

Revisiting Child Second Language Acquisition: Rejoinder to Commentaries

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In my target paper, I formulate several *claims* concerning possibilities and limitations of successive acquisition of bilingualism during early childhood, and I present theoretical and empirical *evidence* in support of these claims. The particular perspective developed here is based on *assumptions* based on research results drawn from a large body of studies investigating simultaneous as well as successive acquisition of bilingualism.

The research interests pursued in this work are motivated by theoretical as well as practical considerations. As for the former, they focus on the question of what the human language making capacity (LMC) is able to achieve. The past 25 years of research on the simultaneous acquisition of more than one language since birth ((2)L1) have demonstrated that, contrary to earlier beliefs and prejudices, the LMC is equipped to cope easily with the task of developing two or more mental grammars simultaneously. It turned out, in fact, that the LMC is much more robust than previously expected. But there are surely challenges which exceed its possibilities, calling thus for an exploration of the limits of the LMC. Epistemologically, then, the goal set by this research agenda is to gain insights into the nature of the LMC. On a more practical side, the particular interest of this approach resides in the fact that research results obtained by this endeavour are urgently needed in view of the steadily increasing social significance of bilingualism. Insights gained by this research are, in fact, of immediate relevance not only for our understanding of the human mind but also for the search for solutions to socially pressing problems. If, for example, AOA is indeed a crucial factor determining the possible success of L2 acquisition already in early childhood, this has important implications for the large and growing number of children raised monolingually in a bilingual setting.

The *assumptions* on which my claims are based can be summarized as follows:

- (2)L1 and aL2 differ in fundamental ways, i. e. observable differences between these types of acquisition reflect partially different kinds of grammatical knowledge. I am thus adopting a modified version of the Fundamental Difference Hypothesis (FDH).
- These fundamental differences are caused primarily by maturational changes in the brain affecting the LMC.
- As a result, the developmental process is characterized by a number of sensitive phases, affecting different domains of grammatical competence at distinct ages. During the maturationally defined optimal phases, the respective grammatical phenomena are acquired relatively easily by mere exposure to the primary linguistic data (PLD).
- Parameterized principles of UG define the most important grammatical domain subject to maturational changes.

The *claims* which I have tried to argue for can be summarized in the following way:

- Age of onset of acquisition (AOA) makes a significant difference in language acquisition, distinguishing successive acquisition from both monolingual and bilingual L1 development.
- The relevant changes happen at a much earlier age range than is commonly assumed, namely already at around age 4;0. Consequently, L2 research should pay special attention to the study of learners whose AOA lies in the age range between 3;0 and 8;0 years.
- Not only parameterized principles of UG, but discovery principles as well are affected by maturational changes.
- In view of the limited prognostic power of linguistic and neuro-cognitive theories in this respect, identifying the grammatical properties affected by AOA and the particular developmental schedules represents a serious challenge for acquisition research.
- Theoretical as well as empirical considerations suggest that, contrary to what has been claimed recently, inflectional morphology represents one of the areas in which cL2 resemble aL2 learners and differ from (2)L1 children.

A number of commentaries to these hypotheses brought forward in my target paper emphasize that factors other than inaccessibility of parts of the LMC can contribute to an explanation of the differences between monolingual or bilingual first language acquisition, as compared to successive language acquisition by children or adults. This is undoubtedly correct, and I find these proposals helpful for our efforts to attain a more comprehensive understanding of the underlying causes leading to differences between various types of acquisition. In fact, although I do

want to maintain the claim that maturation of the neural system is the major causal factor for differences in linguistic knowledge between successive and simultaneous acquisition of two or more languages, it is certainly not the only one. Let me thus state explicitly what I thought was obvious, namely that by focusing on one specific aspect of child L2 acquisition, i. e. maturational effects, my intention was not to implicitly claim that no other factor could possibly be relevant. On the other hand, the role of age of onset of acquisition is not minimized by acknowledging the relevance of additional factors.

