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FUNCTIONALISM YES, BIOLOGISM NO: A COMMENT ON 'OPTIMALITY AND DIACHRONIC ADAPTATION'

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As I see it, Haspelmath pursues two distinct goals in his paper. First, he wishes to reinterpret the constraints of Optimality Theory (= OT) in functionalist terms, more precisely in terms of 'user optimality'. Second, he wishes to interpret user optimality as a form of 'linguistic adaptation' (= "grammatical structures are adapted to the needs of language users"), with the understanding that "linguistic adaptation is in many ways *analogous* to biological adaptation" (emphasis E.I.).

It seems obvious to me that Haspelmath achieves his first goal perfectly well. His functionalist reformulations of OT constraints are convincing, and he even manages to show that the representatives of OT have themselves been groping for some sort of functional motivation for their constraints. By contrast, it is doubtful whether Haspelmath achieves his second goal. It is this aspect of his paper that I will discuss in what follows.

There is a natural urge to view a less well known (or less prestigious) phenomenon as *analogous* to a better known (or more prestigious) one. The history of science is full of examples of this type of analogy-making. Some such attempts have been successful, others less so. As instances of unsuccessful analogies I mention the following. Aristotle extended the notion of purposive action (e.g. house-building) to inanimate nature (cf. Itkonen 1991: 181-182); Spinoza extended Euclidean axiomatics to ethics; Hobbes and Hume extended, respectively, Galilean and Newtonian mechanics to the explanation of human behavior (cf. Itkonen 1983: 298-302); Toulmin (1972) extended the notion of natural selection to the explanation of scientific progress. Today Haspelmath, Croft, and others (mentioned in References) would like to apply the biological analogy to diachronic linguistics.

For my part, I cannot see how they could succeed. As biological analogues to linguistic change, Haspelmath offers the development of antifreeze proteins in some fish species and the development of long necks in giraffes: in both cases

"the useful genes spread in a species because of the greater reproductive capacities of their bearers". In just the same way, Haspelmath argues, in the linguistic domain "frequency of use is determined primarily by the *usefulness* (or *'user optimality'*) of linguistic structures". There is, however, a fundamental *dis*analogy between the two types of cases. In the case of fish or giraffes the 'usefulness' (of genes) is not experienced, but in the case of speaker-hearers the usefulness (of linguistic structures) *is* experienced, at least unconsciously. Now, it is precisely this experience, and nothing else, which *explains* why people change (or forebear to change) their language in certain ways and not in others.

Saying that a given linguistic structure is (experienced as) 'useful' really amounts to saying that it is (experienced as) a *means* to achieve some *goal*. (Haspelmath mentions the goals of 'saving production energy', 'avoiding articulatory difficulties', 'eliminating threats to comprehensibility', 'avoiding ambiguity' etc.) Thus, we have here the means-end schema characteristic of human behavior in general (for a very extensive discussion, cf. Itkonen 1983). Notice also that this is how 'causal' and 'teleological' aspects of human behavior are reconciled: *Because* (= 'causal') I have the *goal* (= 'teleologial') G and believe that the action A is a *means* (= 'teleological') to achieve G, I set out to do A; and in language, in particular, social control determines whether A will be accepted (or imitated) by the community (cf. Itkonen 1983: 49-53, 201-211, and 1984).

There is, in other words, an *application of intelligence* in linguistic change which is absent in biological evolution; and this suffices to make the two domains totally disanalogous. This becomes even more obvious when, instead of OT-type constraints, we consider such a prototypical diachronic-linguistic process as grammaticalization (which may produce structures conforming to OT-type constraints as end results). There is today a general consensus to the effect that grammaticalization is a two-stage process consisting of reanalysis and extension. The former is an instance of *abduction* whereas the latter is an instance of (analogical) *generalization*. It is impossible to deny that abduction and generalization are cognitive processes, ultimately serving the goal of problem-solving, which intelligent entities like humans *must* perform all the time, but which biological entities like genes *cannot* perform. Trying to eliminate this basic difference leads to confusion.

This is how Cohen (1986: 125) refutes Toulmin's (1972) Darwinist explanation of scientific change/progress: "Hence no evolutionary change of any kind came about through the application of intelligence and knowledge to the solution of a problem. That was at the heart of Darwin's idea. ... And that is why Darwinian evolution is so deeply inappropriate a model ... for the understanding of scientific progress - as if scientific progress could occur without the application of intelligence and existing knowledge to the solution of new problems." In just the same way I am trying here to refute the Darwinist explanation of linguistic change.

An analogy between genes and humans could be understood in one of two ways: either genes behave like humans, i.e. they perform abductions and generalizations; or humans behave like genes, i.e. they lack the capacity to perform abductions and generalizations. Both options should be rejected. Why? - because they are *false*.

Haspelmath is not unaware of these problems. Thus, having offered a Darwinist account of linguistic change, he wants to take it back: he admits that linguistic change also contains intentional elements; and he adds that innovations in the speech of individual speakers are often (?) non-random. However, no consistent picture emerges from this (apparent) compromise. The inconsistency becomes explicit in the abstract of the paper, where it is claimed that biology deals with intentional actions. This is most emphatically not the case.

In sum, linguistic change as well as its results (which conform to OT-type constraints) can be exhaustively characterized in psychological and social terms. To put it roughly, innovation (not 'variation' in the sense of 'mutation') is psychological whereas acceptance (not 'selection') is social. Adding an account in biological terms brings no new information; instead, it either distorts or eliminates existing, fully validated information. So why should anyone advocate the biological analogy? - because biology is thought to be more prestigious than linguistics (or psychology, or sociology). Unlike traditional Indian or Arab linguistics, modern Western linguistics has always suffered from an inferiority complex vis-à-vis the 'hard' sciences (for documentation, see Itkonen 1978).

To avoid any misunderstandings, it needs to be emphasized that in my view Haspelmath certainly establishes what I take to be his main point, namely a

vindication of the functionalist point of view, as opposed to either 'neutral' or openly formalist interpretations of OT constraints.

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A note for the editors: Haspelmath's text contains a reference to 'Itkonen (1984)', but there is no such item in References.