

Reply to Göran Hammarström's comment on Esme Winter-Froemel's paper

One of the central terms in Göran Hammarström's comment to my paper is complexity; he concludes his observations with the following remarks: "My ideas about language change are fairly simple and Winter-Froemel's ideas as evidenced in her paper are fairly complex. Am I right in thinking that in many cases simpler ideas are preferable?" My answer to this question would entirely be with Ockham and his razor; nevertheless, I would like to point out two things: first, the main aim of my paper was not to present a coherent approach to the study of language change, but to critically discuss various previous accounts (the S-curve model, Keller, Croft and Coseriu/Aristotle). In this perspective, I have argued that certain refinements to Keller's and Croft's approach prove necessary, most importantly with respect to the notion of diffusion, which can be approached from both a micro-level and a macro-level perspective, and with respect to Keller's notion of static and dynamic maxims in the course of the diffusion process. In this sense I have argued for more complexity. Moreover, I have tried to show that we might overcome or at least attenuate the sharp confrontation between causal and intentional/final accounts by returning to Aristotle's distinction of four types of causes which he perceives as being complementary. Again, this can be seen as a step towards more complexity, but (at least to my view) it might be a helpful step, as it permits us to gain new insights into our domain of investigation.

Second, I have tried to show that both Keller and Croft have specific standard examples or subtypes of language change in mind and develop their theories based on these examples and subtypes of change. However, if we compare the two approaches, we can see that these examples are fundamentally different: while Keller focuses on genuinely intentional innovations, Croft mainly analyzes innovations by form-function reanalysis that are not even perceived by the speakers. In my view, a comprehensive account of language change should encompass both kinds of changes (and possibly others). In this sense, the necessary complexity of linguistic analyses of language change depends on the complexity of the subject, and in order to produce an adequate account, we have to include the various subtypes of changes, including intentional, but also non-intentional changes, speaker-induced and hearer-induced changes, spontaneous and natural change as well as planned or "artificial" change (cf. Keller's remarks on natural vs. artificial phenomena vs. phenomena of a third type, to be explained by invisible hand-processes; in Hammarström's comment the domain of planned innovations is also referred to as "the case where an authority imposes a change"). And again, introducing terminological distinctions for certain subtypes of change (e.g. the alternative between what I have proposed to label catachrestic vs. non-catachrestic innovations) might help us to develop a more fine-grained understanding of the various factors at work, as well as their interplay, in individual processes of language change. So here again, I would say that more complexity may help us to gain a better understanding of language change as a whole.

At the same time, I have argued that a usage-based perspective can serve as a constant (and simple) guideline for our reflections on language change. What seems to be central here

is the distinction between two levels of analysis, the actual-individual level of the speaker's utterances on the one hand (cf. Keller's micro-level) and the historical level of the structures of language/the language system on the other hand (cf. Keller's macro-level). Based on this distinction, we can formulate the claim that all explanations of language change have to be grounded on the level of the individual utterances. In previous research, however, we can observe that the two levels of analysis are not always clearly separated. This is true to a degree in some of Hammarström's observations too. In his argument "Most changes are from a well functioning expression to another well functioning expression. If so, they have no purpose from a functional viewpoint." I would tend to interpret the first sentence as referring to elements of the language system, as this statement talks about a change in the language system and evaluates the overall "functionality" of the (abstract) linguistic elements before and after the change. The second sentence, in contrast, assesses the purpose or utility of the linguistic elements involved, which, following a usage-based approach, should only be referred to at the level of the individual speakers and their utterances. At this level, we can observe that all the various innovations have a certain functionality.

Moreover, Hammarström's observations "Less often the new expression functions better than the old one." and "Less often the new expression seems to be less well functioning than the old expression." raise the question whether functionality can be evaluated and compared along a scale. Hammarström illustrates the former case by various examples of he calls *homonymie dangereuse* and what is also discussed under the label *homonymie gênante*.¹ The latter case is illustrated by the examples of English *you* (being used for addressing one or several persons and thus having become ambiguous) and English *gay*, where the old meaning "can no longer be used after it has taken the present meaning of 'homosexual'". However, for both examples we can think of potentially positive aspects of the innovations: for the speakers who introduced the new meaning of *gay*, this use was clearly motivated by pragmatic strategies (e.g. of designating the taboo concept indirectly, of suggesting a positive value or instead suggesting a deprecatory value based on an ironical/negative understanding of the literal meaning, etc.). For the case of *you*, the innovation could e.g. be motivated by cognitive ease, as the complexity of the morphological paradigm is reduced (but I must admit that these are mere speculations, since I have not

¹ As a side-note, I do not agree with Hammarström's analyses here; to my view, authors such as Eugenio Coseriu, Peter Koch and Wulf Oesterreicher have convincingly argued that from the perspective of the speakers, most cases of homonymy are not at all problematic, and our current research on ambiguity as well as other recent studies also support the idea that the dangers of ambiguity have been considerably over-estimated in previous research. See e.g. the arguments discussed by Coseriu, Eugenio (1958): *Sincronía, diacronía e historia. El problema del cambio lingüístico*. Montevideo: Universidad de la República (Reimpresión fotomecánica, Tübingen 1969); Koch, Peter/Oesterreicher, Wulf (1996): "Sprachwandel und expressive Mündlichkeit", in: *Zeitschrift für Literaturwissenschaft und Linguistik* 102, 64-96; Koch, Peter (2005): "Sprachwandel und Sprachvariation", in: Schrott, Angela/Völker, Harald (Hgg.): *Historische Pragmatik und historische Varietätenlinguistik in den romanischen Sprachen*. Göttingen: Universitätsverlag Göttingen, 229-254; Winter-Froemel, Esme (2012): "Ambiguitätsphänomene im Umfeld des Verbs in der Diachronie der romanischen Sprachen", in: *Romanistisches Jahrbuch* 63, 139-171; Wasow, Thomas/Perfors, Amy/Beaver, David (2005): "The Puzzle of Ambiguity", in: Orgun, C. Orhan/Sells, Peter (eds.): *Morphology and The Web of Grammar: Essays in Memory of Steven G. Lapointe*. Stanford: CSLI Publications. <http://www.stanford.edu/~wasow/Lapointe.pdf> (last accessed 09.12.2011); Piantadosi, Steven T./Tily, Harry/Gibson, Edward (2012): "The communicative function of ambiguity in language", in: *Cognition* 122, 280-291.

studied these changes in detail). Again, these examples thus show that there is a complex range of factors at work, and this leads me to a final remark.

Hammarström seems to understand the term 'cause' in a slightly different sense than I do in my paper, referring to a sufficient condition that inevitably provokes a certain process of language change (cf. his remark "I cannot see that there is a complete cause at work anywhere in language change."). I think that this would be too strong a requirement, as we should assume that most (if not all) processes of language change are characterized by multicausation, so that there are always different causes (or, in Hammarström's term, causal elements) at work. In this sense, I have argued, causal aspects of language change can be analyzed, but I fully agree with Hammarström that it will most probably be methodologically impossible to identify complex sets of causes or causal elements that would permit us to predict language change. To conclude, I would say that the notion of complexity highlighted by Hammarström is indeed fundamental for studies of language change, and that the question how much complexity is needed in a satisfactory theory of language change requires a nuanced answer. Simplicity in description will only lead to clarity if the phenomenon is simple; if it is in reality complex, a simple description will be *less* clear than a complex one.