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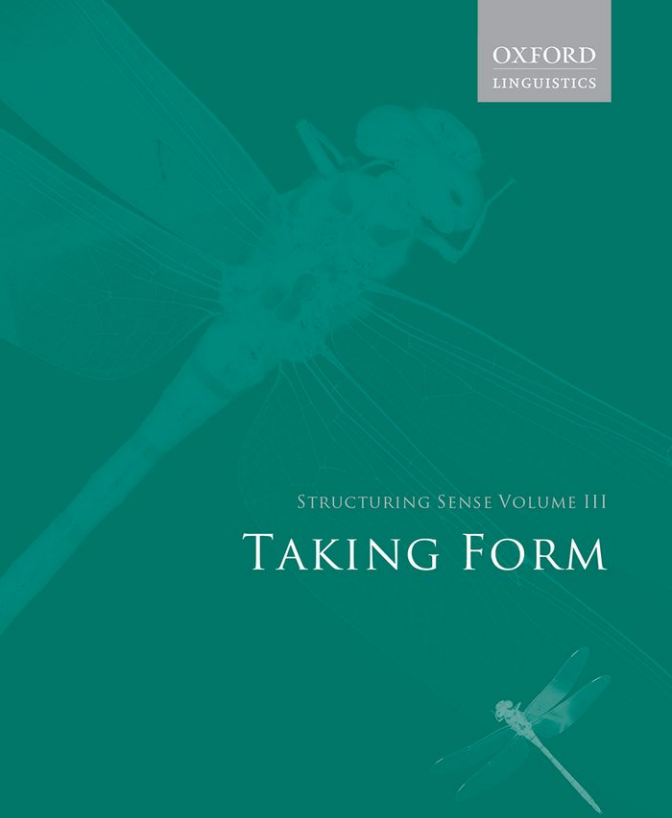

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Taking Form

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T'was [$_{A}$ brillig], and *the* [$_{NP}$[$_{A}$ slithy]toves]
Did [$_{V}$ gyre] and [$_{V}$ gimble] *in the* [$_{N}$ wabe]:
All [$_{A}$ mimsy] *were the* [$_{N}$ borogoves],
And *the* [$_{NP}$[$_{A/N}$ mome] raths] [$_{V}$ *out*grabe]

Jabberwocky (annotated)
Lewis Carroll, *Through the Looking Glass*

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Acknowledgements

Sometime during the summer of 1980 as I was pondering possible dissertation topics, and against the background of a surging interest, within generative linguistics, in comparative syntax, it occurred to me that language variation is not about central switches controlling major circuits, but rather about tinkering with very small screws controlling very small face plates; about making structures, broadly construed, fit into a rather narrowly defined inventory of connectors and joiners, defined, specifically, by grammatical formatives—as the terminology would have it at the time—and their formal properties. Rather central to my thinking, at the time, was my own experience as an L2 learner, as well as a better understanding of the rather neglected third type of operation proposed in Emonds (1976)—the Local Transformation.

The idea was hardly popular at the time, but I nonetheless pursued it as best I could, becoming, in the process, painfully aware of the absence of theoretical tools to properly deal with the class of items that I was foregrounding, as well as my own ignorance of the literature that had been attempting to make sense of the behavior of grammatical formatives—the morphological literature.

The research agenda thus conceived has remained the overriding force that has driven my inquiries in the past three decades. Setting aside the occasional love affairs with Subjects, with Control, and with Stylistic Inversion, my work on argument structure as well as my interest in nominal structure, as represented in Volumes I and II of *Structuring Sense*, all came about as matters that had to be understood before the core of the problem can be handled, the core of the problem having remained, tenaciously and consistently, a better understanding of formal joiners, however defined, and their relationship with structure and with syntax. In short, the subject matter typically described as "morphology".

In turn, anyone who has seriously attempted to formalize morphology is woefully aware of how outstandingly difficult the task is. For one thing, there is no universal agreement on what the subject matter of the inquiry is. Is it attested words? Is it possible words? Is it productive processes? Is it lists? In fact, there isn't even an agreement on what the intuitive notion "word" might correspond to, and whether there is such a formally coherent object that can be studied. Even beyond that, there is little agreement on what a morpheme is, a matter that this study is quite concerned with. At the core of the difficulty, I believe, there lies a fact that is hardly in dispute, but the significance of which remains elusive: during any language use situation, the majority of syntactic objects are formed on the fly and are phonologically realized and semantically composed and interpreted as such. By contrast, the majority of (complex) "words" appear to be pulled from a list, where they are stored together with their phonology and their meaning. But why should that be so, and what other differences does this fact correspond to? The resolution of the matter is hardly made easier by the fact that some syntactic objects are clearly listed (e.g. phrasal idioms), and "words" certainly **can** be formed on the fly and routinely are. As a result, it cannot

be taken for granted that the distinction between listedness and on-the-fly, when attested, signals fundamentally distinct formal systems. It nonetheless remains the case that because so many "words" are listed in **some** sense, and because listing, by its nature, is more tolerant of idiosyncrasies, any generalizations concerning the formal properties of "words" as well as their construction and their sub-units remain extraordinarily slippery.

During the three decades that I have been attempting to gain some foothold on these slippery grounds, my own thinking on the subject matter has undergone important changes, the culmination of which is in the conviction that there is little reason to segregate the formation of complex words and the formation of complex phrases, a conviction which I attempt to make explicit in this rather hefty volume. The final result remains, nonetheless, incomplete in crucial ways. It is riddled with acknowledged open issues and with tentative results, and peppered with junctures at which further research is clearly called for. Nor have I attempted to go far beyond the boundaries of English morphology (with some side detours to Semitic), or to cover, even within English, the full array of relevant facts (thus "inflection" and compounds are only scantily addressed, and argument-structure changing morphology altogether set aside). I do believe, however, that the work represents a step forward, and little more, truly, can be hoped for in any body of scientific work.

Commensurate with the increase in my own knowledge of morphology and morphological matters, my respect for previously gained morphological knowledge, indeed, my ability to appreciate it and learn from it, have grown tremendously. Any reader of this book cannot fail to recognize the fundamental impact on my morphological thinking that comes from that of Steve Anderson, Mark Aronoff, Noam Chomsky, David Embick, Morris Halle, Paul Kiparsky, Rochelle Lieber, Alec Marantz, Lisa Selkirk, and Edwin Williams. Other major intellectual debt is owed to Peter Ackema, Maya Arad, Mark Baker, Robert Beard, Anna-Maria Di Sciullo, Joe Emonds, Heidi Harley, Ray Jackendoff, Jean Lowenstamm, Ad Neeleman, Tom Roeper, and Pavol Štekauer.

The empirical core of this book consists of a study of derived nominals. The profound impact on my understanding of these constructions that originates with Jane Grimshaw's work should be self-evident. I have gained much additional insight in attempting to understand these constructions from the work of Artemis Alexiadou, Noam Chomsky (again!), Ilan Hazout, Angeliek van Hout, Alec Marantz (again!), Isabelle Roy, Tom Roeper (again!), Bożena Rozwadowska, and Tal Siloni.

Finally, my attempts to make sense of Semitic morphology owe substantial debt to previous work of Maya Arad (again!), Outi Bat-El, Shmuel Bolotsky, Edit Doron, Noam Faust, Jean Lowenstamm (again!), John McCarthy, and Alan Prince.

During the many years that I have been attempting to cut a path through the morphological jungle I have benefited tremendously from input from colleagues and students. For outstandingly insightful discussions and comments I would like to thank audiences of seminars and talks primarily at the University of Southern California, at Stuttgart University, at Seoul University, at LISSIM IV, and at Abralin, as well as Peter Ackema, Paulo Acquaviva, David Adger, Artemis Alexiadou, Elena Anagnostopoulou, Marijke De Belder, Marcel den Dikken, Edit Doron, David

Embick, Joe Emonds, Martin Everaert, Noam Faust, Daniel Harbour, Heidi Harley, Jean Lowenstamm, Alec Marantz, Jason Merchant, Andrew Nevins, Sarah Ouwayda, Gilliam Ramchand, Malka Rappaport-Hovav, Isabelle Roy, Ivy Sichel, Andrew Spencer, Michel Starke, Peter Svenonius, and Jean Roger Vergnaud. Andrew McIntyre offered important observations on parts of the manuscript for which I am extremely grateful. Special heartfelt thanks go to Agnieszka Łazorczyk for a careful reading and detailed insightful comments on many parts of this manuscript that no doubt served to improve it substantially. Finally, the keen sight and astute observations of Melisa Rivaldi did much to put this manuscript in its final shape.

During the five years or so that I have been writing this book, my own life has undergone major changes. The world, likewise, is not the same place it was when I first became ensconced in the comfort of this project. As I am emerging from this particular ivory tower into an altogether harsher world, wishing for a just world seems rather unrealistic, but nonetheless a hope to hold on to. I would thus like to dedicate this book to all those in Palestine and outside of it who continue to struggle, tirelessly, against the Israeli occupation and for peace with justice in the Middle East.

HB

London, April 2012

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Abbreviations, Terms, and Orthographic Conventions

√	root general use (not necessarily as a phonological index)
$^{\pi}$√XYZ	root, specifically as a phonological index
/$_{\pi}$*xyz*/	reference to an actual phonological realization (phonetically accurate representations not attempted)
≪e≫	open value, in need of range assignment
≪X^{MN}≫$_{B}$	value-assigned head within a head pair. Examples: [PST ≪V^{PST}≫$_{T}$]; [FUT≪e^{FUT}≫$_{T}$]
{Ex [X]}	the set of ExP-segments that make up the Extended Projection that takes the category X as its CCS
AS-nominals	Argument Structure Nominals (roughly Grimshaw's Complex Event Nominals)
ASP$_{Q}$	telicity (quantity) inducing ExP-segment, member of {Ex [V]}
ATK	"ation and kin": the set of phonological realizations associated with the C-functor, C$_{N[V]}$ in English: *-ation*, *-ance*, *-ence*, *-ancy*, *-ency*, *-ment*, *-al* (possibly also *-age*). Does not include *-ing*.
binyan	in Semitic languages, morphological template, guiding the arrangement and the vocalization of root radicals; verbal
C	"lexical" category (V, N, A; possibly ADV and P)
C-core	the C-component dominated by {Ex [C]} (see definition in Ch. 6, section 3)
CCS	Categorial Complement Space
C-functors	categorial functions; C$_{X[Y]}$
C$_{X[Y]}$, C$_{X[Y]}^{/\pi\text{-}xyz/}$	C-functor which projects X and takes Y as its CCS and which is phonologically realized as /$_{\pi}$*xyz*/
C=X	C-equivalent (i.e. a constituent without an inherent categorial label occupying a categorial space otherwise reserved for X-labeled constituents), e.g. *the* [$_{C=N}$$^{\pi}$√FORM]
DM	Distributed Morphology Model
E	an event structure ExP-segment, member of {Ex [V]}; may license an *Originator* interpretation
ExP	Extended Projection; {Ex [X]}
ExP-segments	segments of Extended Projections (e.g. for a verbal Extended Projection, T, G-ASP, ASP$_{Q}$ and so on); abbr. ExS
ExS	ExP-segment
F^{SHL}	Shell F; semantically vacuous ExP-segment
(G)	corpus-based example, Google search

G-ASP	grammatical aspect (viewpoint aspect), an ExP-segment (distinct from event-structure aspectual nodes such as ASP_Q and E)
Head Pair	the grouping of S^X and the open value to which it assigns semantic range
LOH	The Level Ordering Hypothesis
LPM	Lexical Phonology and Morphology
mišqal	Semitic morphological template, guiding the arrangement and the vocalization of root radicals; nominal
Originator	event-participant role; roughly an *internal causer*
P-RaD	a (well-defined) phonological rule application domain; in English and Hebrew, that of primary stress
P-Voice	ExP-segment, member of {Ex [V]}, responsible for passive voice (abbr. P-Vc)
Q-nominals	Quality nominals; de-adjectival nominals which do not embed (stative) event structure
R-nominals	derived nominals which do not embed event structure
S-functors	semantic range assignors to open values; also S^X
Shell F	semantically vacuous member of {Ex [X]} (F^{SHL})
S-marking	the phonological realization of a value assigned to ≪e≫ when occupied by C; example: $\ll V^{PST} \gg \ll {}^{\pi}\sqrt{XYZ}^{P\text{-}VC,PST} \gg$
S-nominals	state nominals; de-adjectival nominals with an embedded stative event structure
S^X	an S-functor with the semantic range S, to be assigned to an open value which, as a consequence, becomes of categorial type X; example: $[THE^D \ll e \gg] \rightarrow [THE^D \ll e^{THE} \gg_D]$
X	an X projection (either *min* or *max*, unless otherwise specified)
XP	X^{max}; with the exception of DP, always notated as such, maximal and minimal instantiations of categories are only marked as such when salient
XS, XSM	Exo-Skeletal Model

Orthographic Conventions

abc.def:	phonological marking which does not entail a constituent boundary (e.g. in this system, *dog.s*)
abc-def:	constituent boundary within a phonological string (e.g. *transform-ation*)
Italics (lower case)	informal reference
Shaded bold gray	silent copies (e.g. $[_A[_A[_{C=V}\ {}^{\pi}\sqrt{CATCH}]\ ABLE]\ [_{C=V}\ {}^{\pi}\sqrt{CATCH}]]$)
Upper case	Functors (e.g. THE, MOST, ABLE)
Upper case, italics	Content (e.g. *CAT*, *DOG*, *CAR GEARBOX*)
SpecX	Specifier of X

A Note on Hebrew Transcription

The pronunciation of biblical texts was codified in the 10th century by the Tiberian School by means of adding diacritics to what was, previously, an unvocalized text. While some syntactic and morphological information concerning Biblical Hebrew is clearly discernible from the texts as they predate that time, other information, including vowel quality and gemination, is not encoded directly in earlier texts. Phonological information concerning the pronunciation of Rabbinical (or Mishnaic) Hebrew (as well as Aramaic) was likewise incomplete. In this book, Tiberian Hebrew is the term used whenever statements are made which concern phonological aspects of Hebrew that were codified by the Tiberian School. Biblical Hebrew, when the term is used, rather refers to those aspects of Hebrew (e.g. the aspectual system, prefixation, suffixation, and so on) which can be unambiguously discerned from older, non-vocalized texts, and specifically, texts which predate the important linguistic changes that Hebrew underwent roughly from the 6th century BCE onwards, and which by 200 BCE gave rise to texts classified as Rabbinical Hebrew. Of the many important changes, some of relevance are the reanalysis of the aspectual perfective/imperfective system as tense (past/future, respectively), and the change of word order from VSO to SVO, accompanied by a fuller agreement on the verb.

Syntactically, Modern Hebrew (MH) is a descendent of Rabbinical Hebrew, via Medieval Hebrew. Phonologically, on the other hand, it is clearly distinct from Tiberian Hebrew as well as from Medieval Hebrew (used throughout the Mediterranean basin). It is further phonologically distinct from all recorded (and distinct) vocalizations of Hebrew that existed well into the 20th century, both in Europe and throughout the Mediterranean basin, the Middle East, and the Arabian Peninsula. Interestingly, but rather non-surprisingly, the greatest historical linguistic continuity is morphological, with the template system of Biblical Hebrew, Rabbinical Hebrew, and Modern Hebrew being virtually identical. One important area of potential difference, however, emerges precisely from the distinct phonology of Modern Hebrew. In Tiberian Hebrew, binyanim III and VII (as well as IV, the internal passive of III) involve the gemination of the middle radical. Also of some importance is the gemination, for binyan II, of the first root consonant in the imperfective, typically taken to indicate an assimilation of the *n*-prefix associated with that binyan. In Modern Hebrew, however, gemination is altogether phonetically absent, raising the legitimate question of whether the relevant binyanim have been reanalyzed and the relevant binyan-specific gemination altogether gone. The matter is not a simple one, because although gemination, as such, is never directly in evidence, arguably it is present in some abstract form nonetheless, thereby accounting both for the failure of post-vocalic spirantization of the middle radical in III/IV (when compared with I, cf. (1)) as well as for the overwhelming friendliness of III and VII templates towards

quadro-radical roots (cf. (2)), or the different patterns of reduplication in bi-radical roots as in (3):

(1) ROOT: √ʔBD I: ʔabad III: ʔibbed
Tiberian H: [*ʔavad*] [*ʔibbed*]
Modern H: [*avad*] [*ibed*]
'work' 'cultivate'

(2) ROOT: √TRGM (⋆I,⋆II,⋆V) III: tirgem VII hittargem
'translate.TRANS' 'translate.INTRANS'
(MH pronunciation: [*hitargem*])

(3) ROOT: √BZ
a. I: i [*baz*] ii [*bazaz*]
'rob' 'rob'
b. III: [*bizbez*] VII: [*hitbazbez*]
'waste.TRANS' 'waste.INTRANS'

Seeking to characterize what is common to the different stages of the language (or to the languages, for that matter) and attempting to make morpho-phonological relatedness as transparent as possible, I have opted to represent III, IV, and VII, as well as imperfective forms of II, throughout, as involving gemination, thereby allowing the discussion to proceed on the basis of the morphological data available, uniformly, in all accessible historical periods in which the morphological system appears, otherwise, fundamentally the same. A similar rationale dictates the decision to phonologically represent at least some of the root radicals as they are phonologically encoded in Tiberian Hebrew—and by assumption in Biblical Hebrew as well (to judge on the basis of orthographic distinctions)—even when the relevant phonological distinctions have become obscured in Modern Hebrew, insofar as such radicals continue to inform phonological processes, especially when guttural and pharyngeal, which would otherwise be difficult to describe. We note that the biggest phonological distinction between Tiberian Hebrew and Modern Hebrew involves the loss of vowel length distinctions in the latter, a matter that goes unrepresented in this work altogether, where vowel length in Hebrew goes unmarked across the board, thus corresponding to Modern Hebrew pronunciation. I further opted not to represent gemination outside the verbal system or orthographically existing but phonetically absent distinctions in borrowed forms. In general, and although I do subscribe to the view that Modern Hebrew phonology must have retained abstract distinctions that are no longer directly pronounced, transcription decisions were made primarily based on ease of exposition, and theoretical phonological claims are by and large not intended. Theoretical claims are at times made in this text that do impact phonological representations. When that is the case, these claims are explicitly articulated in the discussion.