Moreover, if my discussion of maturational changes focuses on negative consequences for the L2 learner, this is motivated by the research interests briefly reiterated at the beginning of this rejoinder. It is precisely because of the impressive efficiency of the LMC as evidenced by recent research on monolingual and bilingual first language acquisition that one wonders what limits there are for the LMC – and this is the interest pursued in the target paper. But this does not imply that (2)L1 – L2 differences will in all or even most instances materialize as deficits, as is pointed out correctly by *Johanne Paradis* and by *Suzanne Schlyter*. How could one possibly disagree with *Paradis*' statement that maturation will also – in most cases, in fact – result in new abilities? Note that, according to the assumptions underlying my discussion, neither lexical learning nor the acquisition of “story grammar” or of narrative competence is subject to maturational changes of the sort discussed in my paper. *Paradis*, *Schlyter*, *Tracy* and *Verhagen & Schimke* are undoubtedly correct when pointing out that the rate of acquisition of cL2 learners tends to be faster than that of adult L2 learners, an observation already made by *Pienemann* (1981) and others since. The example of a faster rate of acquisition in cL2 learners not only in comparison to aL2 learners but also in contrast to L1 children, presented by *Paradis* and mentioned in passing by *Tracy*, is nevertheless intriguing and deserves further investigation. But, to pursue my negative line of thought, we also need to determine whether earlier achievement of new abilities indeed enhances the capacity of language acquisition. That this is not necessarily so is suggested by *Felix* (1984); cf. 2.2, in the target article. He argues that increased cognitive and memory capacities of L2 learners may interfere with the efficient language learning mechanisms of the young child. Successful acquisition would thus depend on inhibiting the newly developed capacities. A similar argument is, in fact, made in *Goodluck*'s commentary where she reminds us that “less may be more” in L1 development.

As one of the commentators who remind us that factors other than the ones discussed in the target paper can also account for L1 – L2 differences, *Helen Goodluck* argues that changes in sentence processing mechanisms should be taken into account. This is certainly a plausible

assumption, compatible, in fact, with what I alluded to in section 2.2 as a possible explanation of the changes affecting processing and discovery principles. Yet it remains to be seen whether this will indeed lead to a better rather than to a more comprehensive understanding of the differences between types of acquisition. At any rate, given that the rather scarce literature on non-native language processing focuses on syntactic processing, it is by no means a trivial matter if one attempts to draw far-reaching conclusions for morphological processing. It has indeed been argued that “word-level processing and morphosyntactic feature matching between adjacent or locally related words” (Clahsen & Felser 2006b: 565) in L2 learners may not differ essentially from L1 processing. More specifically, in an ERP study, Sabourin (2003) found similar (P 600) effects in adult L2 learners and in L1 speakers processing subject-verb agreement and gender concord. Findings like these lead Clahsen & Felser (2006a) to the conclusion that at least highly proficient “L2 learners can employ the same mechanisms for morphological processing as L1 speakers” (Clahsen & Felser 2006a: 12), whereas this does not seem to be the case in the domain of sentence processing.

Another factor possibly causing differences between various types of acquisition which is not discussed explicitly in the target paper and which is alluded to in several commentaries is the nature of the data the learners are exposed to; see *Rothweiler*, *Schlyter* and *Unsworth & Hulk*. *Schlyter* reminds us that cL2 learners are not necessarily exposed to the standard variety of the target language. Consequently, some particularities of the speech of child L2 learners may actually reflect L2 usage in the children’s linguistic environment rather than specific cL2 learning mechanisms. A similar point is made by *Unsworth & Hulk*. This is indeed an important point which primarily concerns ‘heritage learners’, cf. *Montrul* (2004) and immigrant children like the Moroccan Arabic or Turkish L1 learners of Dutch and German investigated by *Hulk & Cornips* (2006) or by *Rothweiler* (2006). From all we know, the German children on whose acquisition of French I report are consistently exposed to the ‘colloquial standard’ variety of French, and the linguistic background of the children at the French school in Hamburg is more homogeneous than seems to be the case in the French school of Stockholm. Some of them are indeed bilingual, involving languages other than French or German, but they are a small minority which we excluded from our study. *Monika Rothweiler* remarks that our corpus consists of immersion data and comments that “this setting is sub-optimal in the sense that outside of kindergarten, French has no relevance in the daily life of these children.” (section 1., point (1)). I will refrain from speculating on whether this last remark is true or not, but I do want to reject the implicit claim that it might severely limit the validity of our data.

