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# Cognitive linguistics' seven deadly sins

Ewa Dąbrowska, Northumbria University

*Cognitive linguistics is an approach to language study based on three central premises: that the function of language is to convey meaning, that linguistic description must rely on constructs that are psychologically real, and that grammar emerges from usage. Over the last 40 years, this approach to studying language has made enormous strides in virtually every aspect of linguistic inquiry, achieving major insights as well as bringing about a conceptual unification of the language sciences. However, it has also faced problems, which, I argue, must be addressed if the approach is to continue to flourish. Some of these are shared with generative linguistics, while some are peculiar to the cognitive approach. The former include excessive reliance on introspective evidence; paying only lip service to the cognitive commitment; too much focus on hypothesis formulation (and not enough on hypothesis testing); ignoring individual differences; and neglecting the social aspects of language. The latter include assuming that we can deduce mental representations from patterns of use and equating distribution with meaning. I conclude by sketching out how these pitfalls could be avoided.*

Cognitive linguistics is a broad church encompassing several distinct strands. Linguists who describe themselves as cognitivists investigate a wide range of phenomena using a variety of methods. Nevertheless, I think it is fair to say that there are at least three central tenets that all cognitive linguists subscribe to, namely:

- (1) that language exists to convey meaning (which includes both “content” and construal, so grammatical morphemes and constructions are regarded as meaningful), and that therefore the goal of linguistics is to explain how speakers use linguistic expressions to convey meaning;
- (2) that “accounts of human language [should] accord with what is generally known about the mind and brain from disciplines other than linguistics” (Lakoff's “cognitive commitment”: Lakoff 1991: 54); and
- (3) that grammar emerges from usage, at both developmental and historical level, and therefore usage should be centre stage in all accounts of language.

In the forty years or so since its inception, cognitive linguistics has made enormous strides. We now have detailed descriptions of a variety of lexical and grammatical phenomena in many languages, including some very little studied ones; we have a better understanding of how meaning is constructed, why languages are the way they are, how they are acquired and how they change, and we have laid the theoretical foundations for understanding language processing and impairment. Furthermore, areas which were previously marginal – such as construal, embodiment, the meanings of grammatical items, metaphor, metonymy, and iconicity – are now at the centre of linguists' interest; and, perhaps most importantly, we have achieved conceptual unification of many aspects of language study.

Nevertheless, there are some serious problems (“sins”) which are prevalent in the field and which will need to be addressed if the field is to continue to prosper. In this contribution, I discuss the “sins” and how they can be avoided, and conclude with a few comments about the future.

## 1. Excessive reliance on introspection

The foundational work in cognitive linguistics (e.g. Fauconnier 1985; Lakoff 1987; Lakoff and Johnson 1980; Langacker 1987, 1991a, 1991b; Talmy 1988, 2000) relied strongly on introspective evidence. This is very common, and probably unavoidable, in the early stages of theory development in the human sciences. However, cognitive linguistic theory has matured considerably since then, and at the same time, substantial progress has been made in linguistic methodology. While this has led to a noticeable shift towards more rigorous empirical methods (see

Janda 2013), many cognitive linguists continue to use their own intuitions (or at best those of a few colleagues) as their main data source. This is due partly to historical reasons: much of 20<sup>th</sup> century linguistic inquiry, especially in the generative tradition, was based on the linguists' own introspective judgements, and many linguists do not have the technical knowledge to use other methods. A second reason is ideological: some cognitive linguists appear to believe that objective methods are incompatible with the spirit of cognitive linguistics, which is a non-objectivist theory (for discussion, see Geeraerts and Cuyckens 2007: 18).

However, as many researchers have pointed out before me, the use of the analyst's own introspective judgements is problematic for two reasons. First, many aspects of our mental life are not accessible to introspection, and these, obviously, have to be studied using different methods. More seriously, introspective judgements are influenced by our knowledge and beliefs, and often demonstrably false (see Dąbrowska 2010; Gibbs 2006). This is particularly problematic when the person who provides the judgements has strongly held beliefs about the subject matter, which is usually the case when linguists base an argument on their own judgements. As Schütze (1996: 187) points out, "... one cannot find a more biased subject than the investigator". Furthermore, even if the researchers' judgements are not biased, they may not be representative of the entire population of speakers.