Finally, Tiberian Hebrew has a spirantization rule which affects non-emphatic stops post-vocalically, and which is bled by gemination, and thus [b,p,d,t,g,k] → [v,f,ð,θ,ɣ,x]. Of these, MH observes only [b,p,k] → [v,f,x], the sounds [ð,θ,ɣ] having altogether vanished from the language, and in two of these cases [v,x], the output of such

spirantization renders it indistinct from consonants that are otherwise attested in the language and which are not derived (the erstwhile [w] in Tiberian Hebrew now pronounced [v], and the erstwhile [ḥ] now pronounced as [x]). Throughout this work, and again guided by expositional considerations, spirantization remains unmarked (unless relevant), given the fact that by assumption, it has little if any effect on the phenomena under consideration. For more specific clarifications in relevant contexts, see footnotes in Chapter 11. The table in (4) is a representation of the transcription notations used relative to the Hebrew alphabet. Here, and throughout this text, quotative forms for verbs are perfective.3.SG.M. All glosses of quotative forms in isolation, however, abstract away from tense/aspect/agreement markers:

(4) **Transcription, Hebrew Consonants**

Orthography	***Tiberian Hebrew***	***Modern Hebrew***	***Transcription***
א	[ʾ]	[ʾ]; ∅	*ʾ* (word initially and root radical); ∅ otherwise
בּ	[b]	[b]	*b*
ב	[v]	[v]	*b*
גּ	[g]	[g]	*g*
ג	[ɣ]	[g]	*g*
דּ	[d]	[d]	*d*
ד	[ð]	[d]	*d*
ה	[h]	[h]; ∅	∅ word-final; *h* (otherwise and root radical)
ו	[w]	[v]	*v*
ז	[z]	[z]	*z*
ח	[ḥ]	[x]	*x*
ט	[ṭ]	[t]	*ṭ* (except for borrowed forms)
י	[y]	[y]	*y*
כּ	[k]	[k]	*k*
כ	[x]	[x]	*k*
ל	[l]	[l]	*l*
מ	[m]	[m]	*m*
נ	[n]	[n]	*n*
ס	[s]	[s]	*s*
ע	[ʔ]	[ʿ]	*ʔ*
פּ	[p]	[p]	*p*
פ	[f]	[f]	*p*
צ	[ṣ]	[c]	*c*
ק	[q]	[k]	*q* (except in borrowed forms)
ר	[r]	[r]	*r*
שׁ	[š]	[š]	*š*
שׂ	[s]	[s]	*s*
תּ	[t]	[t]	*t*
ת	[θ]	[t]	*t*

1

Introduction—Words? What Words?

1.1 Introduction

This book is the third in an ongoing investigation of the interaction between syntax and words. In its two predecessors, I developed a model which I named the Exo-Skeletal Model (XS Model), in which all grammatical (non-phonological) properties are computed on the basis of syntactic structure in conjunction with syntactically merged semantic operators. Insofar as XS, like any structuralist approach to language, must assume a list of some sort—at the very minimum linking some arbitrary sign (sound, gesture) with some conceptual meaning, or Content—I explicitly assumed that neither the sign component nor the (conceptual) Content component—nor, indeed, the pairing of the two—can inform the construction of syntactic or formal semantic structure in any way. XS, then, challenges claims made at least as early as Chomsky (1965), according to which the listing of substantive Content units comes with a set of diacritics which are, effectively, a set of instructions to the syntax.[1] At their most developed incarnation, such list-, or lexicon-based models translated into the claim that syntactic structure is largely the trivial output of the combination of lexically encoded instructions with general principles which guide the construction of hierarchical constituent structure (e.g. X′-theory). Such is, broadly described, the system developed in various versions of Lexical Functional Grammar as well as many aspects of the system developed in (early) Government and Binding, where it is explicitly assumed that D-Structure is GF_{theta}—the direct result of composing lexically specified grammatical information with X′-theory. Models which endorse the central grammatical role of listed units may vary substantially concerning not only the scope and the formal nature of the diacritics under consideration, but also the extent to which formal operations may affect them and thus affect the output to hierarchical syntax. They do, however, share the substantial claim that listed Content units, however otherwise described, have properties which can inform both syntactic structures and formal-semantic operations, although they are not derived syntactically or semantically. Most broadly, all these models share the assumption that listed Content units come with a category label (noun, verb,

[1] The separation of Content from elements of formal semantics thus appeals to Frege's distinction between *Sinn* and *Bedeutung*, in turn supplemented by the explicit claim that the former is grammatically inert.

adjective, and so on), and that they come with some instructions concerning their insertion frame—be it subcategorization, a configuration of thematic role assignments, lexical semantics which guides the formation of predicate-argument structure, or some other equivalent systems. To illustrate, the fact that e.g. *table* is listed as a noun (as a count noun, specifically), makes its merger licit, licenses it, so to speak, in the context of determiners, in the context of plural marking and cardinals and so on. The fact that *kick* is listed as a verb with two participants of a particular ilk, allows it not only to occur with tense, but also to occur with a direct object and a subject with a particular interpretation, and specifically in a structure in which the subject c-commands the object.

Presumably, in any account, something must be said about the syntax such that it would account for the occurrence of *table* in the context of *the*, and for the occurrence and interpretation of *walked the dog*. The challenge that systems such as XS and similar thus face is the modeling of a sufficiently restricted grammar which can adequately describe these effects without availing itself of information listed in conjunction with substantive Content units. This is the task I undertook in *In Name Only* and in *The Normal Course of Events*, the first elaborating on the construction of nominal constituents, and the second on the construction of event structure, all without recourse to information listed with Content units.

At the core of the theoretical approach, however, there remains an important matter which is in need of a thorough investigation. Thus far, the XS investigation of e.g. *the table* or *walked the dog* proceeded from a starting point that the formal (non-phonological) properties of such expressions can be fully accommodated without availing ourselves, at any point, of information which is uniquely connected with *table*, *walk*, and *dog*, respectively. Rather, both the syntax and crucial aspects of the formal semantics can be computed on the basis of the syntactic structure of functors and the semantic formulas which such functors name; in this case, the functors that we can refer to, informally, as THE and PAST. As a result, the investigation focused little on the actual properties of *table*, *walk*, and *dog*. What, however, **are** *table*, *walk*, and *dog*? The matter is important in two rather distinct ways. First, *table*, *walk*, and *dog* do come to be associated with some Content. Even if such Content may be inert syntactically and in formal-semantic terms, it nonetheless does get associated with these expressions at some point, and a full language description must take account of that fact. More important from a grammatical perspective, however, is the question of what the specifically grammatical properties of *table*, *walk*, and *dog* are. For instance, do they have a syntactic category? Do they have a syntactic category in isolation? Do they have a syntactic category within a larger syntactic constituent? Is that syntactic category constant across their occurrences? And finally, given that by assumption in XS they are not **listed** with such a category, if they do come to have one, how do they come to have it? Similarly, and assuming *table*, *walk*, and *dog*, at the very minimum, correspond to some phonological representation, what is this phonological representation associated with? Some atomic units, call them **table**, **walk**, **dog**? Some derived unit, created from combining more than a single constituent? If the former, we must now address the question of what **table**, **walk**, and

dog are, such that they are linked to a phonological representation. If the latter, we must determine what the larger unit under consideration is, such that it would be assigned the relevant phonology, and ask whether it is the same across all the relevant phonological occurrences of **table**, **walk**, and **dog**.

Crucially, note that, at least as phonological strings, *table, walk*, and *dog* must be dissociated from any syntactic information, for the very same phonological string may occur as both noun and verb. Hence *the table* but *to table a motion*; *walked the dog* but also *the walk to the hills*, and *it's not nice to dog people like that* and so on. In Borer (2005a, b) I conclude that the best way to handle this fact is by assuming that in and of themselves, *table*, *walk*, and *dog*, whatever they turn out to be, do not have a category, and that their verbal or nominal instantiation is dependent on their syntactic context (a matter I return to in Chapter 7). If on the right track, this provides at least one answer to the set of questions above, insofar as *table, walk*, and *dog*, if atoms, have no syntactic category, thereby allowing them to occur in a multitude of syntactic contexts.

But if so, then even these relatively simple cases already raise a number of puzzles. First, while one may concur that **table**, **walk**, **dog** **are not** (atomically) syntactic units, this hardly provides an answer to the question of what they **are**. Second, while *dog* (or *table*) may certainly occur in both verbal and nominal contexts, the Content of *dog* (henceforth *DOG* or *TABLE*) in these different contexts is quite different. While certainly the verbal Content can be related to some properties of the nominal Content, the Content relation is not a deterministic one. While *to dog* in English means to pursue tenaciously to the point of harassment, it could have, presumably, picked up on some other canonical property of domestic canines to end up with a Content such as *to be loyal,* or *to follow scent. Table*, as a verb in English, actually has two diametrically opposite readings—one means to submit a motion (presumably, metaphorically, put it on the table), the other to postpone its submission, further illustrating the failure of deterministic Content relations to occur. Nor should this come as a surprise. "Words" do acquire unpredictable Content, and such unpredictable Content is frequently associated with categorial polarizing. This, after all, is one of the reasons for the powerful hypothesis that words are listed in some sense that phrases are not. The question for an XS-type approach, then, becomes how to represent such unpredictable Content in a system where, to begin with, it is not entirely clear what *dog* or *table* are, altogether. To the extent that there **are** such entities that we can refer to as *dog* or *table*, do they have Content? Do we expect this Content to be constant across all their occurrences? Do we expect such Content to be modified when they are in nominal or verbal contexts, and how can we model such modification of Content? And are there any limits on the ways in which such Content can be modified?

The matter acquires additional complexity when we consider 'complex' words such as *transformation* or *globalize*. If one could argue that in isolation *table* and *walk* do not have a category, a similar claim concerning *transformation* or *globalize* seems rather dubious. **The globalize* or **transformationed the committees* are clearly ungrammatical, representing what can probably be otherwise demonstrated rather

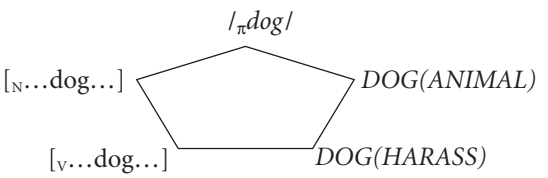
amply, which is that English derivational suffixes correspond to categorially marked constituents. If, however, one subscribes to a model in which only structures and functors have formal properties but never listed Content items as such, then the inevitable conclusion is that *globalize* and *transformation* cannot be "listed Content items" and must have a structure and may contain functors. While, presumably, the compositional operations that give rise to *globalize* might still be distinct from those that give rise to *in the park*, note that by assumption, the output of *globalize* cannot be listed (in the relevant sense) any more than the output of *in the park*. More accurately, insofar as by assumption the construction of the grammar is oblivious to the properties of Content units, even if some component were to list *globalize*, the fact that it is listed as a verb cannot be syntactically "checked" any more than the fact that *dog*, presumably, is not. In view of this, one is driven to question the advantages of an independent component which derives forms such as *globalize*, given that not only the internal structure, but also the output of such a component would be syntactically inert.

Suppose, then, we assume instead that there may be exactly one (non-phonological) hierarchy-forming computational device which utilizes identical formal operations when it constructs *globalize* and *in the park*. But then, the question we already faced relative to the verbal and nominal instantiations of *table* and *dog* becomes even trickier—*transformation*, presumably, is ambiguous between a Content that can, broadly speaking, be paraphrased as "the act of transforming", and a Content that, broadly speaking, can be paraphrased as "a particular formal operation on a grammatical representation". The former, but not the latter, has an interpretation fully predictable from the Content of the verb within it, plus whatever contribution comes in from *-ation*, say "action". The latter still has the "action" component to it, sure enough, but the verb embedded within it, *transform*, does not share the technical Content that is associated with the nominal (to wit *I performed a transformation on this structure to derive this word order* ≠ *I transformed this structure to derive this word order*). Certainly, insofar as the technical Content of *transformation* is not predictable from its parts, that Content needs to be listed. But how can such listing be accommodated within a fundamentally syntactic approach to the formation of complex words such as *transformation*? And why list *transformation*, but not *in the park*?

Attempting to phrase the question here in the broadest terms, and abstracting away from formal functors, an utterance in Natural Language is typically associated—directly or derivatively—with some syntactic information, some phonological information and some Content information. Presumably, each of these modules defines within the terms of its own formal language units of various size. Within syntax, we can assume the minimally sized unit to be a category (where by category I specifically refer to so-called lexical categories, i.e. N, V, A, possibly Adv, and some instances of P). I will take Content, or conceptual Content, here to refer to aspects of meaning which are not rigid designators (in the technical sense, as defined later in the chapter), and which, broadly, correspond to conceptual knowledge, however structured. I will also assume that conceptual knowledge is internally organized in ways

which determine what is, or what isn't, an appropriate unit of Content.[2] For phonology, I will by and large assume that neither a segment nor a syllable (or a foot) as such define domains in the relevant sense. In a language such as English, rather, the minimal domain would turn out to be that which coincides with the innermost phonological cycle and which contains no internal boundaries (and see below, section 1.4, for a more precise description). The question under consideration here, then, is what is the relationship between these different module-specific units? Suppose we assume now, as is reasonable, that e.g. /$_{\pi}$*dog*/ is a minimal phonological unit insofar as it defines a single cycle.[3] The concrete question then becomes what the relationship is between /$_{\pi}$*dog*/, [$_{N}$ . . . dog . . .] and *DOG(ANIMAL)*, and to what extent it overlaps with the relationship between /$_{\pi}$*dog*/, [$_{V}$ . . . dog . . .] and *DOG(HARASS)*:

(1)



In principle, any relationship is possible between any of the objects in (1) and in any direction, although, presumably, some are less plausible than others. There is no prima facie expectation, we note, for these minimal units to correspond to each other. It is certainly perfectly plausible, formally, for them to go their separate ways, so that, for example, a minimal syntactic category, say N, would nonetheless correspond to multiple possible PH (=phonological) realizations, some minimal and some not; similarly, it would be possible for a single Content unit, say *DIE*, to correspond to a complex syntactic constituent as well as a non-minimal PH unit, and for a single PH unit to correspond to a complex syntactic constituent or complex Content, say /$_{\pi}$*kill*/ if we assume that *kill* is indeed complex, in some sense. In turn, some of these degrees of complexity may be derived from one another or alternatively correlate with each other symmetrically in some meaningful way. The matter, ultimately, is an empirical one.

Within generative grammar, and for the past forty years or so, there has been one dominant answer to this question—the lexicalist answer, according to which phonological, syntactic, and Content properties are all associated with listed items which we may refer to as "words". As the purpose of the present work is to challenge this answer, it is worthwhile to embark upon a brief historical review of its emergence and its justifications.

[2] The question receives massive attention in the philosophical literature, which I will not attempt to summarize. Most notably, see Quine (1960) and much follow-up discussion.

[3] Material enclosed in slashes with a π (/$_{\pi}$*xyz*/) is in reference to some appropriate phonological representation. Actual phonological representations are largely not attempted. Italicized capitals indicate (conceptual) Content. See the glossary at the start of the book for a full list of notational conventions and abbreviations.

1.2 The *Remarks* Challenge

In 1967, in *Remarks on Nominalization* (published 1970), in a move that was as controversial as it was influential, Chomsky proposed that certain operations of word formation, previously assumed to be within the jurisdiction of the syntax, were to be moved to the lexicon, a specifically non-generative component of the grammar. As such, their properties were assumed to be on a par with other listed properties such as subcategorization, selectional restrictions, category specification, and phonological properties, many already proposed to reside in the lexicon in Chomsky (1965). The primary rationale for moving the relationship which holds between e.g. *destroy* and *destruction* to the lexicon was twofold. On the one hand, it was heuristic. Syntactic derivations of de-verbal nominals, and of complex words in general, proved extremely detrimental to attempts to formally constrain the syntax along more universal lines, a disadvantage that was, in fact, to lead to the formal collapse of those syntactic models which rejected the *Remarks* move. Moving word structure and word properties to the lexicon, on the other hand, allowed the development of a more constrained syntax precisely within those sub-areas of grammar for which progress could be most beneficially made at the time.

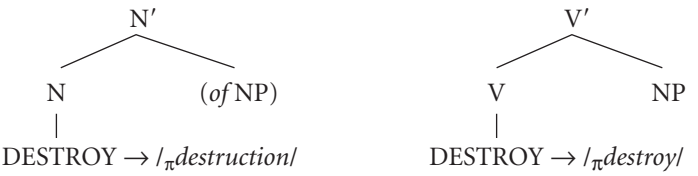
The second rationale for moving complex-word internal properties to the lexicon was formal. Chomsky (1970) puts forth a series of arguments designed to show that the formation of words, however achieved, is not a generative device, but rather must avail itself of lexically listed information. The lexicon, thus extended, was specifically targeted as the locus not only of idiosyncratic information associated with individual words, but also as the locus of relationships between pairs of related words, by assumption potentially arbitrary and unpredictable.

Reasoning on the basis of a detailed comparison between complex nominals arguably derived from verbs, and gerunds, Chomsky constructs a typology of syntactic vs. lexical operations. Thus he points out that while gerunds are entirely regular and predictably share the properties of the verbs embedded within them, that is not the case for de-verbal nominals, where both interpretational and syntactic idiosyncrasies are common, and where the systematic inheritance of verbal properties cannot be taken for granted. The appropriate insertion of de-verbal nominals into syntactic structures, i.e. their present-day merger, Chomsky reasons, must avail itself of unpredictable listed information, thereby necessitating their removal from the syntax and their listing, leading to an enrichment of the lexicon.

