AUTHOR'S RESPONSE

A sparkling apéritif or old wine in new bags?

PIETER MUYSKEN
*Centre for Language Studies, Radboud University
Nijmegen*
p.muysken@let.ru.nl

In the keynote article "Language contact outcomes as the result of bilingual optimization strategies" (Muysken, published online May 31, 2013; henceforth KA), I have tried to accomplish three things:

(a) linking a number of fields of language contact research (code-switching, Creole studies, contact-induced language change, bilingual production), by

(b) assuming four roles that the contributing languages may play ((i) first language dominant, (ii) second language dominant, (iii) neither language dominant – patterns common to the two languages, and (iv) neither language dominant – language-neutral communicative strategies), and

(c) modeling these four roles in terms of bilingual optimization strategies, which may be implemented in an Optimality Theoretic (OT) framework.

Bilingual strategies are conditioned by social factors, processing constraints of speakers' bilingual competence, and perceived language distance. Different language contact outcomes correspond to different interactions of these strategies in bilingual speakers and their communities.

The authors of the peer commentaries on the KA come from very different backgrounds, and I am grateful for their valuable contributions and for pointing out its current weaknesses and limitations, which will serve to help improve this attempt at developing a model for outcomes of language contact. The reason that it took me so long to write and submit this keynote article was precisely that I was not sure about the value of the model myself. I perceive it as a new set of glasses with which hopefully language contact phenomena can be viewed. The commentaries suggest that this is sometimes, but not always, the case. In this response, I will try to clear up some points that have led to misunderstandings and indicate some new directions. A model starting from the linguistic perspective (essentially that of language differences and distance) necessarily is reductionist in terms of a number of other dimensions – time, ethnicity, ideology, processing, interaction, to mention just a few — and thus carries the risk of oversimplification.

Starting with pidgins and Creoles, both MIKAEL PARKVALL and BERND HEINE stress the need to separate individual factors (like component languages) from the actual time course involved in the creation of a pidgin and Creole, a move obvious in the case of the process of grammaticalization, for example. A case in point would be the use of the Papiamentu nominal plural marker *nan*, identical to the third person pronoun. Parkvall makes the valuable point that "limited access" should be qualified as a factor in the genesis of new languages. Often a different target is aimed at by the L2 learners creating Papiamentu than the European lexifier. The story in the KA is unduly phrased in terms of this limited access rather than in terms of identity construction and creating a separate, sometimes incomprehensible new variety. This does not take away the different weight of contributing languages, the main point of the discussion about Creoles in Section 2.3 of the KA. Both Parkvall and Heine warn against circularity in historical accounts of language genesis. Fortunately our knowledge of demographic history and the weight of different lexical contributions has improved dramatically, allowing us to be more confident about scenarios of genesis, although I doubt creolists will ever agree on this. Parkvall wonders whether deliberate simplification, Ferguson's (1971) Foreigner Talk, would not be the main mechanism in the genesis of many pidgins. This is an old idea, but the actual pidgins often look different from what one would expect if they were the basic result of Foreigner Talk. The KA discusses different conceptions of Foreigner Talk. The continuity between the Carib pidgins described by Taylor and Hoff (1980) and Huttar and Velantie (1997) suggests that such strategies can be maintained over several centuries. A final issue regarding Parkvall's commentary is that he argues that reduction processes are crucial in understanding language genesis, and contrasts this with my "universals". In this regard, I would like to point out that I am avoiding the terms "universals" and "universal grammar" and use instead "universal principles"(UPs) to mark very basic communicative styles, essentially without the functional categories that many newly created languages have discarded or reduced (see Muysken, 2008). Thus, our respective positions may be closer than Parkvall thinks.

In his helpful commentary on code switching, RAKESH BHATT notes that the correlation between insertional code-switching and asymmetric power relations may be observationally adequate, but is not sufficiently embedded in a social theory. Also the model does not really deal with more complex cases of code-switching, where more than

two languages are involved and where several types of switches co-occur in the same bilingual interaction. The model is not sufficiently constrained to handle such cases, and the data set is richer than portrayed in the KA. I should note that in Muysken (2000), the basis for the treatment of code-switching in the KA, a richer data set is used, providing evidence that the three strategies distinguished there are linked to specific social settings and language attitudes. However, in the earlier paper, the issue of three languages interacting is not adequately treated either, and the backflagging strategy discussed in the KA is not given prominence.

Regarding Optimality Theory (OT), in her insightful and exciting commentary, DEVYANI SHARMA argues that the KA uses OT metaphors and terminology rather than concepts central to OT as generally thought of. This critique is probably correct, but in part I think it is a matter of formulation, partly due to lack of space. The notions of L1 and L2 are not "universal building blocks of human language" in my view. I agree with Sharma in this respect. However, they can be viewed as bundles of particular grammatical choices (parameters in the generative tradition) that constitute the competence of an individual speaker. My own (perhaps not sufficiently informed) stance would be that many differences between languages can be viewed in terms of different constraint rankings, as in OT, but that trying to reduce all grammatical differences to ranking differences would commit one to a self-defeating enterprise, such as the Cartographic approach, in which all morphosyntactic categories are assumed to be universally given and a selection between them is accomplished by a mechanism of ranking or feature strength (Cinque, 2002 and later work). Many differences seem to come simply from different inventories of functional categories rather than ranking differences. Choosing a particular language involves in large part choosing a particular set of interlocking functional categories, as Myers-Scotton (e.g. 1993) has pointed out repeatedly. Another option I would consider in response to this commentator is the possibility of rephrasing constraints related to the "universal principles" aspect in the KA in terms of OT Economy constraints.

