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AUTHORS' RESPONSE

Parametric variation in acquisition and diachronic change: A response to the commentaries*

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1. Claims and hypotheses

The starting hypothesis of the keynote article (KA) is that language acquisition plays an essential role in processes leading to grammatical change. Consequently, a minimal requirement, to be met by explanations of diachronic change is that they rely on mechanisms which are operative in acquisition. The KA is therefore an appeal for interdisciplinary cooperation between historical linguistics and acquisition research.

The discussion is limited to possible changes of core properties of morphosyntax, i.e. to what the theory of Universal Grammar (UG) refers to as PARAMETERS. The rationale for this choice derives from the special status assigned to parameters. In UG theory, they define the variation tolerated by UG across linguistic systems. From the perspective of language development, parameter setting is of special interest because it involves innate knowledge (provided by UG) as well as knowledge resulting from the learners' analysis of the Primary Linguistic Data (PLD). Parameter setting is claimed to happen fast and uniformly across learners. The acquisition of grammatical properties particular to specific languages, on the other hand, is not constrained by UG and is therefore characterized by a significant amount of interindividual variation.

The fact that parameters set to one of the values offered by UG cannot be reset constitutes an important piece of evidence in support of the starting hypothesis. Independently of how this is explained, see Meisel (1995) for a discussion of this issue, research on first language (L1) development has not yet provided evidence for parameter resetting in individuals, i.e. their grammars do not change in this respect across the lifespan. This observation led to the hypothesis defended in the KA that parametric change must happen in intergenerational transmission.

Starting from this assumption, entertained primarily – though not exclusively – in studies working in the UG framework, I argue in the KA that the evidence provided by L1 research does not lend support to the claim that parametric change happens in L1 acquisition. Moreover, although language contact is frequently invoked as a causal factor triggering grammatical change, investigations of the simultaneous acquisition of two or more languages (2L1) demonstrate that such changes are not more likely to occur in multilingual than in monolingual settings. The human Language Making Capacity (LMC), with UG as its core component, is a sufficiently robust device to enable children to acquire native competences of the target language(s), even under less than optimal conditions, e.g. in unbalanced 2L1 acquisition. In successive acquisition of languages, however, subcomponents of the LMC do not remain fully accessible to the learner; second language (L2) learners may therefore fail to set parameters to the target value. This observation has led to the hypothesis that L2 acquisition plays a major role in parametric change. L2 learners may either be themselves the agents of change, or they may serve as role models for monolingual or bilingual ((2)L1) learners. In the former case, diachronic change can be viewed as an instance of transmission failure, whereas in the latter case, the triggering cues are contained in the PLD, provided, at least in part, by L2 speakers of the target language.

2. Interdisciplinary and cross-theoretical dialogues

The broader goal of the KA, calling for interdisciplinary cooperation, is more ambitious than might appear at first sight, and when it implies dialogues across theoretical boundaries, it is perhaps inevitable that a certain amount of space must be dedicated to clarifying particular claims and assumptions. The challenge is to assess the relevance of an argument within the context in which it was developed or to reinterpret it within one's own theory, rather than dismissing a potential problem as a theoretical artefact.

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Only then can factual claims be addressed. This reply is organized accordingly, since some of the commentaries raise issues not concerned primarily or exclusively with the claims made in the KA but rather with its theoretical framework. Let me begin with claims and assumptions, before turning to the discussion of specific hypotheses.

First, however, let me explain briefly why I see no possibility for a fruitful debate without the type of cross-theoretical efforts just alluded to. The significance of theoretical notions or technical terms can only be determined adequately within their theoretical context, irrespective of whether this theory is ultimately adopted or rejected. Thus, comments on a paper dealing with acquisition and change in the framework of UG theory (*vulgo* generative linguistics) or more specifically Parameter Theory (PT), can be expected to attempt to reconstruct the meaning and relevance of particular claims as they are defined by UG or PT. Yet SALIKOKO S. MUFWENE (Mufwene, 2011, p. 152) (SM), discussing the notion of "reanalysis", contends, for example, "that parameter-setting applies, perhaps in ways different from the received doctrine [*sic*, JMM] in generative linguistics". How exactly parameter setting is redefined and how it operates independently of PT, remains a mystery. But whatever solution, based on whatever doctrine, may be implied here, it will be of little interest for the discussion in the KA if it merely retains the label, attributing a different meaning to the term "parameter-setting".