The complementary distribution of languages over various contexts of everyday life or social domains is, in fact, not unusual for bilingual settings, whether in family or societal bilingualism. If, for example, children growing up bilingually, are exposed to one of their languages exclusively in interaction with one parent, its 'relevance' will also be limited, and yet most of what we know about early bilingualism is based on corpora of this type, and these studies have demonstrated that children can acquire two 'first' languages in this kind of setting. At any rate, I fail to see how the limited 'relevance' of a language would affect the acquisition of specific grammatical devices, e.g. gender markings; at worst it might affect acquisition across the board. I might add that a similar distribution of languages also characterizes the acquisition setting of immigrant children like the ones studied by *Rothweiler* and associates, *Hulk & Cornips* or *Tracy*. They are exposed to the majority language in school (and possibly on the playground) and to the minority language in the family, unless, of course, they do have access to the majority language in other contexts as well, e.g. via the mass media. In this case, however, the additional exposure to the majority language would also have been possible before the children entered kindergarten or school. This would make it virtually impossible to decide on their age of onset of acquisition of this language. In sum, all our data are 'sub-optimal' – and in more than one way. The only optimal solution I can imagine is to engage in more studies of cL2 acquisition. If, then, it turns out that certain findings are specific to one type of setting (immersion, heritage learners, immigrants, etc.), we may raise the question again as to whether this result is due to the nature of the data.

Suzanne Schlyter addresses a further point to be considered in addition to AOA, namely the relation between syntactic and cognitive development, and I largely agree with the arguments presented in her commentary. As I acknowledge in my paper, (2)L1 and L2 do differ in important ways with respect to the knowledge sources available at the initial state. In my understanding, however, cognitive development does not affect the acquisition of the grammatical phenomena investigated in the target paper. With respect to the acquisition of grammatical tenses, on the other hand, Schlyter is undoubtedly correct when she states that it is influenced in crucial ways by the learner's cognitive capacities and probably also by the L1 system. Let me merely add that if L2 learners resort to adverbs in order to express temporal or aspectual notions, this seems to corroborate my claim that they encounter more problems with inflectional morphology than L1 children; see Meisel (1987).

A particularly interesting addition to the proposals developed in the target paper is suggested by *Rothweiler* who argues that the existence of early sensitive phases could explain certain problems encountered by

specifically language impaired (SLI) children, e.g. placement of finite verbs in verb-second (V2) position or in negative constructions. As has been noted before, cf. Håkansson (2001) or Paradis & Crago (2000), among others, SLI children resemble L2 learners in some respects. Since their acquisition of grammar can be severely delayed as compared to linguistically non-impaired children, their acquisition of some grammatical phenomena may happen only after the optimal periods for these particular constructions to be acquired. They may then have to resort to compensatory strategies, much like L2 learners. This hypothesis highlights, once again, that we urgently need to discover the developmental schedule underlying early grammatical acquisition, and, most importantly, that it needs to be defined in terms of relative order of emergence of grammatical phenomena; the particular age ranges should be expected to vary according to type of acquisition.

Quite obviously – or fortunately, in the present context – the commentators not only draw our attention to factors possibly neglected in the target article, they also raise objections, and they suggest refinements and modifications to what is proposed there. Some concern the theoretical assumptions on which my discussion is based, others refer to my claims concerning cL2 acquisition. The core hypothesis of my contribution is that cL2 acquisition resembles in some respects aL2 rather than monolingual or bilingual L1 development. Moreover, I have argued that some of these cL2 – aL2 similarities can emerge as early as at around 4 (AOA). Such claims would be of only very limited interest if one was not able to give reasons for why the postulated similarities and differences between acquisition types emerge. This is why I summarized them at some length in section 2, attempting to make my approach and the research goals of my study transparent and perhaps plausible. I hope to be able to present a more comprehensive account of L1 – L2 similarities and differences in Meisel (to appear). In what follows, I will try to focus on points directly concerned with cL1 acquisition, the topic of the target article. It may nevertheless be useful to briefly comment on those points in my theoretical assumptions which may not have been sufficiently clear in the target article, thus leading to misunderstandings, or which seem to be misrepresentations in the commentaries.

