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AUTHORS' RESPONSE

What is the "Nonce Borrowing Hypothesis" anyway?

MARGARET DEUCHAR
JONATHAN R. STAMMERS
ESRC Centre for Research on Bilingualism in Theory and Practice, Bangor University

In this rejoinder to Shana Poplack's response to Stammers & Deuchar (this issue), we argue that our reformulation of the nonce borrowing hypothesis (NBH) to include specific reference to frequency was needed in order to make the hypothesis more precise and testable. Furthermore, in order to test the assumption that codeswitching (CS) and borrowing (B) are two distinct categories, it was necessary to suspend this assumption in our study. This led us to find support for a possible CS/B distinction, but not for the categorical integration of all borrowings regardless of frequency. In discussing our methods, we maintain that soft mutation is an appropriate measure of morphosyntactic integration in Welsh, and is no more purely phonetic than any other morphosyntactically triggered process.

Poplack (P) (this issue) agrees (under section heading: "The Nonce Borrowing Hypothesis: What it is and what it is not") that our data refute our "reformulation" of the nonce borrowing hypothesis (NBH), that "there is no difference between frequent and infrequent donor-language items in terms of their degree of integration". So how does P's formulation of the NBH differ from ours? In her reply, P presents the hypothesis as capturing "the empirical observation that speakers not only code-switch spontaneously, but may also BORROW spontaneously, and these spontaneous borrowings assume the morphological and syntactic identity of the recipient language even PRIOR to achieving the social characteristics of established loanwords (recurrence in the speech of the individual, and dispersion across the community)" (under section heading: "The Nonce Borrowing Hypothesis: What it is and what it is not"). It is notable, however, that this formulation is still not precise enough to be tested, hence the need for us to suggest a more precise, testable version. If even a reply entitled "What does the Nonce Borrowing Hypothesis hypothesize?" still contains no sufficiently precise formulation, we may well ask what the NBH actually is.

In formulating a testable version of the hypothesis we needed to suspend the assumption (strongly held by P) that there is a distinction between codeswitching (CS) and borrowing (B). P criticises us (under section heading: "The role of frequency in the Nonce Borrowing Hypothesis") for "treating code-switching and borrowing as one" but in order to test whether there is a distinction, we needed to refer more neutrally to candidates for both CS and B as donor-language items. Having done this, and comparing the results for listed and unlisted English (donor-language) verbs, we found (a) that the integration of both types of verb was not categorical but related to the frequency of the verb in the corpus; and (b) that the unlisted verbs of a similar frequency to the listed verbs were less well integrated. This could be interpreted to provide support for a CS vs. B distinction, but not for the categorical integration of all borrowings regardless of frequency.

We now address the methodological issue of whether soft mutation (SM), our test of the morphosyntactic integration of English verbs in Welsh, is actually a suitable test. P argues that her previous work has shown that whereas "morphosyntactic integration is a nearly fail-safe indicator that a [lone other-language item] has been borrowed" (under section heading: "Type of diagnostic: Morphosyntax vs. phonology"), "phonological integration is a poor diagnostic for code-switching or borrowing status, precisely BECAUSE it occurs variably in all of them" (*ibid.). In our article we argued that soft mutation is not merely a phonological phenomenon but part of the grammar of Welsh, and we referred to respected sources (Borsley, Tallerman & Willis 2007; King, 2003). For example, Borsley et al. (2007, p. 19) state that "the distribution of the various mutated forms is generally determined by lexical and morphosyntactic factors and never by phonological factors". Despite these sources P considers that "some scholars regard SM as phonetic, others as phonemic" (under section heading: "Type of diagnostic: Morphosyntax vs. phonology"), and argues that although SM might be triggered morphosyntactically, it manifests phonetically "and does not involve alteration of the morphology or syntax" (*ibid.). She compares it to plural marking on lone other-language items in otherwise French discourse, reporting (footnote 1) Poplack, Sankoff and

Address for correspondence:
Margaret Deuchar, ESRC Centre for Research on Bilingualism in Theory and Practice, Bangor University, College Road, Bangor, Gwynedd LL57 2DG, Wales, UK
m.deuchar@bangor.ac.uk

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Miller's (PSM, 1988) finding that plural marking "showed gradual integration according to frequency". P suggests that whereas phonological integration can indeed be expected to increase with frequency, this is not the case with morphosyntactic integration. However, PSM (1988) did not consider plural inflection to indicate phonological integration, but listed it as one of the indicators of morphological integration. The fact that PSM (1988) find similar results to ours does not justify reinterpreting their results as dealing with phonological integration only. Indeed, PSM (1988, p. 68) report that "[e]xamination of rates of French plural marking in words of varying usage frequencies shows that tendency to receive French affixation increases as a function of the diffusion of the word". Turpin (1998, p. 230), also cited by P as having studied plural marking, reports that "the choice of plural mark [on English-origin nouns inserted in Acadian French] is conditioned by the frequency of use of the lone English-origin noun to which is affixed". Turpin concurs with us in interpreting PSM's study of the plural inflection as dealing with morphological integration, and also reports Poplack & Sankoff's (1984) observation that "the more a loanword is diffused (and accepted) in the linguistic community, the more it is integrated into the recipient language, just as expected if these items are borrowings" (Turpin, 1998, p. 231). As pointed out in our article, the role of frequency in morphological integration was recognised by Poplack before the advent of the NBH in 1990 led to its denial.

As a measure of the morphosyntactic integration of English verbs in Welsh, P prefers the presence vs. absence of a Welsh verbal suffix to the presence vs. absence of soft mutation, and she points out that 97.3% of the English verbs in our Table 2 have a Welsh verbal suffix, thus showing the abrupt morphological integration of borrowed elements that she argues to be characteristic. However, just as she argues that soft mutation is triggered morphosyntactically but manifests phonetically, we would point out that the Welsh verbal suffix is also triggered morphosyntactically and manifested phonetically as [io]. In fact, it is hard to imagine a measure of morphosyntactic integration which is not manifested phonetically, either in terms of a change in word form or word order. Furthermore, we argued that – since the insertion of verbal suffixes on all donor language items would be predicted by the Matrix Language Frame model (MLF; Myers-Scotton, 2002) – we needed a more sensitive measure of morphosyntactic integration which went beyond the predictions of the MLF. The variable application of soft mutation to both Welsh and English-origin verbs provided this more sensitive measure.

We are grateful to Shana Poplack for paying so much attention to our paper, and for giving us food for thought. We share her hope that our study, together with her reply and our rejoinder, will stimulate further study and debate.

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