

# Cognitive Linguistics as a Cognitive Science

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The social sciences and the humanities are currently experiencing a “cognitive turn” at least as sweeping and profound as the linguistic turn in the last century. Many disciplines are struggling to redefine themselves in light of this development, and in the following, I will share some of my thoughts on how successful Cognitive Linguistics has been so far in doing so -- in establishing linguistics as a Cognitive Science.

In order to discuss in what way and to what extent cognitive linguistics, or any other discipline, can be regarded as a cognitive science, we need to know what we mean by “cognitive science“. I will assume the following, uncontroversial definition

Cognitive science is the interdisciplinary study of mind and intelligence, embracing philosophy, psychology, artificial intelligence, neuroscience, linguistics, and anthropology. (Thagard 2008)

I will also assume that by “mind and intelligence“ we mean things like perception, imagination, categorization, reasoning, and memory.

It follows that any discipline that considers itself to be a “cognitive” science, must contribute to the study of the human mind. For two of the disciplines in the definition above, that contribution is self-evident: the human mind and its material basis constitute the principal objects of research of psychology and neurology respectively. I will refer to these as “core cognitive sciences“.

For other disciplines, that contribution is less direct: artificial intelligence, for example, is concerned with the study of systems that *simulate* some of the outwardly visible behavior of the human mind, and to the extent that that simulation is accurate, the mechanisms behind it may be treated as models of the mind. Whether these models are realistic, however, is a question that can ultimately be answered only by empirical research in psychology and neurology.

Still other disciplines have a much more indirect relationship to the study of mind and intelligence; their principal objects of research are related to cognition only in that they are, in some sense, products of human minds. Linguistics and anthropology fall into this category, but also other emerging disciplines that are being sucked into the slipstream of the cognitive turn such as cognitive stylistics/poetics, the cognitive study of art or music, cognitive economics, cognitive sociology, etc.

Unlike the core cognitive sciences, these disciplines have a double task to fulfill if they want to be part of the cognitive enterprise: first, they must put forward scientific models concerning their object of study (language, literature, culture, society, economy, etc.), and second, they must specify some relationship between that object of study on the one hand and the study of human mind and intelligence on the other.

There are two main ways to specify such a relationship, both of which can be found in the recent history of linguistics.

First, a discipline can draw inspiration from the core cognitive sciences by borrowing, more or less selectively, their models, perspectives and terminology. There is nothing wrong with this research strategy, as long as there is a genuine attempt to import more than just the terminology of cognitive science. But since none of the results of this import of ideas ever flows back to the core disciplines, the potential of this strategy for real progress is limited.

Second, a discipline can use its domain-specific expertise to build the best (most descriptively adequate, most exhaustive) model of its object of study. This model can then be confronted with the body of knowledge adduced by the core cognitive sciences in order to achieve some kind of integration, i.e. to investigate what this model may tell us about the human mind and human intelligence. This integration can be attempted by practitioners of psychology and neurology, by adequately trained practitioners of the discipline in question, or collaboratively, by researchers from both fields. There are practical obstacles to this research strategy -- diverging terminology, discipline-internal institutionalization, etc. -- but I believe that it is, ultimately, more fruitful than a mere one-way import of ideas from psychology and neurology.

Before Leonard Bloomfield's brief behaviorist revolution in linguistics, connections between language and mental processes were naturally assumed by authors as Karl Brugmann, Ferdinand de Saussure, Franz Boas, Edward Sapir, Otto Jespersen, Herman Paul and many others. But the first linguistic theory that explicitly saw itself as a "cognitive science" was Chomskyan linguistics in its reaction against behaviorism (I avoid the term "generative" because there are non-Chomskyan generative theories, too).

