

PEER COMMENTARIES

Is morphosyntactic change really rare?

SARAH G. THOMASON

*University of Michigan, Department of Linguistics,
440 Lorch Hall, 611 Tappan St., Ann Arbor,
MI 48109-1220, USA
thomason@umic.edu*

Jürgen Meisel argues that “grammatical variation . . . can be described . . . in terms of parametric variation”, and – crucially for his arguments in this paper – that “parametric settings do not change across the lifespan”. To this extent he adopts the standard generative view, but he then departs from what he calls “the literature on historical linguistics” (by which he means the generative literature only) in developing the arguments leading to his major claims: that only “transmission failure” resulting from L2 acquisition can produce parametric morphosyntactic change; that any L2 learners, children or adults, may be the agents of change; that such changes “happen less frequently than is commonly assumed”; and that, “in larger and more complex societies, situations in which L2 learners exert a major influence on a language are most likely to emerge in periods of substantial demographic changes” (his example is a plague that kills most members of a speech community). Adult L2 learners, according to Meisel, can only be agents of parametric change if they provide most or all of the input for the next generation’s L1 acquisition.

Meisel’s discussion of relevant results of bilingual and monolingual L1 acquisition studies is impressive, as are his criticisms of serious conceptual and empirical problems with previous accounts of parametric change. But it is difficult to assess his arguments for his own hypotheses about grammatical change, because he provides too little information about their foundations. Perhaps most crucially, what counts as parametric change? He does not say. He does give a few examples, primarily concerning V2 syntax. But he does not say which other morphosyntactic features are parametric, and he does not say how one can find out which ones are parametric. Without this information, it is not possible to tell whether or not his belief about the paucity of parametric changes is plausible.

He also doesn’t say how one is to determine the fact of morphosyntactic change. He cites Poplack and Levey (2009) approvingly on this topic; they analyze three cases of proposed contact-induced change in detail and conclude (convincingly, for these three cases) that no change has taken place at all in two cases and the change was not due to language contact in the third. They then speculate, on the basis of these three cases and brief references to a few others, that “contact-induced change is a good deal less common than the literature suggests”. There is therefore a striking split between Meisel, and Poplack and Levey, on the one

hand, and on the other hand the many authors who have discussed hundreds of examples of contact-induced morphosyntactic change over the years. Poplack and Levey are explicit about what they see as necessary to prove that contact-induced change, or for that matter any change, has occurred: they insist on the application of rigorous variationist sociolinguistic methods to rule out (unchanging) synchronic variation and other possible explanations for the phenomena under consideration. In other words, they reject the last hundred and fifty years of research by historical linguists on language change, including all the research by historical linguists on contact-induced language change. (Using rigorous variationist methods to analyze changes that happened hundreds of years ago is unfortunately not an option.) Unlike Poplack and Levey, Meisel does not offer any criteria for determining whether change has taken place or not; his conclusion, which closely resembles theirs, is based on theoretical reasoning, and its primary empirical testing ground is research (his own and others’) on first-language acquisition.

Because the knowledge accumulated by (mostly non-generative) historical linguists is ignored in Meisel’s theorizing, and because he doesn’t characterize “parametric change” or “morphosyntactic change” precisely, his proposals can’t be tested against the available empirical evidence of language change. Take sentential word order as an example. There are numerous cases in the language-contact literature of changes in basic word-order patterns, from SOV to SVO (e.g. Finnish under Indo-European influence and Ma’a under Bantu influence); from VSO to SOV (e.g. Akkadian under Sumerian influence); from SVO to SOV (e.g. Austronesian languages of Papua New Guinea under the influence of non-Austronesian languages of New Guinea); and from VSO or SVO to SOV (Ethiopic Semitic under Cushitic influence). All these changes fulfill stringent conditions for identifying particular (sets of) changes as due to language contact (see e.g. Thomason, 2001, pp. 91–95): the fact of contact is established for the relevant time period; the languages in contact share independent features in several structural subsystems; and in each case the word-order feature can be shown to be old in the proposed source language and innovative in the proposed receiving language. A crucial question, in the present context, is this: are these parametric changes? If so, how is that determined? If not, how is that

determined? Admittedly, surface word order is not an ideal example, since the same word order pattern may be produced by quite different syntactic structures; but it's a convenient, if flawed, example, both because word order is so easily transferred from one language to another and because explaining more complicated contact-induced morphosyntactic changes would require too much space.