Chomsky (1970) does note, however, that alongside potential idiosyncrasies, de-verbal nominals are frequently systematically related to their verbal source, to wit, *destroy* and *destruction*, *defer* and *deferral*, and so on. To capture these regularities, he introduces X′-theory, within which a pair such as *destroy*/*destruction* can be perceived as a single category-less entry with a fixed subcategorization frame. This entry, in turn, may be inserted under an X^0, be it N^0 or V^0. In turn, the syntactic context of the insertion determines the phonologically appropriate form for the entry. If inserted under N, it would be pronounced *deferral* or *destruction*. If, on the other hand, it is

inserted under V it would be pronounced *defer* or *destroy*.[4] Given the provisions of the X′-scheme as suggested at the time, subcategorization relationships which hold between an entry and its complement may be constant across categorial instantiations.[5] Crucially for this execution, whatever operation relates *destroy*, as the verbal instantiation of the relevant entry, and *destruction*, its nominal instantiation, it is not syntactic and is not represented syntactically. In fact, within that approach, it is not clear that the relationship is derivational in nature altogether, as opposed to constituting a salient statistical correlation, an approach explicitly put forward by Jackendoff (1975). The *Remarks*-model structure of *destroy* vs. *destruction* can thus be represented as in (2) (irrelevant details omitted), where DESTROY is an entry presumably marked by some Content as well as by subcategorization and possibly other properties, but not syntactic category:

(2)



A number of crucial properties of (2) are worth highlighting. First, note that the complement of the noun is optional, but that of the verb is obligatory. Chomsky (1970) assumes, explicitly, that this is a structural difference between nouns and verbs, which spans both the object and the subject, the latter optional for nouns and obligatory for verbs as well. In fact, the correlation between the optionality of complements in de-verbal nominals and the optionality of complements in non-de-verbal nominals serves for Chomsky as an additional argument for the lexically rather than (syntactically) derived nature of de-verbal nominals. To wit, if de-verbal nominals have a verb embedded under them, one expects the obligatoriness of both complement and subject, typical of verbs and clearly attested in gerunds. That such obligatoriness is not found in de-verbal nominals therefore serves as an argument that, fundamentally, they are inserted into the tree as nouns, and are not syntactically composed of a verb plus some nominal affix.

A second important observation concerning the structure in (2) is that **syntactically**, *destroy*, a verbal head, and *destruction*, a nominal noun, are equally complex—both are terminals. That one of them is morphologically complex and includes within it a stem that is largely identical to the verbal realization is most certainly not a

[4] For a strict Bare Phrase Structure approach, note, this execution is impossible, as, in principle, the head is assumed to project its categorial properties, if any, and is not inserted under a pre-constructed categorial node. See Chapter 6, section 1 for more comments.

[5] The suggestion that specifiers are subjects had to wait another fourteen years, to be introduced by Stowell (1981). Specifiers, in earlier versions of X′-theory, were typically functional items which nowadays would be assumed to head, or be the specifiers of, separate functional projections (e.g. determiners, degree modifiers, auxiliaries, etc.). Alternatively, specifiers were assumed to host adjectives, adverbs, and other modifiers. See, especially, Jackendoff (1977).

syntactic fact, and in fact, for Chomsky (1970), it is not clear that it reflects any systematic derivational relationship altogether. We note that in such a context even the term "derived nominal" in itself would be a misnomer, which is why the term "de-verbal nominal" has been opted for above. Finally note that, albeit not explicitly acknowledged, the entry for DESTROY must contain **some** phonological information. Were that not the case, the phonological overlap between /$_{\pi}$*destruction*/ and /$_{\pi}$*destroy*/ and similar pairs would become an inexplicable—and repeated—coincidence.

The case for the idiosyncrasy of complex words, and hence their listed nature, was considerably enhanced by Halle, who pointed out in *Prolegomena for Word Formation* (1973) the phonologically unpredictable nature of morphological operations, primarily within the domain of inflection. Observing, among other phenomena, lexically specified stem alternations under affixation, incomplete paradigms, impoverishment, the occasionally idiosyncratic interpretation of some inflectional morphemes (e.g. Russian instrumental case), and the unpredictable fusion of distinct inflectional markers, Halle argued that the erratic nature of the phonological output of word formation, insofar as it clearly necessitates the consultation of listed information, supports the case for the transfer of all complex words and their formation away from the more phonologically well-behaved parts of the grammar, i.e. syntax and, we may add, formal semantics. Halle did, however, propose a semi-formal word formation component, albeit structured so as to allow its output to consult idiosyncratically listed information.

Ironically, in the direct aftermath of Chomsky's *Remarks* and Halle's *Prolegomena*, and with the notable exception of Jackendoff (1975), a burgeoning community of word formation scholars eschewed, collectively, the notion that word formation is summarily non-generative, applying considerable talent to the attempt to systematize and formalize accounts of word structure and word formation.[6] The systems that emerge are largely not only generative, but also suspiciously syntax-like. Beginning especially with the influential distinction of Aronoff (1976) between analytic and productive morphology, we see the introduction of rewrite rules and phrase structure (cf. Selkirk 1982); of heads for words (cf. Williams 1981a); and of subcategorization and constituent structure for affixes (cf. Lieber 1980). In fact, the formal devices used in constructing complex words became gradually so syntax-like, that a special condition has been introduced for the sole purpose of preventing the syntax from interacting with word-internal structure.[7] Alongside the attempt to

[6] Importantly, most of this research sets aside one of the problems that troubled Halle (1973) the most, namely possible but non-existing words. Most of these accounts chose, instead, to focus on the distinction between possible vs. impossible words. This move has enabled substantial progress in the study of word formation. It left, unaddressed, however, the fundamental cognitive difference between words and phrases, best expressed in the fact that the very notion "possible but non-existent phrase" is an incoherent one. While it is clear that the formation of word constituents continues to be available as a generative device throughout an individual life span, it nevertheless remains the fact that this generative ability is invoked irregularly, and is subject to social conditions and pressures of the sort not attested for phrases. Having noted this puzzle, I will, just like my predecessor and contemporary word-formationalists, proceed to set it aside.

[7] Hence the Lexical Integrity Hypothesis of Lapointe (1980), reformulated as the Atomicity Thesis of Di Sciullo and Williams (1987), which explicitly barred the syntax from consulting the internal structure of

build a hierarchical structure for words, we also see a systematic attempt to introduce some order into the chaos by separating those affixes in which a high degree of regularity—morphological, syntactic, and phonological—is observed, from those affixes where such regularity is less frequent (cf. Siegel 1974; Allen 1978; Pesetsky 1979; and most influentially, Kiparsky 1982a, within the general framework of the Level Ordering Hypothesis and more specifically that of Lexical Phonology and Morphology).

Suppose we attempt an admittedly coarse summary of the consensus among lexicalist scholars working on word formation in the mid 1980s, focusing specifically on approaches to derivational morphology and on the syntactic implications of such approaches. By that time, and integrating many of the generative or semi-generative devices briefly outlined in the previous paragraph, the formation of complex words takes place in a component distinct from the syntax—call it WF, for Word Formation. "Word" in such models is a technical term reserved for formal objects which are the output of WF (including trivial outputs). The primitives of WF are affixes and bases.[8] In that system affixes such as *-ation*, *-al*, or *re-* are functors (to appropriate the term used by Di Sciullo and Williams 1987), insofar as their attachment to some base results in the emergence of some well-defined formal properties. Bases, on the other hand, are by and large inert from a WF perspective. They are assumed to be pairs of sound and some lexical semantics, and they do have a category, but they do not define any grammatical operations, as such. WF manipulates affixes and bases (or possibly, affixes trigger WF manipulations of various sorts), giving rise to "words" (themselves potentially recycled into the WF component as a base for further affixation), and where "words", now, is assumed to consist of a sound–lexico-semantics pairing (from which potentially its argument array is derived), and always with a syntactic category. Crucially, then, such models, although they do assume that, e.g. *destruction* is derived, at some point, from *destroy*, continue to assume that the morphological complexity of *destruction* is syntactically obscured, and that **syntactically**, the representation should fundamentally be as in (2).[9]

Consider in greater detail the justification for an independent formal component of Word Formation, given the development of more generative approaches to its

words. To appreciate the significance of the need for such a condition, we may ponder the absence of any articulated principles barring the syntax from, e.g., consulting syllable weight.

[8] Alternatively, "stems" or "roots". Given the centrality of the term "root" in this work, and with a definition that is rather distinct from that mostly used in WF accounts, the term "base" is opted for throughout when referring to traditional accounts of WF.

[9] This picture is, of course, greatly simplified. Morpheme-based accounts of word formation may differ greatly as concerning the degree of abstractness of affixes, the extent to which they spell out the output of rules, or are themselves the names of rules, and, of course, concerning the type of rules which manipulate morphemes and their possible target, ranging over category, subcategorization, argument array, and so on. From the perspective of these introductory comments, what is important is the assumption, I believe inherent in all lexicalist approaches, that the output of the word formation component is associated with sound and with syntactic properties which determine its merger possibilities, as well as the assumption that the output of WF operations is atomic in the relevant sense which is defined in the text. I turn shortly to a discussion of "realizational" models, which inherently entail a very different formal interaction between the syntax, the phonology, and the formation of complex words.

nature. As a first step, note that while "words" may be defined as the domain of a single primary stress, that only goes so far in accounting even for its phonological properties.[10] A complex "words" cannot **just** be a single prosodic domain. Rather, there has to exist a phonologically relevant combinatorial system which is devoted specifically to putting words together, and which is distinct from prosody. The reason for this is the existence of phonological domains of rule application which are sensitive to morphological complexity, e.g. the cycle (or domains defined by boundary types), and which therefore require setting up some morphological domains as being, hierarchically, internal to others.

A cycle, of some sort, is fundamental to the construction of syntactically complex structures in all hierarchical, constituent-based approaches to syntax I am aware of. What, then, is the justification for the claim that word formation is nevertheless not syntactic, despite the appearance of similar combinatorial principles which are similarly constrained? The justification, as it turns out, harks back to Halle (1973) and to Chomsky (1970) and focuses on phonological and Content unpredictability. First, the specific phonological representation of particular morphological functions is often item-specific. Some affixes cause stress shift while others do not, and the choice of one over the other, in the context of a particular stem, seems item specific. Thus **inventívity (*vs. *invéntiveness*), but *receptívity*. The realization of past tense in the context of the verb *walk* is different from the realization of past tense in the context of verbs such as *run* or *sing*, a matter about which the respective entries of *walk, run*, and *sing* need to be consulted in some fashion. The nominalizer for *destroy* is *-ation*, and the pronunciation of *destroy* in the context of *-ation* is /$_{\pi}$*destruct*/ but although *construction* appears quite related to *destruction*, we do not find /$_{\pi}$*constroy*/. For *defer*, on the other hand, the nominalizer is either *-al* or *-ment*, but never *-ation*. The stem for *receive* is pronounced *ceive*, but the *cept* variety crops up in some affixation contexts. If word formation must crucially refer to idiosyncratic phonological information in lexical entries of stems and affixes, and if one assumes that the syntax is barred from so doing (and if one assumes that the syntax is phonologically regular, in the relevant sense), then word formation must be non-syntactic. Second, morphological cycles, however defined, nonetheless frequently correspond to non-compositional Content.[11] This situation, it is claimed, is (by and large) not found with phrases, where the meaning is compositional, thus suggesting that the output of

[10] Thus functional vocabulary is frequently devoid of primary stress altogether, raising the question of what, if anything, makes *the* a "words", if words are to be defined as the domains of primary stress. The problem, however, is only meaningful if the domain of stress is to be correlated with other, non-stress-related and non-phonological properties. If the domain of stress, on the other hand, is not expected to correlate with anything except stress and whatever phonological effects it has, the fact that *the*, otherwise a spelled out functor, doesn't have primary stress is of little relevance.

[11] The terms "compositional" and "non-compositional" meaning, whether of formal objects or of conceptual Content, are used here in a non-technical (semantic) sense. I will assume a complex expression to have a "compositional" meaning if **some** transparent, systematic combinatorial rules can derive its interpretation on the basis of the meaning of its parts. For example, the interpretation of non-intersecting adjectives, in this context, is transparent and consistent (*red face* being a predictable combination of *face* with *red-for-face; red sofa* being a combination of *sofa* plus *red-for-a-sofa*) and hence, non-technically, compositional, in a sense that e.g. *transmission*, as a car gear, *greenhouse*, and even *blackberry* are not.

word formation is checked against Content in a way which does not apply to phrases, and hence that the formation of words differs fundamentally from the formation of phrases.

Note now that for such lexicalist approaches, "words", as the outputs of an independent WF component, are not only syntactically atomic, as we already noted, but also, importantly, **complete** in providing a juncture of semantic, syntactic, and phonological information. As such, "words" within such approaches are unique formal objects, in that no other terminal or single representation in the grammar is complete in a similar sense. N or D as syntactic terminals are certainly not complete in the same sense, nor is their combinatorial output, DP, complete in the relevant sense. It has syntax, but arguably it doesn't even have an interpretation, but rather must be converted to a semantically appropriate representation to receive one. It certainly doesn't have inherent phonology or Content. Nor are the terminals or the output of phonological operations or semantic ones complete, in the relevant sense. As such, WF has properties that are very distinct from those of its fellow grammatical modules. To wit, the syntax creates representations which are translated into (possibly unique) semantic formulas, which themselves utilize semantic primitives and modes of composition which are distinct from those utilized by syntax. Certainly, one does not assume that semantic objects correlate to unique phonological objects, and even the claim that they correlate to unique syntactic objects is largely not accepted. Not so "words", in the decades following Chomsky (1970) and Halle (1973), and most strikingly so, perhaps, in Lexical Phonology and Morphology. "Words", here, are perceived as units which are morphologically constructed but are nonetheless simultaneously phonological, semantic, and syntactic objects, and where none of these distinct sets of properties is derived from another. Fundamentally, the lexicalist claim here is that a particular combinatorial module, WF, creates hierarchically complex structures which are privileged in creating a domain that must have syntactic properties that function as instructions for syntactic tree construction; they must have (lexical) semantics (some potentially deriving the syntactic properties); and they must come with instructions for phonological rule application. This is so even though, in and of itself, the atoms and the combinatorial processes used by the morphological combinatorial module are neither inherently semantic nor are they assumed necessarily to have fixed phonological or syntactic properties. In contrast, note again that no such relationship holds between syntax and phonology, or syntax and semantics, and such dependency is almost incoherent as a statement of the relationship between phonology and formal semantics. Phonological properties need not treat phrases as privileged atomic units (to wit, liaisons such as *who's* and *isn't* which arguably cross phrase boundaries), nor are phrases atomic semantic or Content units. Rather, they are subject to compositional semantic interpretation as based on their parts.[12]

[12] Not so for example in Construction Grammar, where a phrase, or a sentence, is a template consisting of discontinuous constituents, at times phonologically fixed (e.g. the *his way* construction) and which may presumably constitute a single unit of Content. For some comments on phrasal idioms, see the appendix to Chapter 9.

Within an agenda that was extremely influential, the triple role of the "words" became, effectively, the set of instructions that just about all other components of the grammar were to check. D-structure was but the tree constructed from the syntactic properties of the "word", which then had to be preserved throughout the syntactic derivation. The phonology, likewise, was a direct mapping of the *sound* properties associated with the "word" into phonological–phonetic representation. While some aspects of meaning and syntax continued to be independent of the properties of "words" (presumably, all so-called A-bar operations), the view of the grammar as, effectively, checking the properties of "words" had come to play a progressively more important role. Very telling in this respect is van Riemsdijk and Williams (1981), who partition the syntax into two formally distinct components, the first, NP-structure, fundamentally lexicalist and receiving its "instructions" from "words", the latter, more abstract, interfacing only with formal syntactic objects.

At least one major problem emerges immediately, however, when we observe the central role of the "word" in such approaches, and that is the fact that if we take "words" to be, at least phonologically, primarily prosodic units with a single main stress, then it turns out that identical grammatical information is represented as phrasal in some languages, but as a "word" in others (and see Marantz 1997 for discussion). Causatives may be a case at hand. While English and French opt for analytic causatives, polysynthetic languages opt for a synthetic form, as do Semitic languages. The expression *(Z) make (X) cross (Y)*, in English, is clearly phrasal, contains more than one main stress, and has two phonologically discrete verbal heads which may be separated from each other. Similar facts hold for *(Z) faire traverser (Y) à (X)* in French, with two verbal main stress domains and a potential clitic intervener. On the other hand, Hebrew *(Z) he?evir (X) (Y)* 'cross-CAUSE' is one prosodic unit, has no coherent separable parts, and allows no intervening material inside it. Even more problematic is the fact that the very same language may at times express the same grammatical information as a phrase or as a "word". Consider again Hebrew, where a synthetic inchoative based on a particular adjective may occur alongside a periphrastic construction using that same adjective, and where the following are truth-conditionally equivalent:

(3) a. *ha.binyan* *hichib* *?im ha-zman* [*cahob*: 'yellow']
the.building yellowed in time
'The building became yellow/became yellower in time.'

b. *ha.binyan* *na?asa/nihiya* *(yoter) cahob* *?im ha-zman*
became (more) yellow
'The building became (more) yellow in time.'

Any attempt to reduce the properties of syntactic configurations to properties of "words" would thus need effectively to have two very distinct structures giving rise to a suspiciously similar syntactic and interpretational configuration. To wit, in English and in French, *Y* is the subject of *cross*, but in Hebrew, it would have to be the object of *cross*-CAUSE. In one inchoative construction in Hebrew, 'the building' would be a subject of an adjective, in the other the (unaccusative) subject of a verb, and so on.

And yet, the interpretation of the argument configurations, the event structure properties, both syntactic and semantic, and the internal syntax are all arguably extremely close, if not identical. If constructed in distinct grammatical modules, such formal correlations between the outputs become a coincidence.[13]

1.3 Moving Away from the "Word"

1.3.1 *A snapshot and road signs*

Returning now to the question as broadly outlined at the end of section 1.1, the answer provided by lexicalist approaches to the interaction between (minimal units of) Content, PH, and syntax can be (broadly) diagramed as in (4):

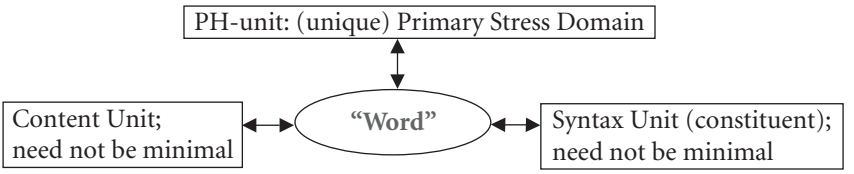
[13] Rather than consider this duality a drawback, the system developed in Reinhart (2002) as well as in Reinhart and Siloni (2004) and Horvath and Siloni (2011) puts this duality forward as a cornerstone of language variation. The "Syn–Lex Parameter", specifically, allows a grammar to opt for either syntactic *-arity* reducing operations or for lexical *-arity* reducing operations. The latter, specifically, is local and consists of existentially binding one of the lexically specified arguments of a listed item. The result is an altered set of instructions to the syntax, in that the existentially bound argument fails to merge. *-arity* reducing operations are by assumption illicit as such in the syntax. For instance, a lexical operation would allow a reflexive to form as a result of the binding of the internal argument, thereby giving rise to an intransitive (unergative) structure. A correlating **syntactic** reflexive, however, would only be able to delimit the realization possibilities of an argument. Thus an internal argument must project, but may be realized as a clitic (e.g. *se* in Romance), thereby delimiting the realization possibilities of the direct object.