This is not to say that there is no room for introspection in linguistic enquiry. The researchers' own intuitions are an excellent source of hypotheses we may want to test using more rigorous methods, and are particularly useful in the study of meaning (cf. Talmy 2005). However, such judgements must be treated with caution, and, if there is any doubt, they need to be supported by judgements elicited from a representative sample of linguistically naïve informants.

## **2. Not treating the cognitive commitment seriously**

Given that one of the central tenets of cognitive linguistics is that language relies on mechanisms shared with other aspects of human cognitive functioning and can only be explained by appealing to human cognition, you would expect cognitive linguists to support their analyses by appealing to what is known about human cognitive processing – in other words, you would expect frequent references to work in other areas of cognitive science, and in particular, cognitive psychology. Sadly, this is generally not the case, even if there are some notable exceptions. Although cognitive linguists happily proclaim that language relies on general cognitive processes, many seem to believe that it is not their job to find out how, and in fact do not seem to be interested in what the other cognitive sciences have to say about human cognition.

Consider, for example, Ron Langacker's *Foundations of Cognitive Grammar*. The first volume of this monumental work (Langacker 1987), which sets out the theoretical framework for CG, contains references to thirteen titles relating to cognitive psychology and eight psycholinguistic studies. Volume 2 (Langacker 1991), which applies the framework to a range of linguistic phenomena, mentions four titles relating to cognitive psychology and three which can be classified as 'psycholinguistics'. None of the works cited is discussed in great detail, and none are mentioned in the introductory discussion in volume 1 of the research that helped to shape the developing framework. It is worth noting that Langacker makes an effort to relate the CG framework to some psychological concepts and honestly acknowledges that the connections with psychology are tenuous (cf. 1987: 6). Many other cognitive linguists confidently assume that CG and related frameworks are psychologically real and never even bother to engage with cognitive psychology.

Thus, many cognitive linguists' stand on the cognitive commitment is best described as "believing but not practicing". A similar attitude is revealed in what I have previously (Dąbrowska 2009) described as "explanation by fiat". This involves "explaining" the grammatical properties of a construction simply by asserting that it involves a particular construal of a situation, without providing any evidence that this is in fact how speakers construe the situation. Achard's (2007) analysis of French complementation is a typical example of such work. Achard notes that when the subordinate clause subject is the same as that of the main clause, the subordinate verb occurs in the

infinitive (*Marie aime aller au cinéma*), but when the subject is different, the subordinate verb must be finite (*Marie aime que son frère aille au cinéma avec elle*). This, he argues, is because

the presence of a finite complement ... reflects an objective construal of the scene coded in the complement.... Because the vantage point from which it is construed is external to its scope of predication ... the whole scene, including the subject of the subordinate process, is part of the objective scene and thus profiled. (792)

This may very well be the case, but unless we can independently verify whether speakers construe the scene objectively or subjectively or whether the vantage point is internal or external to the scope of predication, the explanation is circular: Construction X has properties Y because involves construal Z. How do we know that it involves construal Z? Because it has properties Y.

Cognitive linguists often criticize the generativists for claiming that language can be studied independently of other aspects of human cognition. And yet, while they maintain that such a position is untenable, this is what many do in practice! True, cognitive linguists may not be quite as “sinful” in this regard as generative linguists, in that they do at least appeal domain-general cognitive processes, but in many cases the appeals have a definite “Sunday morning Christian” feel to them. It is time that we stopped simply asserting that language is part and parcel of “general cognition” (whatever that may be), and started thinking more about how the analyses that we propose fit in with what is known about how humans represent and process language. Some crucial issues that should be centre stage include, for example, what it means for a speaker to “have” a particular construction, how constructional knowledge is represented (e.g. whether it is declarative or procedural), how it is accessed during real-time comprehension and production, and what cognitive processes are involved in construal (and how they can be measured).

