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Concerning (Radical) Construction Grammar

Construction grammar (= CG) is a school of thought that has developed under the umbrella notion of Cognitive Linguistics (= CL). While there are a few theoretical problems that are characteristic of CL in general and of CG in particular, they are likely to remain implicit. Their existence has, however, been accentuated with the emergence of 'Radical Construction Grammar', presented in Croft (2001). Some aspects of (R)CG will be scrutinized in this paper. It should be read together with Itkonen (2006).

1) Basic Confusions

It is the central tenet of (R)CG that "constructions, not categories and relations, are the basic, primitive units of syntactic representations" (Croft 2001: 46; originally with emphasis). In consequence, "the categories of the construction are defined by the construction itself. Hence, categories are unique to each construction" (p. 54; repeated on p. 59).

What are constructions? They are illustrated in Table 1.3 on p. 17 and in Figures 1.11–12 on p. 26. For instance, there are the Intransitive Construction [Sbj IntrVerb], the Plural Construction [Noun-*s*], and the Adjective Construction [Adj], but also lexical units like *green* are constructions, and represented as [*green*]. The fact that lexical units are constructions already guarantees that there will be in practice an infinite number of constructions, i.e. 'primitive units of syntactic representations'. But because constructions (including sentences) are explicitly allowed to contain constructions (including sentences), recursivity is a built-in feature of RCG. Therefore it is necessarily the case that there are indeed an infinite number of 'primitive' units. This is the **first basic defect** of RCG, and it already amounts to a *reductio ad absurdum* of RCG in its entirety. To be sure, RCG shares this characteristic with all other versions of construction grammar.

RCG sees itself as a culmination point in the history of linguistics. All rival theories and approaches are simply rejected. Why? — because they fail to offer "a rigorous scientific method for discovering the properties of the grammar of a language" (p. 41). The same claim is made concerning pre-RCG typological research of cross-linguistic data (p. 31). We are also

taught that “heuristic definitions [like those given by every non-RCG approach] are no substitute for sound methodology and theory” (p. 64). Such statements make the reader intensely curious. What is Croft’s **own** answer to the age-old problem of grammar-writing? What is the “rigorous scientific method” that he **himself** uses “to discover the properties of the grammar of a language”? Above all, how does he discover or identify his own “basic, primitive units”, namely constructions?

Incredibly, Croft simply refuses to answer this question: “The best way to understand how to identify constructions is to use the results of psychological research into categorization and the formation of taxonomies, a domain that regrettably will not be discussed in detail [read: at all] in this book” (p. 53). This is **second basic defect** of RCG. It goes against all the norms of “sound methodology and theory” that Croft, who refuses to answer the question, strongly criticizes anyone who, unlike him, has tried to answer it. When combined, the first two defects produce the result that RCG postulates an infinite number of “basic, primitive terms”, without giving any indication how these could or should be identified.

RCG claims, to repeat, that the categories contained in a given construction are ‘defined’ by this very construction. This is problematic, however, because — as we noted above — categories are themselves constructions; and the opposite is also the case: “constructions represent categories” (p. 27). It is also repeatedly claimed (e.g. on pp. 52–53) that constructions and categories result from one and the same process, namely categorization. Because the ‘construction vs. category’ distinction is of such a dubious nature, it has seemed simpler to resort to the such traditional notions as ‘word’ and ‘morpheme’: “In RCG, the grammatical knowledge of a speaker is knowledge of constructions ..., words ..., and the mapping between words and constructions” (p. 46). “[C]onstructions ...can be segmented into their constituent words and morphemes” (p. 48).

