Comments on February 6th dissertation draft, "Lexical flexibility in discourse: a quantitative corpus-based approach" - Bill Croft

The dissertation draft is very well-written, very clear, and a pleasure to read. An enormous amount of effort has been invested in the data curating and analysis: in particular retyping all of the Nuuchahnulth texts and coding them, and processing and manually coding a far larger corpus of English. The proposed metric for lexical flexibility is a good one, and the comparison of raw frequencies with corpus dispersion is useful. The introductory theoretical chapter—the longest chapter in the dissertation—provides a very good overview of the issues.

Having said this, however, there are a number of important theoretical issues that I have with the framing of the dissertation topic. I believe these need to be addressed to some degree. These would also have consequences for what the empirical study can tell us about the theoretical issues. It is clear that properly addressing these issues would require major revisions of the dissertation—the empirical study itself, not just the text of the dissertation. I raise the issues here, starting with the biggest issues and going to lesser ones. I also lay out what I think are at least the minimum revisions I would like to see in the final draft. (Page references are to the pdf version of the dissertation draft.)

The biggest issue is implicit in the description of the phenomenon being examined as 'lexical flexibility'. This description as *lexical* implies that this is fundamentally a phenomenon about *words* or *word classes*. But the dissertation is quite critical of approaches that assume that this is fundamentally a phenomenon about words. It rejects those approaches and endorses a Radical Construction Grammar analysis (see Croft 2001, henceforth *RCG*), and many of the RCG criticisms of word-class-based approaches. Unsurprisingly, I agree. But I think that the dissertation hasn't really abandoned a word-class-based approach to the degree that it should, which leads to some confusing and/or contradictory statements.

Here are a couple of basic examples. On p. 120, Nakayama is quoted to say that he 'treats "adjectivals" as a subclass of verbs' in Nuuchahnulth, without comment. Yet this is problematic in at least two respects. First, the use of terms like 'adjectival' and 'verb' assumes that language-specific word classes are comparative concepts—a position that was argued against in chapter 2. Second, 'class' vs. 'subclass' employs methodological opportunism, something which was also argued against (specifically with respect to class vs. subclass) in chapter 2.

Another example is on p. 135:

> 'In a corpus analysis of English and Mandarin, Thompson finds that property words have primarily two functions in discourse: to introduce new discourse-manipulable referents, and to predicate attributes of an already-known referent. In English these two functions are realized via attributive adjectives and predicative adjectives respectively.'

Not really: these two functions are expressed by attributive (modifier) constructions and predication constructions respectively. These are not properties of words; these are properties of constructions.

The phenomenon that the dissertation is examining is primarily about *constructions*, not words or word classes; it is only about word classes (or individual words) in that their distribution is defined by constructions. This is the basic tenet of Radical Construction Grammar, and in many parts of the dissertation this is asserted or assumed.

The definition offered for the term 'lexical flexibility' is more construction-based, though it is still word-biased. Lexical flexibility is defined as 'the use of a lexical item in more than one discourse function (reference, predication, or modification) with no overt derivational morphology' (139). The definition is not quite accurate as to what the dissertation actually does. In *Sally is a professor* and *Sally is smart, professor* and *smart* are used in the predication function with no overt derivational morphology. The dissertation analysis excludes "predicate adjectives" and "predicate nominals" from "lexical flexibility" because 'most researchers would consider these to be instances of morphologically marked conversion in English' (93).

Referring only to derivational morphology in the definition is too restrictive—but also an indication of the word-biased approach, since morphological derivation creates new words, but using a copula in predication does not. The more general notion is *overt coding* of the discourse function in the predicate adjectival and predicate nominal *constructions*, as opposed to *zero coding*—two different strategies for expressing the function. A more accurate definition of "lexical flexibility" is 'the use of a lexical item in *constructions* expressing more than one discourse function and employing a *zero coding strategy*'. (For the distinction between 'construction' and 'strategy', which goes back to the early days of modern syntactic typology, see Croft 2014, 2016, to appear [henceforth *MS*].)

So the phenomenon of lexical flexibility has to do not only with the words being used in the three discourse functions, but the constructions that express those functions, specifically the constructions that use a zero coding strategy (no derivational affixes, no copulas, no overtly coded deranked forms like gerunds, participles, infinitives, etc. [Stassen 1985, 2009; *MS*]). And yet, there is no section of the dissertation that presents to the reader the reference, predication and modification constructions of English and Nuuchahnulth.

