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REVIEW ARTICLE

Structuring Sense, Vol I: In Name Only. By HAGIT BORER. Oxford: Oxford University Press, 2005. Pp. xvi, 310.

Structuring Sense, Vol II: The Normal Course of Events. By HAGIT BORER. Oxford: Oxford University Press, 2005. Pp. xvi, 400.

Reviewed by CHRISTOPHER POTTS, *UMass Amherst**

1 Introduction

In early transformational grammar, lexical items were essentially lifeless, pushed around by powerful and diverse phrase structure rules and transformations. The balance of power began to shift, though, in the late 1970s. The phrase structure rules became more generalized (Jackendoff 1977; Gazdar *et al.* 1985; Kaplan & Bresnan 1982; Kornai & Pullum 1990), the transformational component shrank (to nothing in some theories), and the lexicon took over as the primary locus of grammatical complexity and theoretical innovation. This trend continues, and it is a significant point of agreement across frameworks and outlooks (Chomsky 1995b; Pollard & Sag 1994; Collins 2003; Bresnan 2001; Joshi & Shabes 1997).¹

In *Structuring Sense*, Volumes I and II, Hagit Borer proposes a reorientation of the lexicalist approaches that have dominated for the past quarter century. In B's hands, the lexicon is once again impoverished. In fact, it is advertised as more impoverished than ever before. The open-class items (listemes)

do not have any formal properties, and are, in this sense, tantamount to raw material, 'stuff' which is poured into the structural mould to be assigned grammatical properties. (Vol. I, p. 108)

The general principles are held to be simple and universal as well. Where, then, is the linguistic complexity? The above quotation begins to get at the answer. While open-class items are devoid of grammatical properties, the functional lexicon is teeming with information-rich, language-specific items. These create the 'structural mould' that determines the full range of grammatical properties.

* My thanks to Ash Asudeh, Seth Cable, Greg Carlson, Tom Roeper, Peggy Speas, and Ida Toivonen for helpful discussion. I am especially grateful to Hagit Borer and Beth Levin for detailed comments on an earlier draft of this review. I alone am fully responsible for the claims and opinions expressed herein.

¹ Semantics theory's history runs in parallel. Early Montague Grammar (Montague 1974) involved many lexically-based rules of semantic composition, but Klein & Sag (1985) moved the field towards lexically-driven (type-driven) theories, which represent the dominant perspective today.

Listemes flesh out these structures with their underlying conceptual content, but such content is held not to be responsible for the important patterns of grammar. This is the big picture idea that guides SS.

Volume I, *In Name Only*, is concerned largely with nominal structure, and Volume II, *The Normal Course of Events*, focuses on verbal projections and the nature and source of aspectual distinctions. (A third volume, presently called *Taking Form*, is in progress.) Though they can be read independently of each other, these two volumes, henceforth referred to collectively as SS, are tightly integrated. Each volume contains the table of contents for the other, they share a basic index (helpfully formatted to distinguish the two books), and there are numerous cross-references between the books, some of which even take the reader to specific footnotes.

SS is, by any measure, a sizable achievement. Since its publication in 2005, it has helped shape framework-level debates in syntax, and its descriptive generalizations (primarily for Hebrew and English, though Italian, Chinese, and Czech play significant supporting roles) have been extensively inspected and debated.

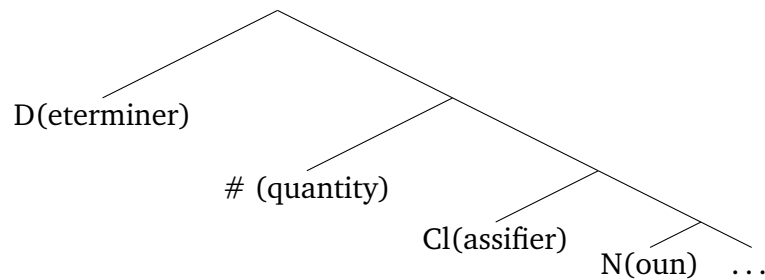
With the present review, I try, first and foremost, to articulate and explore the theoretical underpinnings of SS. The books together weigh in at over 700 pages. This is, in itself, a barrier to understanding and exploring the true nature of the theory they work to define. Thus, the longest section of this review, §2, is devoted to chapter-by-chapter summaries that strive to draw out the major theoretical strands as they develop over the course of the two volumes. I then scrutinize this picture at a higher level (§3) and finally explore what seem to me to be some of its limitations (§4).

2 Overview

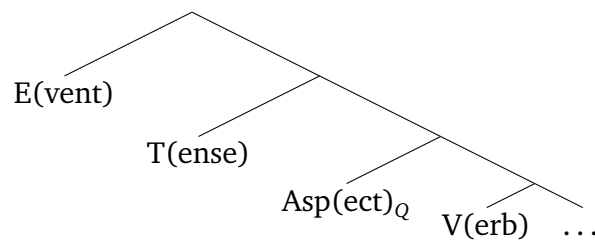
This section moves through all twenty chapters of SS, seeking to distill the major theoretical ideas down to just a few pages. I emphasize major theoretical developments over factual claims and challenges to others' proposals. It is important to assess these aspects of the books (and §4 begins that work), but, to do it effectively, we need to understand the theory itself. Readers wishing to get a sense for the path B takes to this theory, and the tangents pursued along the way, should scan the table of contents, which is quite detailed.

Vol. I focuses on nominal structure, Vol. II on verbal structure. In each domain, the proposed hierarchy of functional projections plays a leading role, so the schematic structures in (1)–(2) are useful to have in mind throughout.

(1) Nominal structure (Vol. I)



(2) Verbal structure (Vol. II)



I've included just the head labels, to abstract away from the details of the version of bare phrase structure (Chomsky 1995a) that determines most of the node labels in SS. I flesh out the above trees as I move through the next twenty subsections.

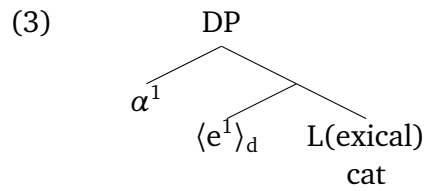
2.1 Vol. I, *In Name Only*

Vol. I, Ch. 1

Ch. 1, 'Structuring sense: Introductory comments', sets the stage, conceptually and theoretically. It opens by describing the quantitative claim that open-class words are fundamentally more polysemous than structures. (I assess this claim more carefully in §3.1 below.) The remaining sections outline B's explanation for this contrast: (i) open-class lexical items (listemes) are unspecified for all grammatical properties; and (ii) it's the job of functional heads, hierarchically arranged in a universal order (p. 24, 28), to specify these features. We're given a sense for the version of bare phrase structure (Chomsky 1995a) assumed throughout the books, and we get a glimpse of some theoretical perspectives that shape the books' development: type shifting is held to be inherently bad (p. 9, 16), not all meaning is compositionally derived (p. 12), and inter-language variation derives from differences in the stock of functional morphemes, rather than in the shape of the tree structures or the principles that govern them.

Vol. I, Ch. 2

Whereas Ch. 1 describes the general theoretical setting, Ch. 2, ‘Nuts and bolts’, begins to develop the novel theoretical ideas of these books. There are two related concepts: *open values* and *range assignment*. These concepts are introduced with examples involving quantificational binding (§2.1.2), but the following simplified nominal example (p. 39) is more representative of how the ideas are employed:²



The range assigner is α^1 . It might be a free morpheme (f-morph) like *the*, it might be a bound morpheme like the Hebrew definite prefix *ha*, or it might be a full DP (§2.1.3). It assigns range to the open value $\langle e^1 \rangle_d$ that sits below it. This open value has some structure: its index connects it with α^1 , and its subscripted feature determines its set of potential range assigners. The open value assigns grammatical properties to its sister lexical item. In this case, $\langle e \rangle_d$ makes *cat* an N, so that is (as far as I can tell) an equally good category label for *cat* here. Most theoretical explanations in SS turn on the assumption that these open values are obligatory (to categorize the listemes) and that they must have range assigned to them properly.

This chapter is a useful resource. I found myself referring back to it often. However, the reader is warned that these chapters do not *define* B’s grammatical theory. They merely begin to introduce it. Many of the details concerning range assignment are introduced during the course of specific analyses, and the class of open values expands as well. For example, fused open values appear first at Vol. I, p. 111 (Ch. 4), and the details of range assignment via feature percolation first turn up in Vol. II, §5, 6.

Vol. I, Ch. 3

Ch. 3, ‘The proper way’, opens Part II. The guiding idea is that the distinction between proper names and other kinds of DP is not a lexical matter, but rather the output of different functional structures:

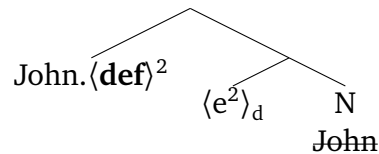
² The node-labeling principles of SS are not fully clear to me, so I have left blank all nodes for which the category label does not play a role in my discussion or B’s.

Aside from social conventions which may favour the interpretation of some nouns as proper names and others as common names, it appears that a noun will be interpreted as a proper name or as a common name (or, at times, as ambiguous between the two) solely based on structural information (p. 76–77).

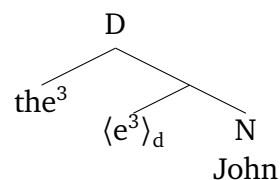
For example, setting aside capitalization conventions, *cat is here* is grammatical if *cat* is construed as a proper name. Similarly, in *The John (I met yesterday)*, the immediate grammatical environment for *John* (its determiner) forces a common-noun-like reading for *John*. Pluralization and quantification are quite generally permitted (*John, every John (in the room)*), with effects that are largely predictable from the meanings of the functional elements. Stepping back, then, we see that proper names are first-class N elements, free to appear in a wide variety of DP configurations.