In fact, the commentary by SM illustrates the limits of cross-theoretical debates in more than one respect. It is certainly to the point in that it addresses the major issues raised in the KA. SM rejects all of them, however, claiming, for example, that parameters can be reset repeatedly across a person's lifespan, that grammatical change need not be associated with bilingualism, and that anybody participating in linguistic interactions qualifies as a potential agent of change. The problem is not that SM disagrees with most or all points made in the KA. The problem is that he exposes his own views but does not address arguments or evidence presented in the KA. Yet two (partially parallel) monologues do not make a dialogue. Mufwene (2011, 154) asserts, for example, to be "sure there is no shortage of examples to prove that parameters can be reset several times in a speaker's/signer's lifetime and parameter-setting is not the exclusive capacity of child learners", and he "take[s] issue with the claim that adult L2 learners produce deviant grammatical strategies from the target language because they have no, or less, access to Universal Grammar" (Mufwene, 2011, p. 154). The findings on which these convictions are based are not revealed, however, and can therefore not be discussed. As a rejoinder, I could point out that access to UG has been the topic of a controversial debate for more than 25 years in L2 research (cf. Meisel, 2011), and I could repeat that L1 research has never encountered evidence supporting the idea of parameter resetting in a person's lifetime. But asserting opposing views hardly qualifies as a stimulating debate. I will therefore refrain from doing so and return to some of the points mentioned by SM later, when discussing specific objections raised by other commentators.

Some of the commentaries are primarily directed against 'generative linguistics' and only secondarily against the arguments presented in the KA. In some of these cases it may be useful to attempt clarifying the intentions of the KA. SARAH G. THOMASON (ST), for example, claims that "[b]ecause 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" (Thomason, 2011, p. 147).

I believe that neither of these criticisms is warranted. Concerning the first, the KA starts from the hypothesis that certain types of change happen in the course of acquisition, and it proceeds by critically examining this idea in the light of insights from acquisition research. Consequently, the discussion refers almost exclusively to studies working under the assumption that acquisition plays a significant role in grammatical change. As ST herself points out, this idea is primarily pursued in generative historical linguistics. Hence the focus on the generative literature which, I might add, is reviewed critically, as is acknowledged by some commentators, e.g. Joseph (this issue).

Focusing the explicit discussion on a limited choice of studies does not imply that knowledge gained by other researchers is ignored. I do not see how the claim that parametric change happens less frequently than is commonly assumed can be interpreted as indicating that previously accumulated knowledge is ignored. This conclusion neither disputes the possibility of parametric change nor rejects the idea of language contact as a crucial factor in this process – quite to the contrary, its importance is emphasized by the significant role attributed to L2 speakers. A well-documented example of parametric change is the one from Latin OV to Romance VO order. A number of substantive changes happened during the period when the Ancient Romance languages emerged, i.e. well before the 9th century. It is not implausible to assume that this occurred in settings of the type referred to in Section 5 of the KA where I alluded to the migration of peoples as a possible scenario. But some of these changes happened earlier, in the period of Roman colonization, i.e. in a setting where L2 learners were very likely to have exerted an important role on the emerging colloquial varieties of Latin. The local elites, e.g. Celts in Gaul, acquired the language as an L2, but many of

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the 'Romans' were actually not native speakers of Latin either.