My first minimum recommendation is to remedy this:

•Discuss the reference, predication and modification constructions of both English and Nuuchahnulth in chapter 3 of the dissertation, lavishly illustrated with examples.

(The one thing that really struck me in reading this dissertation is how few examples there are—and many of the relevant examples are shunted into an appendix [Appendix C].

There really need to be more examples, and the examples in Appendix C should be moved to chapter 3.)

There is some unclarity about constructions in the current draft of the dissertation. This is manifested in the following paragraph:

> 'Nuuchahnulth, on the other hand, has no morphology dedicated to modification, just a conventionalized syntactic construction in which the modifier precedes its head. Some linguists analyze Nuuchahnulth as lacking an adjective class entirely (Nakayama 2001). Since English has dedicated modifying constructions, speakers make use of those constructions for prototypical property words. But since Nuuchahnulth does not have dedicated modifying constructions, speakers avail themselves of other strategies that vary depending on the discourse context.' (146)

To me, the statement that 'Nuuchahnulth does not have dedicated modifying constructions' is incorrect (see also p134). Nuuchahnulth's modifying construction was described just above: 'a conventionalized syntactic construction in which the modifier precedes its head'. That is a dedicated morphosyntactic construction. The fact that there is no morphological inflection or derivation doesn't mean it's not a construction—at most, it is confusing the discourse function of modification with associated constructions such as degree, indexation, etc. This statement also appears to be confusing the existence of a construction—a morphosyntactic structure that expresses a function—and a strategy used to encode that function. That is, it appears to assume that the use of a zero strategy of a function represents the absence of a construction for that function. Similar problematic statements are made in §5.5 at the very end of the dissertation.

The quoted passage also leaves aside the strategy of "compounding", in the morphophonological sense of a phonologically bound combination of two morphemes, which plays a significant role in Nuuchahnulth. There is some discussion of this in terms of alternative strategies for the discourse function of modification for property concepts in Nuuchahnulth, particularly the use of Lexical Affixes (I am capitalizing this label since it is language-specific). In reading Nakayama on Nuuchahnulth Lexical Affixes, it is clear that describing this construction and its relation to the aforementioned zero-coded pre-head modifier construction is not easy. The reader who is interested in understanding or perhaps replicating or modifying the empirical study in the dissertation needs an overview of the relevant constructions.

Unfortunately, constructions are far harder to extract from a corpus than words. This is another reason why the corpus study is word-oriented, despite the use of a largely (radical) construction grammar approach to the phenomenon. The constructions need to be coded manually, which was a central part of the dissertation research effort.

And of course, for any quantitative study, perhaps the single most important thing is how the (qualitative) analysis of the data into coding categories is done, and why. The 'why' is covered reasonably well in the dissertation (despite my quibbles and concerns above). But the 'how' is not, namely, what decisions were made, how theoretically-motivated

categories (like reference, predication, modification) were operationalized, and how to deal with borderline cases. Section 3.3 is much too short. This is my second minimum recommendation:

•Explain in detail how the coding categories are defined, especially in the case of borderline cases, again with many examples, in section 3.3.

Of course, this explanation will be easier once the reader has been introduced to the reference, predication and modification constructions of English and Nuuchahnulth (my first recommendation).

Here is an example of what sort of issues need to be explained to the reader, one that can be inferred from the small number of examples in the current draft. On pp. 92-93, it is said that so-called "compounds" are excluded, that is, *back* in *back yard, hard back book* and *back burner* were excluded. But in Appendix C, *street* in *street clothes* is included, and coded as a modifier (165). How are "compounds" and "nouns functioning as modifiers" differentiated? Can they really be differentiated? See *MS*, §5.2.1 on typifying modification constructions (Koptjevskaja-Tamm 2013) and Pepper (2020) on binominal lexemes, also discussed in *MS*, §5.2.1.

This just happens to be an issue where there were examples in the current draft that led me to notice this. But when I started to look at the Nuuchahnulth examples, I felt I had little understanding of how the discourse functions were coded. Discourse functions in another language are at least partly identifiable by the English translations, that is, the translations to some extent reflect the information packaging of the original language utterance. Of course, it's not perfect, and the linguist may not be thinking about how they match. But there are a lot of examples where I don't understand the coding. For example, why is 'quite a while ago' (part of?) the predicate in the first line? Why is *hitac'u* a "Name"? (the translation would usually be called a secondary predicate or a predicate complement — actually, something in between a modifier and predicate; see *MS*, §14.3). I think that simply going through the whole passage in Appendix C.2 and explaining and justifying your coding would be very helpful to the reader — and do this in section 3.3, not an appendix.