The notion that lexical items are ‘raw material [...] poured into the structural mould’ (p. 108) defines B’s analysis of these proper-name-containing DPs. Both the definite article and the feature $\langle \mathbf{def} \rangle$ (‘u’ is for ‘unique reference’) are defined as range assigners to $\langle e \rangle_d$, the head of the DP projection. If $\langle \mathbf{def} \rangle$ appears, it is the range assigner to $\langle e \rangle_d$, and the N head moves to support it, as in (4), resulting in a proper-name interpretation. On the other hand, if the f-morph *the* is present, the N stays in its base position (pending some refinements from Vol. I, §6), and a common-noun interpretation results, as in (5).

(4) D (proper noun interpretation)



(5)



By the time of Ch. 6, two new functional projections (#P and ClP) have been introduced, which leads ultimately to the conclusion that proper names move through them, assigning range as they go (§6.3), but (4)–(5) are the heart of this analysis. Cross-linguistic differences should be localizable in the stock of functional morphemes, in particular, in the demands they place on phonological realization (p. 78, 85).

In §4.2 below, I question the notion that this analysis succeeds in eliminating the fundamental lexical distinction between proper names and common nouns (cf. p. 73, 78). The overarching insight is preserved, however: proper names are compositionally flexible, with their overall interpretation given by the functional structure in which they are embedded.

Vol. I, Ch. 4

Ch. 4, ‘Some stuff: On the mass–count distinction’, is pivotal for SS. It contains the major theoretical developments of Vol. I, and it is an important counterpart to the chapters devoted to telicity in Vol. II (Part II). The descriptive focus is the status of the mass–count distinction. This is often traced to the quirks of specific lexical items. That option is not available in SS; the goal is to draw *no* grammatical distinctions in the lexicon. So B seeks instead to derive it entirely from functional structure operating with almost complete freedom on highly malleable lexical items (and highly underspecified denotations).

The case for this is strong. We think of *salt* and *slime* as mass nouns, but a shift to the count domain is often as simple as adding a plural morpheme; setting vagueness aside, it’s easy to see what *salts*, *many salts* or *three slimes* mean. The functional structure guides us. Similarly, *dog* is prototypically a count noun, but *much dog* is meaningful and well formed (though perhaps a bit at odds with how we apprehend dogs; see p. 102). These facts suggest that, at the very least, we won’t get much mileage out of specifying lexical items for their mass–count properties.³

This flexibility is, however, limited to otherwise unmodified lexical items:

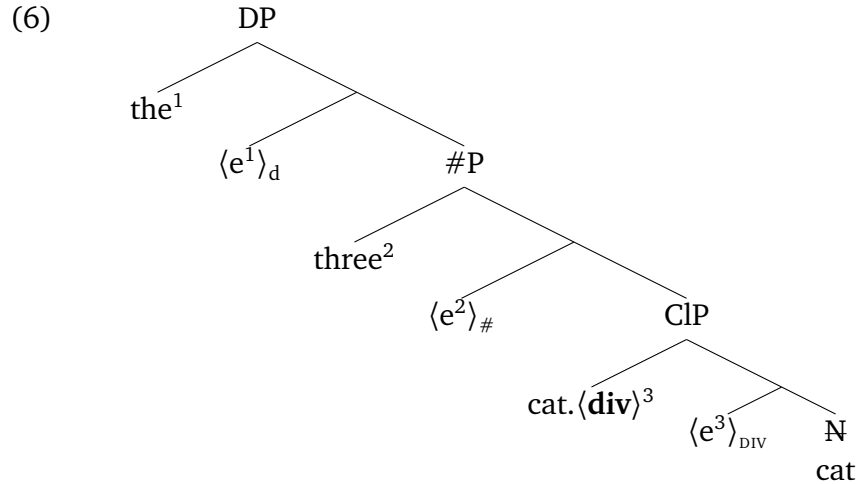
Thus grammatically marked plurality cannot be coerced into a count interpretation, nor can grammatically marked mass phrases be coerced into a count interpretation (p. 105).

Once we have formed *three dogs*, a mass interpretation is out of the question, and **much three dogs* is ill-formed morphosyntactically, never mind questions about its conceptual status (see p. 104 for additional examples along these lines).

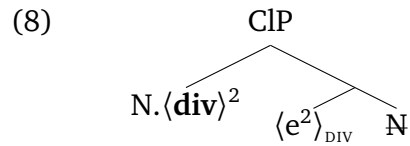
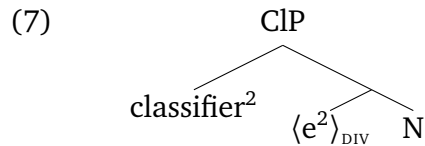
The simple DP structures of the preceding chapters expand considerably in this chapter. The two new functional projections seen in (1) are introduced here.

³ The noun compounding data of §4.5 demand further study. B argues that, in these limited structures (e.g., *stone throwing/carving*), only world knowledge tells us whether the first N is mass or count. However, these data seem susceptible to analysis in terms of properties of the second N. In addition, the possibilities for grammatical marking on the first N are not as limited as one might initially think — for example, *parks commissioner*, *perennials catalogue* (Sneed 2002), and British English *drugs bust*.

The quantity phrase #P is headed by $\langle e \rangle_{\#}$, with its range assigned by elements like *much* and *seven*. The classifier phrase ClP is headed by $\langle e \rangle_{\text{DIV}}$ and plays an important role in plurality. The following English example illustrates how the pieces fit together:



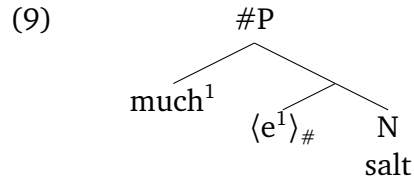
In (6), the N *cat* is merged with the $\langle \text{div} \rangle$ feature. The phonological output for this is the plural-marked *cats*. Thus, the example makes clear that ClP, while initially motivated by classifier-marking languages, plays a role even in languages (and structures) without traditional classifier morphemes. Indeed, for B, the contrast between classifier marking and plural inflection turns entirely on how range is assigned to $\langle e \rangle_{\text{DIV}}$, the feature that heads ClP. The f-morph strategy, (7), is the classifier (Chinese-like) strategy, and the head-movement strategy, (8), is the inflectional (English-like) one seen in (6). (These diagrams are modeled on those on Vol. I, p. 95, which are for Armenian, a language in which classifiers and plural marking co-exist, but in complementary distribution, a nice piece of motivation for the connection B proposes here.)



The range assignment possibilities mirror those for proper names in (4)–(5) above, and all of them are instances of the basic strategies used to introduce range assignment near the start of §2. The parallels are theoretically appealing. However, this chapter must extend the basic idea to allow a single item to assign range to multiple open values. To see the motivation behind this idea, we need

first to briefly review the semantics of mass and count that B envisions for these structures.

The fact that bare (classifier-free) Chinese nouns are interpreted as mass leads B to conclude that mass is a ‘default’ semantic value when the mass–count distinction is relevant (p. 107). Structurally, this is captured by the absence of CLP:



The mass default means that, semantically, ‘the plural is not a function from singulars’ (p. 120), but rather a function from mass denotations. That is, the semantics of $\langle e \rangle_{\text{DIV}}$ involves mapping a mass denotation to something else. According to B, this something else is not, however, a traditional count denotation. It is, instead, a *reticule* (p. 123), which seems, from the description, to be a potentially infinite set of partitions on the mass denotation. The $\#P$ meaning selects a member of the reticule. If the range assigner in $\#P$ is *three*, then a reticule with three members (at some level of granularity or individuation) is selected, for example. So count denotations involve the interaction of $\#P$ and CLP⁴

This proposal leads naturally to the question of how count denotations are derived in the absence of morphemes that we could plausibly place in CLP to assign range to $\langle e \rangle_{\text{DIV}}$ and take the mass denotation as an argument. The following Armenian example illustrates (p. 117):

- (10) Yergu hovanoc uni-m. (Armenian)
 two umbrella have-1SG
 ‘I have two umbrellas.’

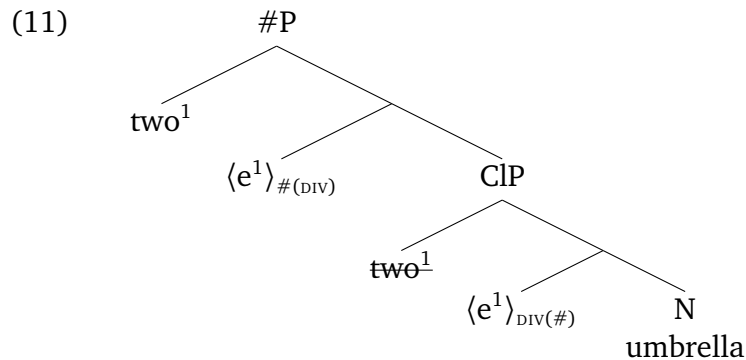
The N head *hovanoc* is not plural, but the interpretation is count. B’s solution to this is to allow some functional elements to assign range to more than one open-value. The idea is initially introduced for the English indefinite article:

Suppose we assume that *a* (as well as *one*) assigns range simultaneously to $\langle e \rangle_{\text{DIV}}$ and to $\langle e \rangle_{\#}$, in essence establishing an identity

⁴ This semantics, and some of the assumptions that support it, are central to the account of telicity in Vol. II, §3, 5. I return to this, in particular, to the role of the distinction between quantity predicates and homogeneous predicates, in my discussion below of Vol. II, §5.

between $\langle e \rangle_{\text{DIV}}$ and $\langle e \rangle_{\#}$. More specifically, suppose we assume that the open values $\langle e \rangle_{\text{DIV}}$ and $\langle e \rangle_{\#}$ in this case are fused (alternatively, coindexed in some fashion) to give rise to complex open values of the form $\langle e \rangle_{\#(\text{DIV})}$, $\langle e \rangle_{\text{DIV}(\#)}$ (p. 111).

