systems neatly describable by Halle and Marantz, yet disallowed by vocabular clarity. The vocabular clarity requirement would still be supported by the facts of actual Polish, therefore, even -*u* were indeed an all-purpose 'elsewhere' suffix.

In Halle and Marantz's analysis, differences in inflectional behaviour among Polish nouns are accounted for by lexical specifications that mention case features such as $[\pm \text{Dative}]$ and $[\pm \text{Locative}]$. What if, instead, recourse is had to features with no independent morphosyntactic interpretation, such as $[\pm\alpha]$, $[\pm\beta]$, $[\pm\gamma], \ldots$, as in Müller's (2004) analysis of Russian nouns? It is not a surprise that Russian nominal inflection classes turn out to be describable in this fashion. Rather, in view of the lack of any independent basis for such features and the consequent freedom with which they can be exploited, it will be a surprise if there is any conceivable inflection-class system that cannot be described in such a way.

There is vastly more that could be said both about syncretism and about checklist analyses of inflectional categories exploiting binary features. On the latter, I will have something more to say in the next chapter, while assessing how successful this book's abductive argument has been. But in this chapter I hope at least to have persuaded readers that my relative neglect of these topics is not a deficiency, from the point of view of this book's goals.

9

Conclusions

9.1 Evaluating the abductive argument

As we noted in Chapter 1, language evolution is a field of inquiry in which no hypotheses can be tested directly by experimentation. But this is not a fatal difficulty; palaeontologists and cosmologists face it too. Instead of experiments, we must rely on 'arguments to the best explanation', that is on abductive arguments. A good abductive argument is one which supports plausible assumptions about what is not observable by establishing links between a number of apparently unrelated facts and showing that they follow from well-established observations along with the assumptions that are being tested. An even better abductive argument is one that leads to novel predictions: in our case, predictions about characteristics of language that have not been previously noticed or, if noticed, have proved difficult to accommodate within currently fashionable theories. So how successful is the abductive reasoning offered in this book? To answer that, we need to identify the 'apparently unrelated facts', the 'well-established observations', the 'plausible assumptions', and (if appropriate) the 'novel predictions'.

As apparently unrelated facts, the following are relevant:

A. The fact that derived words and compounds show a greater tendency to acquire semantic and other idiosyncrasies (and hence to require lexical storage) than syntactic constructs, i.e. phrases, do—which accounts, at least in part, for the confusing usage of the term 'lexical'.
B. The relative independence of affixal inflection and non-affixal inflection (that is, stem alternation), as illustrated earlier in Russian and Italian especially.
C. The tendency for stem-alternation patterns to be either uniform (as in Italian and Dhaasanac) or nested (as in Russian and Polish).

As well-established observations, we have relied on the following:

D. The operation of a Principle of Contrast in vocabulary acquisition by children, so that new words are always assumed to mean something different from words already known.

E. The evidence for a similar principle at work in the cognitive processes of chimpanzees and at least one dog.

F. The maintenance of 'vocabular clarity' (or 'linguistic clarity') in multi-vocabular (or multilingual) speech communities.

G. Phonetic assimilation between neighbouring items in the speech chain, whereby a single item can over a few generations acquire two or more shapes whose distribution is no longer phonologically predictable.

As plausible assumptions about a prehistoric stage of language (more specifically, a stage at which there was a vocabulary but not yet necessarily any syntax), we have invoked the following:

H. Characteristic D already applied, because its presence in both chimpanzees and humans, and perhaps among mammals more widely (observation E), is mostly naturally explained as a shared inheritance, expediting the acquisition or fine-tuning of sign systems or call systems generally.

J. Characteristic F already applied, because the social and cognitive pressures militating against vocabulary mixture would already have operated in prehistoric human speech communities.

K. Characteristic G already applied, because the lack of any settled syntax need not entail a slow, halting rate of delivery in what was at that time not an unusual or deficient form of language but rather the only form of language in existence.

L. Some collocations of vocabulary items would have become institutionalized, and in some of those institutionalized collocations one of the items would have been both relatively unselective (exhibiting, in Matisoff's phrase, 'juxtrepository productivity') and relatively short (hence particularly vulnerable to phonetic erosion or divergence from its shape in other contexts).

M. Semantic relationships between some of these short unselective items would, even before the evolution of syntactic categories or 'parts of speech', have yielded 'cliché-pattern menus' as described in Chapter 5.

In the rest of this section we will consider how observations D–G and assumptions H–M account for facts A, B, and C. The question of what novel predictions, if any, follow from D–G and H–M will be taken up in section 9.2.

Fact A, treated in Chapter 7, is puzzling because, as shown in Chapter 2, there is no logical connection between being a complex word and being lexically listed. But the puzzle substantially disappears if morphology is a

product of processes that operated perhaps before syntax existed, and at any rate independently of it. At a time when there was only one linguistic category, namely 'vocabulary item', any institutionalized collocation or cliché (on the basis of L) had no choice but to be a new 'vocabulary item'. That by itself would have been enough for the brain to treat it as a candidate for listing. Furthermore, since all vocabulary items had to mean something different (by D and H), the brain needed to ensure that no two clichés were synonymous. That need would have favoured semantic drift, away from a meaning predictable on the basis of the cliché's components towards something more idiosyncratic. So, inasmuch as contemporary 'words' are the counterparts of protolinguistic 'vocabulary items', it is natural that they should retain this propensity towards idiosyncrasy. That is not to say that phrases cannot acquire idiosyncratic meanings too (so as to become 'idioms'). But this happens more seldom than with complex words, because phrases are subject to no inherited expectation of idiosyncrasy.

Facts B and C both derive from the tug-of-war between semantic distinctness (cognitively required, by H) and phonological multiformity (arising from characteristics of our articulatory apparatus, by K). The seeming lack of connection between B and C reflects the different mechanisms to which the brain would have resorted in order to deal with relatively long selective items and relatively short unselective items respectively. Long selective items plausibly provided the ancestral template for what in contemporary morphology we call 'roots' or 'stems', while short unselective items provided the template for 'affixes'. Only the latter, thanks to their participation in cliché-pattern menus (by L and M), lent themselves to differentiation through being interpreted as members of rival 'vocabularies', by virtue of F and J. The former (distinct 'stem alternants' in modern terms) would have had to differentiate themselves in some other way. Sometimes there would have been a convenient semantic or syntactic contrast to latch on to, as when a modern German noun, if it has two stems, uses the umlauted one in all and only plural contexts. But where that was not the case, our brains were content with differentiation on a purely formal basis. This might involve the phonological context, with one alternant appearing before vowels and the other before consonants (for example). Or one or more of the stem alternants could signal its own distribution, provided this distribution was consistent. Or a given alternant in a particular paradigmatic cell or cells could function as not a signifier but rather what is signified by a given alternant in one other cell— only one other cell, however, not two or more, or else synonymy would again rear its head (Chapter 6). Thus synonymy avoidance, by a roundabout route, accounts for both the kind of uniform distribution of alternants that we have

noted in Italian and Dhaasanac, and the kind of nested distribution that we noted in Russian and Polish.

There is nothing strange in all this once one recognizes that some aspects of the phonological shape of an utterance may reflect nothing outside morphology itself, serving only for synonymy avoidance. The view that morphology can operate 'by itself' has achieved wide acceptance (though not wide enough) since Mark Aronoff published *Morphology by Itself* in 1994. What I am suggesting here is a precise and deep-rooted cognitive function for many and perhaps all the kinds of allomorphy that Aronoff draws attention to.