But Chomskyan linguistics added an interesting twist to the issue: it posits a specialized mental module that contains our linguistic competence, an autonomous language faculty that does not share any representational principles with other aspects of cognition and that interacts with them only in the process of linguistic performance, a process that is explicitly placed outside of the domain of linguistic theory. This is a very elegant postulate: it allows Chomskyan linguists to do cognitive science simply by linguistic analysis (building models to account for their own acceptability judgments on constructed sentences). They do not have to pay attention to anything that is known about any other aspect of human cognition. For a linguistic theory with cognitive aspirations, this is a genuinely ingenious theory-building mechanism: it ensures that any discovery about the structure of language automatically becomes a fact about the human mind.

As I said, it is an elegant postulate, but there is a fundamental problem with it: it is not falsifiable. That does not mean that it is actually false -- it just means that we can never know whether it is false, and thus, it is fruitless as a departure point for the scientific study of linguistic cognition. Incidentally, it is also fruitless as a departure point for the scientific study of language itself -- there is an open-ended number of theories that are consistent with a convincingly large subset of linguistic data to appear plausible, and the because of the autonomy postulate -- the idea of an encapsulated linguistic competence that cannot be accessed -- there are no competence-external criteria that could distinguish between them.

Cognitive linguistics -- the family of theories associated with authors like Ronald Langacker, George Lakoff, Leonard Talmy and Giles Fauconnier -- avoids such a postulate. This makes its relationship to the core cognitive sciences potentially more fruitful but also much more complicated. In cognitive linguistics, we see evidence for both of the strategies I mentioned earlier.

A brilliantly laid-out example of the first strategy is found in Langacker's *Cognitive Grammar*, as developed in the two volumes of his *Foundations of Cognitive Grammar* (1987, 1991). Langacker himself refers to his work, quite aptly, in my view, as "an exercise in speculative psychology" and he summarizes its relation to the study of the human mind as follows:

I have adhered rather strictly, in developing my proposals, to the dictates of both psychological plausibility and linguistic necessity; I have relied almost exclusively on *seemingly indisputable cognitive abilities* (e.g. the ability to compare two events and register a discrepancy between them), and I invoke them for linguistic constructs that must somehow be accommodated in any comprehensive and explicit analysis (1987: 6, my emphasis).

In other words, Langacker builds his linguistic theory entirely on supposedly general cognitive processes, but he is highly selective in choosing the processes he considers relevant: cognitive routinization ("entrenchment"), the ability to recognize similarities and differences, schematization, the ability to track changes over time, and figure-ground perception.

Langacker is not just selective, though. In addition, he characterizes these processes in very general terms not tied to any specific model from the core cognitive sciences. He adopts a kind of "enlightened common-sense" perspective on the human mind -- enlightened in that it is generally informed by 20th century psychology (for example, gestalt principles), and common sense in that it defines its concepts in terms that don't require any knowledge in psychology or neurology. Langacker is not alone in this strategy: Lakoff and Johnson's *image schemas*, Leonard Talmy's *force dynamics*, Lakoffs *polysemy networks* and Fauconnier's *blending* are all examples of such "common-sense" cognitive processes.

There are good reasons for a selective common-sense approach to the cognitive sciences. Recall that we are talking here about a one-way import of ideas from the core cognitive sciences to theoretical linguistics, with no serious expectation yet that

their application to language will yield results that will flow back to the core cognitive sciences. Given this situation, consider the alternative to such a selective approach -- the full-fledged adoption of a particular model of cognition: every time this model is revised or amended, as it doubtless would be, the linguistic theory based on it would have to be revised and amended as well, regardless of whether the linguistic data call for such revisions. Cognitive linguists would be in a position comparable to that of third-party software developers developing software for the Windows operating system: Every time a new version of Windows is released, they have to change their applications to work with this new system, even if the application was working just fine. On the other hand, these developers have no influence on future versions of the OS, suggesting particular potential functions that would benefit their application.