It's not clear to me what B has in mind by way of coindexation, but her dominant notation, in this chapter, is the parenthesis-based one. In terms of the tree structures, it means that, alongside (7) and (8), we have the following kind of plurality marking and count denotation composition (p. 114):



The structure for *a dog* is comparable. (Thus, while B lumps *a* and plural marking together in many descriptions, this doesn't fully capture the proposal for how the indefinite article works. §6.1 specifies further differences.)

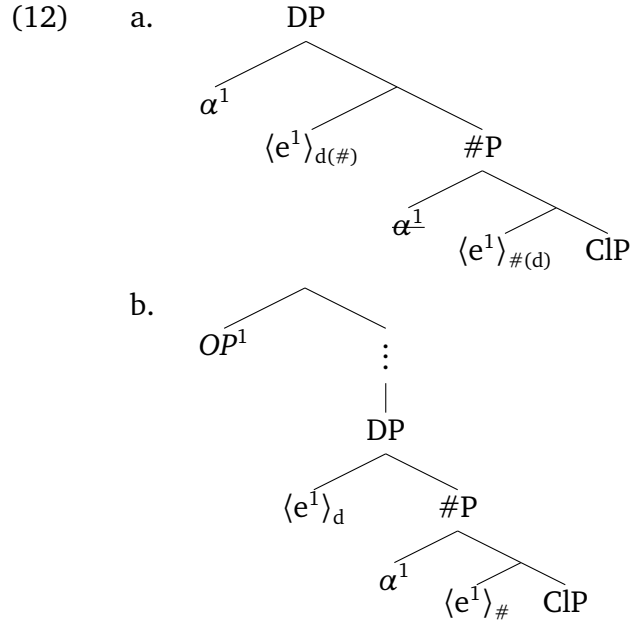
A variety of different DP configurations are possible with the theory of range assignment developed to this point. This chapter includes a helpful summary chart (p. 119), which should facilitate comparison with other extended attempts to derive a proper DP typology from a specified set of universal morphological features (Giorgi & Longobardi 1991; Longobardi 1994; Chierchia 1998; cf. Chung 2000).

Vol. I, Ch. 5

The previous two chapters raise a number of questions about the internal structure of DP. These turn largely on how certain open values are assigned in the absence of overt functional morphemes that could plausibly be doing the assigning. Ch. 5, 'Things that count: Null D', addresses one such question: how is range assigned to $\langle e \rangle_{\text{d}}$ in structures like *much salt* and *three cats*; (p. 136), which contain $\langle e \rangle_{\#}$ open values but seemingly no $\langle e \rangle_{\text{d}}$ range assigner.

B proposes two mechanisms for assigning range to $\langle e \rangle_{\text{d}}$ absent an overt D: (i) merger of a single functional morpheme in both #P and DP, where it can

assign range directly to $\langle e \rangle_d$, as in (12a); and (ii) indirect range assignment via existential closure or binding by a quantificational adverbial, (12b).⁵



Whereas inherently quantificational operators like *every* and *most* (Partee 1987) are held to demand option (12a), the second half of this chapter is given over to arguing that (12) represents a semantically contentful ambiguity for (some) morphological indefinites. The case is made based on the distribution of scope-taking possibilities for a variety of indefinites. These facts are held by B to support the ambiguity in (12). This leads to a discussion of two strategies for marking indefinites overtly in Hebrew: with the clitic element *xit*, which always takes primary scope, and with the (mainly) suffixal element *'exád*, which can have only narrowest scope. This culminates in the proposal that *xit* must assign range to $\langle e \rangle_{d(\#)}$, whereas *'exád* cannot assign range to $\langle e \rangle_{d(\#)}$ and therefore triggers the indirect range assignment option (12b). In §4.3 below, I critically assess the extent to which these syntactic options help with the semantic variability at hand.

⁵ Throughout the chapter, structures like (12a) are given with the simple features $\langle e \rangle_d$ and $\langle e \rangle_{\#}$ (e.g., p. 143). However, I cannot see any relevant differences, formally or conceptually, between these cases and those analyzed as involving fused features in §3 (see (11) above), nor does B discuss the issue, so I've continued with fused values here.

Vol. I, Ch. 6

The highly articulated functional structure in (1) is challenging in two major respects: (i) in simple examples like *hippos*, it isn't immediately clear how all those open values are assigned range; and (ii) in certain complex ungrammatical cases, it often isn't immediately clear what has gone wrong, since there seems to be sufficient room for everything. Ch. 6, 'Things that count: Null # and others', seeks to address a range of such issues. The reader should keep in mind that, while open-class lexical items are grammatically impoverished and largely homogeneous, the functional lexicon is supposed to be highly articulated, with each element carefully crafted. DPs are highly variable in their shape, and that information needs to be stored somewhere. Thus, it should not be surprising that special restrictions need to be placed on *a* (vs. its near synonym *one*; cf. *the one/*a dog*; see p. 163), on *all* (p. 172–174), and so forth. These are all, by hypothesis, members of the functional lexicon, and thus each is its own complex, idiomatic morphosemantic bundle.

If we step back from these details, we do see a larger theme emerge from this chapter: typically, in simple DPs, one or more elements will have to move through each of the semantically required functional projections, assigning range as it goes. Thus, the final proposal for how proper names work involves the N (the proper name) moving through ClP and #P before assigning range to $\langle e \rangle_d$ (§6.3), and *the* merges in #P before moving upwards in simple DPs like *the cats*. These movements strike me as a consequence of the assumption that each functional element must project its own phrase (as opposed to having complex feature bundles on nodes, as in Pollard & Sag 1994; Sag *et al.* 2003; Bresnan 2001; Dalrymple 2001).

Vol. I, Ch. 7

Ch. 7, 'One is the Loneliest Number', begins Part III. Ch. 7 and Ch. 8 together form an extended, wide-ranging analysis of indefinites in Hebrew. They are application chapters, in that they attempt to use the preceding theoretical apparatus and conclusions in the analysis of a few different configurations in the Hebrew DP. Ch. 7 extends the discussion of indefinites and specificity that closes Ch. 5, and it addresses Hebrew definite marking and quantification. It is factually rich, and thus useful even to readers who don't subscribe to the specific theoretical implementation.

Vol. I, Ch. 8

Ch. 8, ‘Cheese and olives, bottles and cups: Notes on measure phrases and container phrases’, concentrates on an unusual class of nominals involving measure and container phrases. These are special in a range of respects affecting their morphology as well as their basic structure. There is not space to delve too deeply into these analyses. This chapter is, though, my favorite of the two volumes. It moves the spotlight off the well-worn terrain of normally shaped DPs and onto a more unusual (but productive) subclass. It makes a convincing case that we can learn a lot by looking to the fringes of language (Culicover 1999).

Vol. I, Ch. 9

Ch. 9, ‘Some concluding notes on language variation’, is a brief conceptual summary of Vol. I. It provides a fast overview of the technical details of range assignment, but its main purpose seems to be to emphasize that, in the theory developed in SS, inter-language and intra-language variation are due to variation in the stock of functional elements. (Readers looking for more closure on their study of SS should look to the final chapter of Vol. II.)

2.2 Vol. II, *The Normal Course of Events***Vol. II, Ch. 1**

Ch. 1, ‘Exo-skeletal explanations: A recap’, is largely a compressed version of Vol. I, §1, 2 (though the material on idioms is new). I think that the two volumes of SS can profitably be read independently of each other, but readers planning to study just Vol. II might nonetheless read the first two chapters of Vol. I as well, as they provide a fuller introduction to the theoretical ideas that are particular to these books.

Vol. II, Ch. 2

Range assignment to open values remains the central theoretical mechanism in Vol. II, but the set of functional heads and the underlying semantics is different. Ch. 2, ‘Why events?’, outlines these new elements: it introduces the functional projection Asp_Q (see (2)), a counterpart to $\#P$ in the DP, and it describes how the functional projections rally together to recreate the patterns of argument structure realization that we might once have attributed to lexical items.

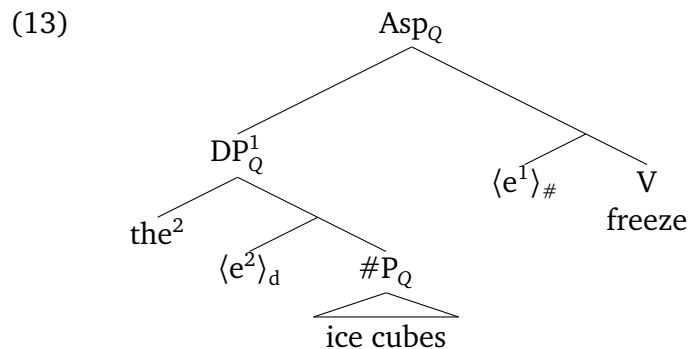
This chapter also uses unaccusativity to broaden the argument that lexical items are like clay waiting to be molded by the structures in which they appear.

Unaccusative verbs take a single nominal argument, but that argument often has many properties of an internal argument. B rehashes some of the debate about whether unaccusativity is syntactically represented (with the surface subject as an underlying direct object) or merely reflects semantic generalizations. On the basis of Hebrew possessor datives (p. 39–42), and after rejecting a handful of well-known arguments (Levin & Rappaport Hovav 1995 is a more extended discussion and historical overview), she opts for a syntactic approach, which is developed more extensively in Ch. 3.

Vol. II, Ch. 3

The final section of Ch. 2 is called ‘Severing the *internal* argument from its verb’. The title is a play on Kratzer (1996), ‘Severing the external argument’, which is an argument that verbs are not functions on their subjects, but rather only on their internal (direct object) arguments. B seeks to push this a step further, by turning verbs into mere event modifiers, with all the arguments interpolated via functional heads (in the manner that Kratzer develops). Ch. 3, ‘Structuring telicity’, begins to show us how this will work syntactically (the semantics is largely deferred until Ch. 5). Throughout, VPs are nonbranching structures immediately dominated by a new projection. For telic structures, this is Asp_Q . (Atelic structures are the focus of the next chapter.) Asp_Q is where the verb’s internal argument is realized.