9.2 A novel prediction: Inflection classes as 'vocabularies'

I classify as 'novel' the prediction that, in affixal inflection classes, obedience to vocabular clarity (other things being equal) predicts the non-occurrence of lavish inflection-class systems in which there is no limit in principle to the number of distinct classes that a given affix with a particular grammatical function can appear in. This prediction rules out the sort of inflection system illustrated schematically at (1), where there are four distinct inflection classes, yet every affix (represented by a lower-case letter) violates vocabular clarity:

(1)	Class A	Class B	Class C	Class D
Cell 1	a	a	b	b
Cell 2	c	d	c	d
Cell 3	e	e	f	f
Cell 4	g	h	h	g

To be more precise, the prediction is that, if an inflection-class system that looks something like (1) presents itself, there must be factors other than vocabulary membership that govern the distribution of at least some of the affixes. That is indeed what we discovered in respect of Polish masculine nouns. At first glance, as illustrated in Table 6.5, Polish presented severe difficulties for the notion that each affix must belong to just one vocabulary (or else function as the default for its cell, outside the domain of the contrasting vocabularies). Yet the difficulties melted away once we noted the syntagmatic factor (namely stem alternation) that accounted for the distribution of the locative and vocative suffixes.

The effect of this prediction is not new, inasmuch as it is the same as that of the No Blur Principle proposed by Carstairs-McCarthy (1994) and defended in respect of Polish by Cameron-Faulkner and Carstairs-McCarthy (2000). The basis of it is new, however, in that, whereas the No Blur Principle rested

on Eve Clark's (1993) Principle of Contrast (that is, directly on synonymy avoidance), I am now suggesting that the restriction on inflection classes is a special case of the way in which synonymy avoidance is implemented in multivocabular situations, such as those of Dyirbal, Guugu Yimidhirr, and Javanese.

The fact that this novel prediction has the same empirical effect as an older one raises an obvious question. Since the No Blur Principle (or 'NBP') was proposed in 1994, has it withstood confrontation with further evidence? My answer is: well enough to support vocabular clarity as a plausible contributor to explaining the distribution of potentially synonymous inflectional affixes. Yet this answer may seem surprisingly optimistic in the light of a discussion of the NBP by Stump (2005: 283–92), headed 'The No Blur Principle is not valid'. Stump claims that the Principle is incompatible with certain Sanskrit data. What's more, he concludes: 'Readers familiar with other heavily inflected languages will have no trouble uncovering comparable counterevidence to the NBP. In view of such counterevidence, it is clear that . . . the NBP is itself invalid' (2005: 292).

Despite Stump's confidence, however, I am aware of only three other explicit attempts to disprove the NBP by appeal to specific evidence. The languages are:

- Old Russian, mentioned in n. 8 in Chapter 8;
- Polish (Halle and Marantz 2008), already discussed in Chapters 6 and 8;
- Icelandic (Müller 2005).

I will deal with in Icelandic in section 9.2.2; but first, we need to take a look at Stump's Sanskrit arguments.[1]

9.2.1 *Possible counterevidence from Sanskrit*

Within the immensely complex inflectional morphology of Sanskrit, Stump identifies three pairs of affixes whose behaviour is said to violate the NBP in that each affix neither identifies its inflection class (so as to exhibit 'vocabulary clarity') nor serves as a 'class-default' affix, so as to appear in all those inflection classes that lack an affix of their own:

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[1] Müller (2007: 13–14) mentions a number of studies that supposedly present counterexamples to the No Blur Principle. But none of these discusses the Principle directly, so far as I am aware; and indeed one of them (Trommer 2008) deals not with affixal inflection at all, but rather with stem alternations in Amharic verbs—which, as mentioned in section 6.4, seem to observe perfectly the constraints proposed in Chapter 6.

(2)   In Classical Sanskrit, *i* and *a:m* as singular locative suffixes on nouns.

(3)   In Classical Sanskrit, *i:* and *au* as dual direct-case (i.e. nominative and accusative) suffixes on nouns.

(4)   In Vedic Sanskrit, *(a)n* and *ur* as 3rd person plural active indicative suffixes in the perfect and aorist tenses of verbs.

It is evident straightaway that Sanskrit does not display wholesale paradigm mixture of the kind presented at (1), with blurring in every cell of the paradigm. What we see instead is apparent blurring in two out of about nineteen nominal cells and two out of several hundred verbal cells.[2]

As regards (2), Stump points out that several classes of nouns tolerate both *i* and *a:m* as locative singular suffixes: 'they alternate dialectally if not freely in some paradigms' (2005: 290). These suffixes thus resemble -*t* and -*ed* in English, where *dreamt* and *knelt* compete with *dreamed* and *kneeled*, for instance. For English one can reasonably expect that one or another form will ultimately triumph in each dialect. It is harder to know what to make of the situation in Classical Sanskrit, for which we are reliant on texts that were nearly all composed long after the language had ceased to be anyone's mother tongue. So it seems risky to use the *i* and *a:m* forms as part of the basis for a judgement on the NBP.

As regards (3), as Stump acknowledges, gender looks to be at first sight a differentiating factor, with *i:* on neuter nouns and *au* on masculine and feminine nouns. However, he rejects this, on the ground that one class of feminine nouns, represented by SENA: 'army', takes *i:* (thus *sena:-i:* 'two armies'), while other feminines take *au*. But this objection is undermined by the fact that the form meaning 'two armies' is not in fact *sena:-i:* but *sene*, and similarly for all nouns of this class. If we classify *e* as a third rival suffix, then it is unique to one class and so satisfies the No Blur Principle, while *i:* and *au* in their remaining unequivocal occurrences correlate neatly with neuter and non-neuter gender respectively.

Stump no doubt has in mind a productive process of vowel assimilation in Sanskrit (one kind of 'vowel sandhi') that turns all underlying sequences of the form /a(:) i(:)/ to [e]. But that does not mean that surface [e] must always correspond to a synchronic underlying sequence ending in the vowel /i/. It may be that historically the suffix -*e* in *sene* is derived from earlier **-a:-i:* or

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[2] The vague figure 'about nineteen' reflects the fact that, although Sanskrit has eight cases and three numbers, there are arguably fewer than twenty-four relevant cells, because no noun distinguishes more than three forms in the dual. But the analysis of the inflectional syncretisms there need not concern us.

suchlike; but that is not a basis for positing /i:/ as the suffix synchronically, any more than one would posit an underlying representation /sain/ rather than /sen/ for the stem.

That leaves (4). The pattern is complicated, and I will not attempt a detailed reply to Stump's analysis. Suffice it to say that problematic distribution of the suffixes *an* and *ur* is restricted to one mood (indicative, not subjunctive or optative) and two tenses (imperfect and aorist, not present or perfect). Within imperfects it is further restricted to the so-called '2nd conjugation' (there being nine other conjugations), and within aorists it is limited to so-called 'root aorists' that have a root vowel other than *a:*. The most sensible reaction, it seems to me, is to note *an* and *ur* as a problem, but by itself not a sufficiently serious problem to warrant abandoning at once the attractions of the hypothesis about morphological evolution presented here.

9.2.2 *Possible counterevidence from Icelandic nouns*

The inflection of Icelandic nouns has been discussed in two articles by Gereon Müller (2005, 2007). In Icelandic, a limited repertoire of inflectional suffixes (of the shape *V*, *Vr*, *s*, *um*, and *(n)a*) is distributed over an apparently generous array of inflection classes, which Müller presents as in Table 9.1. Emphasizing the lavish use that Icelandic makes of limited resources, Müller focuses on homonymy, both intraparadigmatic, i.e. between cases, and also (as he puts it) transparadigmatic, i.e. between inflection classes. His complaint against the No Blur Principle is that it draws attention away from this homonymy and overemphasizes the importance of affixes that are peculiar to one inflection class (2005: 259–61). Yet one could also argue that, if the No Blur Principle turns out after all to be observed even in a system with the degree of suffixal homonymy (whether systematic or not) that Icelandic nouns exhibit, then its observance is all the more interesting! So what is the verdict? I will offer reasons for thinking that, for the hypothesis of vocabular clarity in inflection, the news from Icelandic is by no means bad.