In a situation where the relation between the core cognitive sciences and linguistics is a one-way street from the former to the latter, then, the kind of selective common-sense psychology we see in the works of Langacker, Lakoff, Talmy and others is the only viable option. It is not without problems as a basis for interacting with psychologists, neurologists and computational scientists, who *do* tend to adhere to specific theories of the human mind and who are often exasperated with us cognitive linguists because of our fast and loose approach to cognitive science models and terminology. Still, this strategy does have a potential to yield results that will feed back into the core cognitive sciences if and when it is combined with an empirical approach to linguistic data that is lacking almost completely in the work of the authors I have mentioned.

But before I return to this issue, let me turn to the second strategy mentioned above -- the one, where a discipline applies its domain-specific expertise to the analysis of its object of research and then turns the results over to the core cognitive sciences for an investigation of how these results may be accounted for within a general theory of the human mind. I claimed that there were examples of this strategy in cognitive linguistics, too, and I would like to mention two of them: metaphor research and construction grammar.

The interesting thing about both of these areas is that they did not initially import ideas from the core cognitive sciences in the way that Langacker's Cognitive Grammar or Fauconnier's theory of Blending did. Instead, they started out from linguistic analysis.

Now, this is not to say that they did not make claims relevant to the cognitive sciences. Take Lakoff and Johnson's Conceptual Theory of Metaphor -- it takes them less than one paragraph to get to their central claim:

We have found ... that metaphor is pervasive in everyday life, not just in language, but in thought and action. Our ordinary conceptual system, in terms of which we both think and act, is fundamentally metaphorical in nature (Lakoff and Johnson 1980: 3).

But consider the evidence on which they base these far-reaching claims, and you will find that it is exclusively linguistic, as they themselves readily admit on the very next

page (Lakoff and Johnson 1980: 4). Essentially, their evidence consists of two facts: first, that metaphor is pervasive in everyday language, and second, that metaphor is best described in terms of general, highly systematic mappings between concrete and abstract semantic domains. Both of these observations are purely linguistic (and neither of them was new at the time when Lakoff and Johnson pointed them out). So, at first glance, Lakoff and Johnson's strategy is very similar to the Chomskyan strategy: they uncover a fact about language and automatically assume that it is also a fact about the mind. What distinguishes their strategy from Chomsky's is the fact that they do not posit an encapsulated language faculty, and therefore their claims are, in principle, testable: they make predictions about the human mind in general, not just about the part of it that is concerned with language. And thus, behavioral evidence can be brought to bear on the issue.

Now, based on what we currently know about metaphor in the human mind, I think it is fair to say that the claims by Lakoff, Johnson and others were too sweeping -- matters are considerably more complex. But interestingly, we can also say that they were generally right. We *do* have empirical evidence for a correlation between linguistic metaphors and general patterns of thought.

Interestingly, not all of this evidence was adduced by cognitive linguists aware of metaphor theory: it is well-established in Social Psychology, for example, that people associate moral failure with physical pollution: experiments show that people have the urge to wash when they have thought or talked about disagreeable, unpleasant or morally questionable acts (Fairbrother et al. 2003, Zhong/Liljenquist 2006) and, conversely, they have a higher tolerance for such acts if they wash their hands first (Schnall et al, in press). It is suggestive to explain these results with reference to linguistic metaphors like the following:

- (1)
  - a. the *stain* of guilt/sin/illegitimacy
  - b. *impure* thoughts/soul/character
  - c. a *smutty* manner/joke/film/novel
  - d. a *filthy* word/mouth/mood/mind
  - e. an *unclean* thought/spirit/mind
  - f. a *dirty* mind/look/word/secret
  - g. to *contaminate* a relationship
  - h. to *taint* someone's reputation
  - i. to *wash away* someone's sins
  - j. to feel *soiled* by an experience
  - k. to *cleanse* oneself of negativity
  - l. to *pollute* someone's mind/thoughts

What these studies leave open is the causal mechanism underlying such correlations: do we think of disagreeable things as DIRTY because there are linguistic metaphors that make this connection, or are the linguistic metaphors simply an expression of a preexisting mental connection. It is here that the speculative-psychology approach of the leading theoreticians in cognitive linguistics reaches its limits -- empirical research is necessary within the field of cognitive linguistics itself.

