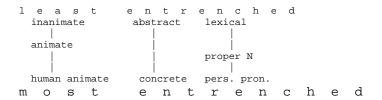
- p. 3, 12: "Does the extent of lexical flexibility observed for a language correlate with size of corpus? Does it correlate with the token frequency of the lexeme? (Chapter 2)" -> Not sure I'm getting how this is an interesting question (at least when formulated like that). So, you have *shoes* as a noun in (1a) and *shoe* in (1b); let's assume in the SBCAE, shoe is used 3/4 as a noun and 1/4 as a verb now why is there an expectation that those proportions change with corpus size and why would that be interesting? Is corpus size here a not-so-well-named proxy for something more long the lines of corpus diversity or register plurality? Then, ok, maybe, but corpus size per se I don't see what that might do/explain. p. 3, 12: in that combination: Regardless of what you want to do with corpus size and even more so if you mean something other than mere size, I think the token frequency of the lexeme is insufficient. In fact, I myself have shown in the last few years in a variety of papers that corpus dispersion is often more predictive than corpus frequency. One of the best examples: the words enormous and staining are equally long and equally frequent in the Brown corpus (37 times), but the former shows up in 36 parts of the 500 Brown corpus parts, the latter in 1! And this effect is particularly pronounced in middle frequencies of the type you're likely to be interested in. Thus, while Bybee and others don't get that, dispersion or contextual diversity have been shown time and again to be more relevant predictors of things that usage-based linguistics has ascrined to frequency.
- p. 4: "Do certain semantic domains tend to exhibit more lexical flexibility than others? Is lexical flexibility sensitive to the animacy hierarchy? Are property concepts more flexible than time-stable items and/or events?" question 3 seems to be a more precise version of question 2 do you have specific hypotheses here and, more importantly, do they lead to falsifiable predictions? If yes, make sure you state them (either there or later of course).
- p. 4: "Does the current and/or previous choice of grammatical role for a lexeme correlate with choice of lexical category?" -> Are you asking whether, for instance, whether the fact that *shoe* is used in a subject slot increases its probability of being a noun??
- p. 4: "Does information status (given vs. new vs. activated) correlate with choice of lexical category?" -> Why would it? (UPDATE: p. 10f. talks a bit about that, ok.)
- I realize that you s're saying that these things will be discussed below, but a little more guidance for the reader would be nice so as to not have ppl be annoyed at having to keep all this in memory for later.
- p. 5: "This structuralist approach to lexical categories, which came to be known as the distributional method (Harris 1951:5), constituted a major advance in the typological study of parts of speech, and essentially became **the sole method of syntactic analysis in modern linguistics** (Croft 2001:11)." (my emphasis) -> This seems like a reasonable statement to make given your committee members, but it is just as reasonable when you apply for a job where there's 1 functionalist, one generativist, and one literature person on the committee?

- p. 5, 10: "prototypal" -> "prototypical"
- p. 13: what idea of normalization (for different corpus sizes etc.) do you have in mind wouldn't a measure such as entropy (of counts per item, perhaps smoothed) not be enough?
- p. 15: I have some severe problems with the topicality hierarchy and I am the best person to admit that. ;-) In a nutshell, many versions of the hierarchy are kinda crap because the notion of hierarchy implies that it's a unidimensional scale along which items are ranked, but that is not true. In fact, in my very first publication I used Deane's entrenchment/topicality hierarchy and showed that it was significantly correlated with particle placement (*He picked up the book* vs. *He picked the book up*). But later in my dissertation, I had to admit that that was all non-sense; from my 2003 book (with my emphasis added now):

Third, I would like to draw attention to a most fundamental flaw that underlies many of the previously discussed approaches and that will be illustrated in the following sections in much more detail. This Haw is related to the criticism levelled at some of Fraser's arguments in the beginning of this section and derives from the still current prevalence of monocausal approaches to syntactic variation. If a variable is investigated empirically in isolation, it might very well be the case that a significant effect can indeed be detected, but the problem, especially with more complex phenomena, is that one needs to test whether the impact found is due to the variable as a whole or due to some conceptual subparts) of the variable. In the case at hand, the entrenchment hierarchy is a rather complex variable, consisting, as it were, of a combination of several more primitive variables, each of which has already proven to be relevant for a variety of linguistic phenomena; this is represented in Table 2:4 [where Deane's entrenchment/topicality hierarchy is just a not well thought-through conflation of three different but correlated hierarchies].



If the above reasoning is applied to the case at hand, one has to test whether the explanatory power of the entrenchment hierarchy is superior to that of its constitutive subparts in combination: in Gries (1999), a highly significant Spearman rank correlation was found between the position of the referent of the direct object on the entrenchment hierarchy and the choice of construction (rs = .68; ptwo-tailed = .001). However, if, for the same data set, a multifactorial analysis is calculated for the relation between all the constitutive subparts of the entrenchment hierarchy (and their interactions) and the choice of construction, it becomes obvious that the explanatory power of the individual subparts of the entrenchment hierarchy and their interactions is much greater than that of the entrenchment hierarchy alone: R = .89; F(10, 136) = 50.912; p<.001. In other words, taken together, the individual variables account for particle placement much better than their joint combination into the entrenchment hierarchy - in fact, we lose explanatory power if we use the entrenchment hierarchy rather than its subparts. This result is further supported by comparing the partial correlations'' for these variables in a multifactorial analysis: the partial correlation of the entrenchment hierarchy with the choice of construction is the second smallest of all the above variables entering into the equation. In sum, it seems as if the role of entrenchment (at least when measured using Deane's (1992) or Grics's (1999) entrenchment hierarchy) simply does not exist - it is merely a statistical artefact that has arisen from a mono- causal analysis without considering the simultaneous interrelationships between several variables, a problem which has important methodological consequences [...]

In other words, I strongly advise against the application of any such hierarchy because they conflate multiple hierarchies/causes in a way that makes it very hard to discern which of their included hierarchies and which of their levels are actually responsible for anything. Not to step on any one's toes, but apart from their roles as a first heuristic, these hierarchies are simplistic junk.

p. 16: no specific comment as yet, but I will be curious to see your operationalization of information status: many existing ones leak and ignore important components that contribute to accessibility (such as superordinate or subordinate terms, partonymic relations etc.) Just saying: this will be something where careful operationalization is required because I have shown in other work that the way in givenness etc. is operationalized can determine whether it's more or less important than syntactic criteria in alternation studies.