From a formal perspective, however, the Syn–Lex Parameter is rather illustrative of the inherent formal problems for the duality of representations. Intuitively, the meaning of *-arity* reduction is fairly clear, but the intuition here is based on a discourse function—that is, on the generalization that an argument needs to be defocused or eliminated. Formally, no unified statement is available that could range over the existential binding of a lexically listed argument, on the one hand, and its realization as, e.g., a null pronominal or a clitic in the syntax. The parameter as stated, then, allows UG to force a choice between two formally entirely distinct operations, whose outputs happen to converge in terms of their discourse function. The difficulty is seriously amplified by the fact that no **general** formal parametric choice is—or could be—available and that *-arity* reduction is relativized to a construction: *-arity* reduction in reflexives, *-arity* reduction in causative-inchoatives, *-arity* reduction in passive, and so on. But this presupposes that something like "reflexives" exists independently of its syntactic structure or its argument instantiation, a rather difficult concept to grasp outside the domain of discourse function, given the fact that it is not clear that any of these "constructions" are more than a linguistic terminological convenience.

Quite possibly, however, the most problematic aspect is the fact that the model is explicitly not committed to the (morpho-)phonological reality of the listed items to which *-arity* reduction operations apply, and that in that sense, they appear to be more akin to Beard's (1995) "Lexemes" than to "words", as used, for example, in Williams (1981a, b). I return to this point, and to the role of phonology, in section 1.3.2 below, as part of a more general discussion of realizational models.

As a historical footnote, a syntax–lexicon parameter was suggested in Borer (1984), and pursued, for instance, in Borer (1990). Crucially, however, what was parameterized in that model was not a formal operation of any sort, which was in all cases identical and responsible for the concatenation of particular morphemes. Rather, the variation involved the merger possibilities of the output. A **Lexical** choice entailed that the morpheme combination merged as such at D-structure. A **Syntactic** choice entailed the merger of the morpheme combination as a super-tier, of sorts, of an already existing syntactic structure, providing it matched it phonologically. Morpheme combinations, in all cases, were syntactically opaque. A merger as a super-tier, however, left the syntactic 'under-tier' available, thereby giving rise to the appearance of word-internal transparency. No such transparency emerged for a D-structure merger, as no syntactic parallel structure existed to correspond to it.

(4)



Crucially, the notion of "word" as it emerges from (4) cannot be derived. In other words, there is no sense in which we could define words as a set of three properties, precisely because the "word", by assumption, has properties which are meta-theoretical insofar as they do not emerge from the properties of its phonology, its syntax, or its Content, most important among them its atomicity. Nor do the constituent structure or the Content properties need to be minimal, and in fact, they are not constrained in any obvious way by the system. Rather, it is the existence of a listed item, a particular "word", that serves as their *raison d'être*, so to speak. Thus compounds such as *blackboard* or *kitchen towel* are certainly not a single constituent, nor do they clearly have minimal Content, and arguably, the same holds for *arguably*, which corresponds neither to minimal Content (i.e. it is either *ARGUE+able+ly* or *ARGUABLE +ly*), nor is it a minimal constituent. Nor is there a necessary connection between the complexity of constituent structure, i.e. syntactic complexity and Content complexity—within quite a few lexicalist accounts it is assumed that transitive *break* is a superset of intransitive *break* in terms of its semantic properties (i.e. that intransitive *break* is derived from transitive *break* and entails an external causer), making transitive *break* syntactically minimal, but with complex Content. In fact, as is entirely clear from the model in (4), the only consistently well-defined and minimal domain that can be associated with e.g. the English "word" is phonological, and as a consequence the picture in (4) amounts to the claim that what is, in actuality, a minimal phonological domain (specifically for the assignment of stress), nonetheless has privileged properties that allow it to define an atomic domain that goes beyond the domain of phonology and extends into both syntax and Content assignment (and see Marantz 1997 on this latter point).

If we wish to reject the a priori privileged status of "words" in lexicalist models, and given the fact that the only robust definition of what a word is appears to be phonological, suppose we assume that words, or at least substantive words (as opposed to function words), are a prosodic unit of a particular (language-specific) size, or, wishing to be potentially broader, that phonological domains can be usefully defined so as to constrain the application of phonological rules, and that one such domain corresponds to what in English, as well as in a good many other languages, would correspond to the prosodic domain of a single main stress. Suppose we now call this particular domain "Phonological Rule Application Domain", or P-RaD (with the understanding, of course, that it refers to a well-defined phonological domain among possibly other larger or smaller ones). Returning to (1) and to the potential correlations between Content, PH, and syntax, we can now fix the PH tip of our pentagon as some specific P-RaD, and ask what, if anything, P-RaD corresponds to within the area of well-defined syntactic or semantic properties.

When we now consider WF, and taking WF to refer specifically to combinatorial operations which give rise to complex structures of some sort, the inevitable conclusion that we reach is that for many accounts, WF is **definitionally** all combinatorial operations which are internal to the P-RaD, while syntax is definitionally all combinatorial operations external to the P-RaD (and see Aronoff 1994 for the same conclusion). This claim is implicit in any account that subscribes to the Lexical Integrity Hypothesis or to the Atomicity Thesis, and is explicitly espoused in Ackema (1995) and in Ackema and Neeleman (2004). What is, however, rather striking is that none of these accounts offers a definition for what a syntactic "word" or even a morphological "word" is, such that it is independent of P-RaD; i.e. independent of whatever domain is defined by the assignment of primary stress. To illustrate, in Lexical Phonology and Morphology, "word" is essentially defined in terms of a boundary type, #, itself no more than a diacritic marking a specific domain for phonological rule application. However, and following the original insights of Allen (1978) and Pesetsky (1979), Kiparsky (1982a) proposes that such a boundary defines, as well, a domain for both syntactic (i.e. constituent composition) and semantic operations. To wit, it is assumed that a + boundary can separate an affix from a non-word (presumably, a constituent devoid of Content), and hence, at least possibly, a category-neutral item; but not so #, which may only attach to what is already, itself, a word; the latter, it is claimed, necessarily with a category and with Content. Combinatorial Content across a # boundary is predictable, or so the claim goes, but not necessarily across a + boundary, and so on. Beyond its phonological role, however, what a # boundary is, from the perspective of categorial constituent structure or from a Content perspective, remains undefined.[14]

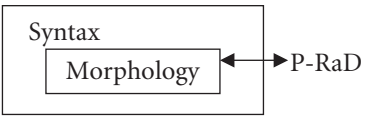
The conceptualization of the issues under consideration here might be facilitated by some diagrams. In (5a) there is an internal domain and an external domain, and P-RaD defines, or is defined by, the internal domain. The picture in (5b), on the other hand, associates P-RaD with some point in the syntactic structure. The picture on the left does not, of course, exclude the existence of a single combinatorial computational system (outside phonology). The system on the right, however, makes a stronger statement—it excludes, in principle, the existence of more than one combinatorial

[14] Essentially, LPM in particular and the Level Ordering Hypothesis in general view the issue in terms of domains, and claim that what we are calling here P-RaD is an atomic domain, and hence, presumably, must be complete in some well-defined sense. We note in this context that if true, then lexicalism, from this perspective, becomes axiomatic, rather than a matter to be empirically decided, as already suggested in the discussion of diagram (4).

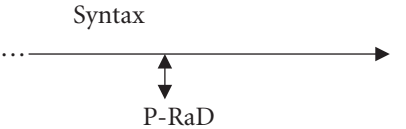
The claim, in turn, bears non-trivial similarities to Chomsky's (2001) "Phase", insofar as a "phase" defines a domain which is syntactically defined, but which nonetheless must be complete in some phonological and semantic sense. When comparing these two distinct domains and the theoretical assumptions that underlie them, in turn, three rather distinct issues must be addressed. First, whether it is altogether a theoretically and empirically sound move to create a unified domain for the satisfaction of phonological, syntactic, and meaning properties, where meaning spans both formal interpretation and Content. Second, assuming it is a good move, is such a domain to be defined phonologically, as in the Level Ordering Hypothesis, or syntactically, as in Minimalism? And finally, assuming the latter issue is settled, what, exactly, *is* the relevant domain, and from the perspective of the discussion here, most crucially, does P-RaD constitute one such domain? I take up this matter in Chapter 9.

computational system (again outside phonology). Crucially, in (5a) it is possible to define formal operations which apply to the inner box but not to the outer box, and that is, indeed, the primary task which faces proponents of an independent, non-syntactic WF. The diagram in (5b) excludes, in principle, the existence of such operations:[15]

(5) a.



b.



Suppose, then, we adopt the stronger claim, according to which there could be only one unified computational component which is responsible for all constituent-building and all constituent-manipulating operations; call it "Syntax". Within such an approach, P-RaD might correspond to some particular domain in the incremental merger system, or possibly more than one.[16] If that is the case, then the formation of words, whatever they turn out to be, **and providing of course it is combinatorial and hierarchical**, cannot differ from the formation of phrases.[17] However, in the absence of a distinct module of WF, and in the absence of "words" as conjunctions of phonological, semantic, and syntactic properties, we must otherwise address the concerns put forth by lexicalist accounts which have motivated the lexicalist shift to begin with. These concerns are phonological, i.e. those that involve the way in which entry-specific material impacts the phonological spellout of words; they are morphological, i.e. they concern the particular choice of affixation in particular contexts; they are Content related, i.e. they concern the emergence of non-compositional Content for complex words; and they are syntactic, i.e. they concern the emergence of syntactic properties, including category and insertion frame. To illustrate, within the area of phonology, such an account would need to address itself, at the very least, to defining a domain within which one can find stem alternations such as *ceive*/*cept* or *sing*/*sang* and to how information about such alternations and their environment is to be encoded. Within the area of morphology, it would have to show itself capable of capturing the fact that e.g. *transform* is nominalized as

[15] Note that the relationship between the inner box and the outer box need not be linear and was explicitly assumed not to be so in Parallel Morphology (cf. Borer 1991 i.a.). That the inner box must be distinct, formally, from the outer box was argued expressly in Borer (1998b), as based on distinct formal properties of morphological and syntactic operations. Many of these arguments are developed and augmented in Ackema and Neeleman (2004), although quite a few of them have been made obsolete by the shift away from X′-theory (see Chapter 6). My own change of perspective is ultimately based on the conclusion that the modules cannot be formally separated without compromising explanatory adequacy, and that insofar as formal distinctions may still be discerned, they are a set of instructions for a future research agenda, rather than its bottom line.

[16] The choice of a spellout domain for a particular form could, in turn, interact with language specific phonological factors, and in particular, the phonological realizational properties of what I will call, below, S-functors. See section 1.5.4 as well as Chapter 9, section 2 for the relevant discussion.

[17] A characterization, I believe, true of so-called derivational morphology, but not necessarily of so-called inflection, and see sections 1.3.2 and 1.4 below as well as Chapter 6, section 3 for some comments.

transformation, but *defer* as *deferral*. Content-wise, the task would be to account, e.g., for the possible emergence of a specialized Content for *transformation*, but never for gerunds; and finally, syntactically, at the very least it would have to account for the emergence of specific categorial behavior for (*a*) *cat* and (*to*) *walk*, on the one hand, as well as for (*a*) *transformation* and (*to*) *verbalize* on the other. Much of this book is devoted to constructing a model which, I believe, is capable of giving at least some answers to these questions. In the remainder of this introduction, I will briefly outline some general core ideas I will be following throughout. A detailed elaboration as well as justification for the model will be pursued in Chapters 6–10.

1.3.2 *Phonological considerations and realizational models*

We noted that the perception of the "word" as a unit that is at the same time phonological, syntactic, and semantic, is crucial to lexicalism. In turn, this perspective is rooted in the view of the morpheme as the smallest unit of sound–meaning correspondence, coupled with the essentially structuralist view entrenched in Chomsky (1965), according to which syntactic structure is incrementally constructed from categorially marked primitives. Certainly, insofar as the building blocks of "words" are themselves already units of sound and meaning and possibly syntax, one can hardly expect the output to be devoid of such properties. As it turns out, and in addition to the consequences already outlined, the fundamental grouping of these properties also gives rise to a very specific view of the interaction between the formation of words and the syntax. One such consequence is noted in Borer (1998b) and consists of the fact that lexicalist models are perforce linear—that is they assume the existence of a single point of interaction between "words" and the syntax, and specifically, that the output of the lexicon (assuming the lexicon to allow the internal modification of listed items) is the input to the syntax and specifically to D-structure, or to primary merge. In the strongest articulation of these claims, that of Kiparsky (1982a), this entails that word phonology and inflection as well must be part of the lexicon and hence must precede the syntax. In other accounts, lexical representations may be underspecified, phonologically, at merger, but only insofar as aspects of phrasal phonology may impact them at a subsequent point.

To appreciate the fact that this execution is just about inevitable within an approach that strictly views the morpheme as the smallest sound–meaning unit, it might be worthwhile to consider a large number of syntactic accounts of word formation prevalent in particular in the 1990s and subsequently, all inspired by Baker's (1985) Mirror Principle. The accounts under consideration are specifically those which allow for the syntactic merger of inflectional morphemes, complete with phonological information, in an attempt to thus piece together, so to speak, the inflected phonological word.[18] It is, I believe, fair to say (and see Anderson 1992 and

[18] Baker's (1985) original argument was not based on inflection, but rather on the interaction of syntactic structure and argument-structure changing morphology (passive and causatives) which, unlike inflection, is typically extremely phonologically stable, and is certainly very stable in the polysynthetic languages studied in Baker (1985, 1988). From the perspective of the model to be articulated here, it is thus quite possible that the Mirror Principle, as a descriptive generalization, **is** valid, for morpheme ordering,

Halle and Marantz 1993, as well as Borer 1998b) that this particular research agenda has been unsuccessful, insofar as after two decades, it is rather clear that any such account is in principle incapable of handling the degree of phonological idiosyncrasy found, typologically, in inflectional systems, involving, among many other factors, unpredictable gaps; different orders of markers even within the same language; a single marker that correlates to two syntactic distinctions; two markers that correlate to a single syntactic distinction; markers that don't have an obvious syntactic correlate; etc. The consensus, in the morphologically informed literature, is that the task of relating syntactic structure to phonological realization cannot be thus accomplished. This particular conclusion, if coupled with the view of inflection as a morpheme in the classical sense (=the smallest sound–meaning unit), thus results in the inevitability of a morpho-phonologically rich lexicon.

As it turns out, however, the classical morphemic view itself has come under a fair amount of criticism. In a series of extremely influential works, and following insights originally in Matthews (1972, 1974/1991), Beard (1981, 1995) presents a system of word formation in which phonological realization is explicitly severed from any morpheme structure, a hypothesis that has come to be known as the "Separationist Hypothesis", and which has given rise to a class of models commonly referred to as "realizational". Most crucially, within realizational models, phonological form need not correspond strictly to constituent structure or to any listed unit. Indeed, in much of his own work, Beard denies the use for any such constituent structure. It therefore follows that even if there is a lexicon with listed units, such units are not listed with fully specified phonological form, but rather their phonological realization may be potentially sensitive to a variety of independent factors including aspects of the syntactic derivation. Thus at least one obvious way to execute this insight would be to assume that phonological realization operates on syntactically (in)formed constituents, which would then entail that it is post-syntactic, i.e. involves so-called "Late Insertion".

Thus viewed with the benefit of hindsight, we may now conclude (as would Halle himself, no doubt) that it was probably an error to conflate the phonological irregularities within the inflectional domain with the syntactic and Content irregularities within the domain of so-called derivational morphology, so as to use inflectional irregularities as supporting evidence for the listed nature of all outputs of word formation operations. While the realization of inflection certainly does appear to be contingent on listed information, and while an account of "word" unpredictability within the domain of syntax and Content is certainly required, it nonetheless appears rather clear that the formal characterization of the former may be quite different from the formal characterization of the latter, and that little is to be gained from conflating them. This is all the more so because, as I will show in Chapter 9, the domains for "inflectional irregularity" and "Content irregularity" do not converge.

insofar as derivational functors, I will suggest, are syntactic terminals and hence, trivially, "morphemic". The translation of Baker's original insight into the inflectional system emerged, originally, as a result of Belletti's (1990) attempt to derive the inflection of the Italian verb from the split INFL system proposed in Pollock (1989).

This said, realizational approaches do not necessarily all agree on the precise objects to which phonological realization applies, or, for that matter, on the nature of what, if anything, is the listed residue and on the extent to which it may or may not be subject to lexical manipulations. Beard himself (1995, 1998) subscribes to the view that **all** processes which impact the listed item, the "Lexeme", result in a possible phonological modification, a generalization which he holds to be true for both (traditional) inflection and derivation. Specifically, then, insofar as one can refer to some words (e.g. *sings*, *singing*, *singer*) as complex, their complexity is not cumulative or hierarchical, but, rather, marks such complex forms as having undergone some process which results in a distinct phonological realization. To illustrate, in the mapping from *sing* to *sang* as well as in the mapping from *walk* to *walked*, neither the phonological change in *sang* nor the one in *walked* corresponds to the existence of a chunk of structure that we can refer to as PAST and with which we can associate some well-defined phonology. Rather, a process that we may refer to as PAST is responsible for changing the phonological information associated with *sing* and *walk* respectively so as to give rise to a distinct phonological realization of the output. Similarly, in deriving e.g. *deferral* from *defer*, we can assume a process called NOM which modifies the phonological information of *defer* so as to result in the phonological form corresponding to *deferral*. Crucially, there is no sense in which *walked* has more complex **constituent structure** than *walk*, or *deferral* is more complex than *defer*. The reader might note that in making this latter assumption, Beard's system is rather akin to Chomsky's (1970) original view of the relationship between pairs such as *destroy* and *destruction* (see (2) and related discussion). Finally, insofar as the processes under consideration apply to **some** object, that object, the Lexeme, is for Beard fundamentally a Content unit associated with a categorial label and some basic (underspecified) phonological information. The Lexeme, we note, cannot have meaningful internal complex constituent structure, making e.g. *walk* and *patronize*, or *verbalize*, effectively identical grammatical objects, and likewise *boy* and *dancer*, with the agentive content of the latter representing **semantic** derivational complexity, rather than one that might involve a complex constituent structure.