Before I turn to a discussion of empirical research, let me briefly discuss my second example: construction grammar. Note that, just like metaphor theory, construction grammar was not inspired by models from the core cognitive sciences, but by purely linguistic considerations: authors like Lakoff (1976) and Fillmore (1985) became increasingly aware of the extent that the grammar of natural languages was suffused by irregularity and idiosyncrasy. Construction grammar was developed as a framework capable of capturing these idiosyncrasies and the generalities that, of course, also exist, by a uniform mechanism: the grammatical construction. Construction grammarians vary in their willingness to make claims about the human mind based on their theoretical considerations, but Lakoff (1987) and Goldberg (1995) clearly assume that their grammatical analyses reflect the way in which grammatical knowledge is mentally represented.

Again, I would argue that we are a long way away from a complete understanding of the nature of grammatical constructions, of how constructions emerge, develop and disappear in language change, of how they are learned, of how they are represented in the mind and of how they are put to use in producing and comprehending language. But, as in the case of metaphor theory, I think it is safe to say that we have empirical evidence of their existence and of their relevance to any characterization of grammatical knowledge (cf. the early work by Gibbs 1995, work by Michael Tomasello, Elena Lieven and their associates, and recent work by Goldberg 2006, also my own work on negative evidence, Stefanowitsch 2006, 2008).

So let me turn to the issue of empirical research for the final minutes of my talk. Because the fate of cognitive linguistics as a theoretical framework depends crucially on the willingness of its practitioners to adopt state-of-the-art empirical methods.

Currently, cognitive linguistics as practiced by the majority of its adherents cannot be classified as a cognitive science quite simply because it is not a science. Most cognitive linguists are happy to rely exclusively on the first strategy I've discussed today: they import, first-hand or, more typically, second-hand via authors like Lakoff and Langacker, selected perspectives and partial models from the core cognitive sciences, analyze linguistic data in terms of these models, and leave it at that. This unfortunate fact is responsible for endless series of dull and repetitive talks at national and international cognitive linguistics conferences about metaphors for LOVE in Chinese, Russian or Old Norse, about the polysemy of the English preposition *over*, about the most pleasing way to draw a network for the resultative construction, about the prototypical meaning of this or that Korean verb and about the embodied way in which we talk about the human body. More importantly, the self-sufficient reliance of cognitive linguists on a few conceptual imports and pseudo-imports from the core cognitive sciences is responsible for the fact that very few people in the cognitive sciences pay attention to cognitive linguistics.

If you follow the development of the field, you may be less pessimistic with regard to the role of empirical research. Because if you look at the conference programs over the past fifteen years or so, there seems to be a growing interest in empirical research: from the mid-nineteen-nineties onwards, second-generation researchers in cognitive linguistics began to turn to psycholinguistic methods (and some psycholinguists

began to take an interest in cognitive linguistics research issues, such as metaphor and polysemy). And from the late nineteen-nineties onwards, second- and third-generation cognitive linguists began to turn to corpus-based methods (cf. Tummerts et al. 2005, Stefanowitsch, to appear). The results generated by these empirical researchers are often spectacular and they are certainly highly relevant to theoretical considerations at the heart of cognitive linguistics.

In the domain of metaphor, for example, there is now evidence for more than just a correlation between metaphorical language and metaphorical thought: it seems that there is a causal direction from metaphorical language to metaphorical thought. Lera Boroditsky has shown that speakers of English, who talk about time as moving horizontally from front to back also reason about time in this way, while speakers of Chinese, who talk about time moving vertically from top to bottom reason about time correspondingly (Boroditsky 2001). I myself have uncovered language-specific differences between the metaphors associated with near-synonyms such as *joy* and *happiness* (Stefanowitsch 2004), and Ben Bergen and his colleagues have provided evidence that suggests that such differences may affect non-linguistic cognition (Tseng et al. 2006).