In Croft’s view, the distributional method utterly fails to discover or identify such syntactic categories as noun, verb, and adjective. This is the justification for his claim that “noun, verb, and adjective are not categories of particular languages” (p. 63). As shown by the quotations in the previous paragraph, he seems to take it for granted that discovering or identifying the words and the morphemes of a given language is a trivial undertaking. But this is not true, as shown in great detail by Dixon & Aikhenvald (eds. 2002). The contributions to this volume establish a set of phonological criteria, on the one hand, and of grammatical criteria, on the other, for defining ‘word’. It is demonstrated beyond any doubt that, depending on the language in question, these criteria may converge to a higher or lesser extent. Thus, discovering words (and, by implication, morphemes) is a difficult but still feasible task. (Surely it goes against sound methodology and theory to

simply accept those orthographical conventions that have, as a matter of historical accident, been adopted in the codification of this or that language.) Ignoring all these complications is the **third basic defect** of RCG. When combined, the three defects exposed above produce the following result: RCG postulates a infinite number of primitive units; we do not know what they are; they are analyzed into smaller units, but we do not know how. Taken together, these three defects constitute what, for want of a better term, might be called a **mega-muddle**.

2) Mending the Damage

Some mistakes can be explained whereas others cannot. In this section I try to expose at least some of the reasons that may have given rise to the oddities discussed in the preceding section.

Pre-Croftian linguistics is claimed to be irreparably vitiated by its **circularity**: constructions are used to define categories, and categories are in turn used to define constructions (p. 45). It is in order to avoid this (alleged) circularity that Croft purports to define categories on the basis of constructions, and leaves the latter undefined, with the undesirable consequences pointed out above.

Croft is mistaken in thinking that every circle is necessarily a vicious circle. It is commonly known that there is also a **non-vicious** type of circle, called **hermeneutic circle** or, rather, **spiral** (cf. Itkonen 1993). Let us consider such traditional notions as 'sentence structure', 'noun', and 'verb'. What is their mutual relation, in reality? The language learner encounters entire utterances which he tentatively analyzes into smaller units, including those that exemplify (preliminary versions of) such categories as 'noun' and 'verb'. Next, he encounters new utterances which he now conceptualizes, tentatively, as sentence structures constructed out of these categories. Such conceptualizations are not satisfactory, however, which leads to a **revision** of 'noun' and 'verb'. The revised nouns and verbs, in turn, produce revised versions of 'sentence structure'; and so on. This 'dialectical' back-and-forth movement is the essence of the **inductive method**, as it applies to linguistics. It goes on first at the level of a single language, and then at the cross-linguistic or typological level. Notions like 'noun' and 'verb' are inductively generalized from the first level to the second one because (*pace* Croft) there are sufficient similarities to support such generalizations (cf. Itkonen 2001a). But, in the dialectical fashion, the tentative cross-linguistic categories in turn influence the corresponding language-particular categories; and so on.

In Itkonen (1978: Chap. 11) I applied the hermeneutic circle to solve the philosophical (pseudo-)problem known as the 'paradox of analysis': Let A and B stand for analysandum and analysans, respectively. Now, if A = B,

the analysis is trivial; and if A ≠ B, then the analysis has failed. This is what I said:

"I do not think that this problem can be solved at all in that formal and static frame of reference which is generally characteristic of analytic philosophy. What needs to be seen, is that analysandum and analysans are not just concepts or expressions which are being compared with each other; rather, they represent different stages of a **process**. Analysandum represents a body of knowledge in its prescientific, atheoretical state. Analysans represents a **different** state of the **same** body of knowledge, viz. its scientific or theoretical state. This explains why the two are simultaneously identical and different; the relation between them is a conceptual or necessary one, but it is not logical equivalence in the sense of formal logic. Rather, this relation can be adequately characterized only in terms of Hegelian or **dialectical** logic. Hegel sums up his thinking by saying that it deals with "das werdende Wissen", knowledge in the process of becoming. This aspect is badly neglected in analytic philosophy, which is still today the prevailing trend in Anglo-Saxon philosophy" (p. 300–301).