Endorsing a realizational approach in some domains, Anderson (1982, 1992) proposes a specific model which allows e.g. *sing* or *walk* to be marked as PAST in the relevant syntactic context, while at the same time continuing to maintain that they are not complex constituents, and that e.g. PAST is not a morpheme, where by "morpheme" here we mean an affix of some sort (and hence "Amorphous Morphology"). Anderson is clear, however, in maintaining that a distinct, hierarchical system (albeit a lexical one), complete with morphemic representation, may exist alongside such a realizational system, with the latter covering roughly (but not precisely) what is at least at times referred to as "Derivational Morphology". In his system, then, such hierarchical structures may, in principle, be capable of combining listed items which are smaller than Lexemes in the sense of Beard (1995, 1998), and specifically, items that may fail to be triplets of Content–syntax–phonology.

An approach that is simultaneously morphemic and realizational, finally, is put forth within the framework of Distributed Morphology, beginning with Halle and Marantz (1993), and subsequent work primarily by Noyer (1997), Embick (2000,

2004, 2010), Embick and Halle (2005), Embick and Marantz (2008), Bobaljik (2000, 2005, 2008), Nevins and Nevins et al. (2008, 2010, 2011), and others. Within that approach, both inflection and derivation are morphemic insofar as they correspond, specifically, to a well-defined merging syntactic constituent, with meaning, but no sound. Thus insofar as, e.g., *sang* and *walked* are associated with PAST, and insofar as PAST is a (syntactically merging) morpheme, both *sang* and *walk* are (at least) bi-morphemic and hence syntactically complex. The realization model in Distributed Morphology, however, is considerably richer than that assumed in either Beard (1995, 1998) or Anderson (1982, 1992) in allowing radical manipulation of the syntactic constituent structure prior to the actual spellout, or the phonological realization. Possibly the most radical manipulation involves the "flattening" of the structure (to give rise to "M-Structure"), to be followed by a variety of modifications that operate on a linear string and which may involve fusion, fission, reordering, insertion, deletion, and so on, all prior to defining the specific units which are then subject to phonological realization (or "Vocabulary Insertion", in the terminology of Distributed Morphology). Like Beard and presumably Anderson, Distributed Morphology avails itself of a list of (basic) Content units with some well-defined properties, with which other morphemes may merge. Unlike Beard (1995), however, listed units, explicitly, cannot be complex and do not have a syntactic category. They are, rather, "roots". As should become evident, the model to be developed here shares, with Distributed Morphology, the assumption that the basic listed item, the root, is not complex and is devoid of a syntactic category. Nonetheless, the notion of root to be developed here will be distinct in other rather important respects from that utilized in DM. I return to this matter, briefly, in section 1.4 below, and at greater length in Chapter 8.

1.3.3 *In defense of derivational constituents, preliminary*

When considering much of the realizational literature, of which the three approaches above are fairly representative but are by no means an exhaustive sample, one is rather struck by the overwhelming focus on inflection coupled, nonetheless, with conclusions which are asserted to apply to derivation as well.[19] That is certainly the case in Halle and Marantz (1993), as well as in Noyer (1997), Bobaljik (2000, 2009), and Nevins (2010). It is even more striking in Beard (1995, 1998). In Beard (1995), an extremely sketchy discussion of (category changing) derivation (Marchand's "Transposition") is summarized as follows: "The importance of discussing transposition at this point is that it allows us in future chapters to exclude from consideration all operations which simply change lexical class". Beard (1998) then asserts on the basis of an equally sketchy discussion that there is no need for any hierarchical representation for any of the processes typically referred to as "derivation" (excluding compounds). And yet, in the present author's native language, as in all Semitic languages, just about any phonologically well-formed unit is the output of what would otherwise

[19] And where by "inflection" here I refer, rather loosely, to markers which are typically assumed to be conditioned by syntactic dependencies such as tense, agreement, case, aspect, plural marking, and so on, and by "derivation" primarily to the affixation of category-marked affixes (and setting aside non-category changing prefixes).

be labeled a "transpositional" or "functional" morphology, and the matter is hardly as "simple" as Beard suggests, nor is it "purely" phonological in any theoretically helpful sense.[20]

Suppose we consider now "inflection" vs. "derivation" from the point of view of the correlation between what we may broadly think of as Form and what we may broadly think of as Function, and taking as our starting point *-ed* and *-tion* as Forms, and PAST as well as a categorial label such as N[V] as Function (and where N is a projecting category and [V] defines its categorial complement). The fact of the matter is that in neither case can the Form be predicted from the Function. N[V], in English, at the very least may be spelled out as *-ment*, *-al*, *-ance*, *-ence*, *-age*, and possibly *-ure* as well.[21] PAST in English is associated with a broad range of unpredictable stem allomorphs, sometimes in conjunction with no dedicated past tense marker at all and at other times possibly maybe with *-t*, and hence *sang*, *ate*, *broke*, *caught*, *dreamt*, etc. Consider, however, the predictability of Function from Form. Although *-tion* at times attaches to stems which do not have a clear category (e.g. *nation*, *potion*), it never attaches to anything that is categorially distinct from V, and while there are sporadic cases of verbs that end in *-(a)tion*, these are all clearly reanalyzed erstwhile nouns.[22] Moreover, there are, to be sure, some stem allomorphs in conjunction with *-(a)tion* (*destroy–destruction*; *perceive–perception*), and *-(a)tion* itself has three (rather minimally different) variants, but these are really quite limited, when we compare them to the domain of inflection. A similar situation holds for other so-called derivational suffixes. While the spellout of V[N], a Function, may not be predictable, and may be instantiated as *-ize*, *-ate*, and *-ify* (and possibly as *en-* and *be-* as well), the Functions of *-ize* or *-ify* are entirely predictable from their Form. When we turn to the area of so-called inflection, however, what we find is not only that Form is not predictable

[20] Beard (1995) discusses three types of operations as (putatively) coming under "derivation": category-changing morphology (transposition, e.g. *form→formation*), functional morphology (*recruit→recruiter; →recruitee*), and expressive morphology (evidential marking, diminutives, augmentatives, etc.). Because he is committed to the non-hierarchical representation of these processes, and because he believes compounds have a hierarchical structure, compounding is summarily expunged from the realm of WF.

Most of the arguments put forward by Beard (1995, 1998) are intended to challenge the assumption, inherent in hierarchical approaches to WF, that morphemes are listed. His challenge to listedness, on the other hand, is based on the comparison of properties of morphemes to properties of what he presupposes without much discussion to be the paradigmatic listed items, namely Lexemes. Insofar as "Lexemes", for Beard, are a triplet of syntactic information, semantic information (=Content) and phonological information, the expectation is that if (derivational) morphemes are listed items, they should have a similar set of properties, which, Beard argues, they do not. In the model under development here, derivational morphemes are the spellout of a syntactic function (at times in conjunction with a semantic one), and Lexemes, as such, are an ill-defined notion and certainly do not correspond to listedness. As a consequence, most of the criticism advanced in Beard is not applicable. For a fuller discussion see Chapter 7, section 5. We note, finally, that the distinction between "transposition" and "functional" morphology is not clearly well motivated. Concerning so-called "expressive" morphology, it appears rather likely that it is, indeed, to be treated on a par with inflection, which is to say, as marking that is integrated into Extended Projections and is primarily realizational (and see section 1.5 below as well as Chapter 6, section 3).

[21] As well as, of course, *-ing*, *-er*, and possibly *-ee*, all three with well-defined distinct semantic functions. I return to this matter at some length in section 1.5.1 below as well as in Chapter 6. See also Chapters 4 and 12.

[22] E.g. *condition*, *question*, *ration*, etc. I return to these cases in some detail in Chapter 7, section 4.4.

from Function, but that Function is altogether not predictable from Form. Thus there are no cases in which *-(a)tion* fails to return a N, even if at times N is homophonous with a (reanalyzed) V. English *-s*, however, may mark plural, verbal inflection, and case. Once we turn to *-ed*, we find that there are at least two major cases in English in which *-ed* fails to return PAST, one involving participial cases and the other adjectives. Nor is it the fact that across its occurrences *-ed* always attaches to (attested) V—to wit, *winged*, *legged*, etc. Much more crucially, and regardless of the specific analysis of past tense marking in such cases, the Function of the marking in pairs such as *give–gave*, *break–broke*, *catch–caught*, or, for that matter, *goose–geese* is, to put it mildly, not obvious. The lack of the ability to predict Function from Form in inflection is so severe that few grammarians think of *-ed* or of *-s* as meaningful items in themselves. Rather, the common assumption is that inflection, even if it corresponds to a constituent, or a morpheme, is abstract—e.g. PAST or PL. Once cast in terms of an abstract Function, "inflection" becomes, of course, entirely regular. It is, in fact, **only** in terms of the abstract Function that one could claim that "inflection" is more regular than "derivation". V+PAST is a combination with entirely predictable properties, not only syntactically, but also semantically. V+*ation* may be (just about) predictable in terms of its category, but its Content very frequently falls short of such predictability. It thus emerges, very roughly put, that "inflection" should be characterized in terms of its syntax and its semantics, while "derivation" should be characterized in terms of its syntax and its phonology. Insofar as realizational models have been extremely successful in bypassing intractable problems of "inflection", this is precisely because "inflection" is very often the ad hoc phonological realization of what is, otherwise, a semantically and syntactically fully predictable generalization. In fact, so predictable that inflectional marking is almost superfluous, and thus can afford, so to speak, to be largely missing in many languages, and very erratically marked in others. If, however, "derivation" is a broad description for a class of generalizations which are always syntactic and frequently **only** syntactic, and which need not have a semantic value, nor, necessarily, give rise to predictable Content, it is at least plausible to assume that whatever function they do have, and insofar as such a function may not be otherwise discernible from either semantics or Content, it may need to link to a more regular set of phonological realizations. But if at all on the right track, this suggests that radical realizational models cannot work for derivation; that derivation is, quite possibly, morphemic in **some** sense, and that attempts to conflate "derivation" and "inflection", either along the lines suggested by Beard (1995) or by Distributed Morphology, are on the wrong track.

Wishing to translate the informal intuition in the previous paragraph into an actual rigorous model, I will assume that inflection is, indeed, radically realizational, which is to say, I will assume that it is amorphous, and that e.g. *sang*, *walked*, and *dreamt* are all non-complex. As a consequence, I will also assume, as in Anderson (1982, 1992), but not as in Halle and Marantz (1993), that although there certainly is something that we may refer to as PAST, what *sang* or *walked* correspond to is not a combination of a stem (or a root) + PAST, but rather the spellout of a stem (or a root) marked as PST. There is, differently put, no morpheme boundary inside *sang*, nor is there one inside *walked* or *geese*, for that matter. Not so *-ation*, or more accurately,

the categorial functor that comes to be spelled as /$_{\pi}$*ation*/. I will argue at some length that the relevant functor merges and projects syntactically, and that as a result, any combination of *-ation* with any stem is at the very least a binary branching structure.

The complexity of structures realized with *-ation* vs. the absence of such complexity in e.g. *sang*, or for that matter [$_{N}$ jump], is discussed at some length in Chapters 6–11. Most crucially, a notion of locality will be developed and argued for in this book which would specifically distinguish between the local domain defined by e.g. *formation* and that defined by *sang*. For the time being, then, it is hoped the reader will bear these differences in mind, while awaiting a discussion of their justification and ramifications to be undertaken in Part II of this work.

1.4 Roots, Preliminary

If, indeed, categorizing affixes are constituents, it follows that in contrast with e.g. Beard (1995), it cannot be assumed that /$_{\pi}$*dancer*/ is the spellout of a single constituent. Rather, we must assume it to correspond to a complex structure consisting of two merged terminals which spell out as /$_{\pi}$*dance*/ and /$_{\pi}$*er*/ respectively. Assuming for the sake of the discussion that we actually know what function /$_{\pi}$*er*/ spells out, this still leaves us with the question of what /$_{\pi}$*dance*/ is. By extension, and assuming that functors which spell out as /$_{\pi}$*er*/ or /$_{\pi}$*ation*/ attach to **something**, what is that something, and what list or reservoir does it come from? In accordance with the assumptions made so far, it could only be defined as some subpart of P-RaD, to which combinatorial principles may apply. Suppose we call such basic, underived units "roots" and proceed to notate them, at least initially, as √XYZ.[23] Importantly, roots are not "words". A "word", i.e. a P-RaD, can clearly consist of multiple roots—to wit, /$_{\pi}$*saber tooth tiger*/—in which case it is clearly derived, and thus by definition cannot be the basic underived unit we are in search of. A root, furthermore, need not be a P-RaD, need not even be a **possible** P-Rad, and quite possibly can never be a P-RaD. But what, then, **are** basic underived units? More importantly, how do we know one when we see it?

To illustrate some of the difficulties, suppose we take /$_{\pi}$*cat*/, following the seminal discussion in Marantz (1996). Is /$_{\pi}$*cat*/ a spellout of √CAT? Not so, claims Marantz (1996), and Borer (2005a, b) (as well as here) agrees—/$_{\pi}$*cat*/ is not the spellout of √CAT but rather of a larger syntactic unit, which, at the very least, also includes the information that it is a noun. The claim embeds two important related assumptions, both of them radically non-lexicalist: first, the claim that the basic underived unit, the root, is in and of itself devoid of syntactic category, and second, that syntactic category becomes available through syntactic structure. It is precisely this separation of category from root that allows us to propose that /$_{\pi}$*cat*/ is the spellout of some

[23] The term "root" replaces here the term "listeme" used in Borer (2005a, b). Although many of its erstwhile properties remain largely unchanged, the general perception of what roots are as well as specific claims concerning their Content have changed. See directly below and Chapter 8 for extensive discussion.

structure which is distinct from √CAT.[24] But these assumptions are by no means self-evident, nor is the three-way distinction between √CAT, /$_{\pi}$*cat*/, and [$_{N}$ . . . cat . . .]. There is also a theoretical claim put forth here which is likewise less than self-evident: that a root in itself need not, and possibly cannot, serve as an independent domain for phonological spellout.

The assumptions, as well as the claim, do receive preliminary support from Semitic languages. In Semitic languages, as is well known, all verbs and most native nouns and adjectives contain what is, indeed, traditionally referred to as "root", and which consists of a group of ordered consonants, or radicals, ranging from two to four. If, indeed, such a group of radicals is the basic underived unit in the sense of our (technical) root, then it is patently clear that it can never amount to a well-formed P-RaD in the relevant sense and that it does not have a category. In turn, all additional phonological information which would render these consonants pronounceable—vowels, affixes, gemination, and so on—comes at a syntactic price. Specifically, their addition is only compatible with the resulting form belonging to a particular syntactic category, and within the verbal domain, typically also as being inflected for a particular tense and voice. Thus it follows that in Semitic languages, it is never the root that is pronounced by itself, but rather, at the very least, the root in conjunction with whatever syntactic structure is responsible for its categorization.

But if roots do not have a category and can never be pronounced by themselves, what are they? Specifically, what are the Semitic roots √KTB, or √XŠB, or √PQD? Could they, for instance, correspond to Content? By way of a preliminary answer, suppose we consider the cases in (6), with each cell containing forms derived from the same root:[25]

(6)

√KTB		√XŠB		√PQD	
katab	'write'	*xašab*	'think'	*paqad*	'order'
niktab	'be.written'	*nexšab*	'be.considered'	*nipqad*	'be.absent'
		xiššeb	'calculate'	*piqqed*	'command'
hiktib	'dictate'	*hexšib*	'esteem'	*hipqid*	'deposit, entrust'
hitkateb	'correspond'			*hitpaqqed*	'be.counted'
katab	'correspondent'	*xašab*	'accountant'	*paqad*	'sergeant'
miktab	'letter'	*maxšeb*	'computer'	*mipqad*	'census'
makteba	'desk'	*maxšaba*	'thought'	*mipqada*	'army HQ'

The Content commonality appears sufficiently robust to find it tempting to say that e.g. √KTB has some conceptual Content to the effect that it is related to writing, although we note that deriving predictably the Contents *DESK* or *DICTATE* from *WRITE* may not be a trivial matter. Any proposed putative Content for √XŠB, now, would need to be considerably vaguer, related possibly, but rather loosely, to high cognitive processes. Clearly, however, the predictive powers here are extremely

[24] Although XSM categorization follows a very different route from Distributed Morphology categorization. See Chapter 7 for a detailed discussion.

[25] See *A Note on Hebrew Transcription*, p. xxiv, for conventions used in transcribing Hebrew.

limited, insofar as the Content *CALCULATE* cannot be predictably related to that of *ESTEEM* or *BE.CONSIDERED*, nor is it obvious how either one emerges from the interaction of the root with the morphological pattern it is embedded within. Matters go considerably further downhill when we consider the case of √PQD. While historically one could possibly link the emergence of these different Contents to some common source related to (numbered) troops, it is also clear that this history is but an anecdote to the current learner or speaker, and that for all intents and purposes, √PQD is either (at least) a three-way homophony, or, alternatively, has no Content independently of its categorized occurrences.

To the best of my knowledge, the only study that has attempted within a rigorous derivational system to associate Content with roots is that of Arad (2005), who sets up a number of entailments as a condition for assessing the commonality of root Contents in distinct morphological environments. In the final analysis, however, her study yielded an extremely small number of root–Content clusters that consisted of more than a single output form, and even within these, predictability was extremely limited. To wit, even if we assume that there exists a root √PQD (one of three homophones) with the Content "count (specifically people)", and another with some "command"-related Content, it still remains a mystery why the *mipqad* 'census' form should be related to the "count" root, but the morphologically identical, but feminine marked, form *mipqada* should be related to the "command" root. Nor does the specific meaning of *paqad*, 'sergeant' rather than say 'colonel' follow in any way. I return to this matter in Chapter 11, but it seems rather clear that if we are to find an answer to the question of what a root is, and if Semitic roots are our paradigmatic root, a reliance on Content as a foundation for what a root is, is at best weak and at worst circular.[26]

Lest the reader conclude that Semitic roots are simply another kind of root, consider the case of English /$_{\pi}$*round*/. Presumably, somewhere within all pronunciations of /$_{\pi}$*round*/ there is buried the root √ROUND. But what **is** that root? Is it, for instance, a Lexeme, in the sense of Beard (1995) and much subsequent work, where by Lexeme we mean here very specifically a unit of Content which may have variable phonological or syntactic realization? Well, we may think we have a relatively clear notion of what the Content of *round* is, but upon closer inspection, it is evident that √ROUND shares a surprising number of properties with √KTB or √XŠB. If we generalize over all occurrences of /$_{\pi}$*round*/, we are likely to find a (somewhat vague) conceptual Content that they have in common. This said, as a rigorous foundation for the Content that we actually get for the different instances of /$_{\pi}$*round*/ in e.g. (7a–f), it appears rather limited:

(7) a. a round of applause
b. a round of poker
c. a round building
d. to round the barn
e. to round the numbers
f. to round up (the children)

[26] This is not to deny the existence of semantically rigorous correlations within Hebrew morphology, a matter I return to in Chapter 11, section 5.