Up to now, I have referred to the discourse function constructions with zero coding, and the words that occur in the relevant role in those constructions. This is of course what the dissertation aims to explore. In the word-centric language of the dissertation, it is said, shortly after the definition of "lexical flexibility" quoted above,

> 'But to lump flexible forms in with overtly derived forms ignores the fact that there is something unique about them—namely that they can appear in different discourse functions with no overt derivational morphology. Just how prevalent is this phenomenon? Why do these words in particular behave this way while others do not? How productive is it? Are the meaning shifts that occur in functional shift different from or the same as the meaning shifts that occur in cases of overt derivation? An attitude that treats flexible forms as a problem to be solved preempts these kinds of

questions—or at least shifts focus away from them. Regardless of one's theoretical analysis of flexible forms, their behavior is substantively different from non-flexible ones, and this fact merits investigation.' (140-41)

Actually, zero vs. overt coding as alternative strategies for constructions is not that unique. It is found all over the constructions of languages, as can be seen from skimming *MS* for example. So the current study needs to be thought of in that context. For example, some languages are happy to use the same word to mean 'to be PROPERTY' and 'to become PROPERTY', while others are quite strict, using overt derivation for the inchoative meaning or the resultative meaning. And some languages are happy to use the same predicate form no matter what the valency, while others are quite strict, requiring for example causative or anticausative morphology to change transitivity. Yet the response of theoretical linguists to the variation between zero and overt coding in aspect and valency is rather different to the response of theoretical linguists to zero vs. overt coding in discourse function constructions.

But the assertion in the last sentence of the quoted passage, 'regardless of one's theoretical analysis of flexible forms, their behavior is substantively different from non-flexible ones' has not been demonstrated in this dissertation. (And the opposite is suggested on p81: 'This would suggest that functional expansion [= lexical flexibility] follows the same principles as overt derivation'.) Worse, the current study in this dissertation cannot demonstrate it, because it examines only "flexibility", that is, words that occur in zero-coded constructions expressing more than one discourse function. The only way one can demonstrate the assertion in the last sentence is to compare such words to words that occur in overtly-coded constructions expressing those same discourse functions. The question in the middle of the passage, 'Are the meaning shifts that occur in functional shift different from or the same as the meaning shifts that occur in cases of overt derivation?', is definitely the right question to ask. But one has to look at the overtly coded constructions as well in order to answer that question.

Obviously, adding overtly coded constructions to the empirical study in the dissertation is not practical in the short time available. So my minimum recommendations here are:

•Include constructions using overt coding strategies in teh presentation of the reference, predication and modification constructions of both English and Nuuchahnulth in chapter 3 of the dissertation, again lavishly illustrated with examples.

•Acknowledge the limitation of looking only at zero coded constructions without comparison to overtly coded constructions.

I think that the limitations of not comparing overtly coded constructions should be raised in chapter 2 or maybe chapter 3 where it is made clear that the focus of the empirical study in the dissertation is zero-coded constructions only. But the point can be raised again in §5.4 with suggestions for future research.

The last major issue has to do with semantic shift. I agree with this statement, of course:

> 'Methodological opportunism, in which researchers select the definitional criteria for lexical categories that best support their theoretical commitments while dismissing or deemphasizing contradictory criteria (Croft 2001b: 30), is a rampant problem in research on word classes.' (140)

I hate to say this, but one could argue that the dissertation could also be accused of methodological opportunism, since it dismisses or at least sets aside semantic shift, which seems to be treated as not important to the goals of the dissertation. I strongly disagree with this. First, of course, in the functional-typological analysis of parts of speech that I propose, the semantic type of the word is as important as the discourse function of the construction. So I predict that cross-linguistically, 'paint' in *This is paint* displays morphosyntactically different patterns than 'paint' in *Carol painted the bathroom last weekend*, even though both represent predication and both involve paint in some way. The dissertation discusses this to some extent; I will return to this below.

Second, for any constructional approach, form-function pairings are fundamental for any grammatical unit, i.e. [*paint*] as well as [SBJ *be* X] or [SBJ X-TNS OBJ]. The dissertation basically looks only at word *forms* without meaning. This structuralist-formalist approach to word forms sits rather uncomfortably with the largely functional-typological-constructional approach adopted in the dissertation.