In sum, and assuming that the 'construction vs. category' distinction can be given a coherent interpretation (corresponding e.g. to the 'sentence structure vs. noun' distinction), their relation is not the one given in Figures 1 or 2, but rather the one given in Figure 3:

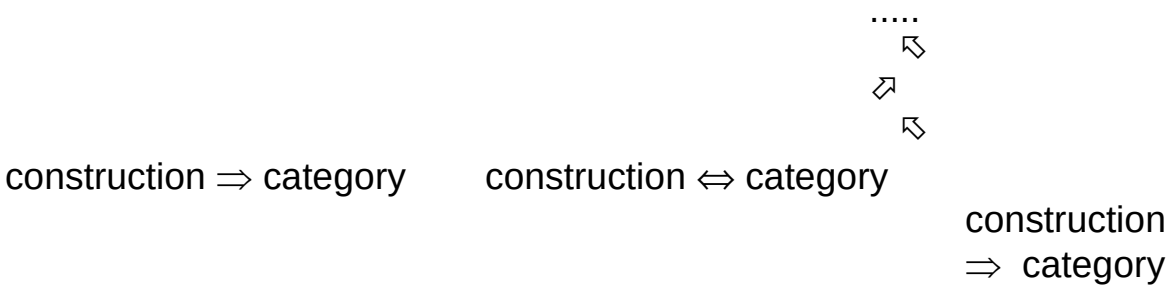

Fig. 1 Fig. 2 Fig. 3

The notion of hermeneutic circle/spiral is well known in Continental philosophy whereas — in spite of its factual accuracy — it has, for whatever reason, remained largely unknown in the Anglo-Saxon analytical philosophy. Nicholas Rescher may be mentioned as a laudable exception. This is how he characterizes his method of 'coherentist inductivism':

"Here there is a definite place for a dialectical process of cyclical structure, where one returns repeatedly to an item already 'established'. For the process of confirmation is now more complex, and a thesis might first appear on the status of a mere datum of low plausibility, later as one of higher plausibility, and ultimately even as a validated truth.

Rather than proceeding linearly, by fresh deductions from novel premisses,

one may be in a position to cycle round and round the same given family of prospects and possibilities, sorting out, refitting, refining until a more sophisticatedly developed and more deeply elaborated resolution is ultimately arrived at. [...] This cyclic process of reappraisal is such that one can even — in suitable circumstances — dispense with the need for 'new' data-inputs in an endeavor to squeeze more information out of the old" (Rescher 1979: 75; for discussion, cf. Itkonen 2003: Chap. 9).

In accordance with the preferences of the Continental tradition, the hermeneutic cycle is generally thought of as a method applied within such 'interpretative' or 'soft' disciplines as philosophy, historiography, and linguistics. Rescher's primary concern, however, is with the methodology of the physical sciences, which just goes to show that we have to do here with a figure of thought to be applied within any intellectual undertaking. To further illustrate this point, let us quote Pawley (2001: 262) on how to reconstruct the Papuan protolanguage:

"This ideal selection can seldom be achieved first up. Often the subgrouping of the languages is poorly understood; and usually the best choice of witnesses does not become evident until the comparative work is well under way. It is to be expected that both the reconstructive hypotheses and the sample of languages compared will have to be revised from time to time."

3) Why 'Constructions' at all? — The Underlying Fallacies of CG

As was already mentioned above, some of the oddities discussed here are not peculiar to RCG, but are shared by all varieties of CG (and by the best-known varieties of CL). Consider the following quotations:

"Fillmore et al. (1988) argue that we should accept the existence of idioms as **constructions**. ... Constructions are like lexical items... Thus, there is a **continuum** between the lexicon and syntactic constructions. Fillmore et al. (1988) also make the next logical step: regular syntactic rules and regular rules of semantic interpretation are themselves constructions. ... The constructional tail has come to wag the syntactic dog... The final step is to recognize that the internal structure of words are also constructions" (Croft 2001: 16–17).