We could, of course, give up and claim that (uncategorized) roots are but a phantom of a deluded collective linguistic mind. Rather, the logic would go, what (7a–f) show clearly and conclusively is that the basic units we are playing with here are, at the very least, categorized constituents, and that [$_{N}$round], [$_{A}$round], [$_{V1}$round], [$_{V2}$round], and [$_{V}$round (up)] are different listed entries, complete with category and Content. The regrettable side effect of such an approach, however, is that it would render the phonological similarity, indeed, identity, a mere coincidence. Pairs such as *table–table*, *chair–chair*, *floor–floor*, *paper–paper*, *blackboard–blackboard*, *chalk–chalk*, *walk–walk*, *kiss–kiss*, and so on would likewise become coincidences, as well, of course, as *round-about*, *roundtrip*, *round robin*, and the nominal expression *a round up*, altogether a somewhat unfortunate result. A rather forceful illustration of the very same point emerges from the following paradigm, originally discussed in Clark and Clark (1979):

(8) a. The factory horns sirened throughout the raid.
b. The factory horns sirened midday and everyone broke for lunch.
c. The police car sirened the Porsche to a stop.
d. The police car sirened up to the accident site.
e. The police car sirened the daylight out of me.

As noted in Borer (2005b), each of the occurrences of *siren* here, as a verb, has rather different Content. That, of course, in addition to whatever Content comes with *siren* when it occurs as a noun. A Content-based listing would thus force us to list *siren* five times, thereby overlooking the fact that in none of these cases does the Content actually come from *siren* itself—rather it comes from its syntactic context; as well as the fact that the picture, on the whole, is characterized by innovative word use and is hence the least likely to represent a list.

We could also assume that of all the cases of /$_{\pi}$*round*/ in (7a–f) and elsewhere, (or for that matter of all cases of /$_{\pi}$*siren*/), one is basic (e.g. [$_{A}$round]; [$_{N}$siren]) and the others are derived from it by conversion or zero affixation of some kind. The latter approach, however, will hardly resolve our Content conundrum. Insofar as the Content of [$_{N}$round], [$_{V1}$round], [$_{V2}$round], and [$_{V}$round (up)] as well as the assorted compounds is different and unpredictable from the Content of [$_{A}$round], even if they are derived from [$_{A}$round], they would need to be separately listed, rendering the conversion here a vacuous operation.[27]

Needless to say, the problem is endemic and is not restricted to (apparent) monomorphs. Thus consider /$_{\pi}$*form*/, occurring as a phonological string not only as a noun and a verb (and with very clear Content similarity), but also in *formative*, in both its transparent and technical use. Relating the verbal and noun instantiations of *form*, as well as the transparent adjectival derivative (as in *a formative experience*) to one common Content seems plausible enough. But are we then to say that *formative*, as in *grammatical formative* is separately listed, and the fact that it can be segmented

[27] I return to the matter of zero categorial affixation in Chapter 7. The choice of *round* here and elsewhere is in recognition of the work on zero affixation (or lack thereof) by Pennanen (1971, 1983).

into portions which elsewhere make up for a productive combinatorial operation is a coincidence?

The bottom line, and as is entirely evident from both the Semitic and the English examples, is that the commonality displayed among the forms in (8) or in (7a–f) is neither that of Content nor that of structure. To be sure, that commonality may display Content correlations of varying degrees of vagueness. What is clear, however, is that such Content correlations have little, if any, formal status, and that the drive to give any uniform source, of any kind, to all instantiations of *round* is fundamentally, and crucially, phonological. If, indeed, there is a root √ROUND which is embedded in all occurrences of /$_{\pi}$*round*/ and a root √KTB, or even √PQD which underlies all the relevant occurrences in (6), at the very minimum it **must** contain phonological information. It is of course possible that it contains more, but I submit that had the phonological identity not been there, the question wouldn't have even arisen. This is, in fact, exactly the conclusion reached by Aronoff (1976), and which has led him to challenge, altogether, the notion that "morphemes", the basic building blocks of morphology, are, as traditionally defined, the smallest sound–meaning pairs. I return to some of his specific arguments in Chapters 6 and 8. Here, I will assume that this is precisely correct for roots, but that functors, including affixes, display a different set of properties, to which I turn shortly.

In Borer (2005a, b), I propose that roots are effectively an indexed place holder, and that the index, specifically, is phonological. Sharpening this claim somewhat, I will assume that an array of (pure) phonological indices is made available for merge, and that what we call "root" is an instance of such merger, licit precisely where no formal information, syntactic or semantic, is otherwise required. The notation $^{\pi}\sqrt{\text{CAT}}$ thus now should be taken to mean a phonological index that may be available at merge, and which, under certain circumstances, would spell out as /$_{\pi}$*cat*/. Seeking to make more specific what a phonological index is, I take it to refer to a packet of root-related phonological information. Such a packet can be trivial, of course, as indeed it would be in the case of $^{\pi}\sqrt{\text{CAT}}$, where, to the best of my knowledge, realizations are exactly restricted to /$_{\pi}$*cat*/. In other cases, however, the realization information may be quite rich. More concretely, I assume such information to be specific enough to exclude the possibility that suppletive forms such as e.g. /$_{\pi}$*go*/ and /$_{\pi}$*went*/, or /$_{\pi}$*die*/ and /$_{\pi}$*kill*/, are realizations of an identical root (and see Chapter 8 for discussion). It would, of course, also exclude the existence of QPD or PQR as alternative instantiations, in Semitic, of the root $^{\pi}\sqrt{\text{PQD}}$. The phonological information nonetheless is abstract enough to allow a single root to spell out as /$_{\pi}$*catch*/ or /$_{\pi}$*caugh(t)*/ in well-specified contexts, or as /$_{\pi}$*sing*/, /$_{\pi}$*sang*/, /$_{\pi}$*sung*/, /$_{\pi}$*song*/ should the context require the spelling out of such distinctions. Even more specifically, the index refers to information about phonological selection which a root may exercise in some well-defined local domains. Thus the fact that the affix N[V] may spell out as /$_{\pi}$*ment*/ or /$_{\pi}$*ance*/ but not as /$_{\pi}$*al*/ in the context of $^{\pi}\sqrt{\text{GOVERN}}$ emerges as a local phonological selection property of the root, a matter I return to in some detail in Chapters 6–8. Crucially, however, I will assume that the phonological indices under consideration, "roots", are not associated, as such, with any Content, a matter I discuss in detail in Chapter 9. Even more crucially, roots are devoid of any

syntactic or formal-semantic properties, which I take to mean that they are devoid of any markers or properties which translate into a rigid designation, the latter by necessity, in XS, a piece of structure—no category, overtly or covertly marked, no inflection, covertly or overtly marked, no quantificational properties, and no operator-like properties of any kind.[28] Given the architecture of the grammar, the inventory of positions in which such objects, otherwise devoid of syntactic or semantic properties, may merge and still result in a converging derivation is limited, and so, de facto, this would serve to delimit the range of not only classical syntactic structures, but also traditional combinatorial WF operations, a matter I return to in Chapter 6, section 3.

One might wonder about the source of the relevant inventory of phonological representations such that they may be roots and specifically, whether the relevant inventory emerges from a fixed listed pool. The answer, however, is that while packets of phonological information may form a cluster, and thus would be listed, such listing is not necessary, and the "inventory" of potential roots consists, in actuality, of all phonological strings that could give rise to phonological well-formedness. In principle, then, a syllable such as /$_{\pi}$*ba*/ or for that matter the syllabic string /$_{\pi}$*bábabàga*/ may merge as a root if once associated with a category and structure, the output is well-formed phonologically (and thus *a ba*, *a pretty ba*, *bas*, *to ba*, *baing*, *baify*, as well as *a (pretty) bábabàga*, *having bábabàgad*, *bábabagàtion*, etc.). Any resulting anomaly would not be associated with the merger of *ba* or *bábabàga*, or from any of the resulting syntactic or phonological properties. As *Jabberwocky* tells us, the anomaly wouldn't even be semantic, as such, insofar as an expression such as *every bábabàga merged with some ba* has fixed rigid semantics, as does the expression *I wanted to bábabàga the car, but it didn't ba out*. The anomaly, rather, would emerge from the fact that none of the possible domains in which /$_{\pi}$*ba*/ or /$_{\pi}$*bábabàga*/ are contained would return any Content. The only difference, then, between *baify* and *nation* would not reside in the properties of the root, in both cases never corresponding to Content as such, but from the fact that *nation* does correspond to Content, but not so *baify*. I return to these matters in some detail in Chapters 8 and 9. I note now, in anticipation of the discussion in the rest of this book that insofar as it would emerge as an advantage to derive all categorial and Content occurrences of e.g. /$_{\pi}$*round*/ or /$_{\pi}$*siren*/ from the same root, and insofar as $^{\pi}\sqrt{\text{SIREN}}$ or $^{\pi}\sqrt{\text{ROUND}}$ may turn out to be no more than packets of phonological information, one of the fundamental tasks of this work would be to convince the reader that what is, in actuality, no more than a convention intended to preserve phonological faithfulness could be converted into a meaningful building block in accounting for the properties of words.

Subsequent to Borer (2005a, b), and specifically within approaches that subscribe to a syntactic approach to complex words, a number of accounts have emerged which divorce Content from roots altogether.[29] Thus Acquaviva (2008b) proposes that

[28] Thus note that although agreement, e.g. as on an adjective, conveys neither Content nor formal semantics, it is triggered in the context of particular syntactic categories, and hence is a marker of such a category and may not be associated with roots.

[29] Within Distributed Morphology, roots do have Content, minimally consisting of selecting an internal argument, and at times considerably more. I return to this matter in Chapter 8.

roots are, indeed, indices, but not phonological ones (and see also Harley 2009c). His roots, then, retain some constancy across their occurrences, but that constancy cannot be described in either phonological or Content terms. An even more radical proposal is put forth by De Belder and van Craenenbroeck (2011), according to which roots correspond to a radically empty structural position. Finally, Ramchand (2008) proposes that roots, altogether, are structurally superfluous and that both Content and what I have referred to as P-RaD correspond to structural configurations which are, so to speak, "headless" in the relevant sense. I return to a fuller review of these proposals in Chapter 8. Fundamentally, I will argue, the reason for a phonological index stems from the need to maintain specifically phonological faithfulness. It is against that criterion that I will evaluate other proposals, to reach what I believe is the empirically and formally optimal formulation.[30]

1.5 Functors, Preliminary

1.5.1 *Two kinds of functors*

Alongside roots and distinct from them, I assume the existence of a finite, UG-defined list of functors, where a functor defines a rigidly designating function, and where by rigid designation I mean a function whose value is constant in all possible worlds.[31] Within the set of rigidly designating functions we find, for instance, determiners, tense markers, plural marking and classifiers (the latter by assumption a count function), auxiliaries, quantifiers, cardinals, aspectual markers including aspectual prefixes and some particles, modals, complementizers, negation, evidentials, switch reference markers, and pronouns and so on. I further assume this class to include categorial derivational affixes eventually to be realized as *-tion*, *-able*, *en-*, or *-ship*.[32]

Functors, I suggest, fall into two very distinct types along at least syntactic and semantic lines, and potentially phonological lines as well, although the latter is in all likelihood a consequence rather than a basic property. Members of the class which includes, e.g., determiners or past tense, and which are typically assumed to be linked with Extended Projections, correspond to some semantic formulas and merge, syntactically, as modifiers. I will refer to them as S-functors. An S-functor is a relationship between some semantic range and a syntactic position with an open value where the semantic range is realized. THREE, to illustrate, is an S-functor that assigns range to, or values, a syntactic position (call it #), a relationship that will be notated as THREE$^{\#}$. Insofar as THREE$^{\#}$ and FOUR$^{\#}$ are semantically related, such

[30] It is perhaps worth noting here that the existence, or lack thereof, of a phonological index as a faithfulness-ensuring device is independent of the question of early or late insertion. Certainly, an early merger of a phonological index ensures faithfulness, but such faithfulness can be likewise achieved if an identical phonological form is associated with all occurrences of the same root index, however otherwise linked and even if such association is late. For more discussion of this point see Chapter 8.

[31] In thus defining functors I follow an informal suggestion in Gajewski (2010).

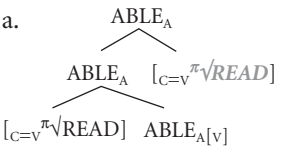
[32] Non-categorial derivational affixes, notably English prefixes, are a harder class to characterize, and, like prepositions, are in all likelihood a mixed bag. Some characterizations and some open questions concerning prefixes are in section 6 of Chapter 7.

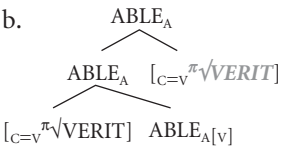
relatedness is mediated through the fact that they are (effectively) operators ranging over the same type of open value.

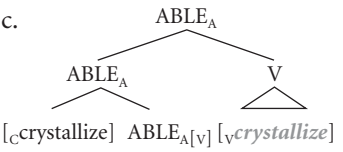
In contrast, functors which ultimately spell out as /$_{\pi}$*ation*/ or/$_{\pi}$*able*/ define primarily a syntactic function whose role is, effectively, to divide the categorial space. It is, thus, a relation between a projecting categorial node, and a particular Categorial Complement Space (CCS), and by Category here I refer to a member of the traditional "lexical" category set N, V, A, and possibly Adv and P. Potentially, some such functors may also have a semantic function (*-able* is a clear case), but as I shall argue, such a semantic function is neither necessary nor sufficient to define this type of functor. I will refer to them as categorial functors, or C-functors. To illustrate, *-able* is a C-functor that projects A and defines its CCS as (equivalent to) V, and is hence an instance of $C_{A[V]}$.[33] We note that the designation $C_{A[V]}$ is not unique, and an identical designation characterizes, e.g., *-ive*. Likewise, a representation such as $C_{N[V]}$ would characterize, at the very least, *-ing*, *-er*, *-ation*, *-ment*, *-al*, *-ance(y)*, *-ence(y)*, as well as *-age* and *-ure* and possibly others. Concerning such cases, they consist, as I will suggest, of two rather distinct sub-cases. Some represent distinct spellout possibilities for the same function. Such, I will suggest, is the case for *-ation*, *-ment*, *-al*, and *-ance(y)*/*-ence(y)*, as well as possibly *-age*, all of which involve an otherwise semantically bleached function which could be well described as (a pure) $C_{N[V]}$. That is not, however, the case when we consider *-able* and *-ive*, precisely because *-able*, but not *-ive*, comes with a modality interpretation. Insofar as *-ive* does appear bleached of any particular semantic value, we can think of it as a spellout of $C_{A[V]}$. Insofar as *-able* does come with an additional semantic function, we can think of it as $ABLE_{A[V]}$, and with ABLE naming the relevant semantic function. Similarly, of course, $ER_{N[V]}$. In Chapter 4 I will suggest that *-ing*, when a nominalizer, has well-defined aspectual properties not shared by the *-ation and kin* group (henceforth ATK for *-ation and kin*), making it, by the same logic, $ING_{N[V]}$. Importantly, and as I shall show, in contrast with S-functors, C-functors merge and project along conservative lines. A first approximation of the structure of what is eventually to spell out as, e.g., /$_{\pi}$*readable*/ or /$_{\pi}$*crystallizable*/ is in (9). Note that by virtue of having a V-equivalent Categorial Complement Space, CCS, $ABLE_{A[V]}$ will render the roots $^{\pi}\sqrt{\text{READ}}$ or $^{\pi}\sqrt{\text{VERIT}}$, otherwise by assumption devoid of category, V-equivalent (C=V), and that the same would be the case for *crystallize*. That the latter is already V (by virtue of the presence of *ize*, itself a spellout of $C_{V[N]}$) is, we note, of little consequence,

[33] In earlier versions of this system, S-functors are referred to as F-functors while C-functors are referred to as L-functors. While the previous notation certainly corresponds to commonly used labels (F for functional categories, L for "lexical" categories), I opted, at the risk of confusion, to relabel the F/L distinction, which under the present execution is at best a misnomer, and rather invoke the C- vs. S-selection distinction, originally due to Grimshaw (1979), as it seems to correspond considerably more accurately to the actual properties of the functions under consideration. A valid question concerns the division of labor here, and specifically, why should it be the case that S-functors correspond to the non-terminal nodes of Extended Projections, while C-functors should reside at the "bottom" of such Extended Projections. The matter is taken up in section 3 of Chapter 6.

as V is, trivially, V-equivalent (and see section 4 of Chapter 6 on linearization as well as on the adjunction structure in (9). Shaded italics for silent copies):

(9) a. 

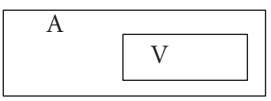
b. 

c. 

In Chapter 6, I return to an in-depth discussion of the syntactic, semantic, and phonological properties of C-functors; indeed, to a detailed justification of their independent existence.