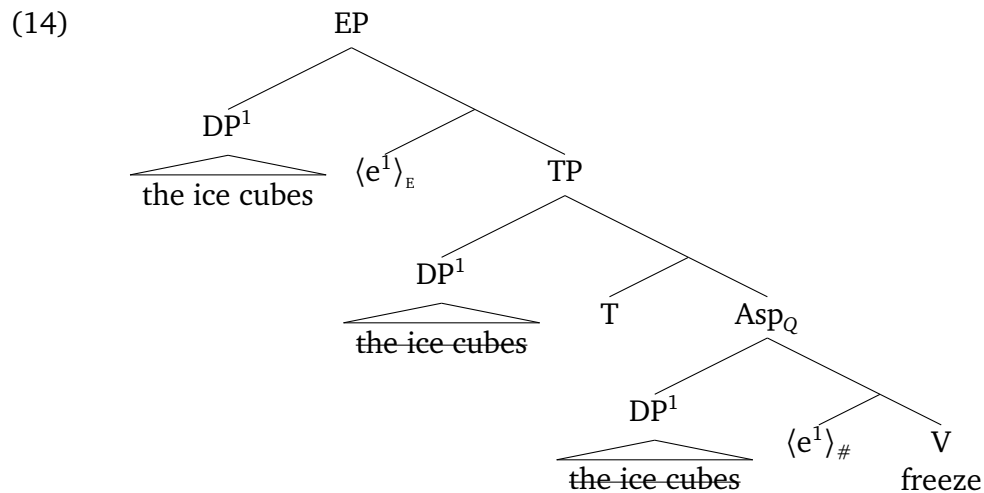
I find it fruitful, in thinking about how these pieces fit together, to begin with the limited structure in (13). Depending on how one builds from it, one can end up with either a transitive structure or an unaccusative one.⁶



⁶ Throughout this chapter, the node dominating the verb is labeled ‘VP’. However, the theory of bare phrase structure outlined in Vol. I, §2.1.4, and repeated at Vol. II, §1.2.4, does not distinguish VP from V in this configuration, and the considerations on Vol. I, p. 44–45 (Vol. II, p. 19–20) suggest that structures like the complement to $\langle e^1 \rangle_{\#}$ in (13) should be given as I have done here, and not as they are given throughout this chapter.

The open value $\langle e \rangle_{\#}$ is the same one that was the focus of Vol. I, §4, where it headed $\#P$. Asp_Q and $\#P$ are in fact intimately related in the present theory. The descriptive connection is between count interpretations in the nominal domain and telic interpretations in the verbal domain. At the theoretical level, this is achieved via range assignment. In (13), the DP receives a count interpretation. It is presumed then to be a suitable range assigner for $\langle e \rangle_{\#}$, which delivers a telic structure. The implicit assumption seems to be that some quantity feature on the $\#P$ inside DP percolates up to the DP level, so that range assignment happens by specifier–head agreement. Following B, I’ve indicated this percolation with the feature Q , but, like her, I won’t explore where such projection can or must happen.

The structure in (13) has an unaccusative continuation: the DP can merge copies in TP as well as in the second functional projection introduced in this chapter, E(vent)P. The overall structure is then (14) (see p. 79, 84).



If, instead, a second DP merges into the TP (and the EP), then the result is a transitive structure (e.g., *Sally froze the ice cubes*).

Thus, in the system that emerges, all nominal arguments are introduced and manipulated by functional elements. Their relationship with the verb that, in lexicalist theories would govern them, is now a mediated one, and it is difficult to see how a given verb could impose special requirements on any of its arguments. I return to this point in §4.1 below, looking to assess the precise nature of this (lack of) connection and what it means for things like subcategorization.

The second part of this chapter concerns PPs. These figure centrally in the argument of this chapter because they can (like some functional morphemes) change the telicity properties of the predication: *ate the cake* can be telic, whereas *ate at the cake* is atelic. The basic idea is that this change in telicity is a result

of the fact that the object, embedded inside a PP, cannot make it into a position from which it can assign range to $\langle e \rangle_{\#}$, and thus the configuration for telicity (see (14)) is not achieved. The aspectual status of the result is then presumably a function of the semantics of the preposition involved.

Vol. II, Ch. 4

Ch. 4, '(A)Structuring atelicity', rounds out the picture of aspectual syntax by giving shape to atelic structures. The title reflects B's view that atelicity is characterized by the absence of certain features present in the telic cases. One way to think about the proposal is this: atelic structures begin from non-count nominals. Thus, there is no range assigner for $\langle e \rangle_{\#}$; a structure like (13) would fail if the object were instead simply the mass DP *ice*. Thus, Asp_Q does not project in these cases; 'atelicity is the absence of quantity structure' (p. 160). Instead, a nonce projection F^sP immediately dominates the VP and provides a home for the object (p. 109). Finnish case-marking provides the empirical basis for this approach. The situation is roughly as follows: objects in telic structures receive accusative case, whereas objects in atelic structures receive partitive case. In B's terms, Asp_Q is the locus of accusative case, and F^sP is the locus of partitive case. Each is abstractly assigned in English as well.

Vol. II, Ch. 5

Ch. 5, 'Interpreting telicity', returns to telic predications, but this time with an emphasis on interpretation. The basic descriptive claim comes as no surprise at this point in SS: the telic–atelic distinction is not a lexical one, but rather one determined by highly articulated structures, with the open-class lexical items playing a relatively minor role (but see Vol. II, p. 70, n. 1).

This chapter is intimately related with Vol. I, §4.4, which sketches a semantics for the mass–count distinction. In my discussion of Vol. II, §3 above, I noted that it is the direct object that assigns range to $\langle e \rangle_{\#}$ and, in turn, determines a telic structure. Not just any direct object can do this, however. For example:

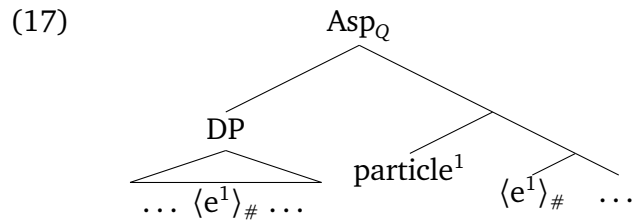
- (15) Representative telic cases
- a. Ed ate the apples
 - b. Ed ate more than three apples
 - c. Ed ate every apple.

- (16) Representative atelic cases
- a. Ed ate apples.
 - b. Ed ate fruit.
 - c. Ed pulled the wagon.

The major work of Ch. 5 (in particular §5.3) is to find a semantic property that divides the examples properly into the telic and atelic cases. B considers a number of alternatives before settling on a basic opposition, derived from work by Kiparsky (1996) and Krifka (1998), between *quantity* and *homogeneous* denotations (p. 147; see also Vol. I, p. 127). The definitions are given in terms of predicates. Roughly speaking, predicate *P* is homogenous iff its denotation is closed under unions (mereological sum formation) and divisive in the sense that, if it is true of some entity *x*, then it is true of every part of *x* as well. Together with B's assumption that bare plurals are homogenous (see Vol. I, p. 122–123; Vol. II, p. 146), this is claimed to properly divide the class of nominal denotations in a way that determines the telic/atelic division as it is usually construed. (However, (16c) is treated separately; see §5.2.) Indeed, B replaces 'telic' with 'quantity' (p. 147), a usage shift that continues for the rest of Vol. II.

Vol. II, Ch. 6

The previous chapter opens with a reflection on the various ways in which range might be assigned to $\langle e \rangle_{\#}$ in Asp_Q . B notes that, to this point, we have seen such range assignment only in specifier–head relationships with the internal argument, as in (13) above. The system, however, predicts more variation than this. There should be an f-morph strategy, comparable to the way the definite article assigns range to $\langle e \rangle_d$, there should be a head-raising analysis, comparable to English plural marking, and there should be external binders (as in (12b)). Ch. 6, 'Direct range assignment: The Slavic paradigm', begins to explore this predicted variation (which continues into Ch. 7). A well-studied group of Slavic aspectual markers is the empirical focus, with most of the data drawn from Czech. The general claim is that these particles are range assigners to $\langle e \rangle_{\#}$. To make this work, B exploits the idea (briefly floated in the previous chapter; p. 124) that range-assignment can run, not only from specifier to head, but also from head to specifier. In (17), for example, it is $\langle e^1 \rangle_{\#}$, having received range from the particle, that in turn assigns range to $\langle e^1 \rangle_{\#}$ inside the DP.



Semantically, the particle assigns range to $\langle e \rangle_{\#}$, and this delivers a quantity denotation for the DP in the specifier of Asp_Q . B calls this ‘range assignment through specifier–head agreement’ (p. 155), and it provides the central method for understanding how verbal particles can affect the range of available interpretations for morphologically bare nominals in Slavic (see especially §6.3).

Vol. II, Ch. 7

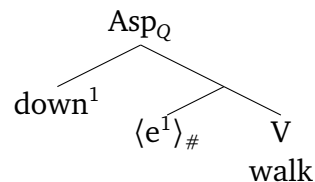
Ch. 7, ‘Direct range assignment: Telicity without Verkuyl’s generalization’, is in many ways a continuation of Ch. 6, since it continues to find new ways in which telic readings can arise as a result of range assignment to $\langle e \rangle_{\#}$ in Asp_Q .

(18) Verkuyl’s Generalization (p. 73)

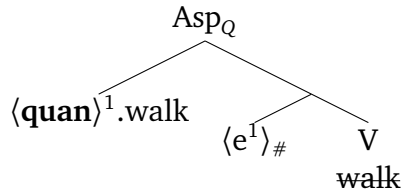
Telic interpretations can only emerge in the context of a direct argument with property α .