As a result, there is hardly any discussion of the meanings associated with word forms in the dissertation. For example, in the 100-item samples in Appendix B, in the English table there is no discussion of the meaning variation. It is assumed that an English speaker's native intuitions are sufficient (but not all readers of the dissertation will be native English speakers). And even for myself, a native speaker, I was stumped with the mention in passing in the dissertation text that *know* occurs in a zero-coded reference construction. The only example I can think of is the fixed multiword expression *in the know*. Appendix B.1 says there were 7 instances in the 100-item English sample. Were they all *in the know*? Anyway, this shows that even a native speaker won't always be able to recognize uses of a word in every discourse function construction, including their semantic shifts. Another example is from Table 4.3, p131: I can't think of what the 1,345 occurrences of *little* in reference are. *A little* in the sense of 'not very much'? If so, I'm not sure I'd call this reference.

In the Nuuchahnulth table in Appendix B there is just a single "Gloss" given, which also provides no information about the range of semantic shifts. I looked up one example in Stonham's dictionary, ʔuˑq 'good' because it was the one word that was equally distributed in the three functions (110). Unfortunately, the dictionary gave just complex words containing ʔuˑq, not syntactic examples.

Based on these observations, my minimum recommendation is:

•Indicate somehow the range of semantic shift of the words examined in the study.

I know that doing this comprehensively is impossible; it was stated somewhere in the dissertation that this data was not collected. All that can be done is some representative illustration.

For example, some zero-coded constructions across languages have a general (i.e. productive) semantic shift. For example, a property concept word or action concept word plopped into a zero-coded reference construction often shifts from denoting the property or action to denoting a person or thing that possesses the property or plays a core role in the action. I believe this is the case for Nuuchahnulth. Another common semantic shift is for a property concept word plopped into a zero-coded predication construction to denote 'become [PROPERTY]'. I don't know if this is the case for Nuuchahnulth. Some of these semantic shifts might be mentioned in the presentation of the discourse function constructions of Nuuchahnulth (or, for that matter, English; see below).

Also, examples could be given of semantic shifts discussed in the dissertation. For example, the examples containing the three occurrences of ʔuˑq 'good' could be given, with a brief comment of what the word means in the three different discourse function constructions attested in the corpus; and a couple of the examples of *know* in a reference construction; and so on.

Another issue with semantics, even if it is not explored in the dissertation, is the defining of terms. The section of the current dissertation with the most in-depth discussion including actual examples is the discussion of "property modification" in Nuuchahnulth in section 4.6.2. I was surprised to see that many of the "properties" are numerals and quantifiers, in both the examples and Table 4.4 (see also p 21). Table 4.4 lists a wide variety of semantic types of modifiers, only a minority of which are usually called properties (for a classification of semantic types of modifiers, see *MS,* Table 4.1, page 95). I would definitely not include numerals or quantifiers as "prototypical modifiers", or even 'proper' (example 38a, page 135).

I am not sure what recommendation to make here. Certainly some explicit definitions of terms like 'property concept', 'object concept' and 'action concept', in part because they don't appear to be the same as the definitions in my 1991 book, which I think are more "standard" (or at least more precise). I might suggest classifying the concepts in the 100-item lists; but I already suggested that those concepts should be revised to represent the semantic range of the words.

I have been critical of the dissertation's ignoring of semantic shift, in part because it is important to the Radical Construction Grammar analysis of parts of speech that I have argued for. Of course, the dissertation doesn't ignore this, and discusses why semantics is ignored in section 2.3.3.4. But I found this counterargument a little odd. It seems to be saying that one can ignore semantic shift because every use of a word involves a slight difference of meaning. I totally agree with this; see Croft 2000 (especially chapter 4); Croft and Cruse 2004 (especially chapter 4; Alan Cruse wrote that chapter, but I fully endorse what he said); and Croft (2010:11-12). Even in my 1991 book, I pointed out that

object concept predication involves some degree of semantic shift, to a predicational or equational interpretation, because object concepts are nonrelational but predication is relational (predicated of its argument(s)).

But what matters is how much of a semantic shift is there, and in what direction, and how conventionalized it is. (This is mentioned only in a throwaway comment at the end of section 2.3.3.4; page 67.) Some discourse function constructions conventionalize a semantic shift, for example from a property to an object possessing that property. In other cases, a substantial semantic shift may have originated with a word denoting a concept being used in a discourse function construction not prototypical for that concept; but the semantic shift has become conventionally associated with that word in that (formerly nonprototypical) discourse function construction.