Although I can only invoke in a general fashion the scenario under which the parametric change from Latin OV to Romance VO happened, what matters is that it is not in conflict with previous non-generative research results. Yet most of these studies did not consider issues like learnability or cross-generational transmission and do not provide information which would allow us to decide whether the phenomena investigated can plausibly be assumed to be instances of the kind of change discussed in the KA. If, however, I am correct in arguing that such changes definitely do not happen across the lifespan of an individual and that they are unlikely to occur in monolingual or multilingual L1 development, unless L2 learners are involved, it does not stand to reason to believe that the "hundreds of examples of contact-induced morphosyntactic change" which Thomason (2011, p. 146) is alluding to qualify as such. These doubts are justified by what is known about acquisition, and there is no reason to assume that processes which are not possible in the present could have occurred in the past. This is why Thomason's 'anything goes' hypothesis of contact-induced change (see Thomason, 2001, or Thomason & Kaufman, 1988, among others) is unsatisfactory, for the claim that any linguistic feature can be transferred from any language to any other language (Thomason & Kaufman, 1988) – to the extent that it is at all correct – is more confusing than enlightening in that it conceals substantial differences in transferability. Many of the "hundreds of examples" are not instances of parametric change and do thus not fall into the range of phenomena discussed here. Moreover, as suggested in Section 6 of the KA, some cases of alleged parametric change may be artefacts of particular grammatical analyses and can easily be accounted for without postulating changes of parameter settings. In other cases, no change has actually taken place, as is demonstrated by Poplack and Levey (2010). In view of such conflicting research results it is not sufficient to merely list examples of apparent or real change without providing evidence suggesting that fundamental grammatical change can indeed happen in these settings, and one should certainly not expect this work to be done by scholars who are critical of the claim that anything goes in contact-induced grammatical change or accuse them of ignoring previous research if they fail to do so.

This brings me to ST's second criticism, that I do not explain in sufficient detail what characterizes and what counts as parametric change. Critical objections referring to the notion of parametric change are also raised by SM and by Stephen Matthews and Virginia Yip (M&Y). Three issues seem to be involved here: (i) the presentation of PT in the KA, (ii) the question of how to distinguish empirically between parameter setting and other acquisition processes, and (iii) the validity of PT as an explanatory tool in historical linguistics.

Concerning the first point, I admit that the presentation of PT in the KA is sketchy, and the fact that PT changed considerably over the past 30 years may be confusing. However, answers to most of the questions addressed by ST and SM are already offered in earlier publications; e.g. Meisel (1995). As for the insufficiently detailed summary of PT, I do not want to claim that this could not have been done better, but one should remember that the purpose of such a summary cannot be to introduce the reader to the theory but only to remind those who already have a basic understanding of PT of its major characteristics. This is why the KA mentions some revisions of PT, emphasizes the claim that parameters refer to uninterpretable features of functional heads, and suggests that both micro- and macroparameters need to be part of our inventory of theoretical notions; see Roberts and Holmberg (2010) for a recent discussion of this issue.

The KA also discusses the issue of distinguishing parameter setting empirically from other acquisition processes. It is not intended to be an exhaustive treatment of this question, and ST is correct when she observes that it primarily refers to empirical evidence from acquisition. This is entailed by the hypothesis that what does not happen and arguably cannot happen in contemporary instances of bi- or multilingual acquisition, did not happen in the past, and claims based on such processes do not constitute convincing evidence for contact-induced change. This is a deductive line of argument, but contrary to what ST asserts, this does not mean that no criteria are offered for determining whether change has taken place; see Section 1 of the KA. At any rate, there is no need for SM's admonition that "(i)t is imperative that syntacticians address the fundamental question of what it really means to say that the learner has set the parameters to such-and-such language-specific values" (Mufwene, 2011, p. 154). Leaving aside the fact that this issue is primarily dealt with in acquisition research rather than in syntax and that it does not concern rule learning, the questions asked by SM have all been discussed in much detail in the relevant literature, and some answers are summarized in Section 3 of the KA, for example those concerning the triggering of parameter setting (the learner as folk linguist in SM's terms) or the difference between inductive learning and triggering, the latter implying abrupt changes. For detailed treatments of these issues, see Meisel (1995 and 2011, Chapter 2).

I do want to acknowledge, however, that ST raises a potentially serious problem because not all empirical criteria developed in acquisition research are applicable to studies of diachronic change. To mention only one example, abrupt change in individual grammars may not spread in a "catastrophic" fashion across members of

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the speech community. Yet empirically testable criteria do exist, based on which it is possible to determine the parameter values instantiated in grammars at a given point of diachronic development, e.g. the cluster of superficially unrelated grammatical properties; cf. Section 1 of the KA. There also exist a sufficient number of uncontroversial parameters allowing one to put to a test the hypotheses about parametric change developed in the KA.