The property α is left undefined here; in general, (18) just stakes our the general claim that the internal argument governs aspect. The previous chapter began challenging this connection using Slavic particles. The present chapter deepens the challenge, by tracing telicity to a variety of different sources: additional particles, a variety of bound morphemes, and prepositions. The diagrams in (19) illustrate the first two cases. Licensing by PPs is more complex; I refer the reader to §7.3.2 for the details.

(19) a. walk down



- b. Ivan po-guljál po górodu (Russian; p. 187)
 Ivan po-walk.PST around town
 ‘Ivan took a short walk around town.’



These configurations are familiar from earlier chapters. The first involves f-morph range assignment (see (5)), and the second involves range assignment paired with head movement (see (4)). (The status of long-distance range assignment to $\langle e \rangle_{\#}$, in parallel with (12b), is left somewhat open; p. 211.)

Vol. II, Ch. 8

The title of Ch. 8, ‘How fine-grained?’, suggests that the primary question will be how much content we ascribe to the narrow semantics — the conventionalized meanings of words and phrases — and how much we ascribe to pragmatic enrichment. The chapter does open with this question, but it quickly moves to focussing on *which* aspects of meaning are conventionally encoded, rather than on how many divisions they make, as well as on which aspects of structure express these meanings. The first major section is §8.2.1, which critically assesses proposals for decomposing basic telic events and resultatives into smaller meaning chunks. Though this is all framed as though the issues involved the very question of whether or not to decompose, the conclusions are actually more limited, as we see here:

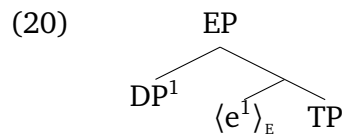
The failure of causality suggests either that decomposing is the wrong move, or alternatively, that decomposing should be otherwise conceived (p. 225).

The next major section, §8.2, addresses the grammatical reality of activity readings, arguing ultimately that this class (unlike stative and telic structures) is not grammatically identified. This conclusion is in keeping with the general conclusion of Ch. 4 that atelicity is the absence of structure (whereas telics are determined by Asp_Q ; see above for more details). Finally, §8.3 addresses (at long last, the reader might say) the fact that lexical items are not as malleable as SS’s guiding thesis might lead one to expect. Rather, we see a great many apparent idiosyncracies reflected in limitations on where and how lexical items

can appear in structures. I return to this in §3.2 below. Suffice it to say, for now, that the general conclusion is that these linguistic patterns are better left to an investigation of the ‘conceptual component’ (p. 252), as distinct from the narrowly grammatical one that is the focus of SS.

Vol. II, Ch. 9

Ch. 9, ‘The existential road: Unergatives and transitives’, opens the final part of Vol. II. This is the first extended appearance of the EP projection seen in my summary schematic for Vol. II, (2). EP is regarded as the final resting place for (most) subjects and, semantically, the locus of event binding. The standard configuration is (20):



Structures like this are not the focus of Ch. 9. They are just its starting point. The examples of interest are those involving unergative and transitive predications in which there seems not to be a DP element as the left daughter of EP. The examples in (21) are representative:

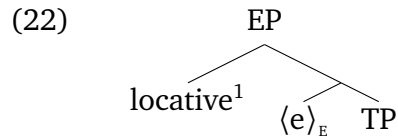
- (21)
- a. ‘abad šam (kama, šloša) gananim. (Hebrew; p. 275)
worked here (several, three) gardeners
‘Several/Three gardeners worked here.’
 - b. Hi canten molts nens. (Catalan; p. 276)
there sing many boys
‘Many boys sleep there.’
 - c. En este parque juegan niños. (Spanish; p. 276)
in this park play children
‘Children play in this park.’
 - d. Above them pranced the horses on the Parthenon frieze.
(p. 287; an attested example cited by Levin & Rappaport Hovav 1995:224)

B concentrates on Hebrew, Catalan, and Spanish examples. The extension to English locative inversion cases like (21d) is tentative (p. 287, n. 22).

Clauses with post-verbal subjects and telic interpretations raise two questions in the context of SS: how is range assigned to $\langle e \rangle_E$, and how is range assigned

to Asp_Q (thereby delivering telicity)? This chapter is largely about how $\langle e \rangle_E$ is assigned range; the licensing of Asp_Q in these cases is left to Ch. 10.

After rejecting the notion that the post-verbal subjects are, at some abstract level, daughters of the EP node, B proposes that, in these cases, it is the locative expression itself that assigns range to $\langle e \rangle_E$ and achieves the requisite sort of binding of the event argument in the semantics (p. 283). ('locatives are one of the crucial building blocks of event structure in general', p. 305). The configuration is as follows:



The locative element can be a free-standing locative phrase, as in (21c), or, if the lexicon permits it, a locative clitic, as in (21a, b). (In the clitic case, the verb moves to adjoin to the locative element, to support it morphologically.)

The account predicts that, all else being equal, it should be possible for locatives to assign range to $\langle e \rangle_E$ even in transitive structures. §9.6, emotively titled ‘Transitive expletives? In Hebrew??’, provides evidence that this prediction is correct (p. 304, (96b)):

- (23) hipcicū šam metosim ’et ha.°ir. (Hebrew)
 bombed there planes OBJECT.MARKER the.town
 ‘Planes bombed the town.’

The second major question to which Ch. 9 is addressed is why the post-verbal subjects in these cases are invariably weak indefinites. The proposal builds on the complex range assignment configuration that was a focus of Ch. 6. There, a DP daughter to Asp_Q had its $\langle e \rangle_{\#}$ value assigned through specifier–head agreement with the $\langle e \rangle_{\#}$ value in Asp_Q , whose range assignment was a particle. B claims that the locative expressions in these cases have been ‘slavified’ in the sense that they too perform this complex multiple range assignment (p. 301). We see more slavification in the next chapter.

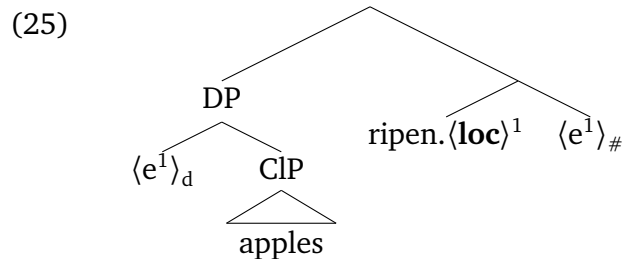
Vol. II, Ch. 10

The previous chapter is unusual in its emphasis on post-verbal subjects in unergatives and transitives. The more famous such subjects in otherwise SV languages are the unaccusatives. Ch. 10, ‘Slavification and unaccusatives’, turns to them. Most of the discussion concerns the Hebrew paradigm, and locatives are again

the proposed range assigners. The following examples (p. 307, 308) provide initial motivation:

- (24) a. *hibšilu šlošet (ha.)tapuxim (ʿal ha.ʿec). (Hebrew)
 ripened three (the.)apples (on the.tree)
 ‘Three apples ripened.’
- b. hibšilu po šlošet tapuxim (ha.qayic) (tok xamiša šabuʿot).
 ripened here three apples (this.week) (in five weeks)
 ‘Three apples ripened here.’

B thus assumes that, as in the previous chapter, the locative is the range assigner to $\langle e \rangle_E$ in these cases. This leaves, however, the question of how range is assigned to $\langle e \rangle_\#$ in Asp_Q . The proposed solution is at least optional cyclic movement of the locative phrase through Asp_Q and EP. The final loose end concerns the internal DP argument. It turns out that, in Hebrew, this can be weak (p. 308, (11), (12)) or strong (p. 313, (18)). Where it is weak, it is assumed that $\langle e \rangle_d$ has range assigned by the locative (p. 315). Where it is strong, the DP itself is the range assigner to Asp_Q , as in (25).



This leaves two classes of example unaccounted for: (i) the limited class of unaccusatives that can be licensed even in the absence of a locative, and (ii) the limited class of achievements that cannot be post-verbal under any circumstances (p. 308, (10)). For (i), both $\langle e \rangle_E$ and Asp_Q appear to be without range assigners:

- (26) Sono arrivati studenti. (Italian; p. 307)
 are arrived students
 ‘Students arrived’
- (27) parca mehuma (ha.boqer). (Hebrew; p. 307)
 erupted.F.SG riot.F.SG (this.morning)
 ‘A riot broke out this morning.’

The examples have quantity (telic) interpretations, but the post-verbal subject is required to be weak (p. 320). B’s analysis capitalizes on the results concerning

locatives from this chapter and the previous one: these examples are analyzed as having a covert, lexically specified locative, with range assignment into the DP also happening at an essentially lexical level, and the stubborn achievements, class (ii) just above, also receive lexical solutions. Thus, §10.3 is another extended appeal to a highly articulated functional lexicon.

Vol. II, Ch. 11

Ch. 11, ‘Forward Oh! Some concluding remarks’, starts with a near-verbatim repeat of Vol. I, §9. However, the rest of the chapter is new and worth studying. The central section, §11.2, is called ‘Some final notes on the nature of listemes’. It is SS’s most frank discussion of the fact that the lexical ambiguities highlighted throughout the work are perhaps not the norm, not within English and even more clearly not in more highly inflected languages. (See also Vol. I, §8.3.) The general conclusion is that the system described here ‘overgenerates’, with the goal of the chapter being to ‘point to some ways in which this overgeneration can be curtailed’ (p. 347). In general, the suggestions that follow involve associating idiosyncractic information with specific listemes. In some cases, this information includes category information and projection structure (e.g., p. 354). Of course, if too much of this is allowed, the force of claiming that open-class listemes ‘do not have an inherent category, but inherit it from the dominating functional structure’ (Vol. I, p. 21) will be seriously eroded. I return to this point in §5 below.