1.5.2 *Category labels—a clarification*

It is worthwhile pausing briefly to elucidate the formal nature of categories, clearly a fundamental task given the claim that it cannot be taken for granted that terminals, as such, come with (lexical) categories. From this perspective, consider again C-functors, by assumption encoding a relationship between some projecting "lexical" category label, N, V, A, or possibly P and Adv as well, and its Categorial Complement Space (CCS). Suppose we pursue the spatial analogy, whereby C-functors divide the categorial space. ABLE$_{A[V]}$, then, creates a categorial space as in (10), where the V-space is fully contained within the A space, and where, by assumption, such containment corresponds to the hierarchical configurations in which following the merger of A and its complement constituent, it is A that projects:

(10) 

If we take the relevant case under consideration to be that of e.g. [$_A$[$_V$realiz] able], the categorial labeling as well as the categorial function of C-functors appear clear and consistent enough. Consider, however, the configuration [$_{C=V}$ $^{\pi}\sqrt{}$READ] in the structure in (9), by assumption a case of V-equivalence. What, exactly, is the notion of V-equivalence, such that it applies to [$_{C=V}$ $^{\pi}\sqrt{}$READ], and such that it could be made to be one and the same as the categorial spatial division in (10), given the fact that the V-ness of *read* appears, at least prima facie, to be of a very different nature than that of e.g. C$_{V[N]}$ when spelled out as /$_{\pi}$*ize*/, as in *realize*?

Viewed differently, consistency emerges precisely from the assumption that [$_{C=V}$ $^{\pi}\sqrt{}$READ] is not V, but rather, it is V-**equivalent**. Specifically, we note that the

V-ness of the CCS of $ABLE_{A[V]}$ should, and typically could, be established on the basis of its availability to merge with something which already has clear verbal properties such as *realize* or *classify*, constituents which are verbal by virtue of being headed by an instance of $C_{V[N]}$ or $C_{V[A]}$ realized as /$_{\pi}$*ize, ify*/. From this perspective, then, $^{\pi}\sqrt{\text{READ}}$ is V-equivalent in (9) not by virtue of having **become** V but by virtue of having come to occupy a syntactic V-space, and by virtue of such a syntactic V-space having particular well-defined properties. Computationally speaking, the inner box in (10), the CCS, defines **an equivalence class**: in the context of e.g. $ABLE_{A[N]}$, *classify*, *realize*, and $^{\pi}\sqrt{\text{READ}}$ are V-equivalent. There is, then, a single notion of N, V, or A in this system, as defined exclusively by C-functors. What the system does allow for beyond explicit category labels, however, is the emergence, in structurally well-defined contexts, of equivalence classes, the latter specifically defined as the complement space of otherwise projecting categorial labels. For otherwise category-less roots, such categorial equivalence is the sole way in which they come to "have" a category, be it V, as in *readable* or *to read*, or N as in *the good read*.

Yet another central issue of labeling is in need of sharpening. In the vocabulary of present-day syntactic approaches, the term "category" is in reference to a label of a particular sort which identifies a constituent. By common assumption, categories can be divided into two rather distinct pools. Those in (11a), typically referred to as "lexical", and those in (11b) typically referred to as "functional":

(11) a. N, V, A, Adv, P
b. D, T, ASP, #, . . .

The reservoir of "lexical" categorial labels is thus named because, typically, any N label includes some lexical element which, by common wisdom, is N. Notwithstanding the validity of this common wisdom, it is reasonably well established that there do exist grammatical constituents, however headed and categorized, which we can usefully refer to as NP, AP, and so on. While the existence of functional elements such as determiners or tense markers is certainly well established and not much in dispute, what continues to be in dispute is the claim that any or all such elements are heads (in whatever sense) which project and give rise to a discrete maximal projection. The controversy, as it stands, reflects several factors. "Lexical" categories constitute an extremely small set (N, V, A, and possibly, but not clearly, some instances of P and Adv) which define constituents with reasonably clear—and distinct—distribution. By contrast, and according to some executions, functional projection lines may consist of independently projecting features, each with its own label, of which there may be anywhere between three and thirty in any one projection line. And yet, the sense in which each of these presumed constituents can be distinguished from those directly embedded within it or directly dominating it is not always clear. What, for instance, is the evidence for the existence of #P (QuantityP) within DP, given the fact that both, perforce, dominate some projection of N and have the distribution of what we may broadly call a referring nominal? What, specifically, would go **structurally**

wrong, if grammatical aspect (G-ASP) is not an independently projecting node, but a modifier of T?[34]

The matter, up to a point, is theory internal. In the execution to be adopted here (see below as well as Chapter 6, section 3), some functional labels are assumed, although, as should become clear from the ensuing discussion, their "categorial" properties, such as exist, are entirely derivative from their semantic function. Even more importantly, and as will be outlined in detail in Chapter 6, section 3, the fundamental property of functional structure, so called, is the fact that functional nodes constitute segments of Extended Projections—ExP-segments. As such, they are, in actuality, not entirely categorially independent of each other, nor, arguably, are they independent of whatever lexical category may reside at their core, their C-core (see below). It thus emerges that there is, and there needs to be, a fundamental distinction between the type of labeling associated with "C" elements, and the type of labeling associated with segments of Extended Projections. A labeled string such as (12) for, e.g., *the three classifications*, in other words, is up to a point guilty of obscuring the formal distinction between labels such as N and V, on the one hand, and labels such as D and #, on the other:

(12) $[_{D}$ D $[_{\#}$ # $[_{CL}$ DIV$[_{N}$ $[_{V}$ $[^{\pi}\sqrt{}$CLASS$]$ $C_{V[N]}]$ $C_{N[V]}$ $]$
the three -s class ify (ca)tion

While a formally accurate notation which represents the distinction would be extremely useful, expositional ease militates against such a change, and hence it has not been attempted. As should become clear from the next few subsections and the discussion in Chapter 6, however, the distinction espoused in this work between the set of "lexical" labels in (11a) and the set of "functional" labels in (11b) is formal and substantial.

1.5.3 *S-functors as range assignors*

The properties of S-functors are discussed at some length in Borer (2005a, b), where their role is pivotal in the emergence of (so-called) functional structure. Some aspects of that discussion are summarized below, although the alert reader would no doubt spot some changes in both substance and execution.

[34] To illustrate, suppose we assume, as seems plausible, that there is a semantic dependence between Tense and grammatical aspect (G-ASP). Suppose we even grant that there exists a relevant semantic overriding consideration which forces Tense to scope over G-ASP rather than the other way around, and hence for Tense to dominate G-ASP (although, note, c-command direction is already an independent stipulation on how semantic scope translates into syntactic architecture). Even so, there are at least three different syntactic ways to represent that dependency while maintaining the typologically valid observation that when T and G-ASP co-occur, the latter is dependent on the former. One would be to assume that G-ASP is a semantic complement of Tense. The second would be to merge G-ASP as a specifier of Tense (and with T possibly raising to a higher position). The third, finally, would consist of a relationship between the head of the Tense Phrase, T^{min}, and the head of the G-ASP Phrase, G-ASP^{min}, however stated, through head to head movement, feature matching, Agree, or what have you, but specifically no complementation relationship as such.

Taking functional structure, as typically understood, as our starting point, and observing that functional nodes are always ExP-segments, we note as our starting point that the grammar as it now stands gives us no way to determine the specific internal architecture or the relative order of such ExP-segments. Crucially, notions such as head, complement, and specifier were developed to describe properties of (lexical) categories, capturing what were assumed to be lexically specified or lexical-semantically meaningful relationships, such as selected arguments, external arguments, and so on, and hence they do not carry over in a straightforward way to Extended Projections, nor is there any particular reason why we should assume them to be meaningful in that domain (and see fn. 34 for a relevant illustration). On the other hand, the existence of at least some structure within Extended Projections as well as the fixed hierarchical ordering of at least some ExP-segments with respect to each other can be empirically substantiated. Suppose we assume, then, that at least some properties of Extended Projections are inevitable and universal. Suppose we further assume that at least one such property which Extended Projections share with C-functors is transitivity. More specifically, and taking an Extended Projection to be the maximal set of consecutively merging ExP-segments, that set defines a Categorial Complement Space, where by Category, again, we refer to a "lexical" inventory. It is, in fact, precisely this transitivity that is responsible for the obligatoriness (and the uniqueness) of a "bottom", so-to-speak, to any Extended Projection that consists of a "lexical" categorial label, as the term is typically construed, or, in the terminology used here, a core consisting of some C-labeled item, call it C-core, and where such a C-core is fundamentally formally distinct from ExP-segments.[35]

I will further assume that ExP-segments (excluding the C-core) fundamentally correspond to semantic functions, and that their availability as well as their order relative to each other within each Extended Projection is universally fixed. Differently put, insofar as we can substantiate the existence, in any grammar, of $[_{D}\ [_{\#}\ [_{CL}\ [_{N}$, then the ExP-segments D, #, CL are universally available in **every** grammar as part of the inventory of ExP-segments which select, or effectively define, a nominal C-core. Furthermore, their merge order relative to each other is fixed (i.e. D must merge

[35] (i) *Extended Projection*$_{def}$:

a. There must be a unique C-core such that it is dominated by all segments of the Extended Projection (ExP-segments);
b. The relative order of merger of ExP-segments within any Extended Projection is universally specified;
c. Subject to A, every ExP-segment is optional, but its presence/absence may have interpretational consequences.

(ii) *C-core*$_{def}$:

a. α is a C-core iff α is C-equivalent and there is a β such that β is contained in α and β is intransitive, and for all x, α dominates x and x dominates β, x is C-equivalent;
b. α is maximal iff there is no γ such that γ is C-core and γ immediately dominates α (and where C stands for the traditional inventory of 'lexical' categories. Note that trivially, all instances of C are C-equivalent).

See Chapter 6, section 3 for a more complete discussion of Extended Projections.

above # and # above CL).[36] The reader should note that at least some of the fixed properties of Extended Projections assumed here should be viewed primarily as heuristic devices. This is to say that such properties are not necessarily axiomatic or innate as such—ultimately, it may very well turn out to be the case that they could be derived from grammatical or, for that matter, extra-grammatical principles. Once such principles are in place, it may also turn out to be the case that they may vary across grammars, or even internal to any one particular grammar. Pending the discovery of such entailments, however, it appears heuristically preferable to simply fix at least some of the properties of Extended Projections as we descriptively know them to be the case, and to proceed to investigate other properties on that basis.

Consider now the following paradigm:

(13) a. During the summer, water in the pond mostly evaporates.
b. Hummingbirds always die young.

(14) a. Water in the pond is mostly lost through evaporation.
b. Hummingbirds always drink from our birdfeeder.

The examples are familiar and represent cases of so-called unselective binding (see Lewis 1975, Heim 1982, and Doetjes 1997, among others for some relevant discussion). Of particular importance from the perspective here is the well-known fact that the adverbs of quantification in (13)–(14) may range either over the interpretation of the event (a reading most salient in (14)) or over the interpretation of the subject alone. Even more crucial, from our perspective, is the fact that the readings are mutually exclusive. Setting aside the specific reasons for the difference in the preferred reading for (13)–(14), we note that even if it were plausible that all hummingbirds in the world drink from our birdfeeder, under that reading it wouldn't necessarily imply that they are constantly doing so. Likewise, (14a) cannot mean that most water is mostly lost. Finally, under the nominal reading, the DP under consideration cannot include any other quantifier. Examples in (15), under the relevant reading, are ungrammatical (i.e. the adverb of quantification can only range over the event):

(15) a. Most/all hummingbirds always die.
b. Most/all water in the pond mostly evaporates.

It thus emerges that adverbs of quantification, when associated with a nominal expression, are in complementary distribution with DP-internal quantification. In the discussion of this paradigm in Borer (2005a), I concluded that such complementary distribution emerges from the fact that under the relevant reading, the adverb of quantification values, or assigns range, in some syntactically and semantically well-defined sense, to some functional structure within the DP, a relationship,

[36] And see Borer and Rohrbacher (2003) for some learnability considerations in this context. Note that I specifically do not assume that every single universally available ExP-segment is always present. Quite to the contrary, I assume, as in Borer (2005a, b), that ExP-segments merge optionally, but their merger, or failure to do so, has deterministic semantic consequences, including, potentially, underspecification.

I assume, which falls under some version of syntactic binding. It is precisely because the adverb assigns range to some otherwise unspecified value within the nominal, that a DP-internal quantifier may not do so and is hence obligatorily excluded, under the assumption that these would constitute cases of vacuous quantification. Clearly, a DP-internal quantifier and an adverb of quantification are not in structural competition concerning their specific merger site—one is internal to the DP and the other clearly external to it; nor do the two clash when the adverb modifies the event. Where the competition does occur, however, is relative to the presence of a single value in need of being provided. If it is bound by the adverb, a DP internal quantifier may not occur. If it is bound by a DP internal operator, the adverb must seek to assign range elsewhere, i.e. within the domain of the event. From this perspective, however, it emerges that the head which is interpreted as *most* within the DP in either (15b) or (13a), say the # head, is best viewed as an open value, and its binder is best viewed as merging elsewhere. This picture thus contrasts with one in which *most* would itself merge within the DP and project # directly. It further suggests that # (for quantity) is not a singleton, but a pair consisting of a binder and a bindee.

An illustration of the same point is available from cases such as (16):

(16) a. the dog's ear
b. a dog's ear

As is well known, (16a) is a definite description, but (16b) is not. Furthermore, as is equally well known, an additional article is barred in (17) (note that all possible combinations of definite and indefinite articles are blocked):

(17) a. *[the/a dog's] the/an ear
b. *the/a [the/a dog's ear]

How, specifically, does *the dog's ear* become a definite description, with properties identical, for all intents and purposes, to those of *the ear*? Under most current accounts, *the dog* or *a dog* occupy some specifier within the DP structure. Opinions may vary regarding where *'s* is, but we note that whatever the properties of *'s* and regardless of where it lives, it certainly cannot be responsible for rendering (16a) definite or (16b) indefinite. Rather, it appears we have here a case of the (in)definiteness of the possessor translated to that of the head, corresponding directly to the non-availability of any direct marking of definiteness for the head itself. Yet again, this picture receives a natural explanation if we assume that the (in)definiteness value of the specifier perforce binds some head within the nominal Extended Projection, and that in this manner, it transfers to that head its (in)definiteness value, resulting in the appearance of agreement in (in)definiteness (cf. 16a) as well as the impossibility of overt articles. Yet again, the picture suggests that ExP-segments are headed by open values, and that cases such as (17) are ruled out due to the presence of two range assignors competing for a single open value to bind.[37]

[37] Importantly, note, this case also shows that the relationship of range assignment is not that of logical variable binding. If we take THE to be a discourse anaphor, then it is clear that when dealing with *the dog's*

The concrete formulation of this proposal in Borer (2005a, b) postulates heads of ExP-segments such as # and D as open values with a specific categorial label. The open values are in turn bound by S-functors which assign range to them, which translates into the statement that the range of the relevant functor is delimited by the specific syntactic position it binds. Suppose we consider the open value in cases such as (13)–(14) to be that which projects as #P, notating such an open value as $\ll e\gg_{\#}$. *Most*, as well as *mostly*, in turn, define a rigidly designating S-function which assigns range to $\ll e\gg_{\#}$, and therefore MOST$^{\#}$, MOST(ly)$^{\#}$:[38]

(18) a. $[_{\#} \quad \ll e\gg_{\#} \ldots \quad [_{C=N} \quad] \,]$

b. $[_{\#} \quad \text{MOST}^{\#} \quad \ll e^{\text{MOST}}\gg_{\#} \quad [_{C=N} \quad] \,]$

c. $\text{MOST(ly)}^{\#} \quad [_{\#} \quad \ll e^{\text{MOST}}\gg_{\#} \quad [_{C=N} \quad] \,]$

Note now that insofar as the S-functions MOST and MOST(ly) are specified to assign range to #, the categorial specification of # on the open value appears redundant and we are free to assume that it is inherited from the range assignor. In that case, what in actuality is part of the numeration and which merges and eventually projects is an empty set, which is licensed and effectively categorized by an S-functor (see Chapter 6 for discussion).

The properties of *the dog's ear* vs. simply *the ear* or *the dog* can be accounted for in similar terms, as illustrated by (19). To be explicit, we note that the possessor DP here is not a functor as such, and its ability to bind the open value of its head stems from some form of specifier-head agreement, however stated (see fn. 37). It is thus the definiteness value or feature of the entire DP in the specifier that is effectively copied onto the $\ll e\gg_{D}$ of the head:

(19) $[_{D-1}[_{D-2} \quad \text{THE}_{\,D} \ll e^{\text{THE}} \gg_{D} \; [_{C=N2}(\text{dog's})] \,] \; \ll e^{\text{DEF}}\gg_{D} \; [_{C=N1}(\text{ear}) \,]]$

Note now that the notion of "head", as it emerges from the previous discussion, deviates architecturally from what is typically assumed by canonical phrase structural accounts. Specifically, in e.g. (18b) or (19), the syntactic head of # and D respectively—the element that projects—is the open value, an empty set. Viewed differently, however, the duality of the open value and its range assignor amounts to separating the syntactic portion of the structure from its semantic function, allowing us to generalize over occurrences of the former despite distinct semantic contexts. Within

ear, what is being copied onto *ear* is not the discourse antecedent of *the dog*. Rather, it is the formal properties of THE/DEF, thus enabling it to seek its own, separate, discourse antecedent.

The argument can, in actuality, be made considerably stronger if one accepts, as in Borer (2005a), that indefiniteness is but the failure of definiteness, and that the indefinite expression *a dog's* binds # but not D. As *the* must merge in #, the binding of the open range in # by *a dog* blocks the merger of *the* in spite of the fact that the semantic value of the two is different. The reader is referred to Borer (2005a) for a fuller discussion.

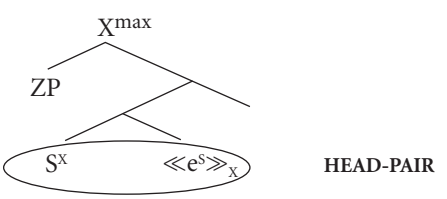
[38] In seeking to avoid confusion with the notation of Type Theory, the notation $\ll e\gg$ replaces the <e> in Borer (2005a, b).

the system as so far outlined, it is the open value that projects, giving rise to a constituent, and it is the open value that constitutes an ExP-segment. By this logic, $\ll e \gg_{\#}$, $\ll e \gg_{D}$, $\ll e \gg_{T}$ are pieces of structure, and MOST, EVERY, THE, MAY are semantic functions that are responsible for the interpretation of that structure. In this system, what, e.g., MOST and EVERY have in common is that both can assign range to an open value that is (or that would thus become) $\ll e \gg_{\#}$. What MAY and MUST have in common is, likewise, the fact that they assign range to the same open value and so on. In and of itself, however, $MOST^{\#}$ is not # nor is $WILL^{T}$ an instance of T. Indeed, they have no syntactic category at all.