### **3. Not enough serious hypothesis testing**

When you push a cognitive linguist on the subject of psychological reality, the answer that you are likely to get is that the proposed analysis is “just a hypothesis” about how a particular aspect of linguistic knowledge may be represented in speakers' minds, and that it is up to somebody else (presumably psychologists or psycholinguists) to test this hypothesis. There is nothing wrong with such a position in principle: formulating hypotheses is a critical part of scientific enquiry. The problem is that, by and large, psychologists are not very interested in testing linguistic theories. There are two reasons for this state of affairs. One is historical: many psychologists got their fingers burned in the 1960s in testing what came to be known as the derivational theory of complexity (the idea that the sentences whose derivation required more transformations would take longer to process). As long as the experimental findings supported the theories put forward by linguists, everyone was happy; but as soon as they psychological experiments began to produce evidence that contradicted claims made by linguists, they suddenly became “irrelevant”. (One wonders whether cognitive linguists would be more receptive to refutations from psychologists. I suspect not: my guess is that, like generative linguists, they would find good reasons to ignore the psychologists' findings.) A second and more serious reason is that many linguistic hypotheses are simply not formulated precisely enough to be empirically testable.

This is perhaps most clear for CL accounts of construal. Construal is one of the fundamental notions in the cognitive approach to meaning and has been appealed to in the description of a vast range of phenomena. There is no question that it is an extremely useful concept. The problem is that the majority of attempts to explain linguistic phenomena by appealing to alternative construals of the same situation are based on the analysts' own intuitions about the data. How do we decide then if a particular description is in fact correct? And what do we do if different analysts disagree?

In many ways, the cognitive notion of construal operations is like the generative notion of deep structure: it is a concept that has tremendous explanatory power and appeals to the imagination, but is very difficult to operationalize and hence to test empirically. This is not a criticism of the notion itself: I am not proposing that we should abandon explanations couched in

such terms, but that we develop ways of operationalizing construal. In fact, a few successful attempts have already been made (see, for example, Bergen et al. 2007; Matlock 2004; Matlock et al. 2004; Tomlin and Myachykov 2015).

No theory provides “the whole truth” about its object, and it is perfectly normal for researchers to disagree about how to interpret linguistic phenomena (or any other phenomena, for that matter). But if cognitive linguistics is to move closer to the truth (rather than going around in circles, like so many other linguistic theories), it cannot merely offer hypotheses about how speakers' knowledge is represented in their minds. We need to derive testable predictions from our hypotheses, carry out the tests, and refine the hypotheses when necessary. If we do not do this, discussions about alternative accounts are going to degenerate into playground arguments of the type “my theory is prettier than yours” or “my intuitions are stronger than yours”. (For further discussion of this and related issues, see Kalyan 2016.)

#### **4. Ignoring individual differences**

The fourth “sin” is again one of not living by what one preaches: while cognitive linguists readily acknowledge the existence of individual differences in theory, in practice most rarely attempt to study them or consider their deeper theoretical implications. Thus, we are told that a particular expression X profiles a particular aspect of a scene construed in a particular way, sense Y is more entrenched than sense Z, and so on. General statements of this kind are, of course, unavoidable: it would be impossible to study the individual mental representations of every speaker of a language; and even if it were possible, most linguists are more interested in general patterns shared across many speakers than in individual idiosyncrasies. So, while strictly speaking it would be more accurate to say “X *typically* evokes such-and-such a construal” and “*for most speakers*, Y is more entrenched than Z”, it would be tedious to add such qualifiers to every sentence one writes.

Thus, most linguists, including cognitive linguists, do not look for individual differences, and tend to sweep them under the carpet when they find them, with the result that they are usually ignored. There are several reasons why this is problematic. First, individual differences are pervasive in language (cf. Dąbrowska 2012, 2015a), and their existence raises some important questions: for instance, if different speakers have different constructions and attach different meanings to the words in their lexicons, how can they understand one another? Secondly, the existence of differences between speakers is predicted by a usage-based theory, and thus offers support for it. Last but not least, the study of individual differences offers a unique window onto the cognitive and experiential underpinnings of language. Although cognitive linguists assume, as a working hypothesis, that our linguistic skills depend on mental abilities that are shared with other aspects of human cognition, there is no reason to assume that all aspects of language rely equally on the same abilities, and in fact there is evidence that this is not the case (see, for example, Dąbrowska under review; Misyak and Christiansen 2011; Street and Dąbrowska 2010). Likewise, certain types of input may facilitate the acquisition of particular aspects of language. This means that by correlating individual differences in linguistic knowledge with measures of cognitive functioning and linguistic experience, we can begin to unpack the cognitive underpinnings of specific constructions.