3 General theoretical ideas

I opened this review by highlighting B’s view of the lexicon as a collection of agrammatical conceptual bundles. I hope my tour of the books, though fast, makes it clear that this big-picture idea does guide the modes of argumentation and technical approaches developed in SS.

Nonetheless, even after studying the two volumes extensively, I still feel that I don’t have a firm grasp of a few really central claims and theoretical constructions. Is it true that words are really more polysemous than structures? If so, what is the evidence for that? If listemes are mere conceptual bundles, then how do they interact with the stuff of grammar? Can we give rigorous definition to the notion of range assignment? The next few subsections are devoted to posing and trying to answer these questions.

3.1 Are words more polysynonymous than structures?

Both volumes of SS are faithful to the opening of *In Name Only*:

Fundamentally, this is a book about polysemy. About why *words* can mean so many things, but structures cannot. An English word, such as *stone*, can be used in a multitude of syntactic contexts as either a noun or a verb, and it can have different meanings in different communicative situations. But not so for structures such as *three stones* and *much stone*, or *to stone a bird*, or *be stoned* (Vol. I, p. 3).

This is a quantitative claim about the words and structures of language. It conflates meaning with morphosyntax in a way that makes it hard to specify what we should count, but its intentions are intuitively clear in the context of SS: listemes impose few grammatical constraints and are thus free to appear in a wide variety of configurations, whereas the addition of functional material — the backbone of morphosyntactic structure — reduces the available structures and senses.

This seems intuitively plausible, but its quantitative nature should lead us to call for hard numbers to support it. SS does not support the claim with any quantitative data whatsoever. On the contrary, as the above quotation suggests, its truth is presupposed. We're given some examples to convey the intuition, but B intentionally stops short of saying that they are representative; as she notes (Vol. II, p. 347), one needs to be selective in one's choice of language and listeme if one seeks the variety attested here with *stone*. It would be much harder to articulate the above claim if we changed *stone* to *inquire* or *hear* or *own*, and moving to a language like Hebrew, with almost entirely bound roots, makes the situation trickier still (Vol. II, p. 348ff).

What kind of data might we gather in order to evaluate this claim? The Statistical Natural Language Processing community has in effect been performing experiments in this area for decades. One of the central problems for part-of-speech tagging and syntactic parsing (among others) is lexical ambiguity. How pernicious is this problem? DeRose (1988) provides some numbers for the Brown Corpus (Kucera & Francis 1967), which contains about one million words drawn from a wide variety of contemporary English sources. Of the 40,000 or so word types in the Brown Corpus, 11.5% are at least two ways ambiguous. The following chart from DeRose 1988 provides a snapshot of the level of ambiguity attested:

(28) Number of words by degree of ambiguity:

| | |
|----------------------|-------------|
| Unambiguous (1 tag) | 35340 |
| Ambiguous (2-7 tags) | 4100 |
| 2 tags | 3760 |
| 3 tags | 264 |
| 4 tags | 61 |
| 5 tags | 12 |
| 6 tags | 2 |
| 7 tags | 1 ('still') |

The Brown Corpus tag-set has about seventy non-punctuation tags. Thus, in principle, a word could be seventy-ways ambiguous. In practice, features of that tagging system make that impossible, but it is still striking that the highest degree of tag ambiguity is merely 7. These numbers are not really at the level one would like in order to draw support for B's claim from them. In fact, one might argue that they *challenge* B's claim. The real problem confronting researchers on part-of-speech tagging is the level of ambiguity at the token level: though only 11.5% of the word types are at least two-ways ambiguous, 40% of the tokens are. That is, the ambiguity is widespread but concentrated in a relatively few words. These numbers might lead one to conclude that the default is a fairly fixed lexical specification, with pockets of freedom (some of it surely accidental, as in things like *can*).

The picture does not become much clearer when we move past the word level. Accurate categorization (and parsing) of naturally occurring data does indeed depend on gathering information not only about the word in question but also about its local linguistic context.⁷ However, this contextual information brings new ambiguities (Church 1988). The short phrase *three stones* seems unambiguous, but what about *wet stones* or (famously) *horse raced*? The more words we add to these strings, the more ambiguous they are likely to get; in some circumstances (e.g., multiple adjuncts), the number of parses can grow exponentially with the word-level length of the string (Church & Patil 1982; Jurafsky & Martin 2000:§10.3; see also Manning & Schütze 1999:§12 for a thorough discussion and some object lessons).

One might object at this point that we are dealing with strings, rather than with structures. B is clear throughout that she is contrasting listemes with highly articulated trees. For example:

⁷ Non-local features like genre are also important, and lexical frequency is vital. The first could fit nicely into a pragmatic theory, and the second is important to the recent work of Joan Bresnan and her colleagues (e.g., Bresnan & Nikitina 2008).

by the absence of grammatical properties, I am referring not only to the absence of category or of argument structure specification, but also to the absence of overt grammatical marking of any sort, be it syntactic, morphological, or inflectional (Vol. I, p. 13).

I am sympathetic with this objection, but I fear that it is easily turned around: the word-level ambiguities that B begins with could be about strings, not lexical items. They tend to rely on the quirk of English that most roots are also well-formed free morphemes, and they trade on an impoverished morphological system. I want to add that, even if we grant that the string-based data are misleading, it might still be difficult to make the case that the number of possible meanings drops as the structures get bigger. Interrogatives like *Where can we buy the newspaper?* are underspecified along a number of dimensions, leading to quite distinct interpretations (Beck & Rullmann 1999; van Rooy 2003). They simply don't admit of unique interpretations, and it seems unlikely that we can motivate enough structural distinctions, given that we are dealing, in some sense or another, with vagueness (persistent underspecification).

One wonders too whether the claim should be limited to open-class items. It's integral to the theory of SS that functional elements are highly specified, and B clearly separates them in her prose and in the theoretical mechanisms she proposes. For example:

The notorious flexibility illustrated for listemes is by and large restricted to the domain of so-called open-class items — that is, to substantive as opposed to grammatical formatives. No such flexibility is attested for closed class items, i.e. for grammatical formatives (henceforth, functional vocabulary) (Vol. I, p. 10).

However, the senses for modal auxiliaries (deontic, buletic, epistemic, etc.) can be sharply curtailed by their syntactic positions (von Stechow & Iatridou 2003; Hacquard 2006).

I want to emphasize that I share B's intuition, both about the way structure can reduce ambiguity and about how this is largely an issue for open-class items (though some of the argumentation in Hacquard 2006 is certainly SS-like, though concerning modal auxiliaries). It might well be that the corpus-based approach misses its mark; SS doesn't offer guidance in how we might find the requisite empirical support. I am optimistic, though, that we can find methods for rigorous assessment.

3.2 The conceptual interface

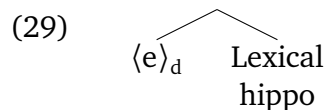
Much in SS depends on getting from nongrammatical listemes and their conceptual content to linguistic structures and (event-style) denotations. This is not an issue that B confronts in SS; the descriptions we get of how this happens are metaphorical:

Where, then, does the grammar meet the substantive listeme? At some very narrow portal, I suggest, where little conceptual packages, hermetically sealed, are passed from one side of the wall to the other [...] (Vol. I, p. 12).

Like B, I hesitate to probe too deeply into the world of concepts and their connection to language. Answers in this area await a highly advanced neuropsychology or a highly refined philosophy. However, model-theoretic semantics is relatively noncommittal about the nature of denotations. For example, whether the denotations are filled with events or our mental representations of events is unlikely to impact most proposals. The action is usually in the combinatorics. So it seems worth fleshing out some combinatoric assumptions that might begin to turn the metaphorical language into a predictive theory.

The first step is a theory of listeme interpretation. B writes, ‘The use of any particular substantive listeme (henceforth simply *listeme*) will return a meaning based fundamentally on its conceptual value’ (Vol. I, p. 11). Thus, let L be the full class of listemes (for some specified language), and let $V^L(\cdot)$ be the function that maps listemes to their conceptual values. The range of $V^L(\cdot)$ is the collection of conceptual values. This much is entirely in keeping with a standard compositional semantics.

Functional elements have fixed meanings as well. It seems sensible, then, to ask about their combinations in configurations like (29), in which *hippo* is a listeme (syntactically categorized as N by $\langle e \rangle_d$):



The output of this structure is discussed extensively in Vol. I, §4, in the context of the reticule-based theory of mass denotations. We need not delve into it here (see my summary of the chapter above). It’s worth positing a logical type for the meaning of $\langle e \rangle_d$, though. Let’s use $(c \rightarrow \sigma)$, where c indexes the collection of contexts and σ is some grammatical output. If we are to stay true to B’s conception, then we must say that c is not part of the linguistic system but σ is. This seems unobjectionable — the combinatorics are just as they would be

for a purely linguistic structure — and we have, at least, defined the requisite interface. The open value $\langle e \rangle_d$ has a foot in the grammatical world (σ) and a foot in the conceptual world (c). It is our ‘portal’.

I should emphasize that a full-fledged semantics for SS might not be entirely compositional. B does not address the issue head-on, but the theory is consistently advertised as ‘constructionist’, and a few scattered comments indicate that B does not subscribe to a strict version of the compositionality principle (‘It is structure that assigns interpretation’, Vol. II, p. 245).⁸

The meaning of the whole might, in this work, be a function of the parts, the linguistic environment, and perhaps some limited world knowledge:

Only when the derivation is over, and the grammar has assigned interpretation to structures, can the conceptual packages be opened. At this point their phonological index can be matched with the appropriate phonological representation, and they are allowed to contribute their conceptual interpretational value (Vol. I, p. 12).