Meisel's theory rules out internally-motivated parametric change completely. He allows for dialect contact as well as contact between different languages as a precondition for change, but this isn't a problem: historical linguists would agree with his view that dialect contact and language contact are the same thing in this context. Examples of internally-motivated change don't involve different dialects (except in a trivial sense in which every idiolect represents a different dialect). Many morphosyntactic changes have occurred – as analyzed by historical linguists, that is – in situations where no significant amount of L2 learning can plausibly be claimed. Are all those changes non-parametric? Or are they not changes in Meisel's analysis? To take one well-understood example from the history of Slavic noun declension, the spread of the animate/inanimate noun-class distinction in almost all Slavic languages proceeded by analogic changes, starting ca. 900 CE. The specific changes were independent of each other – the actual case suffixes often differed from language to language – but the routes of change showed considerable, though by no means complete, parallelism in many instances. The origin of the animacy category might possibly have been due to language contact (Thomason & Kaufman, 1988, pp. 249–250), but that happened before the earliest substantial documentation of Slavic, sometime before ca. 900 CE. But the spread of the category through the declensional systems of Slavic languages cannot be attributed to language contact: the conditions outlined above are not fulfilled, and in any case none of the languages in contact with Slavic languages have such a category. These are certainly morphosyntactic changes, but are they parametric changes?

Aside from foundational issues like the definition of change and the identification of parametric changes, Meisel's proposals reflect an overly narrow, and therefore unrealistic, view of the kinds of social situations in which contact-induced change occurs. His sole reliance on L2 learners as agents of morphosyntactic change runs into the difficulty that not all instances of contact-induced change involve imperfect L2 learning: many changes are brought about by fluent bilinguals who import structures from one of their languages into the other. There is a robust distinction between the kinds of contact-induced changes that are due to imperfect L2 acquisition and the kinds of contact-induced changes that do not feature imperfect L2 acquisition. In the former case, phonological and syntactic changes predominate; in the latter case, non-basic vocabulary predominates (see e.g. Thomason & Kaufman

1988, Chapter 3). (There are a few culturally-determined exceptions to the second half of this correlation, in cultures that ban lexical borrowing.) In focusing on L2 learners, Meisel does not consider the effects of borrowing by fluent bilinguals. He does acknowledge the existence of fluent bilinguals, namely, children who have learned two L1s from birth; but he doesn't allow for contact-induced changes innovated and spread by such people. Consider the bilingual mixed language Michif, for instance. This language, which apparently arose in Manitoba during the 19th century and which still survives on the Turtle Mountain Reservation in North Dakota, comprises French noun phrases (lexicon, phonology, and morphosyntax) combined with Cree verb phrases (lexicon, phonology, and morphosyntax) and sentential structure. It was created, probably with some element of deliberation, by mixed-blood offspring of French (and Scottish) trappers and their Cree wives. That the creators of Michif were fluent bilinguals is shown by the fact that the two components of the mixed language are undistorted. Whether one views Michif as Cree structure imported into French or (much more plausibly) French noun phrases imported into Cree sentences, some parametric changes must surely have taken place – very dramatic changes. There is no evidence of transmission failure, no L2 effects, and yet sweeping contact-induced changes have taken place. This is of course an extreme example; but it is not the only one, and there are a great many less extreme examples too. Such examples, changes made unconsciously or even consciously by fluent bilinguals, seem to me to be problematic for Meisel's theory, which cannot account for examples of contact-induced change in which no transmission failure happened.

Finally, Meisel's proposal that "substantial demographic changes" are required in order for any parametric changes to occur in a sizable speech community is extremely speculative, and he gives just one sketchy example – Basque – to support it. Very much more information would be needed about the Basque case to justify his claim that changes are occurring because L1 learners of Basque are learning the language primarily or entirely from L2 learners. On this point I'd agree with the sociolinguists: one would need evidence that the same parametric changes are not also occurring in monolingual Basque-speaking communities (if there are any), and one would need more than guesswork to establish the point that most (or all?) Basque children are learning their L1 from L2 speakers. In short, one would need more than guesswork to find out exactly how children are acquiring Basque, and from whom.

Not surprisingly, then, I don't find Meisel's major proposal convincing, at least not in its present form. He has strong arguments about relevant aspects of L1 acquisition, both monolingual and bilingual, and his explanation of striking differences between child bilingual L1 acquisition and child L2 acquisition is fascinating and valuable. But

the historical argumentation in the paper is incomplete, and the historical claims are at least partially inconsistent with what is known of contact-induced language change.

References

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