The data at Table 9.1 look at first sight unpromising. In all but one of the eight cells, more than one affix appears in two or more of the twelve inflection classes. But this presupposes that an analysis in terms of twelve independent inflection classes—twelve affixal vocabularies, according to the view suggested here—is correct. Yet the twelve classes are not independent. Each is in fact restricted to just one gender, masculine (M), feminine (F), or neuter (N), the various classes for each gender being distinguished in Table 9.1 as 'a(-stem)', 'i(-stem)', 'c(onsonant-stem)', and 'w(eak)'. At Table 9.2 is a

Table 9.1. Affixal inflection of Icelandic nouns (following Müller)

	I Ma[a]	II Na	III Fa(ʹ)	IV Mi	V Fi	VI Mu	VII Mc	VIII Fc1	IX Fc2	X Mw	XI Nw	XII Fw
SgN	ur	Ø	Ø	ur	Ø	ur	ur	Ø	Ø	i	a	a
SgA	Ø	Ø	Ø (u)	Ø	Ø	Ø	Ø	Ø	Ø	Ø	a	u
SgD	i	i	Ø (u)	Ø	Ø	i	i	Ø	Ø	Ø	a	u
SgG	s	s	ar	ar	ar	ar	ar	ar	ur	a	a	u
PlN	ar	Ø	ar	ir	ir	ir	ur	ur	ur	ar	u	ur
PlA	a	Ø	ar	i	ir	i	ur	ur	ur	a	u	ur
PlD	um	um	um	um	um	um	um	um	um	um	um	um
PlG	a	a	a	a	a	a	a	a	a	a	(n)a	(n)a

[a] The significance of this row of letters and figures is explained in the text. In the leftmost column, 'N', 'A', 'D', and 'G' stand for 'nominative', 'accusative', 'dative', and 'genitive'.

presentation of the data which exploits that fact so as to reduce the number of distinct inflectional 'vocabularies' from twelve to five. The swung dashes indicate instances where vocabular clarity interacts with gender: thus, in class I/II/III the class-specific suffix *s* is found only in masculines and neuters, while feminines take the class-default suffix *ar* that is shared with classes IV/V/VI and VII/VIII.

Underlines in Table 9.2 draw attention to apparent problems for vocabular clarity; yet all these problems are of a kind that, though they certainly invite further investigation, do not suggest fundamental flaws in the hypothesis. In the accusative and dative singular, some but not all feminine nouns of class I/II/III have a suffix *u* rather than no suffix; yet, according to Müller (2005: 232, citing Kress 1982: 66), this *u* appears 'primarily' on stems with the abstract-noun-forming suffix *ing* or *ung*. At once it seems likely that, for some speakers, a syntagmatic factor (the identity of the preceding suffix) cross-cuts and

Table 9.2. Icelandic inflection classes combined on the basis of gender

	I/II/III M∼N∼F	IV/V/VI M∼F	VII/VIII M∼F	IX F	X/XI/XII M∼N∼F
SgN	ur∼Ø∼Ø	ur∼Ø	ur∼Ø	Ø	i∼a∼a
SgA	Ø∼Ø∼<u>Ø/u</u>	Ø	Ø	Ø	a∼a∼u
SgD	i∼i∼<u>Ø/u</u>	<u>Ø/i</u>∼Ø	i∼Ø	Ø	a∼a∼u
SgG	s∼s∼ar	ar	ar	ur	a∼a∼u
PlN	ar∼Ø∼ar	ir	ur	ur	<u>ar</u>∼u∼ur
PlA	a∼Ø∼ar	i∼ir	ur	ur	<u>a</u>∼u∼ur
PlD	um	um	um	um	um
PlG	a	a	a	a	<u>a/(n)a</u>

overrides paradigmatic factors influencing vocabulary choice. In the dative singular, some but not all masculines have *i*; but, since what *i* competes with is a bare stem form rather than another suffix, the question of differentiating suffixal meanings does not arise, and *i* can still be classified as the default suffix for that cell.

In the plural, a new possibility presents itself: perhaps plural and singular are distinct vocabulary domains, rather like respect vocabulary and honorific vocabulary in Javanese. If so, for us to expect vocabulary clarity across all eight cells, both singular and plural, is a mistake. In Polish, we considered only the singular forms of masculine nouns, not the plural forms, because strictly there are no masculine plural forms at all in Polish, only 'virile' plural forms (belonging to nouns denoting human males) and 'non-virile' forms. That consideration does not apply in Icelandic.[3] Nevertheless, if we allow ourselves to explore the possibility, the effect is dramatic:

(5) *Singular-only vocabularies:*

	I/II	III/IV/V/VI/VII/VIII	IX	X/XI/XII
	M∼N	M∼F	(F)	M∼N∼F
SgN	ur∼Ø	ur∼Ø	Ø	i∼a∼a
SgA	Ø	Ø∼Ø/u	Ø	a∼a∼u
SgD	i	Ø/i∼Ø/u	Ø	a∼a∼u
SgG	s	ar	ur	a∼a∼u

(6) *Plural-only vocabularies:*

	I/II/III/X	IV/V/VI	VII/VIII/IX	XI/XII
	M∼N∼F	M∼F	(M, F)	N∼F
PlN	ar∼Ø∼ar	ir	ur	u∼ur
PlA	a∼Ø∼ar	i∼ir	ur	u∼ur
PlD	um	um	um	um
PlG	a	a	a	(n)a

Nearly all problems of vocabular clarity now disappear. The suffix *i*, which alternates with zero in the dative singular, now emerges as the default masculine suffix for that cell, while *u* is the default feminine suffix there and also in the accusative singular. Masculine 'weak' nouns (class X in Müller's numbering) emerge as having no plural forms of their own; they join up with

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[3] Müller (2004: 236–7) argues that singular and plural constitute separate domains (for syncretism purposes) on the ground that a difference in number (singular versus plural) is semantic whereas differences in case are purely syntactic. But it seems strange to deny that the genitive and dative cases, at least, have in some contexts a semantic as well as a syntactic function.

the 'strong' classes I/II in the plural. This helps to make sense of the one remaining problem: the distribution of *na* as a genitive plural suffix in 'weak' nouns (classes X, XI, and XII), alongside *a*. As Müller (2005: 262) puts it: 'The consonantal segment has disappeared to varying degrees in modern Icelandic—almost completely with masculines, to some extent with feminines...and least of all with neuters, of which there aren't many in the first place.' It is surely not an accident that weak neuters (class XI) have a nominative-accusative plural suffix *u* that is vocabulary-specific (or class-identifying), by contrast with weak feminines (class XII), whose suffix *ur* is shared with class VII–IX and hence qualifies as the default for nominative-accusative plural. A vocabulary consisting of *u, u, um,* and *na,* with both *u* and *na* as class-identifiers, would be relatively easier for the learner's brain to pick out than a vocabulary consisting of *ur, ur, um,* and *na,* with just *na* as a class-identifier.[4] So it is understandable that weak feminine plural forms should slide across to the VII–IX class, from which they differed only in the genitive, while weak neuter plural suffixes, being differentiated in more cells, retained their status as a distinct vocabulary.

What I have just said illustrates how complex are the problems that learners' brains are confronted with in making sense of the Icelandic system. But it also illustrates the sort of contribution that vocabular clarity can make towards understanding observed vacillations within the system. In fact, we can go so far as to predict vacillations that should be likely to arise. In (5), as it stands, genitive singular *s* and *ar* are differentiated through belonging to distinct vocabularies: *s* to class I/II and *ar* to class III–VIII. But they are not gender-neutral, in that class I/II contains no feminines and class III–VII contains no neuters. So one is inclined to predict that the brain will be tempted to treat *s* and *ar* as differentiated by gender within a single large class I–VIII, *ar* being for feminines only and *s* for non-feminines. If so, masculines that are indicated as having *ar* in the genitive singular (those in classes IV and VI) should tend to replace it with *s*. And it turns out that some masculine nouns do indeed display genitive singular *s* even while having a nominative plural *ir* (Kress 1982: 68). On the face of it, these constitute a further class not included in Müller's list at Table 9.1. The addition of yet another class would seem to be bad news for an account based on vocabular clarity. But, in terms of the analysis at (5), it suggests that, on the contrary, Icelandic may be on the way towards reducing the number of singular-only vocabularies by combining the first two of them.

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[4] The nominative-accusative homonymies (with *u* and *ur*) are surely systematic, but that fact is irrelevant here.