This leads to the third point: the status of Parameter Theory. M&Y surmise that PT "dictates a severely limited view of what constitutes grammatical change" (Matthews & Yip, 2011, p. 159), and they remark that PT has been abandoned by some of its proponents, insinuating that it is an outdated theoretical approach.

Neither of these conjectures is correct. In fact, the first one stands in stark contrast to explicit statements in the KA. Its theoretical framework does not oblige one to focus on parameters nor does it exclude other phenomena from consideration. I tried to state this unambiguously: "To avoid possible misunderstandings, these are not only legitimate but indeed crucial issues for diachronic studies of language – yet they are not directly relevant for the question pursued in this paper" (Meisel, 2011, p. 122). Let me repeat once again that I decided to focus on parametric change precisely because grammatical systems are particularly stable in this domain whereas other aspects of grammar are more easily subject to change. It is thus not PT which excludes the types of variation from consideration IN THIS PAPER which M&Y find more interesting, e.g. shifts in the frequency of variants, it is the author's choice to focus on different issues. But in the conclusion of their commentary, M&Y even claim that I adopted "a theoretical model which excludes from the outset much of contact-induced change (including anything that does not fall out from parameter settings)" (Matthews & Yip, 2011, p. 161). I find no argument in their commentary which might justify this supposition. It misses the intentions of a paper which actually demonstrates the importance of language contact for change, even if monolingual or bilingual L1 learners are not identified as its major agents. It also misrepresents a theory which is neutral with respect to the possibility of contact-induced change; not to mention the fact that PT cannot and does not exclude developments which are not related to parameter setting and thus do not fall within the domain of this theory.

The only explicit criticism of PT in the commentary by M&Y does not justify the claim that PT cannot account for contact-induced change. It states that binary parameters do not allow for a "third grammar", an option which they claim to be suggested by typology, citing head-initial and head-final constituents in Chinese. In this sense, however, the critique is void. Since parameters relate to the featural specification of functional heads, different values across functional categories can be instantiated in the same grammar. And although PT indeed excludes double settings for one category, distinct options can appear in surface patterns, i.e. in the linguistic properties typology commonly refers to. M&Y cite the example of the Head-Direction parameter, which I did not mention, because, in its traditional form, it relates to surface linearization rather than to properties of functional heads. But as has been demonstrated by Neeleman and Weerman (1997), it can be reformulated in terms conforming to a more recent version of PT; see Meisel (2011, Chapter 5) for more details. At any rate, OV languages like Dutch or German exhibit OV and VO surface order, the latter resulting from finite verb raising; only non-finite verbs appear consistently in post-object position. Thus, although no intermediate or double settings of parameters are tolerated, both OV and VO patterns are generated.

To conclude this section, I will merely say that M&Y are wrong if they believe that Parameter Theory is abandoned. As is to be expected in scholarly debates, it has been the object of critical discussions, resulting in several revisions. Currently, competing models are proposed, differing primarily in how they reinterpret traditional parameters as micro- or macroparameters. Importantly for the current debate, parameters have not been replaced by other concepts in UG theory. Rather, PT is applied successfully in studies of acquisition and diachronic change, as is evidenced by the growing number of recent publications; see Biberauer et al. (2010) or Snyder (2007) as well as the references in the KA.

3. Controversial points

Let me now turn to the critical remarks concerning specific claims and hypotheses rather than the theoretical approach of the KA.

3.1 Parametric changes in mature grammars?

That a parameter set to one of its values cannot be reset across the lifespan of an individual is one of the claims which some commentators object to. This is a crucial point, because the role attributed to acquisition in the explanation of change depends on it to a large extent. If this claim is correct, parametric change is by necessity the result of intergenerational transmission of grammatical knowledge, and processes underlying diachronic change are fully constrained by acquisition mechanisms. If it is not correct, parametric change in the course of acquisition is still a possibility, alongside changes in mature grammars, but restructuring processes are not necessarily constrained by the principles guiding acquisition.