The first thing to notice is the erratic terminology according to which 'rules are constructions', a usage bolstered no doubt by Langacker's (1991: 278) view that "grammatical rules are complex symbolic structures". The second thing to notice is that the argument, attributed to Fillmore et al. (1988), is ominously reminiscent of the non-sensical argument accepted by several representatives of cognitive linguistics: 'literal meaning and non-literal or metaphorical meaning are separate entities; but there is a

continuum leading from the former to the latter; therefore all meanings are metaphorical'. In what follows, I shall try to show what is wrong with the construction-grammar application of this general argument structure, by distinguishing two distinct but related lines of thinking in it.

A) The Fallacy of the 'Constructional Continuum'

The continuum postulated by (R)CG actually conflates two distinct continua: on the one hand, the continuum from large (= complex sentence structure) to small (= affix) and, on the other, the continuum from maximally compositional (= regular syntax) to totally non-compositional (= idioms). Both of these continua are important, but conflating them in the (R)CG fashion only creates confusion.

So why has (R)CG chosen to postulate this hybrid continuum, given that it is bound to create confusion? — because (R)CG wishes to bring out the fact, denied by generative linguistics, that linguistic units (above the phonological level) are form – meaning entities. This is of course the traditional view, codified in de Saussure's notion of *signe linguistique*. The (R)CG-type continuum is just meant to express the insight that not just words, but also constructions above the word-level have abstract meanings that are independent of the particular lexical units that they happen to contain. But this is a modest insight — for instance, the meaning of Intransitive Construction is just 'PRED (ARG)', i.e. "unary-valency predicate-argument semantic structure" (Croft 2001: 21) — and the (R)CG-type hybrid continuum is a wrong way to express it anyway.

Accepting the (R)CG-type continuum amounts to claiming that 'in reality' there are only two linguistic levels, namely sound and meaning. This idea, put forward in Langacker (1987), is quoted approvingly by Croft (2001: 17–18, 20–21, 25–28). But it is a mistaken idea, in more than one way. First, every practitioner of cognitive and/or construction grammar continues to use the terms 'phonology', 'morphology', and 'syntax' exactly as they have been used in the past. Second, there are genuine differences between lexicon and grammar, or between lexical and grammatical meanings, that these terminological innovations are apt to hide.

Let us mention just two of them. First, in all languages of the world, lexical meanings are expressed in the same way, namely by means of the simple form – meaning correlation. By contrast, grammatical meanings are expressed in dissimilar ways, and it is this simple fact, overlooked by representatives of cognitive and/or construction grammar alike, which constitutes the basis of **linguistic typology**. For instance, such a prototypical grammatical meaning as 'plural' (in nouns) may be expressed by suffixes (= Finnish), prefixes (= Swahili), internal change (= Classical Arabic), reduplication (= Indonesian), particles (= Wari'), or zero (=

Chinese), or any combination of some of these formal specifications. Second, in inflectional languages it is customary that (portmanteau) morphemes simultaneously express two or more distinct grammatical meanings (e.g. the Latin *serv-**us*** = 'Nominative', 'Singular', 'Masculine'). By contrast, lexical units express only one meaning at a time, and what may look like distinct meanings stand in taxonomic relations (e.g. *boy* = 'Human', 'Masculine', 'Young'). To be sure, in puns one may wish to express both the literal meaning and the non-literal one simultaneously (e.g. 'The US bombed in Iraq'). — Such de facto differences between lexicon and grammar refute Langacker's (1991: 343) view that "lexicon, morphology, and syntax form a seamless whole".