I have suggested reasons for thinking that Icelandic poses fewer problems than Müller suggests. There is one further point to be made. Müller (2007: 3) emphasizes the desirability of explanatory assumptions that are 'independently motivated', being preferable to 'specific constraints that explicitly impose restrictions on possible inflection classes (. . . like the No Blur Principle)'. He argues that his own approach is based on an independently motivated assumption, namely the Syncretism Principle ((2) in Chapter 8). I agree with him about the importance of independent motivation. However, on the basis of the pervasiveness of homonymy in ordinary vocabulary, I suggested in Chapter 8 that the Syncretism Principle was not well motivated.[5] At the same time, inasmuch as the No Blur Principle is now replaced by the hypothesis of vocabular clarity in inflectional affixation, based in turn on an evolutionary explanation for some of morphology's central quirks, my account is now backed by a kind of motivation that (according to Müller) the No Blur Principle lacked.[6]

In any case, is it really likely that the same mechanism (whatever that may be) can successfully handle both familiar syncretisms ('intraparadigmatic', in Müller's terms) and also 'transparadigmatic syncretism'? Familiar syncretisms raise the issue of what to make of sameness of form where meaning or function are (at least superficially) different. But inflection classes and their behaviour raise a diametrically opposed issue: what to make of difference of form where meaning or function are (at least superficially) the same. This is one aspect of the problem of allomorphy, which (as I argued in Chapter 8) has been neglected recently in favour of what we may call 'homomorphy' (that is, morphological homonymy).

9.3 Summing up: Morphology, the spine, and the peacock's tail

'Nothing in biology makes sense except in the light of evolution.' That is the title of a famous article by the biologist Theodosius Dobzhansky (1973). In relation to language, Dobzhansky's remark has already been cited with approval by James R. Hurford (2007: x). I agree with Hurford wholeheartedly.

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[5] Müller says in a footnote (2007: 3): 'I have nothing to say here about instances of homonymy outside of inflectional morphology.' But a bald assertion of this kind does not constitute an argument that homonymy outside of inflectional morphology is irrelevant.

[6] I say 'according to Müller' because the No Blur Principle was originally presented as a special case of Eve Clark's Principle of Contrast (1993). It therefore rested on findings in developmental psycholinguistics. However, the evolutionary approach defended in this book rests on a broader version of that cognitive characteristic, shared by humans with some other mammalian species.

It is not that all morphology is (or should be) evolutionary morphology, any more than all biology is the study of evolution. There are no doubt many details of the morphology of contemporary languages that morphological evolution sheds no special light on. Yet evolution does turn out to shed surprisingly bright light on the otherwise mysterious fundamental architecture of this branch of grammar.

In Chapter 2 (section 2.4) I mentioned Chomsky's (2004b) discussion of snowflakes and spines from the point of view of design. The spine is an inefficient mechanism for supporting a mammalian body in a vertical posture, but it is the mechanism that we humans have been unavoidably saddled with because of our evolutionary history. I suggested that, whatever one thinks about syntax, morphology at least may be more spine-like than snowflake-like. But perhaps a better analogy is with not spines but peacocks' tails. Peacocks' tails are a cliché example of what looks like a bizarre encumbrance, difficult to account for as an evolutionary adaptation. A biologist would reply that it is not the tail itself that has adaptative advantages, but rather the efficiency with which peahens can pick out the strongest males to mate with, the tail being a reliable indicator of male strength. The tail's size illustrates a kind of hypertrophy—the runaway replication of a colourful design which has taken place simply because there was nothing to impede it. In language, similarly, the human brain's extraordinary capacity for learning and memorizing vocabulary meant that, as soon as phonological processes began to yield noticeably different forms that risked having the same meaning, there was little to impede a bizarre kind of runaway vocabulary elaboration through the attachment to potential synonyms of a multiplicity of differentiating factors. The outcome of this runaway vocabulary elaboration is what we now know as morphology.[7]

Marcus (2008) draws attention to how badly designed the human brain is. Human intelligence has no doubt been honed by natural selection, yet all the same it is an inelegant concretion of ill-fitting bits and pieces. Similarly, grammar (one product of the human brain) does its job well enough, but in most languages this involves the inelegant yoking together of syntax on the

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[7] Morphology, particularly inflectional, has often been cited as a kind of linguistic complexification that certain varieties of language systematically lack, such as creoles. But there is such a thing as complexity in syntax too (see e.g. Dahl 2004; Miestamo et al. 2008; Sampson et al. 2009). My guess is that syntactic complexity is another manifestation of the human brain's capacity for decorative elaboration, instantiated however in areas less closely linked historically to synonymy avoidance at the vocabulary-only stage of language evolution. However, this is only a guess; the complexity issue has only just begun to be addressed.

one hand and, on the other, a hypertrophied conglomeration of synonymy-avoidance devices. In Chapter 2, I envisaged an intelligent Martian asking incredulously why human language should have saddled itself with not one pattern of grammatical organization but two. I hope that any Martian readers of this book will feel that their question has been at least partly answered.

References

Aarts, Bas, Denison, David, Keizer, Evelyn, and Popova, Gergana (eds) (2004). *Fuzzy Grammar: A Reader*. Oxford: Oxford University Press.

Alexiadou, Artemis and Müller, Gereon (2008). Class features as probes. In Bachrach and Nevins (eds), 101–5.

Andersen, Henning (1973). Abductive and deductive change. *Language* 49: 567–95.

Anderson, Stephen R. (1982). Where's morphology? *Linguistic Inquiry* 13: 571–612.

—— (1986). Disjunctive ordering in inflectional morphology. *Natural Language and Linguistic Theory* 4: 1–31.

—— (1992). *A-morphous Morphology*. Cambridge: Cambridge University Press.

—— (2005). *Aspects of the Theory of Clitics*. Oxford: Oxford University Press.

—— (2008). Phonologically conditioned allomorphy in the morphology of Surmiran (Rumantsch). *Word Structure* 1: 109–34.

Aronoff, Mark (1976). *Word Formation in Generative Grammar*. (Linguistic Inquiry Monographs 1.) Cambridge, MA: MIT Press.

—— (1994). *Morphology By Itself: Stems and Inflectional Classes*. Cambridge, MA: MIT Press.

—— (2007). In the beginning was the word. *Language* 83: 803–30.

Bachrach, Asaf and Nevins, Andrew (eds.) (2008). *Inflectional Identity*. Oxford: Oxford University Press.

Baerman, Matthew, Brown, Dunstan, and Corbett, Greville C. (2005). *The Syntax-Morphology Interface: A Study of Syncretism*. Cambridge: Cambridge University Press.

Bánhidi, Zoltán, Jókay, Zoltán, and Szabó, Dénes (1965). *Learn Hungarian* (4th edn). Budapest: Tankönyvkiadó.

Bayer, Josef (1990). Comments on the paper by Frazier. In Booij and van Marle (eds), 107–14.

Bickerton, Derek (1990). *Language and Species*. Chicago: University of Chicago Press.

—— (1995). *Language and Human Behavior*. Seattle: University of Washington Press.

Bierwisch, Manfred (1967). Syntactic features in morphology: general problems of the so-called pronominal inflection in German. In *To Honor Roman Jakobson: Essays on the Occasion of his Seventieth Birthday*. The Hague: Mouton, 239–70.

Bittner, Andreas (1985). Implikative Hierarchien in der Morphologie: das 'Stark-Schwach-Kontinuum' der neuhochdeutschen Verben. *Acta Linguistica Academiae Scientiarum Hungaricae* 35: 31–42.

—— (1996). *Starke 'schwache' Verben—schwache 'starke' Verben: deutsche Verbalflexion und Natürlichkeit*. Tübingen: Stauffenburg.

Blevins, James (2004). Inflection classes and economy. In Müller et al. (eds), 51–95.

—— (2005). Word-based declensions in Estonian. In Booij and van Marle (eds) (2005b), 1–25.

—— (2006). Word-based morphology. *Journal of Linguistics* 42: 531–73.

Blevins, Juliette (1995). The syllable in phonological theory. In John A. Goldsmith (ed.), *The Handbook of Phonological Theory*. Oxford: Blackwell, 206–44.

