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Variation of Semantic Map Display is Necessary

Author's Reply to "What Should Be on a Map?" (Narrog 2010)

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The comment by Narrog (2010) raises various issues concerning my approach to semantic maps as presented in Cysouw (2010a). I thank Narrog for pointing out these issues because his comment gives me the opportunity to clarify a few conceptual assumptions that might not have been made explicit in the original paper (some of those assumptions are discussed in Cysouw (2008), which can be seen as the companion paper to Cysouw (2010a)). As a general conclusion, though, I wholeheartedly agree with Narrog that the endeavor of creating semantic maps is time well spent. My plea to be careful with their interpretation—because semantic maps are always to be interpreted relative to the many analytical decisions made by a researcher (thus, more than one map is possible with the same data)—should surely not be interpreted as claiming that the creation of semantic maps is a futile and wholly relativistic research program. Far from that, I believe that the basic insight leading to semantic maps, namely that linguistic diversity can be used as a tool to investigate meaning, allows for breathtakingly new theories of language. My main problem with the “classical” approach to semantic maps is that it is not using the full power of this insight and unnecessarily restricts itself to a single form of display. More variation in the graphical display of semantic maps is possible, and it is sorely needed in order to deal with the many different kinds of data amenable to the semantic map approach.

As for the issues raised by Narrog, I will reply here to four of them. First he claims that a semantic map based on measurable distances “practically excludes the so-called ‘classical’ or ‘traditional’ maps”. This is a clear misunderstanding because classical maps can be analyzed as being based on a very simplistic measurable distance. The classical maps are thus, strictly speaking, a subset of all maps based on measurable distances. In classical maps, only two possible distances are used, namely “never encoded together by a form in any known language” and “encoded identically in at least some language” (see Cysouw 2007 for full argumentation). So, it is trivial to turn more detailed measured distances into classical semantic maps by, for example, rounding off the distances. Furthermore, it is also unproblematic to turn more detailed measured distances into a graph similar to classical semantic maps. There is actually a large field of graph theory explicitly dealing with the question of how to draw sensible graphs on the basis of complex data. An example of such a method is the Fruchterman & Reingold (1991) graph-layout algorithm which I used in Cysouw (2010b) commenting on Perrin (2010).

Second, Narrog claims that the causative/inchoative alternations used in my paper are not cases of polysemy or polyfunctionality as traditionally used to establish semantic maps. Indeed, there is no direct polysemy between “wake up” and “teach”. However, there is polysemy in the distribution of causative/anticausative morphology for those meanings (as is set out in detail in the appendix to my paper). This is really nothing intrinsically different from the more classical kinds of polysemy used to draw semantic maps.

Third, in discussing Figure 2 in my paper, Narrog says “the first thing that I would have expected was a map [...] presenting the two (or more) dimensions [of the MDS].” Figure 2 in my paper only presents the first dimension of the MDS on the y-axis and something else on the x-axis, arguing that there is a correlation between the two factors. I think the reason behind Narrog’s expectation is that very many presentations of MDS-results are given in the form of two-dimensional graphics. However, the reason for this predominance is only a matter of graphical convenience: two dimensions are easily printed on paper—it is therefore often the first two

dimensions of an MDS that are presented. However, in my paper then I would have had to add a third dimension to argue for the correlation with the additional factor (now simply shown on the x-axis). More importantly, nothing intrinsically in MDS favors the first two dimensions over the others. There are always as many dimensions in an MDS as the number of entities depicted minus one. So, in the case of the 31 lexical concepts in my original paper, there are up to $31-1=30$ dimension calculated by the MDS. Yet, the crucial principle of MDS is that the importance of the dimensions diminishes for higher dimensions. The first dimension captures the largest chunk of variation, the second dimension a slightly smaller (and independent) chunk, and so on. So, only showing the first dimension of an MDS is just as well a possibility as showing the first two dimensions, or the first three. The reason for only showing the first dimension in my paper is motivated by the wish to show yet some other measure in relation to the MDS results.

Finally, Narrog is struck by the position of “die/kill” in Figure 2 in Cysouw (2010a). The closeness to “freeze” and “boil” does not make sense semantically to him. However, he does not make explicit what semantic association would be more suitable; he even asks “what should be represented on a map”. Now, Haspelmath’s (1993) original paper gives a concrete semantic interpretation of the causative/anticausative fraction, namely in the form of the SPONTANEITY SCALE. The more spontaneously an inchoative action can happen, the more linguistically marked the causative is expected to be (i.e. the less anticausative constructions are expected cross-linguistically). It is indeed problematic to assess the position of “die/kill” on the spontaneity scale solely on the basis of causative/inchoative patterns, because the “die/kill” opposition is suppletive in so many languages (so the resulting fraction of anticausatives is statistically not robust for this alternation). However, as argued in Cysouw (2008), the fraction of anticausative encodings is highly correlated with the average difference in length between the inchoative and causative forms across a wide variety of languages. The main outlier on this correlation is “die/kill”. I interpret this result in that paper as showing that the position of “die/kill” on the spontaneity scale can more truthfully be established on the basis of average length difference than on the basis of the fraction of anticausative constructions. This interpretation results in “die/kill” being roughly on the same level of spontaneity as “learn/teach” and “wake up”. I hope that that makes more sense to Narrog’s intuition.

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