As was suggested above, it is much more revealing to examine the notion of **compositionality** on its own, rather than as part of the (R)CG-type continuum. Consider the following pairs of sentences (also discussed in Itkonen 2001b: 198–200):

A-1) The book is on the table	vs.	A-2) The picture is on the wall
B-1) Das Buch liegt auf dem Tisch	vs.	B-2) Das Gemälde hängt an der Wand

The sentences A-1 and A-2 are good examples of compositional structures insofar as their meanings result in a transparent way from the meanings of their constituent words. In particular, the distinction between horizontal (= A-1) and vertical (= A-2) is based on the semantic (or, if you like, 'encyclopedic') difference between the lexical units *table* and *wall*. B-1 and B-1 are, by contrast, **non**-compositional insofar as the horizontal vs. vertical distinction is expressed, in addition, by the semantic difference between the grammatical units, i.e. prepositions, *auf* and *an* as well as by that between the semi-grammatical verbs *liegen* and *hängen*.

It is important to notice, first of all, that although B-1 and B-2 exemplify non-compositional (and perhaps, to some extent, even 'idiomatic') **constructions**, they can quite easily be analyzed into their constituent parts, and both the origin and the degree of their non-compositionality can be stated quite exactly, namely by comparing them to such compositional structures as A-1 and A-2 (for discussion, see also Sinha & Kuteva 1995).

Second, it has been much too seldom noticed that the notion of compositionality (clearly enunciated already by Apollonius Dyscolus on the very first page of his *Peri syntaxe_s*) is just a sentence-level application of what Anttila (1989 [1972]) calls the principle of 'one meaning – one form', or the '1M1F principle' for short (see also Itkonen 1983: 208–210). Why, exactly, are the sentences B-1 and B-2 non-compositional? — because they express one meaning (i.e. either 'horizontal' or 'vertical') by three distinct forms. By the same token, A-1 and A-2 are compositional because

they express these meanings only once.

It is obvious that the non-compositional character of B-1 and B-2 is identifiable as **redundancy** (which entails that the structures of B-1 and B-2 are sentence-level analogues of discontinuous morphemes). The opposite of redundancy is **ellipsis**, and elliptical structures are, clearly, non-compositional in the sense of expressing a given number of meanings by a lesser number of forms. — The 1M1F principle is best illustrated by displaying the instances of non-1M1F both at the paradigmatic and at the syntagmatic level (cf. Itkonen 2001b: 89–90, 2004).

It may be added that the 1M1F principle is called 'isomorphism', and regarded as a subtype of iconicity, by Croft (2003). This leads to the following confusion. The fact that plural nouns are in general longer than the corresponding singular nouns is a prototypical instance of iconicity: the linguistic less vs. more distinction is **explained** by the fact that it corresponds to what is ontological less vs. more. Croft (2003: 102) disagrees. For him, the English pair *cup* cs. *cup-s* is 'non-iconic' whereas the Swahili pair *ki-kombe* vs. *vi-kombe* is 'iconic'. Why? — because it is only the latter pair which expresses the meanings 'SG-cup' vs. 'PL-cup' in accordance with the 1M1F principle (for discussion, see Itkonen 2004).

Croft (2001: 25) accepts Langacker's (1987: 63–76) view that the grammarian deals with "a speaker's knowledge of the conventions of their [*sic*] language", repeated e.g. in Langacker (1991:): "our goal is to properly characterize a speaker's knowledge of linguistic convention". Again, there is much to be corrected here. First, 'speaker's knowledge' is psychological while 'convention' is social. Why can't we say that we are — primarily — describing conventions *tout court*? In the 60's and the 70's it was customary for generativists to anticipate Langacker and Croft in claiming that linguists do not describe (e.g.) English, but the **knowledge** of English. Critics like Dretske, Hutchinson, Itkonen, Kac, Ringen, and Saunders pointed out the pitfalls implicit in this view (for documentation and discussion, cf. Appendix 1 of Itkonen 2003). Today (R)CG perpetuates the same confusion. Lakoff's and Langacker's self-contradictory notion of 'conventional mental image' and some related issues have been discussed in Itkonen (1997).

Second, conventions are **normative** entities. If, according to the Langacker & Croft conception, linguistics ultimately deals with (knowledge of) conventions, one is entitled to expect a systematic account of the concept of normativity. Amazingly, both Langacker and Croft are silent on this crucial issue, which means that their overall conception of linguistics has no foundation. The difficult concept of normativity has been explored on dozens of pages in Itkonen (1978) and (1983). A more recent synopsis is given in Itkonen (2003).