—— (2004). *Evolutionary Phonology: The Emergence of Sound Patterns*. Cambridge: Cambridge University Press.

Bloch, Bernard (1947). English verb inflection. *Language* 23: 399–418. Reprinted in Joos (ed.) (1966), 243–54.

Bloom, Paul (2000). *How Children Learn the Meaning of Words*. Cambridge, MA: MIT Press.

Bloomfield, Leonard (1933). *Language*. New York: Holt.

Booij, Geert and Marle, Jaap van (eds) (1990). *Yearbook of Morphology 1990*. Dordrecht: Foris.

—— —— (eds) (2005a). *Yearbook of Morphology 2004*. Dordrecht: Springer.

—— —— (eds) (2005b). *Yearbook of Morphology 2005*. Dordrecht: Springer.

Botha, Rudolf P. (1981). A base rule theory of Afrikaans synthetic compounding. In Michael Moortgat, Harry van der Hulst, and Teun Hoekstra (eds), *The Scope of Lexical Rules*. Dordrecht: Foris, 1–77.

Bresnan, Joan and Mchombo, Sam (1995). The lexical integrity principle: evidence from Bantu. *Natural Language and Linguistic Theory* 13: 181–254.

Brown, Dunstan, Corbett, Greville, Fraser, Norman, Hippisley, Andrew, and Timberlake, Alan (1996). Russian noun stress and network morphology. *Linguistics* 34: 53–107.

Bryson, Bill (2003). *A Short History of Nearly Everything*. New York: Random House.

Bybee, Joan (1985). *Morphology: A Study of the Relation between Meaning and Form*. Amsterdam: Benjamins.

Cameron-Faulkner, Thea and Carstairs-McCarthy, Andrew (2000). Stem alternants as morphological signata: evidence from blur avoidance in Polish nouns. *Natural Language and Linguistic Theory* 18: 813–35.

Carey, Susan (1978). The child as word learner. In Morris Halle, George A. Miller, and Joan Bresnan (eds), *Linguistic Theory and Psychological Reality*. Cambridge, MA: MIT Press, 264–93.

Carstairs, Andrew (1984). Paradigm economy in the Latin third declension. In *Transactions of the Philological Society 1984*. Oxford: Blackwell, 117–37.

—— (1985). Paradigm economy in Latin nouns. In Jacek Fisiak (ed.), *Papers from the 6th International Conference on Historical Linguistics*. Amsterdam/Poznan: Benjamins/Adam Mickiewicz University Press, 57–70.

—— (1987). *Allomorphy in Inflexion*. London: Croom Helm.

—— (1988a). Nonconcatenative inflection and paradigm economy. In Michael Hammond and Michael Noonan (eds), *Theoretical Morphology: Approaches in Modern Linguistics*. San Diego: Academic Press, 71–7.

—— (1988b). Review of Craig (1986). *Canadian Journal of Linguistics* 33: 173–6.

Carstairs, Andrew (1988c). Some implications of phonologically conditioned suppletion. In Geert Booij and Jaap van Marle (eds), *Yearbook of Morphology 1988*. Dordrecht: Foris, 68–94.

—— (1990). Phonologically conditioned suppletion. In Wolfgang U. Dressler, Hans Luschützky, Oskar E. Pfeiffer, and John R. Rennison (eds), *Contemporary Morphology*. Berlin: Mouton de Gruyter, 17–23.

Carstairs-McCarthy, Andrew (1991). Inflection classes: two questions with one answer. In Frans Plank (ed.), *Paradigms: The Economy of Inflection*. Berlin: Mouton de Gruyter, 213–53.

—— (1994). Inflection classes, gender and the Principle of Contrast. *Language* 70: 737–88.

—— (1998a). Comments on the paper by Noyer. In Lapointe et al. (eds), 286–301.

—— (1998b). How lexical semantics constrains inflectional allomorphy. In Geert Booij and Jaap van Marle (eds), *Yearbook of Morphology 1997*. Dordrecht: Kluwer, 1–24.

—— (1999). *The Origins of Complex Language*. Oxford: Oxford University Press.

—— (2001a). Grammatically conditioned allomorphy, paradigm structure and the Ancestry Constraint. *Transactions of the Philological Society* 99: 223–45.

—— (2001b). Umlaut as signans and signatum: synchronic and diachronic aspects. In Geert Booij and Jaap van Marle (eds), *Yearbook of Morphology 1999*. Dordrecht: Kluwer, 1–23.

—— (2005a). The evolutionary origin of morphology. In Tallerman (ed.), 166–84.

—— (2005b). Phrases inside compounds: a puzzle for lexicon-free morphology. *SKASE Journal of Theoretical Linguistics* 2: 34–42. Available online at <http://www.skase.sk/>.

—— (2008). System-congruity and violable constraints in German weak declension. *Natural Language and Linguistic Theory* 26: 775–93.

Chantraine, Pierre (1973). *Morphologie historique du grec*. Paris: Klincksieck.

Cheney, Dorothy L. and Seyfarth, Robert M. (1990). *How Monkeys See the World*. Chicago: University of Chicago Press.

Chomsky, Noam (1957). *Syntactic Structures*. The Hague: Mouton.

—— (1970). Remarks on nominalization. In Roderick A. Jacobs and Peter S. Rosenbaum (eds), *Readings in English Transformational Grammar*. Waltham, MA: Ginn, 184–221.

—— (1986). *Knowledge of Language: Its Nature, Origin and Use*. New York: Praeger.

—— (2000). *New Horizons in the Study of Language and Mind*. Cambridge: Cambridge University Press.

—— (2001). Beyond explanatory adequacy. Cambridge, MA: MIT Occasional Papers in Linguistics 20. Reprinted in Adriana Belletti (ed.) (2004), *Structures and Beyond*. New York: Oxford University Press, 104–31.

—— (2004a). Language and mind: current thoughts on ancient problems. In Lyle Jenkins (ed.), *Variation and Universals in Biolinguistics*. Amsterdam: Elsevier, 379–405.

—— (2004b). Linguistics in the 21st century. In Noam Chomsky (ed.), *The Generative Enterprise Revisited*. Berlin: Mouton de Gruyter, 147–87.

—— (2005). Three factors in language design. *Linguistic Inquiry* 36: 1– 22.

—— and Halle, Morris (1968). *The Sound Pattern of English*. New York; Harper & Row.

Clark, Eve V. (1993). *The Lexicon in Acquisition*. Cambridge: Cambridge University Press.

Clements, G. N. and Hume, Elizabeth V. (1995). The internal organization of speech sounds. In John A. Goldsmith (ed.), *The Handbook of Phonological Theory*. Oxford: Blackwell, 245–306.

Comrie, Bernard (1989). *Language Universals and Linguistic Typology* (2nd edn). Oxford: Blackwell.

—— (1992). Before complexity. In John A. Hawkins and Murray Gell-Mann (eds), *The Evolution of Human Languages*. Redwood City: Addison-Wesley, 193–211.

Corbett, Greville (1991). *Gender*. Cambridge: Cambridge University Press.

—— and Fraser, Norman (1993). Network Morphology: a DATR account of Russian nominal inflection. *Journal of Linguistics* 29: 113–42.

Côté, Marie-Hélène (2008). Empty elements in schwa, liaison and h-aspiré: the French holy trinity revisited. In Jutta M. Hartmann, Veronika Hegedűs, and Henk van Riemsdijk (eds), *Sounds of Silence: Empty Elements in Syntax and Phonology*. Amsterdam: Elsevier, 61–103.

Craig, Colette (ed.) (1986). *Noun Classes and Subcategorization*. Amsterdam: John Benjamins.

Dahl, Östen (2004). *The Growth and Maintenance of Linguistic Complexity*. Amsterdam: Benjamins.

Dawkins, Richard (1995). *River Out of Eden*. New York: Basic Books.

de Lacy, Paul (2004). Maximal words and the Maori passive [revised version]. In John J. McCarthy (ed.), *Optimality Theory in Phonology: A Reader*. Oxford: Blackwell, 495–512.

