

Linguistic intuitions and beyond

Willem J.M. Levelt

When I wrote my paper for *Advances*, I was fascinated by this 'chapter of psychology' linguists had been writing without asking any of us, psychologists, for permission. In my short career as a psychophysicist/mathematical psychologist I had never come across tree-like representations, but in this chapter they proliferated. What did they represent? Syntactic structures in the mind. And what kind of data were they based on? Linguistic intuitions, grammaticality judgments. The route from judgmental data, such as similarity judgments, to abstract representations was a beaten path for me, but the resulting representations had always been spatial, mostly Euclidian ones (Levelt et al., 1966). Here was a new task for me: collecting syntactic intuitions and developing a mathematical procedure to map them onto syntactic trees. The data I started to collect were subjects' judgments on the degree of syntactic relatedness between the words in a sentence, so-called cohesion data. In the sentence *Ino's birthday is in October* the syntactic relatedness between *Ino's* and *birthday* will be judged to be stronger than the relation between *is* and *in*. This was new stuff (Levelt, 1969), but I missed the boat for the mathematical procedure. Here Steve Johnson (1967) came up with an elegant mapping algorithm that did exactly what was needed.

Would behavioral data on sentence recognition show the same latent hierarchical structure? Yes, when subjects reproduced sentences that were presented in white noise, the conditional probabilities that word j was correctly reproduced if word i was correctly reproduced mapped nicely onto tree structures. These structures reflected the major constituents of the sentence (Levelt, 1970). Now, clearly, linguists were not waiting for behavioral data of this kind. Till the present day they mostly rely on their own linguistic intuitions. At the time, the tacit assumption was that these intuitions are

an almost direct expression of one's underlying linguistic competence, which after all was the *explanandum* of linguistics. This, I realized, was no more than a convenient illusion. In a simple demonstration experiment I showed that trained linguists were hopelessly confused about the grammaticality of pet examples in their own linguistic literature. I concluded that there is "a complete absence of arguments in the literature in favor of the thesis that linguistic intuitions reveal the underlying linguistic competence" and generously advised linguists on how they could minimize the pitfalls of intuitive judgments (Levelt, 1972). That the major empirical base for linguistics was in deep trouble was recognized by many colleagues (see the marvelous review by Schütze, 1996), among them Tom Bever (1970) in *Advances*, but what should be done about it? Labov (1975) correctly pleaded for the use of primary data, i.e. real utterances, wherever possible. He too gave generous advice about the use of grammaticality judgments, which was often cited but never followed up.

But what if linguists would, by some miraculous development, decide to adhere to normal scientific standards of data acquisition? How could reliable intuitive data bear on their theories? This sounds like laboring an obvious point. To know for sure that x is a grammatical sentence and that y is not, is the most elementary support for grammar G that generates x but not y . But it is not so obvious. Any grammar G' that is weakly equivalent to G will be supported by precisely the same data. So, how to distinguish between G and G' ? Grammaticality judgments are to no avail. And I suspect that this is more often than not the theoretical gambit in linguistics. The tension between alternative theories is almost never of the kind "your theory cannot generate sentence x , but mine can". Rather, the fight is about structural and explanatory adequacy. What kind of intuitive data would one need in support of a theory's structural adequacy? (I will leave explanatory adequacy to Bressanone reincarnated). Structural intuitions. What are the cohesions we intuit between words or constituents in a sentence? These are just the kind of intuitions I analyzed in my paper for *Advances*. They tell us something about sentence *structure* and at this point different theories come up with quite different solutions. But for cohesion data it is much less

transparent than for grammaticality data how they would bear on a theory or how they would decide between descriptively equivalent but structurally different theories.