#### **5. Neglecting the social aspect of language**

As pointed out earlier, one of the goals of cognitive linguistics is to explain how our conceptual system shapes the languages that we speak. While there is no doubt that language structure is strongly constrained by our perceptual and conceptual abilities, social and cultural factors play an equally important role; and although few cognitive linguists would dispute this claim, in practice, at least in the first thirty years or so of its development, cognitive linguists have

paid little attention to the social factors that influence language structure.<sup>1</sup> At the risk of stating the obvious, human beings are social creatures through and through, and social influences shape virtually every aspect of our lives from tool use and morality to parenting behaviour and communication. Language is not only an instrument *for* social interaction; it is also a system that emerges *through* interaction, and we cannot hope to understand its structure without considering both cognitive and social factors *and their interactions* (Dąbrowska 2015b; Geeraerts this issue, Harder 2010; Schmid 2015, this issue; Zlatev this issue) – in other words, what we need is a *social cognitive linguistics* (cf. Harder 2010). Cognitive linguists are only beginning to engage more with this broader perspective: examples include work by Du Bois and others on dialogic interaction (see e.g. Du Bois 2014), Schmid's entrenchment-and-conventionalization model (Schmid 2015, this issue), Harder's reflections on the nature of linguistic norms (Harder 2003) and the social construction of meaning (Harder 2010), and my own work on the role of societal structure, community size, and social practices such as literacy (Dąbrowska 2015b). However, such a social cognitive linguistics is not yet mainstream cognitive linguistics.

## 6. Assuming that we can deduce mental representations from patterns of use

All of the “sins” that I have discussed so far are ones that we share with generative linguists. The last two are related to the inferences made from corpus data, and are our very own. The growing awareness of the limitations of introspection, coupled with a strong commitment to the systematic study of real usage led to a flowering of corpus methods within cognitive linguistics which was accompanied by increasingly sophisticated statistical analysis. This resulted in remarkable progress in linguistic description and methodology. The downside was a great deal of confusion about the theoretical status of corpus-based generalizations.

The problem is that we cannot make strong inferences about mental representations – which is what CL claims to be about – from usage patterns (see also Blumenthal-Dramé this issue). Suppose, for example, that our corpus contains 8 tokens of the utterance *Who do you think you are?*, 100 tokens of questions consisting of a WH word followed by *do you think* and a subordinate clause with a gap, and 300 questions with a long distance dependency with various auxiliaries, NPs and verbs in the matrix clause. What sort of constructions can we attribute to the speaker, or speakers, who produced these utterances? The fact that there is considerable variety in the main clause would suggest a fully general construction. On the other hand, the fact that a high proportion of the questions begin with *WH do you think* may be taken as evidence that speakers also have a semi-formulaic template of the form *WH do you think S-GAP?* Likewise, the fact that the very same string (*Who do you think you are?*) is attested 8 times might be taken as evidence that this particular combination of words is also available to speakers as a preconstructed unit. On the other hand, it is possible that questions of the form *WH do you think S-GAP?* are produced more frequently than questions beginning with *WH does Pamela hypothesize* simply because we are more likely to wonder about what the interlocutor thinks than what Pamela hypothesizes (and ditto for *Who do you think you are?* v. *Who did the children believe played Santa Clause?*). And if we assume that speakers do store such lexically specific constructions, are we justified in concluding that these exist alongside the more general pattern? Perhaps they just have the partially schematic template and rely on analogy to produce the more unusual questions? Or maybe some speakers have the general construction and some do not? Without experimental evidence there is just no way to tell.<sup>2</sup>

In some cases there are considerable differences between what corpus data suggest and the

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1 There is, of course, a large and rapidly growing body of research in cognitive sociolinguistics (e.g. Geeraerts et al. 1994; Geeraerts 2010; Kristiansen and Dirven 2008; Pütz et al. 2014; see Geeraerts and Kristiansen 2015 for a recent review). Most of this work, however, has concentrated on language variation rather than social influences on language structure per se.