The question then is what kind of evidence the commentaries offer in support of the idea of alterations of parameter values in mature grammars. As mentioned

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above, SM merely asserts that this must be possible. BRIAN D. JOSEPH (BJ), on the other hand, considers various arguments pro and con. He finds the idea of children as grammar innovators plausible but observes that there "is no empirical evidence that change NECESSARILY originates in the generational transmission process" (Joseph, 2011, p. 156). This observation is correct but implies a requirement which cannot be met in principle. No matter how much positive evidence in support of a hypothesized process is accumulated, nothing in the data can possibly tell us that it will necessarily happen on all future occasions. Note that my claim makes a negative prediction i.e. that a parameter once fixed to a specific value cannot be "reset" to a different one. This can be falsified by means of positive evidence demonstrating that resetting does occur. In the KA article, I refer to studies of L1 acquisition which searched but failed to discover evidence for resetting and to work demonstrating that mature speakers adapt their language use to innovative patterns without, however, showing evidence of modifications of parameterized grammatical knowledge.

As BJ correctly observes, this line of argument relies on the distinction between language use and underlying grammatical knowledge. Consequently, the decision on whether innovations in linguistic behaviour constitute counterevidence to the no-resetting hypothesis depends on whether the new usages are analyzed as reflecting changes in mentally represented grammars; it is therefore partly determined by theoretical assumptions guiding this analysis. We should thus not be surprised to find that there is disagreement on how to classify specific examples. Yet since those favouring the idea of parametric changes in mature grammars do not offer principled reasons for why this should be possible – in fact, they do not discuss the psycholinguistic counterevidence – this debate is limited to a discussion of examples. As for those given in the commentaries, I fully agree with BJ that "adults' production can be affected also by social pressures" (Joseph, 2011, p. 157) – this is what I had in mind when talking about mature speakers' adaptation to their linguistic environment. But this does not involve restructuring of the grammar acquired in childhood, as is confirmed by Joseph's (2011, p. 157) observation: "I find these forms occasionally coming out of my mouth". I also agree that some newly acquired knowledge guides this usage, much like in L2 acquisition. This is also how I interpret the examples mentioned by SM, for example that of "a native speaker of an English dialect with double negation and negative concord [who] can change these particular features after being drilled at school with standard English grammar and interacting with speakers who avoid these features" (Mufwene, 2011, p. 154). Rather than "resetting" parameters in the L1, these speakers acquire a new grammar.

The Greek example cited by BJ, however, is of a different type. It illustrates that a nullsubject language may sometimes require an overt subject. Lexically restricted constructions are indeed well known. But contrary to what BJ seems to suggest, this does not imply permanent resetting of the Null-Subject Parameter since, as BJ correctly observes, Greek continues to be a null-subject language. Note that if these uses triggered a parametric change in that adult speakers of Greek treated it consistently as a non-null-subject language, this would indeed constitute a counterexample to the no-resetting hypothesis.

3.2 Idiolectal variation: Does monolingualism exist?

The problem addressed in the preceding section partly concerns the question of how to interpret variable usage and whether it indicates modifications of speakers' grammatical knowledge. A similar issue is raised, although in a more fundamental fashion, by SM and by DAVID LIGHTFOOT (DL) both of whom, although from different perspectives, question the notion of "language" adopted in the KA. This is an interesting problem, but one much too broad to be dealt with adequately in the present context. Let me nevertheless try to respond briefly to these commentaries.

SM and DL both claim that there is no need to turn to bilingualism when searching for triggers of change; they find all the necessary variation in monolingual settings and regard idiolects as proper objects of investigation rather than what they implicitly (DL) or explicitly (SM) view as a Saussurean notion of language.

DL begins by asking how many languages exist and proposes three answers: one, over six billion, or 7,358; he argues that only the first two have a basis in biology, and he favours the second, thus rejecting "sociologically ill-defined notions like German and Turkish" (Lightfoot, 2011, p. 163). Yet the claim that the third answer (language3) has no basis in biology is incorrect, for language3, too, is an expression of the genotype in that UG constrains grammars of language3 type. If, however, DL intended to say that language3 cannot be characterized exhaustively in biological terms, I fully agree. I do not agree, however, with the conclusion that what cannot be fully accounted for in biological terms should be excluded from the domain of linguistic investigation. Although we focus on I-language and thus on mental grammars of individuals, meaningful insights about acquisition or change can only be gained if we take into account that these individual grammars define type 3 languages and that these ill-defined but factually operative social entities have repercussions on idiolects. "Language" can thus be understood as referring to the unification of properties represented in the mental grammars of individuals, i.e. as

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an idealized abstract object rather than as a Saussurean institution, as surmised by Mufwene (2011, p. 152).