This problem I set out to solve in Levelt (1974), Vol. III. Following standard practice in measurement theory, I developed an interpretation theory that could mediate between linguistic theory and observational cohesion data. For a context free grammar, for instance (and leaving details aside), there would be a cohesion function such that the cohesion of a constituent was always smaller than the cohesion of an embedded constituent. Hence, for *Ino's birthday is in October* the constituent *in October* is more cohesive than the constituent *is in October*. I then defined the cohesion between two elements (words, constituents) as the cohesion value of the smallest common constituent. Hence, the cohesion of the pair (*is, in*) is the cohesion value of the constituent *is in October*, whereas the cohesion of the pair (*in, October*) is the cohesion value of the constituent *in October*. Since *in October* is embedded in *is in October*, the pair (*in, October*) should be intuited as more cohesive than the pair (*is, in*). Such order relations could then be experimentally tested. Notice that this goes beyond the application of hierarchical clustering algorithms - in fact they have become superfluous. It turned out that my experimental cohesion data strongly violated a context free grammar. They were on gracious speaking terms with an *Aspects*-type transformational grammar, but most consonant with a transformational grammar with a dependency-type base. This approach, I believe, is still eminently applicable to a comparison of more recent theories, but apart from the excellent doctoral dissertation of Eric Schils (1983) no further work has been reported along these lines.

Throughout these publications I had kept to my claim that linguistic intuiting was a kind of behavior, rather than a clairvoyant window on linguistic competence. And as Bever (1970) remarked, this invites the study of the intuitional process itself. My ultimate flirtation with linguistic intuitions was just that. Various studies had made it likely that in making a grammaticality judgment about a sentence, the subject would imagine a situation in which the sentence could be uttered. The ease of imagining such a situation would (co-)determine the acceptability of the sentence. This

would predict that a high-imagery phrase would be experienced as more grammatical than a structurally equivalent low-imagery phrase. Levelt et al. (1977) showed that to be the case, and strongly so. More important, however, was the question how much of such interpretation was involved in a grammaticality judgment. Real, all-out, full interpretation? To test this, the same materials were used in a paraphrase task, which did require full interpretation. In both tasks reaction times were measured. It turned out that the imagery variable had a substantially stronger effect on paraphrase reaction times than on acceptability judgment reaction times. This shows that grammaticality judgments involve less than full semantic interpretations. But how much less? Nobody knows

I am now reaching my 1500 word limit for this paper, but I am only seven years from Bressanone. What happened during the next two decades? The Max Planck Society provided me with 'a grant for life' and an ever-growing team of brilliant students and colleagues – among them Ino Flores d'Arcais. Linguistic intuitions were still around in our initial work on linguistic awareness in children, but I decided to concentrate on the most enigmatic of all human behavior, speaking. I would have giggled if anyone had predicted that in Bressanone. But have I lost my fascination about that 'chapter of psychology'? No, I haven't. It is still alive and kicking (see Levelt, 1995).

References

- Bever, T.G. (1970). The influence of speech performance on linguistic structure. In: G.B. Flores d'Arcais & W.J.M. Levelt (Eds.), *Advances in psycholinguistics*. Amsterdam: North Holland.
- Johnson, S. C. (1967). Hierarchical clustering schemes. *Psychometrika*, 32, 241-254.
- Labov, W. (1975). *What is a linguistic fact?* Lisse: Peter de Ridder Press.

- Levelt, W.J.M., van de Geer, J.P. & Plomp, R. (1966).
Triadic comparisons of musical intervals. *The British Journal of Mathematical and Statistical Psychology*, 19, 163-179.
- Levelt, W.J.M. (1969). The scaling of syntactic relatedness: A new method in psycholinguistic research. *Psychonomic Science*, 17, 351-352.
- Levelt, W.J.M. (1970). Hierarchical chunking in sentence processing. *Perception & Psychophysics*, 8, 99-103.
- Levelt, W.J.M. (1972). Some psychological aspects of linguistic data. *Linguistische Berichte*, 17, 18-30.
- Levelt, W.J.M. (1974). *Formal grammars in linguistics and psycholinguistics. Vol III. Psycholinguistic applications*. The Hague: Mouton.
- Levelt, W.J.M., van Gent, J., Haans, A. & Meijers, A. (1977). Grammaticality, paraphrase and imagery. In: S. Greenbaum, *Acceptability in language*. The Hague: Mouton.
- Levelt, W.J.M. (1995). Chapters of psychology. In: R.L. Solso & D.W. Massaro (Eds.), *The science of mind: 2001 and beyond*. Oxford: Oxford University Press.
- Schils, E.D.J. (1983). Cohesion in the sentence. Its use in evaluating grammars. Unpublished doctoral dissertation. Nijmegen University, 1983.
- Schütze, Carson T. (1996). *The empirical basis of linguistics. Grammatical judgments and linguistic methodology*. Chicago: The University of Chicago Press.