Someone might wish to question the claim made in the preceding paragraph. Is it really the case that Langacker's language-conception is

without foundation? Has he not introduced the ‘usage-based model’ into cognitive linguistics, in Langacker (1991: Chap. 10)? And surely **usage** can provide some sort of foundation, especially since the model in question is explicitly claimed to be **based** on it. But one should notice, first of all, the extremely meager data base on which the ‘usage-based model’ itself is actually based. Apart from single words, it consists of the following sentences:

> Tom has an uncle, and Bill does too
> Tom has two ears, and Bill does too
> Tom is painting, and Bill is too
> Tom is talking, and Bill is too
> Tom is writing, and Bill is too
> ? The fire is burning, and the bacon is too
> Well, the fire is burning. Oh my god! The bacon is too.

It is obvious at once that these sentences do not represent any **actual** usage. They have never actually been uttered. They are simple sentences that Langacker’s **own linguistic intuition** tells him to be either correct or (as in the case of the penultimate sentence) slightly questionable. At most, we can say that they are sentences that Langacker may have **imagined** to have actually been uttered. Their status is exactly the same as that of example sentences used in the generativist tradition, for instance, of those 39 sentences that constitute the data basis for Chomsky (1957) (for discussion, cf. Itkonen 2003: Chap. 4). The relation between intuition and observation in linguistics is dealt with at length in Itkonen (2005).

B) The Fallacy of ‘Nonreductionism’: “The Whole is Greater than the Sum of its Parts”

In addition to the (pseudo-)arguments mentioned above, the postulation of ‘constructions’ (as distinguished from ‘regular’, i.e. compositional, syntactico-semantic structures) is also made to appear plausible by claiming that they exemplify the principle that “the whole is greater than the sum of its parts” (Croft 2001: 48). It is this principle (to be called the ‘whole ≠ parts principle’ in what follows) which is claimed to justify the view that “RCG is a **nonreductionist** theory of syntactic representation” (pp. 47–48; emphasis added).

Chapter 10, entitled ‘The reduction of theories’, of Nagel (1961) offers a classic treatment of the present topic in general; and the section ‘Wholes, sums, and organic unities’ (pp. 380–397) is devoted to the whole ≠ parts principle in particular. The point of departure for the entire discussion is constituted by those cases of plane geometry (e.g. a circle and its two

halves) where the whole is demonstrably **equal** to the sum of its parts. This shows, first of all, that in an accurate formulation of the whole ≠ parts principle, the words "greater than" should be replaced by the words "not equal to". Next, Nagel goes on to show that this principle is in fact **trivial** because it applies practically everywhere outside of plane geometry. From the early to the mid 20th century, the most often discussed example of the principle was the nature of water. It was, and is, generally agreed that the nature of water cannot be predicted from, or reduced to, the nature of its parts, i.e. oxygen and hydrogen. Yet, it would be non-sensical to claim, on this basis, that elementary chemistry is a 'nonreductionist' science.

Nagel's argument can be summarized, and simplified, as follows. Consider an unordered set, e.g. {2,1,3}, and an ordered set with the same members or 'parts', e.g. <2,1,3>. It is true by definition that the two sets are unequal to each other and, if we succumb to the temptation to use the vocabulary of 'smaller' vs. 'greater', then it seems natural to say that the set <2,1,3> is 'greater than' the sum of its parts, represented by {2,1,3}. But why do we feel that <2,1,3> is 'greater than' {2,1,3}? — simply because <2,1,3> has a **structure**, namely **order**, which {2,1,3} by definition lacks. Furthermore, we may feel that the whole <1,2,3> may be even 'greater' than the whole <2,1,3>, because the former exemplifies the ascending order of positive integers and can thus be continued indefinitely, whereas the latter seems arbitrary.