This will make it less easy to break structures down into their constituent parts, but I see no way in which it could move us very far away from semantics as usual, though now with a new starting point, c . The ‘making sense’ component (Vol. I, p. 11) seems familiar once reduced to a type-logical perspective.⁹

3.3 Technical details and technical notation

It is not easy to work through the technical details of SS. One could easily finish a careful reading of both books without feeling confident with the formalism. The challenges are largely textual. There is, for instance, no consistent policy regarding node labels for nonbranching structures (despite thoughtful comments on the matter; Vol. I, p. 44–45; Vol. II, p. 20). Maximal projections are sometimes XP and sometimes X^{\max} , and range assignment relations are sometimes given with numerical indices, other times with symbolic ones like \exists , sometimes with arrows, and sometimes not at all. The fused values introduced in Vol. I, §4, are not used consistently in Vol. I and not at all in Vol. II, though the configurations calling for them are extremely common in structures involving merger of the

⁸ As B discusses Vol. I, §1.2.1, the terms ‘constructionist’ and ‘neo-constructionist’ are used to cover a wide range of theories with sometimes quite different premises. For additional discussion, see Goldberg 1995, Ginzburg & Sag (2001), and Levin & Rappaport Hovav 2005.

⁹ It could be that years of doing semantics have left me blinkered, unable to see anything but relational and compositional interpretation. If that’s the case, then I can only say that I am eager to see a worked-out theory of interpretation that doesn’t fall into or near this area.

same item in a few places. When fused values are used, they are not indexed, nor is it easy to see how such indexing should work.

I should emphasize that I am not decrying notational simplifications, especially with trees. However, if one is to absorb a new formalism, one must read with obsessive attention to subtle details of the definitions, text, and examples. Inconsistencies can make such close-reading fraught.

Some crucial concepts are undetermined by the prose as well. To find empirically significant limitations in the theory, we need a clearer description of the feature percolation mechanisms involved in examples like (13) above, and we need to know more about the role that constraints on vacuous quantification play in restricting these relationships (e.g., Vol. I, p. 41; Vol. II, p. 299, 203; see Marsh & Partee 1984).

However, the hardest thing about the formal system of SS is that, as noted in the summary of Vol. I, §2, above, it is introduced gradually over the course of two volumes, with no full definitions to indicate how things have developed. Vol. I contains a summary list of range assignment configurations (p. 262–263), but those definitions hide the assumptions about feature percolation (see (13) above), vacuous quantification (e.g., Vol. I, p. 41; Vol. II, p. 299, 203), and scope-taking limitations (§5.2 and §4.3 below) that, implicitly or explicitly, support them. They do not include the sort of range-assignment through head–specifier agreement that we saw in (17) above. And they misleadingly suggest that indirect range assignment by a quantificational element is local to the phrase containing the open value in question (p. 263, (3b)). They also seem to say that head movement is part of the definition of range assignment, though this should certainly be regarded as a separate pressure.

The overall effect of this is that the reader feels uncertain about how to work with the formal system, test predictions, and make modifications. I thought it would be revealing, for instance, to look for semantic features that unify the class of range-assignment configurations, but I abandoned this effort when I realized that I wasn't sure I could get a grip on how much freedom is allowed in configurations involving long-distance dependencies and feature percolation from inside a DP (as in (17)). The third volume of *Structuring Sense* is still forthcoming, so we can hope that it contains a chapter or appendix that presents the theory with enough precision that the careful reader can get a firm grasp on this important aspect of the trilogy.

4 The limits of listeme freedom

As we've seen, SS argues for the fairly radical (though qualified) view that the lexicon contains almost no grammatical information. This is exactly the sort

of bold pronouncement that sets the theoretical linguist’s mind off in search of counterexamples — which is, I assume, part of the interest in making such claims in the first place. The present section hears this call and begins to answer it. I address just three issues, but they are diverse ones, and I close with some pointers to additional phenomena that look like they might prove to be significant challenges for the SS world-view.

4.1 Subcategorization

Kratzer (1996) shook things up inside the VP by proposing to remove the functional dependency between verbs and their syntactically external arguments. More precisely, if we pack all intensional (event, situation, etc.) arguments into τ , then verb meanings moved from type $(e \rightarrow (e \rightarrow \tau))$ to $(e \rightarrow \tau)$. It sounds like a small change, but it’s surprising stuff, especially to linguists with broadly Motagovian training. It’s the sort of change that reverberates all the way up the clausal spine.

To get a sense for why this is important, we should return to what Partee (1984) calls Keenan’s Functional Principle (Keenan 1974): ‘the (form and) interpretation of a function word may depend on the (form and) interpretation of its argument but not vice versa’ (Partee 1984:161). This gives us the ability to write (if we wish) a single lexical entry for a word like *flat* that returns different values depending on whether the overall phrase is *flat water*, or *flat beer*, and so forth; the following, adapted from Partee’s article, roughs out the sort of denotation we might end up with ($\llbracket \alpha \rrbracket$ is the meaning of the α):

$$(30) \quad \llbracket flat \rrbracket(f) = \begin{cases} \lambda x. f(x) \text{ and deflated}(x) & \text{if } f = \llbracket tire \rrbracket \\ \lambda x. f(x) \text{ and bubbleless}(x) & \text{if } f = \llbracket beer \rrbracket \\ \lambda x. f(x) \text{ and waveless}(x) & \text{if } f = \llbracket water \rrbracket \\ \dots & \end{cases}$$

Kratzer general objection is based on the idea that, while we find a lot of dependences of this sort between verbs and their internal arguments, we find comparatively few of them between verbs and their external arguments. The gap seems nonaccidental. It suggests that the verb does not apply semantically to the external argument.

In Vol. I, §2.4, B severs the verb’s *internal* arguments as well. ‘Argument interpretation is (at least up to a point) independent of the meaning of listemes’ (Vol. II, p. 69). The argument from Keenan’s Functional Principle does not play a major role in this line of reasoning. It is dealt with only in a footnote (p. 63, n. 29)). Curiously, the tack B takes there is to deny that the original

argument is valid. She does this by providing two examples in which the verb seems to select its subject (p. 64):

- (31) a. The wall touched the fence.
 b. Jane touched Amy
- (32) a. Sincerity frightened the boy.
 b. Amy frightened the boy.

I agree with B that we can find data that seem to support subject selection as well. Some additional examples are given in (33)–(37); in each case, the nature of the subject is a significant factor in determining the (preferred) sense of the verb, and at least the (a) cases plausibly involve subject selection.

- (33) a. 2008 found Chris in Amherst. (Events conspired . . .)
 b. The police found Chris in Amherst.
- (34) a. The weather determined the length of the trip. (The weather was a factor in how long the trip lasted.)
 b. The sailor determined the length of the trip. (The sailor did a calculation.)
- (35) a. The watch told the time. (The watch displayed the time.)
 b. The guy with the watch told the time. (He announced it.)
 c. The ancients told the time (using the position of the sun). (They calculated the time using the sun’s position.)
- (36) a. The problem was eating at Sally. (Sally was being annoyed by the problem.)
 b. The monster was eating at Sally. (Sally was being nibbled upon.)
- (37) a. Soft pretzels make good snacks. (Soft pretzels are scrumptious.)
 b. Germans make good snacks. (German baked goods are scrumptious.)

When I first compiled these, I regarded them as a challenge to [Kratzer’s](#) basic generalization. However, upon reflection, I sense that this is not an arbitrary collection. Each of the pairs seems to trade on changes in argument structure that should be grist for the SS mill. There is not space (nor do I have the expertise) to argue that each of these verbs can, in some sense, promote its internal argument to subjecthood on at least one of its readings ([Perlmutter & Postal 1984](#)), but

that line of reasoning seems plausible, and it might vindicate Kratzer's original claim.

If this is correct, though, then we are stuck with the dependencies between verbs and their internal arguments, as Kratzer (1996) has it. This might compromise, in some sense, the notion that lexical items are merely 'modifiers of structure', but it seems to me that it doesn't cut very deep. I see two different approaches one could take.

If we are willing to tolerate disjunction definitions like (30) for the meanings of functional elements (and the extreme amount of ambiguity for prepositions might demand that anyway), then we can always build these dependencies into those functional meanings. The result won't be pretty, but it will get the job done.

Alternatively, suppose that verbs in fact have internal arguments. Most of our conceptual knowledge involves at least the rudiments of argument structure. We shouldn't feel forced, even within the broad confines of SS, to endorse B's view that the patterns reviewed above are merely due to 'world knowledge' (p. 64).¹⁰

4.2 A closer look at proper names

One of the pleasures of Vol. I, §3, is that proper names are given their morphosemantic due. Rather than treating them as inert, compositionally uninteresting expressions, B places the full paradigm of proper-name realizations into the spotlight: *that Sally*, *the Sallys in the room*, *most Sallys*, and so forth, and the examples immediately put us in mind of *Sally-like*, *Joanily*, and so forth (though only the nominal cases are analyzed). These examples make it clear that proper names are first-class members of the nominal community, capable of much that common nouns are capable of.

However, B extends the argument one step too far, in my view. Recall from my summary of Vol. I, §3, above that she proposes to derive *the cat* (the feline) and *Cat* (proper name) from a single lexeme *cat*. This is an intriguing hypothesis, but it obscures important differences between the proper noun and common noun 'starting points' for the two structures. Let's suppose that we are dealing with *John* in its proper name instantiation. Then *Johns* picks out (something like) people who are called 'John', and *the John (I know)* refers to time slices or guises for John. These are rough approximations, but they suffice to highlight the contrasting interpretations that obtain if we begin from a common noun interpretation, say, *john* to mean toilet. Then the interpretations are somewhat

¹⁰ But see §5; perhaps we can accept that these patterns reduce to world knowledge but still regard them as worthy of close linguistic analysis.

easier to get a grip on: *the john* is the unique salient toilet (or else a weak definite; Carlson *et al.* 2006), and *johns* is a rough synonym for *toilets*.