DL argues that monolingual first language acquisition does not exist – not because of individual bilingualism but because children are exposed to numerous idiolects which offer all the necessary ingredients for change. This is true in an obvious sense, referring to the multitude of constructions surfacing in regional, social or situational varieties, yet this is hardly a new insight. At least since Schuchardt (1885), we have been informed about language-internal contact phenomena resulting from the heterogeneity of linguistic systems. However with respect to the issue at stake here, parametric change, DL's statement is probably false. As opposed to true bilingual settings involving more than one language3, no cues for conflicting parameter values are present in situations of theoretical multilingualism – unless provided by dialects of a language3, a case covered by the definition of bilingualism as including bidialectism. Moreover, if "[n]o two children hear the same PLD, because there is too much variation in ambient speech" (Lightfoot, 2011, p. 162), these PLD trigger not only different grammars for each individual but multiple grammars within individuals, as suggested by Roeper (1999). Consequently, DL's second answer must say that there are billions of languages. Either way it leads to what I consider as an unsatisfactory approach because it fails to account for what relates subsets of this plethora of idiolects and thus also for speakers' implicit knowledge about which parts of grammars can be used in a particular setting defined by "language x". It cannot explain, for example, why speakers do not oscillate between different parameter settings. Splitting languages into idiolects may succeed in defining grammars as homogeneous objects – but at the risk of circularity of argumentation. It would be wise to remember that Schuchardt (1885) also demonstrated that neogrammarian sound laws applied without exception within a language only because the decision on which properties to attribute to a language was guided by these laws.

At any rate, as far as I can see DL's observations do not weaken the central argument of the KA because it does not depend on the notion of language, as it is interpreted by DL. When I claim that changes in parameter settings happen less frequently than is commonly assumed in historical linguistics, the truth or falseness of this statement does not depend on how particular parameters, or rather which settings of these parameters, combine in one language or idiolect.

3.3 Acquisition, transmission and diffusion

Several commentators take issue with the role attributed to acquisition or transmission in the KA. I will mention only briefly the ones which are primarily concerned with terminological problems. SM, for example, correctly observes that terms are defined differently under different theoretical premises. But it is not obvious how this affects the argument developed in the KA.

DL, on the other hand, objects to the term "transmission" because he apparently interprets it as implying an intentional activity by the caretakers, and he rejects the term "acquisition failure", arguing that children unfailingly acquire what they encounter in the PLD. But neither of these claims is in conflict with the KA, where "transmission" refers to the fact that speakers of one generation produce the PLD which serve as input for the next. If, then, learners fail to fully reconstruct the grammar underlying the PLD of their linguistic environment, this is termed acquisition failure. L1 children, however, are typically successful and are thus not agents of change resulting from acquisition failure. This is precisely the claim of the KA. L2 learners, on the other hand, may fail to acquire knowledge which L1 children develop rapidly and successfully, in spite of the fact that they are exposed to PLD which contain the relevant cues. This is referred to as "unsuccessful" or "incomplete acquisition" in L2 research.

Let me add that I did not claim that transmission failure resulting from L2 acquisition is the ONLY cause of parametric change. I argued, rather, that it is not uncommon to find that L2 speakers are role models for L1 children. If the latter then develop grammars distinct from those of the previous generation, they are agents of changes which are, however, not the result of transmission failure. I do not rely solely on L2 learners as agents of change, contrary to what ST writes. This is why I like to think that my proposals do not constitute an "overly narrow, and therefore unrealistic, view of the kinds of social situations in which contact-induced change occurs" (Thomason, 2011, p. 147). I even agree with ST's suspicion that changes can be brought about by "fluent bilinguals", provided she is referring here to L2 learners. In the case of Michif, the example mentioned by ST, this must certainly have been the case, even if ST asserts that "[t]here is no evidence of transmission failure, no L2 effects, and yet sweeping contact-induced changes have taken place" (Thomason, 2011, p. 147). Although I am not an expert in this domain, I suppose that when interacting with their partners and children, the French and Scottish trappers and their Cree wives did not use only their respective first languages, but also the L1 of the other parent – i.e. their second language.