The result of the preceding paragraph may be generalized as follows. A structured whole is always 'greater than' the non-structured aggregate consisting of its parts. For instance, the sentence *John likes beer* is by definition 'greater than' the unordered set {*John*, *likes*, *beer*}. This shows that there is no conflict between the whole ≠ parts principle and the notion of compositionality. On the contrary, the former supports the latter.

The confusion beclouding this issue can be illustrated by Lakoff's (1987) view that a 'gestalt' is "a whole [which] is conceptually **simpler** than the sum of its parts", whereas in "technical formal systems, [...] the wholes are just collections of the parts" (p. 486–487; emphasis added). The former quotation implicitly assumes (at least) two distinct stages of a **temporal process**, corresponding, e.g., to 'perceived whole' and 'analyzed whole'. (Compare the analysandum vs. analysans distinction introduced in Sect. 2 above). The statement made in the first Lakoff-quotation is true concerning the first stage, but false concerning the second. By contrast, the second quotation is simply false. It is **never** the case that a structured whole, in 'technically formal systems' or anywhere else, is just **equal** to the (non-structured) sum of its parts.

Now, suppose we try to make use of the whole ≠ parts principle to distinguish between compositional or regular structures and (more or less)

idiomatic constructions, in the way intended by Croft (2001: 47–48). Let us choose his favorite example of an idiomatic construction, namely *kick the bucket* (e.g. p. 26, 56), and let us compare the compositional *John likes beer* with the non-compositional *John kicked the bucket*. We saw above that the principle of compositionality is the sentence-level counterpart of the principle of 1M1F. *John likes beer* has three (lexical) forms and three (lexical) meanings, while the corresponding numbers for *John kicked the bucket* are three (i.e. *John*, *kicked*, and *bucket*) and two (i.e. 'John' and 'died'). The upshot is that, assuming that it is meaningful to speak of 'smaller' and 'greater' in this context, the non-compositional 'construction' is actually **smaller** than its compositional counterpart.

In sum, either the (R)CG vocabulary makes no sense, or the sense it makes is the opposite of what was intended. The same is true of Langacker's (1991: 278) supposedly **nonreductive** usage-based model, assuming that 'nonreductive' is synonymous with 'nonreductionist' as employed by (R)CG.

4) Noun vs. Verb vs. Adjective: The Problems with 'Propositional Acts'

According to Croft (2001: 63), "noun, verb, and adjective are not categories of particular languages. But noun, verb, and adjective are language universals". This view can be deconstructed in the following transparent way.

According to the traditional semantic or **notional** analysis, "nouns denote objects, adjectives denote properties, verbs denote actions" (ibid.); and according the traditional **formal** analysis, there are structural-*cum*-distributional criteria to distinguish between the major word-classes in particular languages. On Croft's view, however, this is all just one big mistake. Why? — First, because the notional analysis is "inadequate" (p. 63) and "discredited" (p. 85); and, second, because "distributional analysis does nothing of the sort" (p. 83).

Once again (cf. Sect. 1), the reader has become intensely curious. As Croft sees it, "RCG offers a solution to the problem of representing syntactic categories, relations, and constructions for particular languages without compromising empirical adequacy and completeness" (p. 85). If neither meaning nor form is of any help, what on earth can this solution be?

The summary of the solution, anticipated e.g. on p. 66, is finally given on p. 87–92. Croft postulates the following three **propositional acts**: 'referring' **identifies** (p. 66) a referent (= object) or **refers** to it (p. 72), 'predicating' **ascribes** (p. 66) something to the referent, and 'modification' **refers** to a property. In other words, Croft takes the ('inadequate', 'discredited') notional analysis, i.e. object, action, property, and postulates

three corresponding acts, namely speaking-of-objects (= 'reference'), speaking-of-actions (= 'predication'), and speaking-of-properties (= 'modification'). The tautological or (viciously) circular nature of the postulated acts is evident from the way they are defined: "**referring refers to a referent**". By the same token, obviously, 'predicating predicates a predicate'.