Dembski, William A. and Michael Ruse (eds.) (2004). *Debating Design: from Darwin to DNA*. Cambridge: Cambridge University Press.

Di Sciullo, Maria and Williams, Edwin (1987). *On the Definition of Word* (Linguistic Inquiry Monographs 14). Cambridge, MA: MIT Press.

Dixon, R. M. W. (1971). A method of semantic description. In Danny D. Steinberg and Leon A. Jakobovits (eds), *Semantics: An Interdisciplinary Reader in Philosophy, Linguistics and Psychology*. Cambridge: Cambridge University Press, 436–71.

—— (1972). *The Dyirbal Language of North Queensland*. Cambridge: Cambridge University Press.

Dobzhansky, Theodosius (1973). Nothing in biology makes sense except in the light of evolution. *The American Biology Teacher* 35, 125–9. Reprinted in Mark Ridley (ed.) (1997), *Evolution*. Oxford: Oxford University Press, 378–87.

Doke, Clement (1973). *Textbook of Zulu Grammar* (6th edn). Cape Town: Longman.

Donaldson, Bruce C. (1993). *A Grammar of Afrikaans*. Berlin: Mouton de Gruyter.

Dressler, Wolfgang U. (1977). *Grundfragen der Morphonologie*. Vienna: Verlag der Österreichischen Akademie der Wissenschaften.

—— (1985). *Morphonology: The Dynamics of Derivation*. Ann Arbor: Karoma.

—— Mayerthaler, Willi, Panagl, Oswald, and Wurzel, Wolfgang Ullrich (eds) (1987). *Leitmotifs in Natural Morphology*. Amsterdam: Benjamins.

Embick, David and Halle, Morris (2005). On the status of stems in morphological theory. In Twan Geerts, Ivo van Ginneken, and Haike Jacobs (eds), *Romance Languages and Linguistic Theory 2003*. Amsterdam: John Benjamins, 59–88.

Emonds, Joseph E. (2002). A common basis for syntax and morphology: tri-level lexical insertion. In Paul Boucher (ed.), *Many Morphologies*. Somerville, MA: Cascadilla Press, 235–62.

Ernout, Alfred (1953). *Morphologie historique du latin*. Paris: Klincksieck.

Errington, J. Joseph (1988). *Structure and Style in Javanese*. Philadelphia, PA: University of Pennsylvania Press.

Evans, Vyvyan, Bergen, Benjamin, and Zinken, Jörg (eds) (2007). *The Cognitive Linguistics Reader*. London: Equinox.

Fitch, W. Tecumseh, Hauser, Marc D., and Chomsky, Noam (2005). The evolution of the language faculty: clarifications and implications. *Cognition* 97: 179–210.

Forsyth, James (1963). *A Practical Guide to Russian Stress*. Edinburgh: Oliver & Boyd.

Frazier, Lyn (1990). Identifying structure under X°. In Booij and van Marle (eds), 87–105.

Gardner, R. Allen, Gardner, Beatrix T., and Van Cantfort, Thomas E. (1989). *Teaching Sign Language to Chimpanzees*. Albany, NY: SUNY Press.

Geertz, Clifford (1960). Linguistic etiquette. (Excerpt from *The Religion of Java*, Glencoe: Free Press.) Reprinted in J. B. Pride and Janet Holmes (eds) (1972), *Sociolinguistics*. Harmondsworth: Penguin, 167–79.

Givón, Talmy (1971). Historical syntax and synchronic morphology: an archaeologist's field trip. *Proceedings of the Chicago Linguistic Society* 7: 394–415.

Grandgent, C. H. and Wilkins, E. H. (1915). *Italian Grammar* (rev. edn). Boston: D. C. Heath.

Gussmann, Edmund (2007). *The Phonology of Polish*. Oxford: Oxford University Press.

Hale, Kenneth (1973). Deep-surface canonical disparities in relation to analysis and change: an Australian example. In Thomas A. Sebeok (ed.), *Current Trends in Linguistics* (*Vol. 11: Diachronic, Areal, and Typological Linguistics*). The Hague: Mouton, 401–58.

Halle, Morris and Marantz, Alec (1993). Distributed Morphology and the pieces of inflection. In Kenneth Hale and Samuel Jay Keyser (eds), *The View from Building 20*. Cambridge, MA: MIT Press, 111–76.

—— —— (2008). Clarifying 'blur': paradigms, defaults, and inflectional classes. In Bachrach and Nevins (eds), 55–72.

Harley, Heidi and Noyer, Rolf (2003). Distributed Morphology. In Lisa Cheng and Rint Sybesma (eds), *The Second Glot International State-of-the-Art Book: The Latest in Linguistics*. Berlin: Mouton de Gruyter, 463–96.

Haspelmath, Martin (2006). Against markedness (and what to replace it with). *Journal of Linguistics* 42: 25–70.

Hauser, Marc D., Chomsky, Noam, and Fitch, W. Tecumseh (2002). The language faculty: what is it, who has it, and how did it evolve? *Science* 298: 1569–79.

Haviland, John B. (1979a). Guugu Yimidhirr brother-in-law language. *Language in Society* 8: 365–93.

——(1979b). How to talk to your brother-in-law in Guugu Yimidhirr. In Timothy Shopen (ed.), *Languages and their Speakers*. Cambridge, MA: Winthrop, 161–240.

Heath, Jeffrey (1998). Hermit crabs: formal renewal of morphology by phonologically mediated affix substitution. *Language* 74: 728–59.

Heine, Bernd, Claudi, Ulrike, and Hunnemeyer, Friederike (1991). *Grammaticalization: A Conceptual Framework*. Chicago: University of Chicago Press.

——and Kuteva, Tania (2002). On the evolution of grammatical forms. In Alison Wray (ed.), *The Transition to Language*. Oxford: Oxford University Press, 376–97.

————(2007). *The Genesis of Grammar: A Reconstruction*. Oxford: Oxford University Press.

Hinzen, Wolfram (2006). *Mind Design and Minimal Syntax*. Oxford: Oxford University Press.

Hockett, Charles F. (1954). Two models of grammatical description. *Word* 10: 210–31. Reprinted in Joos (ed.) (1966), 386–99.

——(1958). *A Couse in Modern Linguistics*. New York: The Macmillan Company.

Hopper, Paul J. and Traugott, Elizabeth Closs (2003). *Grammaticalization* (2nd edn). Cambridge: Cambridge University Press.

Hurford, James R. (2007). *The Origins of Meaning: Language in the Light of Evolution*. Oxford: Oxford University Press.

Jackendoff, Ray (1975). Morphological and semantic regularities in the lexicon. *Language* 51: 639–71.

——(1997). *The Architecture of the Language Faculty*. Cambridge, MA: MIT Press.

——(2002). *Foundations of Language: Brain, Meaning, Grammar, Evolution*. Oxford: Oxford University Press.

——and Pinker, Steven (2005). The nature of the language faculty and its implications for evolution of language: reply to Fitch, Hauser, and Chomsky. *Cognition* 97: 211–25.

Jackson, Jean (1974). Language identity of the Colombian Vaupés Indians. In Richard Bauman and Joel Sherzer (eds), *Explorations in the Ethnography of Speaking* (2nd edn, 1989). Cambridge: Cambridge University Press, 50–64.

Jakobson, Roman (1936). Beitrag zur allgemeinen Kasuslehre: Gesamtbedeutungen der russischen Kasus. *Travaux du Cercle Linguistique de Prague* 6: 240–88. Reprinted (1971) in *Word and Language (Selected Writings, Vol. 2)*, The Hague: Mouton, 23–71,

and in Eric P. Hamp, Frederick W. Householder, and Robert Austerlitz (eds) (1966), *Readings in Linguistics II*, Chicago: University of Chicago Press, 51–95.

Jensen, John T. and Stong-Jensen, Margaret (1984). Morphology is in the lexicon! *Linguistic Inquiry* 15: 474–98.

Joos, Martin (ed.) (1966). *Readings in Linguistics I* (4th edn). Chicago: University of Chicago Press.