Rosemarie Tracy, for example, states that “we cannot simply shift the burden to indirect UG access via some mediating L1 *ersatz/surrogate* solution” (section 1.) and asks why, if this was indeed a possibility, L1 children would not resort to it as well. This, of course, turns the L1 – L2 discussion upside down. Everyone seems to agree that L1 development results in complete knowledge of the target grammar, whereas L2 learners typically do not succeed in acquiring native grammatical competence. Acquisition research therefore cannot dodge the question of how

to explain this difference. The answer which I favour postulates that the LMC offers the optimal solution but that it is not fully accessible anymore to L2 learners who are, nevertheless, still able to learn languages – a fact which also requires an explanation. My suggestion is that L2 learners may resort to other cognitive resources. It is well known that our mind/brain can offer more than one option, that these compete, and that the optimal resource inhibits the ‘sub-optimal’ competitor. Moreover, what counts as optimal may change in the course of development. There is no room here for a more in-depth treatment of this issue, but it should be obvious for anyone familiar with cognitive development that if competing resources are available, the optimal one will inhibit others, and the child cannot play, just for fun, with what has been referred to as a surrogate solution. My own answer to the basic question of acquisition research may well be wrong, but it is at least coherent and it seems to account for the facts. This is more than one can say about approaches which reject the idea of fundamental L1 – L2 differences but fail to offer plausible explanations for the uncontested fact that L2 learners are less successful than L1 children.

Let me add that I actually did not say that the principle of *structure dependency* is no longer available in L2 acquisition, as claimed by *Rosemarie Tracy*. In fact, I did not mean to equate L2 learners with Neanderthal man. What I did write is that under certain conditions “L2 learners incorporate operations into their L2 system which are not structure-dependent”, i. e. alongside structure-dependent ones, thus creating a *hybrid* system. If “in some cases L2 learners will resort to non-UG conform solutions”, this does not imply that the principle is not available anymore. That much subtlety in arguing should be possible in a research paper.

It seems that the age ranges which I mention as potentially critical ones also caused some confusion. Since this is a crucial point, let me try to explain what *Sharon Unsworth* and *Aafke Hulk* perceive as inconsistencies in the target paper. Recall that I started from the observation that a number of linguistic and neuropsychological studies suggest that important changes happen earlier than commonly assumed, possibly at around age 4. I furthermore adopted the hypothesis according to which different domains of grammar are affected by maturational changes at different times of development. Consequently, one should expect multiple changes to occur over an extended period of time. In fact, the state-of-the art article by *Hyltenstam & Abrahamsson (2003)* argues that some such changes happen well before age 3 (e.g. in phonology) and others much later than during the age period dealt with in my paper, not to mention age-related changes caused by other factors than neural maturation. As for myself, I am in no position to suggest an earliest or latest point of development for changes of this type. I proposed instead,

based on the research results available to me, the additional hypothesis, restricting it to morphosyntax, that certain age periods are of particular interest because several optimal periods seem to cluster, e.g. “at around age 6–7, and probably also around age 4, i. e. during the last months of the fourth year and perhaps shortly afterwards” (section 2.1., paragraph 6). These age indications may seem somewhat vague, but the fact alone that they refer to “bundles” of sensitive phases means that it would be implausible to expect more specific ones. In fact, I explicitly stated (2.1) that it would be unreasonable to hope to identify more precise ages. My goal then was to find out whether morphosyntactic changes indeed happen as early as “around age 4”, and I think it is a reasonable research strategy in pursuing this question to include children as of AOA 3. It revealed that most though not all of the children classified as cL2 learners were first exposed to their L2 at or after age 3;7, a finding which I interpret as corroborating the hypothesis postulating maturational changes affecting grammatical development “at around age 4”. For the time being, I have no better explanation than referring to individual variation, in order to account for the fact that one of our children falls into the cL2 group, in spite of an earlier AOA, whereas others who were first exposed to the L2 after 3;7 qualify as L1 learners, at least with respect to the acquisition of gender or finiteness. At any rate, in view of the variability in acquisition rate well documented in monolingual L1 research, this corresponds to the range of variation which was to be expected.