How does Croft identify exemplifications of these acts? He tells us (p. 87) that "predicating, referring, and modifying **constructions** encode the propositional acts" (emphasis added). But we have already seen (in Sect. 1) that Croft is utterly unable to give any criteria for identifying constructions. Therefore we can safely assume that, when speaking e.g. of English, he is simply using (prototypical instances of) the **traditional word-classes** 'noun', 'verb', and 'adjective' (as they have been established by the "inadequate, discredited" notional-*cum*-formal analysis). To be sure, Croft would have it the other way around: "The three propositional act functions are in fact the foundation for the three-way distinction of the traditional major parts of speech" (p. 87). But it is really the other way around. Nouns, verbs, and adjectives equally 'refer' to (or speak about) things, actions, and properties. What we have, are the linguistic categories, i.e. the traditional word-classes, and, less securely, the corresponding ontological categories (= thing, action, property). What we do **not** have, are the three corresponding ('propositional') acts. Insofar as three such dissimilar acts can be said to exist, their dissimilarity follows entirely from the dissimilarities between linguistic categories, on the one hand, and from the ontological categories, on the other.

The point made in the preceding paragraph can be clarified by means of the following analogy. Consider the three acts of splitting a log with an axe, cutting cloth with scissors, and digging earth with a spade. These are clearly separate acts each of which has its own characteristic nature. But this nature is necessitated by, and defined on the basis of, the nature of the instrument, on the one hand, and the nature of that which the instrument is applied to, on the other. For instance, given an axe and a log, there is only one meaningful or rational way to apply the former to the latter. It is **not** possible to proceed in the inverse order, i.e. to start from some 'disembodied' act and to define the axe (and the log) on the basis of this act. (Starting from such disembodied acts is like assuming a smile without a smiling face.) Now, exactly the same remarks apply to the attempt to define the word-classes **on the basis of** the propositional acts.

Let it be added that the problems connected with 'predication' have been known for a long time: "The logical distinction is unclear — as has often been noticed, there seems no logical reason to treat *John hits Bill* as predicating 'hitting Bill' of John rather than as predicating 'being hit by

John' of Bill" (Sampson 1975: 552).

The Croft-type definition of the word-classes is discussed more fully in Itkonen (2001a). Let us just mention here that the reference to Searle (1969: 23–24), meant to justify the postulation of the propositional acts, is inaccurate.

RCG strives after **psychological reality**, which means that the constructions and categories postulated by RCG should be validated by experimental psycholinguistic research (cf. e.g. p. 5, 52–53). In connection with the noun vs. verb distinction, however, this desideratum is simply forgotten. And yet, many experimental studies carried out on speakers of different languages have established the psychological reality of this distinction beyond any doubt. In fact, Langacker (1991: 60) seems to be closer to the mark, in claiming that "all members of the noun class (and not just central members) instantiate an abstract noun schema, while all verbs elaborate an abstract verb schema".

5) Concluding Remarks

P_nini's characteristically trenchant definition of 'word', i.e. *supti_antam padam*, is central to his analysis of Sanskrit (cf. Itkonen 1991: 17). Analogously, Tolkaappiyan̲aar founded his analysis of Ancient Tamil on the *peyar* vs. *vi̲nai* distinction (cf. Itkonen 2000: 81). S_bawaihi, for his part, postulated three word-classes, while Varro needed four and Dionysius as many as eight (cf. Itkonen 1991: 133, 199, 193). The notion of word-class turns out to be the nearest exemplification that we have of **universals of linguistics**, as distinguished from **universals of language** (cf. Itkonen 2001c). This notion is arrived at inductively, starting from its instances in particular languages. It takes an unusual amount of ignorance of — inter alia — history to assume that this notion, so defined, could be dispensed with.

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