Joseph, Brian D. and Smirniotopoulos, Jane C. (1993). The morphosyntax of the Modern Greek verb as morphology and not syntax. *Linguistic Inquiry* 24: 388–98.

Kaminski, Juliane, Call, Josep, and Fischer, Julia (2004). Word learning in a domestic dog: evidence for 'fast mapping'. *Science* 304 (issue 5677): 1682–3.

Katz, Jerrold J. and Fodor, Jerry A. (1963). The structure of a semantic theory. *Language* 39: 170–210. Reprinted (1964) in Jerry A. Fodor and Jerrold J. Katz (eds), *The Structure of Language: Readings in the Philosophy of Language*. Englewood Cliffs: Prentice-Hall, 479–518.

Kiparsky, Paul (1968). How abstract is phonology? Bloomington, IN: Indiana University Linguistics Club. Reprinted (1982) in Paul Kiparsky, *Explanation in Phonology*. Dordrecht: Foris, 119–63.

—— (1973). 'Elsewhere' in phonology. In Stephen R. Anderson and Paul Kiparsky (eds), *A Festschrift for Morris Halle*. New York: Holt Rinehart, 93–106.

—— (1982). From cyclic phonology to Lexical Phonology. In Harry van der Hulst and Norval Smith (eds), *The Structure of Phonological Representations (Part I)*. Dordrecht: Foris, 131–75.

Klavans, Judith L. (1985). The independence of syntax and phonology in cliticization. *Language* 61: 95–120.

Koopman, Hilda (2005). Korean (and Japanese) morphology from a syntactic perspective. *Linguistic Inquiry* 36: 601–33.

Koutsoudas, Andreas, Sanders, Gerald, and Noll, Craig (1974). On the application of phonological rules. *Language* 50: 1–28.

Kress, Bruno (1982). *Isländische Grammatik*. Leipzig: Max Hueber Verlag.

Kuiper, Koenraad and Martin Everaert (2000). Constraints on the phrase structural properties of English phrasal lexical items. In Bożena Rozwadowska (ed.), *PASE Papers in Language Studies: Proceedings of the 8th Annual Conference of the Polish Association for the Study of English*. Wrocław: Aksel, 151–70.

Labov, William (1973). The study of words and their meanings. In Charles-James Bailey and Roger W. Shuy (eds), *New Ways of Analyzing Variation in English*. Washington, DC: Georgetown University Press, 340–73.

Lakoff, George (1987). *Women, Fire, and Dangerous Things: What Categories Reveal About the Mind*. Chicago: University of Chicago Press.

—— and Johnson, Mark (2003). *Metaphors we Live By* (with a new afterword). Chicago: University of Chicago Press.

Lapointe, Steven G. (1981). A lexical analysis of the English auxiliary verb system. In Teun Hoekstra, Harry van der Hulst, and Michael Moortgat (eds), *Lexical Grammar*. Dordrecht: Foris, 215–54.

—— Brentari, Diane K. and Farrell, Patrick M. (eds) (1998). *Morphology and its Relation to Phonology and Syntax*. Stanford: CSLI.

Lass, Roger (1977). Internal reconstruction and generative phonology. *Transactions of the Philological Society 1975*: 1–26.

—— (1990). How to do things with junk: exaptation in language evolution. *Journal of Linguistics* 26: 79–102.

—— (1997). *Historical Linguistics and Language Change*. Cambridge: Cambridge University Press.

Lausberg, Heinrich (1972). *Romanische Sprachwissenschaft III: Formenlehre*. Berlin: Walter de Gruyter.

Le Guin, Ursula K. (1969). *The Left Hand of Darkness*. London: Macdonald Futura.

Lieber, Rochelle (1980). On the organization of the lexicon. Ph.D. dissertation, MIT. [Reproduced 1981 by Indiana University Linguistics Club.]

—— (1983). Argument linking and compounds in English. *Linguistic Inquiry* 14: 251–85.

—— (1987). *An Integrated Theory of Autosegmental Processes*. Albany: State University of New York Press.

—— (1988). Configurational and nonconfigurational morphology. In Martin Everaert, Arnold Evers, Riny Huybregts, and Mieke Trommelen (eds). *Morphology and Modularity*. Dordrecht: Foris, 187–215.

—— (1992). *Deconstructing Morphology: Word Formation in Syntactic Theory*. Chicago: University of Chicago Press.

Lotz, János (1939). *Das ungarische Sprachsystem*. Stockholm: Ungerska Institutet.

Maiden, Martin (1992). Irregularity as a determinant of morphological change. *Journal of Linguistics* 28: 285–312.

—— (2005). Morphological autonomy and diachrony. In Booij and van Marle (eds) (2005a), 137–75.

Marcus, Gary (2008). *Kluge: The Haphazard Construction of the Human Mind*. Boston: Houghton Mifflin.

Markman, Ellen M. (1989). *Categorization and Naming in Children: Problems of Induction*. Cambridge, MA: MIT Press.

Markson, Lori and Bloom, Paul (1997). Evidence against a dedicated system for word learning in children. *Nature* 385: 813–15.

Mascaró, Joan (2007). External allomorphy and lexical representation. *Linguistic Inquiry* 38: 715–35.

Matisoff, James A. (1991). Areal and universal dimensions of grammaticalization in Lahu. In Elizabeth Closs Traugott and Bernd Heine (eds), *Approaches to Grammaticalization* (vol. II). Amsterdam: Benjamins, 383–453.

McCarthy, John J. (1981). A prosodic theory of nonconcatenative morphology. *Linguistic Inquiry* 12: 373–418.

McDaniel, Dana (2005). The potential role of production in the evolution of syntax. In Tallerman (ed.), 153–65.

Meyer-Lübke, W. (1897). Zur Stellung der tonlosen Objektspronomina. *Zeitschrift für romanische Philologie* 21: 313–34.

Miestamo, Matti, Sinnemäki, Kaius, and Karlsson, Fred (eds) (2008). *Language Complexity: Typology, Contact, Change.* (Studies in Language Companion Series 94.) Amsterdam: John Benjamins.

Moro, Andrea (2008). *The Boundaries of Babel: The Brain and the Enigma of Impossible Languages.* Cambridge, MA: MIT Press.

Müller, Gereon (2004). On decomposing inflection class features: syncretism in Russian noun inflection. In Müller et al. (eds.), 189–227.

—— (2005). Syncretism and iconicity in Icelandic noun declension: a Distributed Morphology approach. In Booij and van Marle (eds) (2005a), 229–71.

—— (2007). Notes on paradigm economy. *Morphology* 17: 1–38.

—— Gunkel, Lutz, and Zifonun, Gisela (eds) (2004). *Explorations in Nominal Inflection.* Berlin: Mouton de Gruyter.

Norman, Jerry (1988). *Chinese.* Cambridge: Cambridge University Press.

Noyer, Rolf (2005). A constraint on interclass syncretism. In Booij and van Marle (eds) (2005a), 273–315.

Nyman, Martti (1988). Paradigm economy: a rejoinder to Carstairs. *Journal of Linguistics* 24: 501–13.

O'Grady, William (1998). The syntax of idioms. *Natural Language and Linguistic Theory* 16: 279–312.

Pawley, Andrew and Syder, Frances Hodgetts (1983). Two puzzles for linguistic theory: nativelike selection and nativelike fluency. In Jack C. Richards and Richard W. Schmidt (eds), *Language and Communication.* Harlow: Longman, 191–225.

Peirce, Charles S. (1940). *The Philosophy of Peirce: Selected Writings*, ed. Justus Buchler. London: Routledge & Kegan Paul. Reprinted (1955) as *Philosophical Writings of Peirce.* New York: Dover.

Perlmutter, David M. (1998). Interfaces: explanations of allomorphy and the architecture of grammars. In Lapointe et al. (1998), 307–38.

Piattelli-Palmarini, Massimo (ed.) (1980). *Language and Learning: The Debate between Jean Piaget and Noam Chomsky.* London: Routledge.

Pinker, Steven and Bloom, Paul (1990). Natural language and natural selection [with peer commentary and response]. *Behavioral and Brain Sciences* 13: 707–84.