There are two classic studies that discuss semantic shift in discourse function constructions in (mostly) language use: Downing (1977) and Clark and Clark (1979) (I include Downing here because I believe "compounds" cannot be clearly distinguishing from typifying object modifier constructions; see comments above). These could be cited and briefly discussed in chapter 2 to show a way to deal with the complexity of semantic shift in this domain.

In both these cases, constructional and lexical, there is a conventionalized semantic shift. This is another important continuum apart from degree of semantic shift. There is a continuum from the pragmatics of the communicative situation to the conventions of the speech community. "Lexical flexibility" seems to be often interpreted to mean that a speaker can use the "flexible" word—in extremis, the entire lexicon—in any discourse function construction to mean whatever, and the hearer will successfully interpret what the speaker meant (cf. page 81, bottom). Such events happen; this is probably how some semantic shifts of the type described in the preceding paragraph occur. But these events may or may not become conventions of the speech community, and conventionalization is a gradual process (even if it is sometimes rapid, as with *text* as a predicate). Every word has that potential, but what matters is if that potential is actualized, and how widely it is in the speech community. Conventionalization is by definition a restriction of flexibility (see Croft 2000, chapter 4). The issue is not so much whether every language can use words flexibly—semantically flexibly, not just discourse-functionally flexibly—but what is the interplay between flexibility or innovation and conventionalization or "rigidification".

So I am not sure what we can infer from a corpus-based quantitative study of "lexical flexibility" of the type done for this dissertation, i.e. without reference to degrees of semantic shift or degrees of conventionalization. The latter in particular is not easily extractable from corpora, at least not a corpus as small as the Nuuchahnulth corpus. One can probably safely infer that the vast majority of uses of words in the corpus conform to conventions of the speech community (somewhat simplistically stated); but it is difficult if not impossible to identify the small fraction of the uses which may represent semantically large innovations in the corpus. Conventionalizations are sedimentations of innovation events over the history of the speech community; they have a historical depth

and do not simply characterize synchronic characteristics of the language (in other words, I don't accept the synchrony/diachrony dichotomy implied near the top of page 82).

This leads me to my final minimum recommendations:

•Discuss at greater length the limitations on conclusions that can be drawn in a study that doesn't describe or try to measure degrees of semantic shift.

•Discuss the relationship between "flexibility" and conventionalization, and also the limitations on conclusions that can be drawn in a corpus study where conventionalization of uses of words may be difficult to infer.

I think that these discussions have to occur earlier in the dissertation than in section 5.4. They are probably best put in chapter 2, with §2.3.3.2-2.3.3.4 restructured to make clearer the distinct yet interrelated continua of degree of semantic shift and degree of conventionalization.

There are also a number of minor points, and at least one not so minor point, that should be straightforward to address.

Minor points

p2, fn 1: this is too important to put in a footnote; integrate into the main text.

p7, displaying lexical flexibility: didn't Evans and Osada (2005) also say this?

p20, bottom: actually, modifiers in general are infrequent in discourse compared to referent expressions and predicates; it's not just Nuuchahnulth.

p37: if you prefer *functional expansion*, why don't you use it? I give a lot of reasons above why *lexical flexibility* is a bad description of the phenomenon.

p.38: "adverb" is even bad as a word class; better just to talk about the semantic classes that are frequently called "adverbs"

§2.3.2: this is one of the few really problematic sections in terms of comprehensibility. Another consequence of the word-bias in parts of the dissertation is the difficulty of interpreting the "key findings" described in §2.3.2. The "key findings" are intended to be cross-linguistic generalizations that everyone can agree on. But some of the "key findings" don't make any sense in a constructional approach, so we don't know what the real empirical generalizations are, or even whether they are real empirical generalizations. This is particularly true of the so-called "part of speech hierarchy" in §2.3.2.1. This is cast purely in terms of word classes, which are language-specific and so the labels attached to them aren't really comparable across languages. I critiqued this in *RCG*, chapter 2. Hengeveld wasn't totally opportunistic in the constructions he chose to identify word classes across languages. But I don't think it can straightforwardly be reinterpreted

in a cross-linguistically valid fashion, because of some of the issues I raised in *RCG*. I think the observations in §2.3.2.2 are too fragmentary to call "key findings". Regarding "level" for flexibility (§2.3.2.3), the "key finding" is just that it varies. I also have doubts about separating "morphology" from "syntax" as seems to be done here. Finally, item-specificity (§2.3.2.4) is another way to say that there are no generalizations, that is, there is arbitrariness—a good antidote to some of the more simplistic theories about "lexical flexibility", but something is found everywhere in grammar.