MOSTLY is an adverb, and [the dog] in (19) is a phrase in a specifier position. Where, however, do *the* and *most* merge? An attractive possibility would be to claim that they are specifiers, all the more so since range assignment from specifiers is explicitly assumed. However, even though for both *the* and *most* this is a viable solution, in other contexts it is entirely clear that (overt) range assignment must be allowed to co-exist with otherwise filled specifiers. In lieu of postulating two specifiers and restricting the distribution of S-functors to the innermost one, and in line with the account in Borer (2005a), suppose THE, MOST, and similar S-functors merge directly with the open value which they assign range to as modifiers: non-projecting instances of *min*/*max*.

Supposing this to be on the right track, heads of ExP-segments emerge as possible **pairs** in which one member projects and provides an open value, while the other provides the range assigned to that value. As expected, it is precisely the latter one which is optional, insofar as range assignment can come, as we saw, from an adverb or a specifier. We can thus assume the structure in (20), for some $\ll e \gg$ with S^{X} as its range assignor, and with the circled portion of the structure to be referred to henceforth as "head-pair". Following range assignment, the open value acquires the value S (by assumption the semantic range S). It also acquires a categorial label (X) which, in this context, we will take to be a syntactic way of encoding the fundamentally semantic common denominator of the array of S-functors that may assign range to it, all functors of the type S^{X} (e.g. all quantifiers, and hence $S^{\#}$, all instances of tense and hence S^{T} and so on):

(20)



1.5.4 *S-functors and phonological indices*

As it turns out, THE and MOST, as well as other cases mentioned thus far, have at least one property which does not necessarily carry over to all S-functors. Both THE and MOST—as is also the case for English cardinals and quantifiers, for modals, etc.—are associated with a unique phonological realization. Importantly, they do

not display the sort of phonologically erratic behavior exhibited by, e.g., past tense or plural, and their phonological realization is entirely immune to any neighborhood effects. We would thus be justified in assuming that they spell out in isolation from their context, and we may thus assume that they come with their own index which regulates their realization possibilities.[39]

However, this is by no means the case for all potential range assignors, nor is this in any way a universal situation. Rather, if we take, e.g., English past tense, or English plural marking to be range assignors and hence S-functors, their phonological realization, as already noted, is not only not unique, but is also highly contingent on their immediate environment. In the context of *mouse* (however represented), plural marking ends up as /$_{\pi}$*mice*/, in the context of *child* as /$_{\pi}$*children*/, and in the context of *girl* as /$_{\pi}$*girls*/. We already noted in section 1.3 that such cases are best viewed as subject to late phonological realization. As is patently clear, there is little advantage in associating them with an independent phonological index, for the simple reason that such a phonological index would either have to be effectively vacuous, or require massive subsequent modification. Rather, and in line with a realizational approach, I will assume that the phonological form /$_{\pi}$*mice*/ is a mono-morphemic realization of [$_{C=N}{}^{\pi}\sqrt{}$MOUSE] in the context of plural marking, and that furthermore, /$_{\pi}$*girls*/ is similarly the mono-morphemic realization of [$_{C=N}{}^{\pi}\sqrt{}$GIRL] in the context of plural marking.

Suppose we assume then that plural and past tense are abstract, in the sense that they do not come with a unique phonological index. In being abstract S-functors, they are distinct along phonological lines from, e.g., THE or CAN. We now note, empirically, that the eventual phonological realization of any marking associated with such an abstract S-functor (henceforth "S-marking") is directly contingent on the availability of some phonologically indexed host, in conjunction with which it could be realized, and that this is true regardless of the specific realization of past as "regular" or "irregular". It is in turn the need for such a phonological host for S-marking, I suggest, which is one of the chief forces, if not the only one, of head movement.[40]

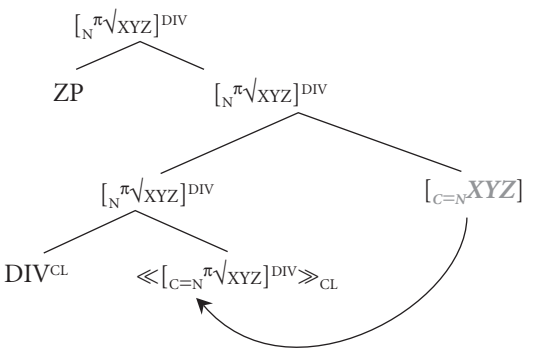
[39] While great phonological stability is attested for quantifiers, determiners, and modals, that is not the case for auxiliaries, raising the possibility that while e.g. realizations such as /$_{\pi}$*may*, *might*, *four*, *every*/ etc. are accessed through phonological indices on a par with roots, this does not apply to e.g. the auxiliary *be* in English, where suppletion is rampant, and where phonological realization would have to be otherwise determined, presumably through whatever late insertion mechanism is also responsible for pairing C_N[v] with multiple phonologically unrelated realizations. For some discussion of suppletion, see Chapter 8. See also Embick and Halle (2005).

[40] We note that it cannot be the case that ExP-segments must be phonologically realized to be licit, and that e.g. no such local phonological realization is available in the case of *mostly* or for possessors such as *the dog's ear*. Even more radically "empty" ExP-segments emerge when we consider cases of zero-realized tense as discussed extensively in Déchaine (1993b) for Chinese as well as for Haitian and a range of African languages. Regarding all such cases, it is rather implausible that an S-functor merges with the head. Alternatively, these are cases where range is assigned to an open value through the mediation of a discourse antecedent, and with the latter functioning, in this respect, exactly like an adverb of quantification. A head-pair, then, is never formed. If a discourse antecedent is at work here as is typically assumed, and insofar as it is clearly necessary for T to be bound by some sentence-external element, it provides independent evidence for the availability of empty heads for functional structure which, in turn, can be licensed from without.

(*cont.*)

Consider, in view of this, the representation of plural-marked derivations in English, depicted in (21), and assuming plural marking to be the spellout of the S-functor DIV, assigning (non-singular) range to a nominal ExP-segment, thereby turning it, effectively, into CL (in the sense of Borer 2005a). As should be clear from the structure in (21), and given the abstract nature of DIV^{CL}, there is little to prevent $[_{C=N}$*xyz*] (effectively N^{min}) from (internally) re-merging with N^{max}. By common assumptions, we note, such re-merger of the head would only be licit if the head retains its categorial status, as well as its projection status (see Ackema, Neeleman, and Weerman 1993, as well as Georgi and Müller 2010). Even more strikingly, however, nothing would prevent the re-merged head from accepting whatever S-marking is associated with the abstract S-functor, giving rise to the emergence of a constituent which projects as N, but which is range marked, specifically, as N^{DIV}:[41]

(21)



The output of such movement, for an array of both roots and non-roots, is in (22):

(22) a. $[_{N\text{-}DIV}$ DIV^{CL} $\ll[_{C=N}\ ^{\pi}\sqrt{\text{TOOTH}}]^{DIV}\gg_{CL}$ $[_{C=N}\ ^{\pi}\sqrt{\text{TOOTH}}]\ldots]$

b. $[_{N\text{-}DIV}$ DIV^{CL} $\ll[_{C=N}\ ^{\pi}\sqrt{\text{WOMAN}}]^{DIV}\gg_{CL}$ $[_{C=N}\ ^{\pi}\sqrt{\text{WOMAN}}]\ldots]$

c. $[_{N\text{-}DIV}$ DIV^{CL} $\ll[_{C=N}\ ^{\pi}\sqrt{\text{BIRD}}]^{DIV}\gg_{CL}$ $[_{C=N}\ ^{\pi}\sqrt{\text{BIRD}}]\ldots]$

d. $[_{N\text{-}DIV}$ DIV^{CL} $\ll[_{N}$ indictment$]^{DIV}\gg_{CL}$ $[_{N}$ *indictment*$]\ldots]$

It thus emerges that postulating empty heads for ExP-segments across the board allows us to considerably narrow the grammatical gap between "hot" and "cool" languages, to use the terminology originally proposed in Huang (1984) in the context of the discourse-licensing of grammatical structures.

We note, likewise, that insofar as S-functors may either be abstract and thereby trigger movement, or correspond to phonological indices, where movement is not required, this is an aspect of grammatical variation, both inter- and intra-language, which is reducible not only to the properties of functional vocabulary, but to the phonological properties of functional vocabulary (see concluding comments in Chapter 13 for some discussion). Contrary to appearances, however, and while it remains the case that in the absence of movement the derivation will crash for phonological reasons, the movement in itself is not assumed to be phonological. See Chapter 6 for some additional discussion.

[41] Some aspects of the derivation are glossed over for ease of exposition. Specifically, I assume that the head re-merges and that the relevant S-functor adjoins to it subsequent to such movement, creating the adjunction structure in (21), where it is the head, rather than the S-functor, which proceeds to project. ZP, if present, merges subsequent to that.

The result of the movement, as is clear, provides us with a constituent which is directly marked by the relevant range. A root embedded within such N is a phonological index, by assumption a packet of information concerning the realization of the root in distinct contexts. It is thus precisely there that information could be located indicating that $[^{\pi}\sqrt{\text{TOOTH}}]^{\text{DIV}}$ would spell out as /$_{\pi}$*teeth*/ and that $[^{\pi}\sqrt{\text{WOMAN}}]^{\text{DIV}}$ would spell out as /$_{\pi}$*women*/. It is likewise exactly in this context and in the absence of any specific information about the spellout of $[^{\pi}\sqrt{\text{BIRD}}]^{\text{DIV}}$ that the default form /$_{\pi}$*birds*/ would emerge. Finally, as root information is not available for [$_{\text{N}}$indictment], being derived, a default realization would emerge as well, hence /$_{\pi}$*indictments*/:

(23) a. $\ll[_{\text{C=N}}{}^{\pi}\sqrt{\text{TOOTH}}]^{\text{DIV}}\gg_{\text{CL}}$ → /$_{\pi}$*teeth*/
b. $\ll[_{\text{C=N}}{}^{\pi}\sqrt{\text{WOMAN}}]^{\text{DIV}}\gg_{\text{CL}}$ → /$_{\pi}$*women*/
c. $\ll[_{\text{C=N}}{}^{\pi}\sqrt{\text{BIRD}}]^{\text{DIV}}\gg_{\text{CL}}$ → /$_{\pi}$*birds*/
d. $\ll[_{\text{N}}\ \text{indictment}]^{\text{DIV}}\gg_{\text{CL}}$ → /$_{\pi}$*indictments*/

The resulting syntactic configurations in (22) and their spellouts in (23) are in turn a specific (alternative) execution of the claim put forth in Anderson (1982, 1992) as well as Beard (1995), according to which formations such as English plural or past tense, and in general inflectional marking of the type typically marked on stems, are not morphemic, but rather represent the phonological realization of particular stems in the context of a particular set of syntactic properties. *Teeth* as well as *birds*, under such an account, are mono-morphemic, where by mono-morphemic what we mean, specifically, is that *teeth* and *birds*, just like *tooth* and *bird*, are the realizations of a single syntactic terminal which is thus in a local relation with its S-marker. As such, then, this account is very distinct from that put forth by, e.g., Halle and Marantz (1993), where PST or PL is a morpheme which attaches to a verb or a noun respectively, and where by assumption any past tense or plural marking on a stem involves, at the least, a tri-morphemic structure consisting of the root, ***v/n***, and a PST/PL morpheme, and hence, schematically:[42]

(24) $[_{\text{PST}}\ [_{\boldsymbol{v}}\ [\ \sqrt{\text{SING}}\ ]\ \boldsymbol{v}\ ]\ \text{PST}\ ]$
$[_{\text{PL}}\ [_{\boldsymbol{n}}\ [\ \sqrt{\text{TOOTH}}\ ]\ \boldsymbol{n}\ ]\ \text{PL}\ ]$

A few final comments might be in order concerning the availability, or lack thereof, of head re-merger. Thus we proposed that an abstract S-functor forces head movement, but have remained silent on whether head movement may occur in the presence of a phonologically specified S-functor. The latter, presumably, would give rise to a morphologically complex form, insofar as it would involve a case of merger between two forms which are otherwise structurally present, each having its own phonological index. Note that although head adjunction is not excluded in XS (cf. (21)) this is not in actuality the structure under consideration here. Rather, the question is whether within a head-pair, the modifying S-functor could be realized,

[42] By way of anticipating a more thorough theoretical discussion in Chapter 6, note that head re-merger, as in (22), is blocked in the presence of intervening C-functors (i.e. *realize* cannot be moved out of *realization*), as C-functors, definitionally, do not bind empty positions and are never ExP-segments.

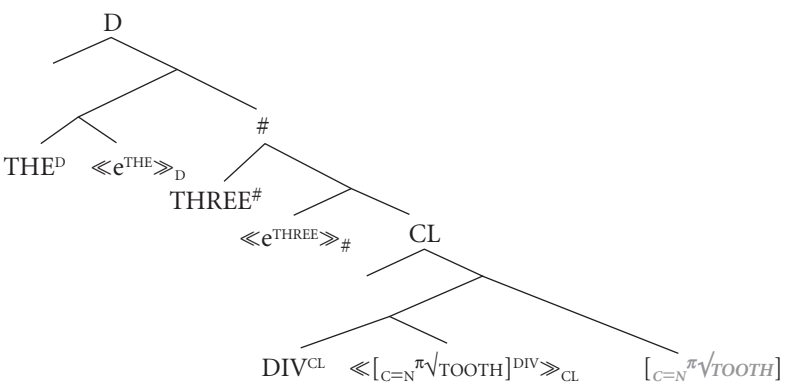
phonologically, independently of some C-core element that has moved to re-merge and re-project as its head-pair mate.

Cases of this nature do not seem to occur frequently. Modals and auxiliaries for example typically pre-empt tense marking, indicating that verbs do not move to T in their presence. Furthermore, stable, discrete bound S-functor morphemes, such that they plausibly have their own phonological index but nonetheless require a lexical base as a host, are rather rare. If head-pairs did systematically allow double phonological realization, such cases would be predicted to occur more frequently, if only as a result of liaison. Ultimately, however, the issue is an empirical one and at least one case, the English progressive, comes to mind where a verbal base does adjoin to a stable potential bound morpheme, *-ing*, and with the latter plausibly signaling some S-functor in conjunction with an aspectual phrase. Under consideration, then, would be the question whether in an expression such as *be dancing*, and assuming $[_{C=V}{}^{\pi}\sqrt{\text{DANCE}}]$ to move, does $[_{C=V}{}^{\pi}\sqrt{\text{DANCE}}]$ move to some otherwise empty head of grammatical aspect (G-ASP), i.e. some instance of ≪e≫, to give rise to the head-pair $<\text{ING}^{\text{PROG}} \ll[_{C=V}{}^{\pi}\sqrt{\text{DANCE}}]^{\text{ING}}\gg_{\text{PROG}}>$. Alternatively, the derivation might involve the assignment of range to some ≪e≫ by ING, and rather than re-project as ING's head-pair mate, $[_{C=V}{}^{\pi}\sqrt{\text{DANCE}}]$ adjoins to ING to give rise to $<[_{C=V}{}^{\pi}\sqrt{\text{DANCE}}]$ $+\text{ING}^{\text{PROG}} \ll e^{\text{ING}}\gg_{\text{PROG}}>$, a position which is not subject to S-marking.

More pertinent to the ensuing discussion, and in particular that of derived nominals in Chapters 3–5, is the status of head movement in cases which involve indirect range assignment, i.e. range assignment by a specifier, as in (19), or by an adverb of quantification, as in (18c), and where a head-pair is altogether absent and where no phonological realization is otherwise associated with the relevant ExP-segment. We note that in such cases there is certainly no impediment to the re-merger of a lower head, precisely because nothing, by assumption, would be otherwise present. Pending reasons to abandon such an assumption, and wishing to make the account the strongest possible, I will assume that all these cases involve a re-merger of the head.

By way of final illustration, (25) is, in broad strokes, an Extended Projection with an N-core:

(25)



D emerges as a result of range assignment by the S-functor THE^{D}, giving rise to a projecting $\ll e^{THE} \gg_{D}$. #, in turn, emerges as a result of range assignment from THREE$^{\#}$, by assumption in Spec,#. CL, finally, involves range assignment by the abstract DIV^{CL}, which requires the movement and re-merger of N. N itself, has emerged as a result of the root containing the root $^{\pi}\sqrt{}$TOOTH rendered N-equivalent, being the CCS of {D, #, CL}. For more discussion of these issues as well as for a more detailed execution, see Chapter 6, sections 2.1 and 3.[43]

1.5.5 *Functors—a brief summary*

To summarize the salient aspects of the functor system proposed here:

A. The functional vocabulary consists of rigidly designating items, each naming a function: functors. The inventory of functions and hence of functors is divided into two formally distinct kinds: S-functors and C-functors. All functors, by virtue of articulating a function, are transitive.
B. S-functors are semantic functions. They assign a semantic range to an open syntactic position. In and of themselves and independently of their merger context, they have no syntactic properties.
C. C-functors are (primarily) syntactic functions. They project a particular category and they define a Categorial Complement Space (CCS). In and of themselves, they need not have a semantic function.
D. In the most standard case, S-functors are modifiers—adjuncts—which do not project and are thus instances of *min*/*max*. They merge with an otherwise open-valued projecting head and assign semantic range to it. The result is a pair, which I have labeled "head-pair".

   An open valued head, however, may be assigned range by items other than its head-pair mate, e.g. by adverbs of quantification or by a specifier. In such cases, the head may not have a mate, and rather, appears to be otherwise null.
E. S-functors may or may not have a unique phonological index. When they do not, head movement is required to allow for the phonological realization of S-marking. The specific form of head movement involves a re-merger and a subsequent assignment of range to the relevant moved head, accompanied by its re-projection effectively as the head of the relevant ExP-segment. As a result, the movement does not give rise to any additional structural complexity.

[43] In allowing the emergence of projecting functional heads within the Extended Projection from an iterative movement of the (head of the) C-core, this system is a direct derivative of Ackema, Neeleman, and Weerman (1993), where it is assumed explicitly that this is the mechanism that drives the formation and the categorial properties of Extended Projections. The account nonetheless differs from Ackema et al.'s insofar as Extended Projections can exist without head movement, and S-functors are adjoined, rather than moved.

We note, finally, that the first instance of merge, in this system, effectively requires the root to merge with itself, and with the higher copy subsequently re-projecting as an additionally labeled ExP-segment (i.e. N-CL).