—— and Jackendoff, Ray (2005). The faculty of language: what's special about it? *Cognition* 95: 201–36.

Poedjosoedarmo, Soepomo (1968). Javanese speech levels. *Indonesia* 6: 54–81.

—— (1969). Wordlist of Javanese non-Ngoko vocabularies. *Indonesia* 7: 165–90.

Pullum, Geoffrey K. and Zwicky, Arnold M. (1988). The syntax-phonology interface. In Frederick J. Newmeyer (ed.), *Linguistics: The Cambridge Survey. Volume I: Linguistic Theory: Foundations.* Cambridge: Cambridge University Press, 255–80.

Pyles, Thomas (1971). *The Origins and Development of the English Language* (2nd edn). New York: Harcourt Brace.

Quine, Willard van Orman (1960). *Word and Object*. Cambridge, MA: MIT Press.

Raverat, Gwen (1952). *Period Piece: A Cambridge Childhood*. London: Faber.

Romaine, Suzanne (1995). *Bilingualism* (2nd edn). Oxford: Blackwell.

Rosch, Eleanor, and Lloyd, Barbara B. (eds) (1978). *Cognition and Categorization*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Rubach, Jerzy (1984). *Cyclic and Lexical Phonology: The Structure of Polish*. Dordrecht: Foris.

—— and Booij, Geert (2001). Allomorphy in Optimality Theory: Polish iotation. *Language* 77: 26–60.

Rycroft, D. K. and Ngcobo, A. B. (1979). *Say it in Zulu*. London: School of Oriental and African Studies.

Sadock, Jerrold M. (1998). On the autonomy of compounding morphology. In Lapointe et al. (eds), 161–87.

Sampson, Geoffrey, Gil, David, and Trudgill, Peter (eds) (2009). *Language Complexity as an Evolving Variable*. Oxford: Oxford University Press.

Saussure, Ferdinand de (1973). *Cours de linguistique générale*, ed. Charles Bally, Albert Sechehaye, and Albert Riedlinger; critical edition by Tullio de Mauro. Paris: Payot.

Sauvageot, Aurélien (1951). *Esquisse de la langue hongroise*. Paris: Klincksieck.

Savage-Rumbaugh, E. Sue. (1986). *Ape Language: From Conditioned Response to Symbol*. New York: Columbia University Press.

—— and Lewin, Roger (1994). *Kanzi: The Ape at the Brink of the Human Mind*. New York: Wiley.

—— Murphy, Jeanine, Sevcik, Rose A., Brakke, Karen E., Williams, Shelly L., and Rumbaugh, Duane M. (1993). Language comprehension in ape and child. *Monographs of the Society for Research in Child Development* serial no. 233, vol. 58, nos. 3–4.

Segerdahl, Pär, Fields, William, and Savage-Rumbaugh, Sue (2005). *Kanzi's Primal Language: The Cultural Initiation of Primates into Language*. Basingstoke: Palgrave Macmillan.

Selkirk, Elisabeth O. (1982). *The Syntax of Words*. Cambridge, MA: MIT Press.

Sells, Peter (2005). Korean and Japanese morphology from a lexical perspective. *Linguistic Inquiry* 26: 277–325.

Simonyi, Siegmund (1907). *Die ungarische Sprache: Geschichte und Charakteristik*. Strasbourg: Trübner.

Sorensen, Arthur P., Jr (1967). Multilingualism in the Northwest Amazon. *American Anthropologist* 69: 670–84.

Spencer, Andrew (1988). Bracketing paradoxes and the English lexicon. *Language* 64: 663–82.

—— (2006). Morphological universals. In Ricardo Marial and Juana Gil (eds), *Linguistic Universals*. Cambridge: Cambridge University Press, 101–29.

Strahan, Ronald (ed.) (1995). *The Mammals of Australia*. Chatswood, NSW: Reed Books.

Stump, Gregory T. (1991). A paradigm-based theory of morphosyntactic mismatches. *Language* 67: 675–725.

—— (1998). Inflection. In Andrew Spencer and Arnold M. Zwicky (eds), *The Handbook of Morphology*. Oxford: Blackwell, 13–43.

—— (2001). *Inflectional Morphology: A Theory of Paradigm Structure*. Cambridge: Cambridge University Press.

—— (2005). Some criticisms of Carstairs-McCarthy's conclusions. In Booij and van Marle (eds) (2005b), 283–303.

—— (2006). Heteroclisis and paradigm linkage. *Language* 82: 279–322.

Swiggers, Pierre (1985). How to order eggs in French. *Folia Linguistica* 19: 63–6.

Szemerényi, Oswald (1996). *Introduction to Indo-European Linguistics*. Oxford: Oxford University Press.

Tallerman, Maggie (ed.) (2005). *Language Origins: Perspectives on Evolution*. Oxford: Oxford University Press.

—— (2007). Did our ancestors speak a holistic protolanguage? *Lingua* 117: 579–604.

Terrace, Herbert S. (1979). *Nim*. New York: Knopf.

Teslar, Joseph A. (1957). *A New Polish Grammar* (7th edn). Edinburgh: Oliver and Boyd.

Thompson, D'Arcy Wentworth (1961). *On Growth and Form*. Abridged, ed. John Tyler Bonner. Cambridge: Cambridge University Press.

Tosco, Mauro (2001). *The Dhaasanac Language*. Cologne: Rüdiger Köppe.

Trommer, Jochen (2008). A feature-geometric approach to Amharic verb classes. In Bachrach and Nevins (eds), 206–36.

Turing, A. M. (1992). *Morphogenesis*. Collected works of A. M. Turing, ed. P. T. Saunders. Amsterdam: North-Holland.

Uhlenbeck, E. M. (1950). *The Krama-Ngoko Opposition: Its Place in the Javanese Language System*. Groningen. Reprinted (1978) in E. M. Uhlenbeck, *Studies in Javanese Morphology*. The Hague: Martinus Nijhoff, 278–99.

Vogel, Irene (1993). Verbs in Italian morphology. In Geert Booij and Jaap van Marle (eds), *Yearbook of Morphology 1993*. Dordrecht: Kluwer, 219–54.

Wasow, Thomas, Perfors, Amy, and Beaver, David (2005). The puzzle of ambiguity. In C. Orhan Orgun and Peter Sells (eds), *Morphology and the Web of Grammar: Essays in Memory of Steven G. Lapointe*. Stanford: CSLI Publications, 265–82.

Wiese, Richard (1996). Phrasal compounds and the theory of word syntax. *Linguistic Inquiry* 27: 183–93.

Williams, George C. (1992). *Natural Selection: Domains, Levels, and Challenges*. New York: Oxford University Press.

Wittgenstein, Ludwig (1963). *Philosophical Investigations*, trans. G. E. M. Anscombe (2nd edn). Oxford: Blackwell.

Wray, Alison (2000). Holistic utterances in protolanguage: the link from primates to humans. In Chris Knight, Michael Studdert-Kennedy, and James Hurford (eds),

The Evolutionary Emergence of Language: Social Function and the Origins of Linguistic Form. Cambridge: Cambridge University Press, 285–302.

—— (2002). Dual processing in protolanguage: performance without competence. In Alison Wray (ed.), *The Transition to Language*. Oxford: Oxford University Press, 113–37.

Wunderlich, Dieter (2008). Spekulationen zum Anfang von Sprache. *Zeitschrift für Sprachwissenschaft* 27: 229–65.

—— and Fabri, Ray (1995). Minimalist Morphology: an approach to inflection. *Zeitschrift für Sprachwissenschaft* 14: 236–94.

Wurzel, Wolfgang Ullrich (1984). *Flexionsmorphologie und Natürlichkeit*. Berlin: Akademie-Verlag. English translation (1989): *Inflectional Morphology and Naturalness*. Berlin: Akademie-Verlag.

Zwicky, Arnold M. (1977). *On Clitics*. Bloomington, IN: Indiana University Linguistics Club.

—— (1985). How to describe inflection. *Proceedings of the 11th Annual Meeting of the Berkeley Linguistics Society*, 372–86.

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