But there are three reasons why the emergence of an empirical tradition in cognitive linguistics is cause for cautious hope at best.

First, the non-empirical majority in the field are quite happy to pay attention to empirical results when these results are spectacular, and when they confirm dearly-held beliefs of the cognitive community. But most empirical research is *not* spectacular and just as often as not it contradicts some dearly-held belief.

Take work by Gerard Steen and his work group on metaphor identification (e.g. Steen 2007) or my papers on metaphorical pattern analysis (e.g. Stefanowitsch 2004, 2006) -- these papers are not particularly dazzling. And I mean that as a compliment: we are grappling with a difficult issue: how to tell when we are dealing with a metaphor. Given the lofty heights at which the theoretical discourse about metaphor takes place, this issue is bound to seem dull. But it has to be done, if we are to make any progress on the deeper questions concerning the relationship between metaphorical language and metaphorical thought.

Take the question of whether metaphorical language causes metaphorical thought or vice versa. In spite of the encouraging results I just mentioned, this question has not been conclusively answered. For example, Chen and colleagues (Chen et al. 2007) were unable to replicate Boroditsky's results regarding the vertical metaphor of TIME. Also, it is at least questionable that the expressions instantiating the MENTAL IMPURITY IS PHYSICAL POLLUTION metaphor in (1) above are widespread and frequent enough in English to cause the behavior observed in the experiments I mentioned. To answer this issue conclusively, we need to have a better idea of how widespread and productive individual metaphors are in individual languages, and this can only be achieved if can reliably identify these metaphors in corpora in order to establish their frequency and productivity.

Second, the growing number of empirical researchers in cognitive linguistics is not due to a large-scale conversion of researchers already active the field -- it is almost exclusively a result of new researchers joining the field (and this despite efforts to provide training opportunities in the form of methodology workshops at national and international conferences and textbooks, collections and monographs on methodology, not all of which are written by experts on methodology, however). This influx of empirical researchers is currently still offset by an even larger group of non-empirical linguists adopting the cognitive linguistics framework. Thus, the empirical researchers are, and will be for the foreseeable future, a minority. And a minority, that is not as widely respected in the field as one might expect, given that they present the best shot the field has for becoming a cognitive science on equal terms with psychology, neurology and artificial intelligence.

Third and last, cognitive linguistics needs its young empirical researchers more than these researchers need cognitive linguistics. There are, after all, well-established traditions of solid psycho-linguistic and corpus-linguistic research that precede the emergence of cognitive linguistics by at least thirty years. And cognitively inspired empirical researchers are welcome there and they typically have at least one foot firmly planted in one of these traditions. If theory development in cognitive linguistics continues to proceed largely independently of the empirical research done at the fringes of the field, I predict that these young researchers will abandon the field, leaving behind a framework that may be inspired by the cognitive turn, but that remains a discipline of the humanities, caught in a hermeneutic circle of intuitions about intuitions.

To end on a more optimistic note, here is what I think the field of cognitive linguistics must do in order to become a cognitive science:

- it must become more linguistic: it must rediscover a focus on detailed linguistic analysis, aided by the most advanced corpus-based methods available;
- it must become more scientific: it must ensure that its basic concepts can be operationalized and that its claims can be phrased as falsifiable hypotheses;
- it must become more methodologically aware: it must adopt strict observational and experimental protocols and state-of-the-art statistical methods;
- it must become less theoretically ambitious: as we become more linguistic, more scientific and more methodologically aware, we will have to restrict ourselves more strictly than we currently do to linguistic constructs that can be operationalized and research hypotheses that can be falsified. This will place large parts of our current models outside of the scope of a cognitive linguistic research program.

Can we, as a field, achieve all this?

Yes, we can.



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