2 In fact, there is solid evidence that speakers have lexically specific templates for questions with long distance dependencies: see Dąbrowska (2008) and Dąbrowska et al. (2009).

picture that emerges when we add information obtained from experiments designed to test hypotheses about speakers' mental representations. Consider genitive case marking in Polish. There are two endings marking the genitive case of masculine nouns, *-a* and *-u*, and their use correlates with various semantic, morphological and phonological factors (see Westfal 1956): for example, about 80% of the masculine nouns designating small easily manipulable objects take *-a*, and approximately the same proportion of masculine nouns designating substances take *-u*.

Furthermore, although there is some variation, different speakers' usage with *real words* is broadly similar. However, experiments with nonce word stimuli, which tap speakers' productive abilities, show that only about 10% of the participants are sensitive to this contrast (Dąbrowska 2008). This is puzzling, since this is a strong pattern which has persisted for several centuries; moreover, new words that enter the language tend to get the ending that is appropriate for their meaning. How is this possible? It seems that consistent usage by the small number of speakers who have extracted the pattern skews the frequency of the endings with the new nouns, such that those that designate small objects are used more often with *-a* while substance nouns are more likely to take *-u*. Speakers who don't have the schema simply memorize the more frequent form, which results in further skewing, and so on. In other words, patterns that are demonstrably present in language may not be represented in speakers' minds, or at least not in all speakers' minds. Speakers may approximate each other's behaviour following different sets of rules. (See also Dąbrowska 2013 for similar observations about English speakers' knowledge about questions with long distance dependencies.)

Corpus analysis is absolutely vital to usage-based approaches such as cognitive linguistics. In the end, however, corpora can only provide information about frequency of items and frequency of co-occurrence of items. If we want to make claims about speakers' mental representations, corpus data needs to be complemented with experimental research.

## 7. Assuming that distribution equals meaning

Similar points can be made about studies that attempt to uncover semantic structure using corpora. Many cognitive linguists took to heart Firth's (1957: 11) dictum that “you shall know a word by the company it keeps”, which resulted in a spate of studies attempting to make semantics more objective by showing that different words, and different senses of words, have different distributional patterns. However, while there is no doubt that differences in meaning *correlate* with differences in form, distribution and semantics are just not the same thing, and the correlation is far from perfect. A particular distributional feature may provide a clue to meaning – or it may not. The assumption that differences in co-occurrence requirements always correspond to differences in meaning is methodologically useful in that it encourages us to look for such correspondences, but it may not be valid: some formal differences may be just that – formal differences. Furthermore, even if a distributional feature does provide clues to meaning, there is no guarantee that a language learner will pick up on the feature. Last but not least, distributional features are often correlated with each other, and most of the statistical models that corpus linguists use (e.g. regression)<sup>3</sup> assume that the predictors vary independently. As a result, analysts have to choose one of a set of correlated features, and the decision which one to include in the model and which to leave out is fairly arbitrary. There is no evidence that language learners do the same: in fact, for all we know, human learners may thrive on multicollinearity. And, of course, different learners may pick up on different features and, because the features are correlated, still end up with similar representations.

So, while distributional features provide important clues to meaning, adequate semantic descriptions must make use of other methods as well. These include a variety of elicitation techniques which tap speakers' knowledge in systematic ways, such as picture or video clip descriptions, paraphrase/definition tasks, semantic similarity judgments, prototypicality judgments, eliciting word associations, listing features, and sorting/categorization tasks. In addition, it is likely that in the future, semanticist will rely increasingly on brain imaging methods.

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<sup>3</sup> Note, however, that this is not true of Naïve Discriminative Learning – see Milin et al this issue.

## 8. A prayer for the future

As the Danes say, it is difficult to make predictions, especially about the future. We don't know where cognitive linguistics will go in, say, the next twenty years. It is unlikely that the problems discussed here are going to dissolve into thin air – after all, humans are sinful creatures, and cognitive linguists are no exception – but I do hope that people who publish in these pages will make an effort to be more virtuous. The changes are already happening: year by year, cognitive linguistics is becoming more empirical as growing numbers of linguists rely less on introspection and do more hypothesis testing; more interdisciplinary, as we increasingly engage with research in psychology, neuroscience, language acquisition, and language impairment; more interested in the social dimension; and methodologically more eclectic, as more and more researchers combine corpus and experimental data with modelling. Let us hope that these trends will continue into the future.

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