Language contact expert DON WINFORD is very critical of the target article. Among his criticisms, three can be singled out. First, he remarks that the idea that in language contact, either the first language or the second language, or approximations between the two languages involved, or universal principles play a role is by no means new (a similar criticism is voiced by Parkvall). In fact, in the KA, no claim of newness is made. I cite (often much) older sources for all these four ideas in different sub-domains. What I do think is new is my attempt to combine these four explanations into a single model. In the models cited in the KA in Section 1.4, generally the first and second language dimensions are the

central focus, and they do not systematically incorporate language distance, for instance. I do not know more complex models that try to accomplish that. Secondly, Winford argues that the model over-reduces, by making central one component: the contribution of the different languages. This is, however, the only way to bring together the wide range of phenomena I aim to cover, since they differ in other crucial dimensions. Thirdly, phenomena are brought together which, according to Winford, are not related. I will focus on the first of several examples he mentions. He states:

For instance, the following are all treated as instances of the optimization of L1 strategies:

(a) "insertion" in code switching
(b) "relexification" in creole genesis
(c) "relexification" in the case of Media Lengua
(d) replacement of a native affix by a foreign equivalent
(e) "classical" relexification, the grafting of an L2 phonetic form onto an L1 lemma
(f) the phonological adjustment of loanwords

In Winford's view, linking all these is not appropriate. Regarding this list, linking (b), (c), and (e) is uncontroversial, I think, given the literature on relexification (e.g. Lefebvre, 1998). Linking insertional code-switching (a) to relexification is also defensible, as in e.g. Amuzu's (2010) book on Ewe–English code-switching. Amuzu shows that in Ewe–English code-switching generally, the rules of the Ewe matrix language are followed, but that several cases need to be accounted for in terms of a more complex restructuring of Ewe lexical frames to accommodate English material. This is very similar to what we see in Media Lengua-style relexification (e.g. Muysken, 1986). In the case of (d), the process is very similar to relexification: semantic and morpho-syntactic properties of the original item are retained, and the phonological shape of the original affix is replaced by one from another language. In the case of (f), I think the case can be made that retaining a native phonology and incorporating a new lexical shape is also a very similar process. Winford has been a strong advocate of Van Coetsem's (1988, 2000) work, and rightly so, but I do not consider that work an improvement upon the model proposed in the KA, since it basically involves only the axis L1–L2.

The issue of priming and psychological consequences is discussed in the contribution by ROBERT HARTSUIKER, who provides a rich inventory of sources on cross-linguistic priming. His contribution constitutes a welcome attempt to reduce the strategies in the target article to basic processing strategies, notably economy of representation and alignment. Note that "language labels" are crucial to the model. Citing Dijkstra & Van Heuven (2002), Hartsuiker proposes that "all words from each language are connected to their corresponding 'language node'". Many psycholinguists assume that lexical items in the

mental lexicon come with a label "English", "French", etc. One of the main hurdles to be overcome in further integrating linguistic and processing approaches lies precisely in determining the theoretical status (Does it have a place in the theory or not?) and operationalizing (Does every lexical element have such a label and how is it determined?) the notion "language label". Possibly, some words have various language labels, or are indeterminate. Hartsuiker objects to my adoption of the term "interference" when describing cross-linguistic influence. I agree that the use of "interference" in much linguistic L2 and multilingualism literature is a bit of a misnomer.

Altogether, the commentaries provide the seeds for new models in the domain of language contact studies, integrating perspectives from language processing, Optimality Theory, and domains such as code-switching and Creole studies. I look forward to seeing some of these models further developed in the near future, and hope that the model in the KA can serve as a useful starting point in this enterprise. The potential lies in integrating a processing perspective such as the one pioneered in MacWhinney & Bates (1989) with the representational perspectives as found in e.g. Prince & Smolensky (2004), within the framework of the cross-linguistic priming literature described in Hartsuiker's contribution.

One thing to bear in mind in this respect is that it is very easy to get lost in terminology. The family of notions of "universals" is a case in point. In the KA I propose a notion of "universal principles" to refer to the potential for language processing not tied to a particular language or to a set of parametric choices. This is of course rather different from the notion of "Universal Grammar", which refers to the variation space encompassing all parametric choices, and again different from "universals" as in language typology, which refers to properties characteristic of all languages.

This notion "universal principles" could be associated with "Economy", as the mode without specific choices, e.g. of particular functional elements such as Gender or Case. However, as Hartsuiker proposes, "Economy" could also refer to maximizing correspondences between two languages in multilingual processing. Future research would need to disentangle these options, or indeed create a synthesis between them, as proposed by Silverstein (1972a, b).

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