Issues of particular theoretical interest are raised by *Ianthi Maria Tsimpli* and by *Martina Penke*. *Ianthi Tsimpli* addresses my claim that L2 learners can resort to solutions not constrained by principles of UG in order to produce surface structures equivalent to the ones required by the target language. She correctly points out that the grammatical status of word order phenomena is not sufficiently well-defined. In fact, recent proposals advanced in the Minimalist framework eliminate linearization operations altogether from “syntax proper”; see Snyder (2007: 12 ff.) for a brief discussion of this issue and its implications for research on acquisition. I concur with Tsimpli’s claim that we need to distinguish between word order regularities related to formal features in functional heads as opposed to those reflecting particular aspects of information structure. These two types of phenomena represent most probably different types of acquisition tasks. Following the logic of my argument, it is plausible to assume that only the former will be subject to maturational changes of the sort discussed in the target paper and illustrated by example (1) presented there. Whether, in this case, the learner indeed operates on the linear order of elements, whether partial structures are involved or rather target-deviant but UG conforming syntactic opera-

tions, is indeed still an open question. Both *Tsimpli* and *Tracy* favour the latter explanation. Admittedly, the brief discussion of this example in the paper does not make a sufficiently strong case for an account in terms of linear sequencing. This undoubtedly requires a more detailed analysis of a larger amount of data.

Martina Penke addresses an even more fundamental issue when she questions the notion of UG as it underlies my approach. She suggests abandoning the distinction between representational knowledge on the one hand, and discovery and processing mechanisms on the other. This is a radical proposal, but it is an alternative which deserves serious consideration. This is a task exceeding by far what can reasonably be accomplished in the present context. Let me emphasize that this statement is not meant as an easy way out, indirectly dismissing Penke's proposal. In fact, my suggestion that not only parameterized principles of UG but also discovery and processing mechanisms are subject to maturational changes, might be construed as an argument in favour of the proposal made in her commentary. On the other hand, one would have to ask what exactly the processes of the brain are under this alternative view, i. e. whether there are not various types of processes which are different in nature, resembling perhaps the distinction assumed under my approach. In other words, where I disagree is with respect to the remark at the end of Penke's commentary, warning against too narrow a focus. I contend that a narrow focus is desirable because it is more easily refutable by empirical findings.

Let me finally turn to the discussion of my analysis of child L2 acquisition. *Rosemarie Tracy* presents results on the acquisition of verb agreement and placement in German by two L1 Russian children. She states repeatedly that this is intended to "support the null-hypothesis for *both*" agreement and verb placement (section 4.). I am not certain what this hypothesis refers to since *Tracy* does not state it explicitly. I also do not know why this should be the null-hypothesis, a decision requiring theoretical motivation. But I suspect that her central claim is that no dissociation of syntax and morphology is found in these case studies, i. e. her children behave in this respect like L1 learners, even if one of them acquires German at a slower rate. We do not know whether these children behave in all respects like L1 learners, i. e. whether they do not use constructions which are not attested in L1 but in aL2 corpora, my main criterion for identifying early cL2 learners. But the claim that successive acquisition "*can* (my emphasis, JMM) proceed like L1 for both syntax and (inflectional) morphology" (section 4., paragraph 8) is not controversial, anyway. In fact, as the attentive reader will have noticed, I showed in the target paper that only some of the children acquiring French at AOA between 3;0 and 4;0 behave like L2 learners. Whether

this is still possible if AOA happens at 4;7, as *Tracy* claims referring to an unpublished study of one boy, remains to be seen. In view of the available information, the age indications offered with respect to this corpus of eight children have to be taken with some caution since they refer to the age at which children “entered kindergarten and encountered German for the first time *on a regular basis* (my emphasis, JMM).” (section 4., paragraph 2). This amounts to saying that they lived in Germany and had been exposed to German before, but the amount of previous exposure and learning is not known. At any rate, I am not sure what kind of conclusion *Tracy* wants to draw from the finding that the two learners presented here seem to acquire German as an L1. She obviously believes that I am overestimating the role of age effects and prefers to account for delays in acquisition by referring somewhat vaguely to personality traits and communicative skills of learners. Note incidentally that, again contrary to what *Tracy* writes, not even I had ever invoked maturation as the *explanans* for acquisition rate (see also Meisel 2007), certainly not as the only one. In sum, I am afraid we cannot learn much about child L2 acquisition from a study of children acquiring two languages successively but allegedly not as L2 but as L1 learners.