B is aware that these meaning shifts strain the theory:

We do note, however, that when *John Smith*, or *cat*, merge a copy in D, they lose their ‘normal’, common name predicative function. Thus, *cat*, as a proper name, does not have the predicate ‘cat’ (i.e. a type of feline) as its extension, and the name *Wolfgang*, in German, does not have ‘wolf walk’ as its extension either. Thus once common names become restrictors of a discourse antecedent, this functional role entirely preempts their original encyclopedic meaning (Vol. I, p. 79).

However, the contrasts just observed between *John* and *john* show that the situation is worse than this. In order to get the meaning differences right for each of the two paradigms involving the string *john*, I had to assume that the noun started out with a particular category — common noun or proper name — and that this had *irrevocable* effects on how the surrounding functional structure worked. (This ambiguity is even reflected in the orthography: if we begin with the proper name *Sally*, then the plural is *Sallys*; if we begin from the common noun *sally*, then the plural is *sallies*.)

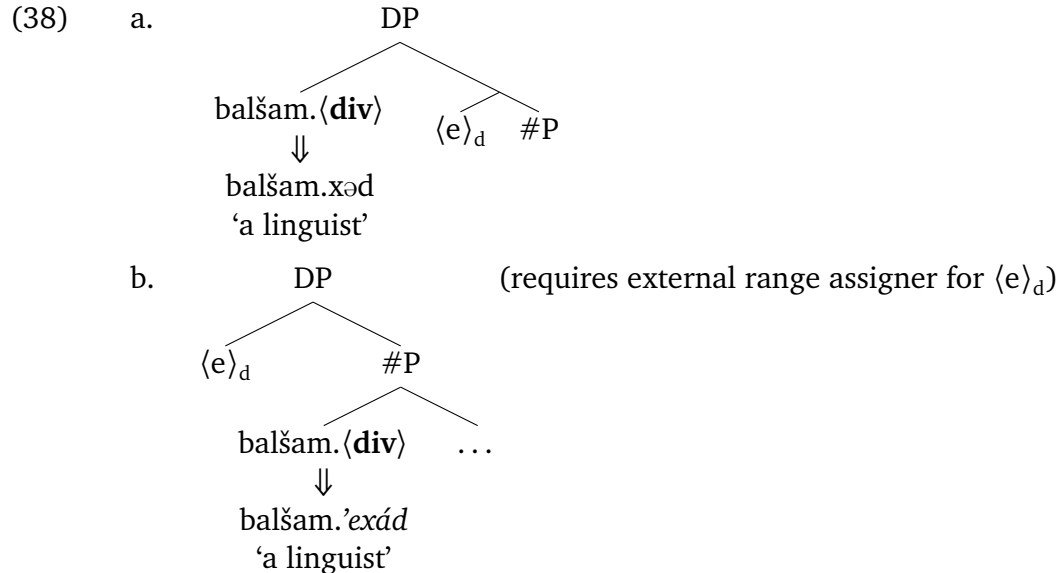
This is not to say that the germ of B’s initial insight is not useful here. The effects of structure are found even after we settle on a proper name or a common noun denotation. Adding a determiner can contribute a layer of expressive meaning (Vol. I, p. 85; and see also Kaplan 1999; Aoun & Choueiri 2000; Potts 2007).¹¹ The effects of pluralizing proper names are highly variable, particularly if we consider instances in which a post-nominal modifier appears (*the Brooklyn of my youth*; *the Kate I know*). And so forth. (For additional discussion, see Vol. I, §3.2.2.) B’s insight about the way functional material shapes the morphosemantics certainly survives my criticism, which shows only that some ambiguities are best left to (accidental or historical) lexical homophony.

4.3 Scope

As I described above, Vol. I, §5, begins the discussion of Hebrew indefinites that continues in Vol. I, §7, 8. Ch. 5 is concerned primarily with the two indefinite markers *xit* and *’exád*. The first must take widest scope, and the second must

¹¹ We need an explanation for why English proper names with definite articles differ semantically from those without them, whereas (for example) Modern Greek proper names require definite articles and some dialects of German optionally allow them without a change in meaning. Vol. I, p. 85, offers a few relevant remarks.

take narrowest scope. The second half of §5.2 seeks to connect these limitations with the internal structure of DP, in particular, with the way range is assigned to $\langle e \rangle_d$. This comes down to two demands: (i) *xit* must assign range to $\langle e \rangle_d$, and (ii) *'exád* cannot assign range to $\langle e \rangle_d$, in which case an external element (existential-closure or an adverbial) takes over, as described in (12b) above. Here are the relevant parts of structures (41) and (42) from Vol. II, p. 157–158:



I have no objection to these structures, but I do not see how they help address the scope-taking limitations that B adduces for these lexical items. Indeed, if existential closure is free to happen at any level in the clause, then the external binding option (38b) will, absent other restrictions, deliver a wide range of readings. If existential closure is strictly local (as one might sense from Vol. I, §2), then only the narrowest-scope reading will arise (absent something like quantifier raising). And (38b) will deliver surface (narrowest) scope unless supplemented by additional mechanisms governing where and how indefinites can take scope with respect to the material around them. The range of readings thus predicted is largely at odds with the observed behavior for these indefinites.¹²

I should say that it is not clear to me how much of the scope puzzle B takes the above structures to address. The relevant passage opens with a broad criticism of certain semantic approaches, but then it switches gears suddenly:

It is thus clear that any approach which seeks to reduce the scopal properties of indefinites to a single structure, with a variable inter-

¹² It is, though, very much akin to the patterns uncovered by Chung & Ladusaw (2004:§2) for Maori indefinites; see also Farkas 1997.

preted freely through existential closure operators, is insufficient to account for the behaviour of specificity markers such as *xit*. On the other hand, if we assume that *xit* signifies the assignment of range to $\langle e \rangle_d$, possibly over and above range assignment to $\langle e \rangle_{DIV}$ and $\langle e \rangle_{\#}$, as is the case for other singulars, the emerging structure will be as in (41) [...] (Vol. I, p. 157).

One expects the material after ‘On the other hand’ to explain how the present proposal captures the scope distinctions. Significantly, it heads in a slightly different direction.

How might we go about capturing the pattern of scope-taking for *xit* and *exád*? It seems to me that the structural contrasts will take us only so far. The task is likely to require an innovative approach to the semantics of indefinites. There is no shortage of such proposals; in addition to the references by Chung & Ladusaw and Farkas just cited, see Reinhart 1997, Winter 1997, Schwarzschild 2002, and Kratzer 1998. B challenges some of this work in passing (see Vol. I, p. 147, 149), but it strikes me as viable to pursue it, perhaps in conjunction with the above structures. My point is simply that the largely syntactic notions explored in SS do not deliver the semantic contrasts that seem to be at the heart of the issue.

4.4 Working with SS

Where else might we find the lexicon or the denotational semantics working harder than SS would lead us to expect? I’m not able to pursue this question much further here, but it is worth reconsidering other lexicalized patterns with the functional lexicon in mind. What should we make of the rich typology of adjectival denotations explored by Kamp & Partee (1995) and helpfully summarized by Partee (1995)? It seems to be a lexical fact that *former* denotes a particular kind of intensional modifier, and this seems intimately connected with its distributional limitations (e.g., **The major is former*). Similarly, Kennedy & McNally (2005) discuss patterns for gradable modifiers. They arrive at a semantic typology, but we see the effects of that typology in patterns of morphosyntactic grammaticality. Adverbs must often arrive in a fixed order in the clausal structure (Ernst 2002). And Schwarzschild (2007) identifies a class of ‘stubbornly distributive predicates’, which block collective readings for their arguments. These patterns *seem* to be fueled by the particularities of specific lexical items (or classes of them). Can they sensible and profitably be reduced to the interactions of functional elements?

5 Convention

In thinking about the grand themes of SS, it might be instructive for readers to set the books next to [Levin 1993](#), which is basically an extensive classification of English verbs, supplemented by an index that reveals the highly interconnected nature of the typology. It's a goldmine for researchers looking at lexicalization patterns and their effects on clausal syntax. What is the status of those patterns according to the theory presented in SS?

The programmatic statements near the start of each volume seem to say fairly directly that [Levin 1993](#)-style classifications are not grammatical patterns at all, but rather only a reflection of the world we inhabit and its regularities. All that should be easy enough for grammatical morphemes to override:

although our perception of the world might lead us to prefer some structural combination over another in conjunction with particular concepts, considerations of world knowledge, it has already been suggested, can be and routinely are overridden by grammatical factors (Vol. I, p. 102).

However, it's important to see that this is not really the view that B advocates (as far as I can tell). For instance, in the final pages of Vol. II, she addresses the fact that, when we look beyond English and beyond a few choice examples, we find that some lexical items just won't budge. 'The XS system, then, overgenerates', she writes (p. 347). Similar themes run through Vol. II especially; the footnote on Vol. II, p. 70, helpfully summarizes some of the important qualifications in this area. More lexically-driven theories of grammatical patterns — type-shifting and lexical rules in particular — come in for a hard time throughout SS (see especially Vol. I, p. 142; Vol. I, p. 158–159; Vol. II, p. 70), but all these theories are, like SS, driven by the common goal of finding deep and surprising patterns in the lexicon. I find the following to be the most telling as to the true nature of SS's claim about lexical impoverishment:

Broadly speaking, then, to acquire a conventional knowledge of a listeme is to have narrowed down the range of its associations with possible conceptual properties sufficiently so as to allow the conceptual exclusion of some computational outputs (Vol. II, p. 251).

This seems exactly right to me, and it strikes me as exactly the sort of thing that linguists should study and develop theories of ([Clark & Clark 1979:809](#)). It's especially important to pursue such conventionalization (and the principles that govern it) if we want linguistics to have a home in a full-fledged theory of

cognition. Language is, after all, a complex symbolic system used by humans to navigate the world and its information. Though SS is advertised in a way that makes these patterns seem incidental, readers will be pleasantly surprised to find that it harbors genuine insights into how linguistic theory might capture them.

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