Concerning the diffusion of change, I readily admit and said so in the KA that the sketches of possible settings involving substantial demographic change are speculative. But I hope that these scenarios are at least plausible.

Finally, several commentators emphasize that the possibility of internally-motivated parametric change cannot be ruled out completely, e.g. Thomason (2011, p. 147). Yet again, this is not in conflict with the argument

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made in the KA, where I repeatedly pointed out that this cannot be excluded, stating, for example that "[t]he evidence scrutinized here is, of course, not sufficient to . . . deny the possibility of internally motivated change" (Meisel, 2011, p. 129), qualifying contact as "possibly a necessary though not sufficient cause" (p. 122) of grammatical change. I understand that this is a less assertive statement than might be desirable, but it captures adequately our current knowledge.

3.4 Types of acquisition

Two commentaries ultimately address specific issues related to acquisition processes. M&Y believe that I underestimate the role of 2L1 acquisition as a source of contact-induced change and claim that cross-linguistic interaction plays a more important role in unbalanced 2L1 acquisition than I am willing to acknowledge. They further argue that the Fundamental Difference Hypothesis (FDH) cannot hold for successive language acquisition by children, pointing to my observation that child L2 acquisition resembles 2L1 in some domains and adult L2 acquisition in others.

Their conclusion does not, however, follow from the latter observation. Recent studies of age and maturation show that different grammatical domains are affected at different ages. The FDH thus does not apply to "language" across the board but to selected domains of grammar, and these are affected at different points of development; see Meisel (2009, 2010) and references therein. The fact that child second language acquisition (cL2) resembles (2)L1 in some domains but adult L2 in others follows directly from these observations and by no means implies that it is impossible to distinguish between 2L1 and L2.

I nevertheless agree with M&Y that the question of whether unbalanced 2L1 can explain grammatical change need not be related to the problem of whether the "weaker language" (WL) resembles L2. In principle, the WL could indeed be the source of parametric change even if it differs from L2 in this respect. But contrary to their claim, it is not only possible but necessary to distinguish between simultaneous and successive acquisition. I argue that cL2 learners are possible agents of change. The controversy thus only concerns unbalanced 2L1 since I contend that the available evidence does not warrant the conclusion that parametric change happens in the WL. The disagreement is about (i) the role of "dominance" and (ii) whether observed instances of cross-linguistic influence reflect changes in the children's grammatical knowledge or in their language use. As for (i), numerous studies have shown that dominance does not necessarily result in such effects and that the WL can also influence the stronger one; see Hulk and Müller (2000). It may well be that more than one factor is at play, as suggested by M&Y, but this is not as trivial an issue as M&Y believe because structural constraints constitute the necessary though perhaps not sufficient condition for this to happen (Hulk & Müller, 2000) whereas dominance is at best an additional factor triggering this process; see Meisel (2007). Concerning the more crucial point (ii), this is hardly a matter of typological distance, as insinuated by M&Y. The in-depth investigation of Basque–Spanish children, for example, revealed no such effects on their grammatical competence; see Meisel (1994). Rather, the fact that the phenomena under investigation typically appear only temporarily and often after a phase during which target-conforming constructions were used exclusively, suggests that we are looking at a phenomenon happening in language production. In sum, I agree with M&Y that it is not implausible to expect the knowledge of the WL to differ from that of monolinguals or balanced bilinguals, even if, in my view, current evidence does not suffice to support this assumption. But M&Y have provided some evidence pointing in this direction, and if this can be corroborated by other researchers, I am prepared to add this scenario (WL in 2L1) to the ones suggested in the KA, as possible settings favouring parametric change.

Fred Weerman (FW), finally, makes an important point suggesting that the opposition between early and late acquisition may be a more important cause of change than bilingualism, late acquisition referring not only to child and adult L2, but also to late L1. This is indeed a possibility which I did not consider and which may play a major role in grammatical change. FW correctly qualifies late L1 learners as a link between L2 learners and new generations of L1 learners. Still, I believe this to be a type of L2 acquisition resulting in L2 islands in L1 grammars, as argued by Meisel, Elsig and Bonnesen (2011). But this is perhaps no more than a terminological quarrel. FW's suggestion undoubtedly deserves further investigation.

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