The problem of defining the age range at which cL2 learners can be expected to differ from L1 children is also addressed by *Ianthi Tsimpli*. She draws our attention to the fact that some aspects of grammar are acquired late even by monolingual L1 learners, more specifically after the age range during which the cL2 learners studied in the target article were exposed to their L2. This raises problems related to those mentioned by *Rothweiler* referring to SLI children. The fact that the Turkish cL2 learners of Greek studied by *Tsimpli* do not converge with the adult Greek norm after 9–11 years of exposure to this language is indeed intriguing and seems to support *Tsimpli's* observation that factors outside of the computational capacity of early acquired syntax interact with the ones acquired late. I fully agree with her *caveat* that such late (L1) acquisitions need to be taken into account by hypotheses concerning early offset of optimal periods. At this point, I can only reiterate that a better understanding of cL2 acquisition will depend crucially on insights into the L1 developmental schedule.

In the target paper, I emphasized not only that we need to discover the developmental schedule underlying the acquisition of grammar, independently of chronological age, but also the importance of defining grammatical areas most likely to be affected at an early age. *Unsworth & Hulk* remind us that “problem areas” cannot be designated independently of language-specific structural particularities. This is undoubtedly correct, and although I did mention in 3.4 that the difficulty of the acquisition tasks depends on how gender is marked in the L2, I agree

that this point could have been articulated more explicitly. They illustrate their point by referring to the relatively transparent Italian gender system which is acquired early and easily – at least in L1. One might add that gender is not a problem area in acquiring languages like English which lack grammatical gender. Somewhat more interestingly, perhaps, we find evidence that in any language pair a given structure may be more problematic for learners of one language than for those of the other. Bonnesen (2008), for example, finds that whereas the German children of the Hamburg cL2 corpus presented in the target paper seem to acquire word order in French subordinates without problems, the same is not true for their French peers recorded in the same school. They encounter major problems acquiring clause-final verb placement in German subordinates, reminiscent of the findings of Sopata (2008) who also investigated the acquisition of German by cL2 learners, Polish in her case. An explanation of these findings would require a detailed account of the syntactic phenomena involved – a task which cannot be tackled in the present context. But it seems to suggest that OV in subordinates represents more difficulties for L2 learners than VO, if the L1 exhibits the other order.

Several of the commentaries allude to transfer as a possible factor characterizing child L2 acquisition. I happen to believe that the role of morphosyntactic transfer from L1 is largely overstated in much of the L2 literature; cf. Meisel (1983, 2000). This remark applies especially to the role attributed to transfer at the initial stage of L2 acquisition. Even with respect to alleged transfer of V2 effects, things are probably more complex than assumed by *Penke*; see *Pienemann & Håkansson* (2007). But there can be no doubt that transfer plays a role in L2 acquisition and that it is one of the characterizing differences between (2)L1 and L2. Evidence for transfer from L1 thus supports my claim that cL2 resembles aL2 in a number of ways. I therefore wonder whether the remarks in some of the commentaries should be interpreted as critical comments on my paper at all. *Rothweiler*, for example, correctly states that the fact that I do not find evidence for (morphosyntactic) transfer in our data “does not mean that transfer does not occur in cL2 in general.” (section 1., point (3)). *Unsworth & Hulk*, too, seem to be disappointed by the lack of evidence for transfer in the cL2 French corpus, given that they detected significant L1 influence on L2 learners of Dutch in their own work. I must confess that I do not see how these comments refer to my analysis of child L2 data. The fact that no such evidence was found in the children’s acquisition of French gender may be unexpected, but it should be accepted as an empirical fact. *Unsworth & Hulk*, however, are not prepared to do so; instead they suggest to re-examine “the apparent [sic!] lack of transfer in the children in Meisel’s study“ (paragraph 13) in