p75, 'semantically more complex' - I don't consider this a characteristic of typological markedness, and I argue against it in *Typology and Universals*

p81, 'Croft admits this possibility explicitly' but I don't think it's *right*

p83 top, Cayuga 'horse' - I think you may have put the cart before the horse here, so to speak: the speaker's intention most likely comes before formulation

p116, 'When lexical items in English and Nuuchahnulth exhibit flexibility, it is typically not to a marginal degree' - this strongly implies semantically distant, distinct uses or senses—conventionalization, rather than "flexibility"

p118, Figure 4.9, Nuuchahnulth - if more than half of Nuuchahnulth words are inflexible, why aren't there most of the dots piled up on the corners of the triangle? (or at least the reference and predication corners) On page 119, you say Nuuchahnulth displays 'rampant flexibility along the reference-predication axis'. How rampant is it if more than half of Nuuchahnulth words are inflexible?
Also, this is probably the point where I should ask, how does the fact that object predication and action reference are so much rarer than action predication and object reference (see Croft 1991) affect the quantitative results?

p122 - it might be useful to compare Nuuchahnulth with an English plot of similar corpus size, to see how similar/different the plots are for the two languages

p128, interaction of high frequency and flexibility: is this because higher-frequency items are more likely to occur in non-prototypical functions at all in such a small sample (see second point under p118)?

p129, top - good, but I fear that this shows that some of the results for Nuuchahnulth are due to small sample size rather than some linguistically interesting difference

p131, Table 4.3 - this would be more useful if examples were given of sentences with each use

p132, end of §4.6.1 - it could be that the "highest-flexibility words" are word forms with discrete senses in each discourse function, and so word-form frequency is really the sum of multiple word sense frequencies.

p134, ex 37a-b - did you exclude these cases from your count? One could argue that these are instances of zero-coded, albeit phonologically bound, modification. But this should be discussed in your coding criteria, as I recommended.

p144, 2nd paragraph - as I wrote above, it is kind of contradictory to talk about "conventionalized flexibility".

p146, 'When property words are used to introduce a new referent into the discourse, referring constructions are used (usually with the definite suffix *ʔi*)' - this is no different from English; modifier constructions are modifiers in referring expressions

p148, middle: Lexemes should also be described in terms of the semantic shifts they undergo in the different contexts, including the possibility of divergence/split

p148, 'Speakers know the range of contexts that a given form may appear in and use it that way' - this is probably due largely to their knowledge of the semantic shifts of the lexeme

pp150-51, looking at other languages: actually, it is *less* interesting to look at a theoretically-biased selection of languages such as this list, rather than a stratified sample of languages that are likely to represent a broader range of types

p152, looking at "flexibility" of corpora over time: actually, it would be better to look at the evolution of discourse function constructions over time, or derivational constructions over time. This would of course also involve looking at the words occurring in those constructions over time

p153 - it's not about a language developing dedicated constructions for different discourse functions; it's about developing multiple constructions for the same discourse function, some using zero coding and others using overt coding.

Appendix A - this is short enough to go into the text (though I'd keep the timeline)

Appendix C - as noted above, these examples all belong in the text

Typos etc.

p50: Hopper & Thompson (1984:745) => (Hopper & Thompson 1984:745)
p51: an action is used as a predicate => do you mean 'as a referent expression'?
p51, bottom: I don't understand why 'ho such metaphor exists for verbalizations'
p54, Hopper & Traugott 2003: do you mean Traugott and Trousdale 2013? Traugott didn't develop her theory of constructionalization till recently
p70, 'semantic space' - I called this a 'conceptual space' in *RCG*, and the term 'semantic space' is a lot older than Finch 2003 (admittedly, a dictionary)
p77: 'restate the dissertation' - ??
p77, in non-prototypical uses = in which non-prototypical uses

p77, next line - remove ‘semantically’ (see Minor comments, p75)
p79, ‘will *always* be marked semantically’ - see Minor comments, p75; this ain’t me
p89, token in (31) => the token in (31)
p96, an referent => a referent
p96, in order to prosodic => in order to take prosodic
p106, *ternary plot* - please give references for the terms used
p130, adopt in dissertation => adopt in the dissertation
p134, flexibile => flexible
p139, abstract: and the considers => and considers
p143, flexibility ~0.6 => flexibility (~0.6)
p143, preicsely => precisely
p143, corpus examined R2. => corpus examined.
p147, flexiiblity => flexibility
p181, *t. givón => T. Givón*

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