a “systematic and carefully controlled study” (paragraph 13). This request for a different type of data is all the more remarkable as this commentary exclusively relies on examples of word order (VO/OV) errors in order to support the authors’ case of L1 transfer. How this might alter the interpretation of my findings concerning the acquisition of grammatical gender or of finiteness for that matter remains totally opaque, especially since a more important role of transfer in cL2 will bring it closer to aL2 and distinguish it from (2)L1, thus strengthening my hypothesis on cL2.

Josje Verhagen & Sarah Schimke raise the perhaps most fundamental objection to the claims made in the target paper when they set out to demonstrate that there is no fundamental change around age 4 affecting morphosyntactic development. They choose not to discuss the empirical evidence presented in the paper on which they comment; they rely instead on a review of studies investigating verb placement with respect to the negator in negative constructions in French, German and Dutch, three languages in which the finite verb is moved to a functional head above the negator, thus preceding it in surface linearization. The choice of this grammatical domain is odd since I stated explicitly and at various places in the target paper that the currently available evidence shows that cL2 learners at AOA around 4 acquire placement of finite verbs in much the same way as (2)L1 children; see, for example, 3.2. In fact, in section 3.3, I not only report that I did *not* find aL2-like errors of this type in the French data of our cL2 corpus, I explicitly refer to an analysis of negative constructions in this corpus which concludes that these children do not behave like aL2 learners with respect to the placement of finite verbs. Surprisingly, the commentary refers neither to these findings nor to the arguments offered by Meisel (1997) in favour of fundamental differences between (2)L1 and aL2 in the acquisition of French and German negative constructions. In sum, *Verhagen & Schimke* refute a claim which is *not* made in the target paper. Their choice of this particular grammatical phenomenon is motivated by the fact that placement of finite verbs involves a parametric option. But although parameters are hypothesized to be affected by maturational changes, I insisted on that not all of them become unavailable simultaneously, and certainly not as early as at around age 4. Hence my call for research on the developmental schedule.

Let me take this opportunity to comment on a methodological problem which becomes apparent in the commentary by *Verhagen & Schimke*. They contrast L1, cL2 and aL2, listing frequencies of constructions types attested in the corpora of various researchers. Although this is certainly a legitimate way to proceed, it cannot provide insights into the *emergence* of these grammatical devices. If, for example, two learners, over a period

of several months, place 6 finite verbs incorrectly after the negator, as opposed to 13 non-finite ones, we need to know whether this usage occurs in the speech of both subjects, whether this happens only during the first weeks, etc. By lumping together frequency counts for several learners over a certain time span, potential information concerning developmental patterns is lost. Yet it is precisely this kind of information which we need in order to decide on possible similarities and differences between acquisition types. Counting error frequencies is useful, but as I argued in section 3.1 (last paragraph), it will not suffice as evidence in this case.

To conclude, the claim that successive acquisition of language in early childhood can justly be referred to as child second language acquisition stands up to scrutiny. As was to be expected, not all commentators subscribe to the theoretical assumptions underlying my analysis of cL2 acquisition. In fact, their suggestions for theoretical modifications or additions are particularly welcome. I also agree with those who argue that in addition to neural maturation, further factors should be taken into account in trying to explain particularities of cL2. But no compelling arguments have been put forth against my claim that maturational changes are of prime importance. In this endeavour, linguistics and the neurosciences need to cooperate; the strongest evidence is provided by studies which demonstrate that neurological sciences and linguistics complement and support each other. To my surprise, my claim that discovery principles too are subject to maturation does not seem to be controversial, nor has the early age range at around age 4 been seriously challenged. Moreover, since my analysis of the acquisition of genders markings has provoked hardly any comments, I conclude that it is on the right track and that inflectional morphology is indeed one of the domains in which cL2 differs from (2)L1 and resembles aL2. Finally, I had hoped to see suggestions concerning the developmental schedule, but on this topic we will have to wait for results from the hopefully manifold number of research projects on cL2.

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