

L3 acquisition and crosslinguistic influence as co-activation: Response to commentaries on the keynote ‘Microvariation in multilingual situations: The importance of property-by-property acquisition’

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I Introduction

I would first of all like to thank the many authors who have provided commentaries on my keynote article ‘Microvariation in multilingual situations: The importance of property-by-property acquisition’ (Westergaard, 2021a). The keynote has generated significant and stimulating debate about central issues on crosslinguistic influence in multilingual language acquisition, and I find it especially welcome that the majority of the commentaries support and enhance the main idea of the keynote, viz. that L2/L3/Ln acquisition takes place as a step-wise process, property by property. The commentaries have provided supporting arguments, additional data, and suggestions for further research, but also new questions and critical comments. This response is divided into sections focusing on the following issues: Full transfer and the notion of copying, the definition of linguistic proximity, some terminological issues, research methodology, predictions, empirical support, and possible misinterpretations.

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II Full transfer in L2A/L3A (second/third language acquisition) and the notion of copying

The most surprising commentary is the one by **Schwartz & Sprouse** (2021a). In their own recent keynote in *Linguistic Approaches to Bilingualism* (Schwartz and Sprouse, 2021b), they embrace the Typological Primacy Model (TPM) of third language (L3) acquisition, which argues for full/wholesale transfer as ‘reduplication . . . (literally, a copy)’ of one of the previously acquired languages at what is referred to as the initial stages (Rothman et al., 2019: 24). Schwartz and Sprouse consider the TPM as a direct continuation of their Full Transfer Full Access (FTFA) model. However, they are now walking away from the idea of full transfer as a literal copy and instead use ‘copy’ in quotation marks, and also add in parentheses that they mean this as a metaphor. In the keynote, I argued that any concept of copying must be metaphorical (Westergaard, 2021a: 10), and Schwartz and Sprouse’s current change of mind now makes me wonder what the difference is between their ‘(metaphorical) “copy”’ and my Full Transfer Potential, which argues that the entirety of the first language (L1) grammar remains available for parsing the second language (L2).

This shows that there has been a certain amount of uncertainty, disagreement and possibly also misunderstanding of the term full transfer over the years (see also Slabakova, 2021). This is emphasized in the commentary by **Stringer** (2021), where reference is made to Hawkins (2001), who stated that L1 influence on the L2 for a particular construction may not occur until it is relevant in the learning process, thus in fact supporting Full Transfer Potential. Furthermore, Sharwood Smith and Truscott (2006, 2008) asked several questions about the status of FTFA and the idea of copying, focusing on what type of input would trigger such a process, and whether a new clone of the L1 would be made every time one is exposed to a few phrases of a foreign language. To my knowledge, Schwartz and Sprouse have never responded to these questions or clarified what they meant. Thus, Rothman et al. (2019: 151), wishing to show ‘fidelity to the construct of full transfer as existing in the published literature’, refer to personal communication with Rex Sprouse in 2018 confirming that full transfer means a ‘full copy of L1 grammatical representations’, which Rothman et al. take to mean ‘transposing a copy of the (entire) L1 system to the initial state of L2 acquisition’. One would assume that a literal copy and what is now referred to as a ‘(metaphorical) “copy”’ cannot be the same thing, so it seems the full transfer people owe the field some clarification. And if the L2 and L3 camps in fact do mean the same thing, they would still have some explaining to do.

Furthermore, **Schwartz & Sprouse** also distance themselves from the concept of cognitive economy as the rationale for full transfer as proposed in the TPM for L3 acquisition; more specifically, a claim that it is more economical for the mind/brain to transfer the whole grammar in one fell swoop than to keep two languages active for an extended period of time, since this would place ‘increased demands on both the linguistic and executive control systems’ (Rothman et al., 2019: 157). Instead, Schwartz and Sprouse refer to their own 1996 seminal article, where they claim that full transfer is preferable (to partial transfer) because the full transfer model is conceptually simpler and more elegant and ‘does not require any additional stipulations to account for the phenomenon of L2 acquisition’ (Schwartz and Sprouse, 1996: 41). In their commentary, Schwartz and

Sprouse do not discuss what this position would mean for their own approach to L3 acquisition, but I would like to point out the following: Under Full Transfer Potential and the Linguistic Proximity Model (LPM), the initial state of L2 acquisition is the entirety of the L1 grammar (as in FTFA) and, similarly, the initial state of L3 acquisition is the entirety of the two previously acquired languages. The acquisition process that follows is the same in both cases: gradual grammar building by parsing, a process that is also acknowledged by the TPM as the normal course of events (Rothman et al., 2019: 23). However, the TPM assumes that, in addition to this process, there is a ‘shortcut of sorts’, where ‘linguistic representations can be changed by, or even copied from . . . other languages, without the direct mediation of language processing’, i.e. what Schwartz and Sprouse (2021b) refer to as the Big Decision. As a consequence, a number of extra stipulations have been added to the model over the last decade: In addition to the initial state, the concept of initial stages has been introduced, although it has so far received no proper definition (except a circular one, stating that it is ‘the period in which structurally driven wholesale transfer from the L1 or the L2 takes place’; González Alonso and Rothman, 2017: 688), and a crude four-level hierarchy of linguistic properties has been postulated, which the parser is argued to follow in the language selection process (Rothman, 2015). Furthermore, it is necessary to make (somewhat unjustified) distinctions between wholesale transfer and cases where property-by-property influence may take place (both before and after wholesale transfer and possibly in L4 acquisition). Thus, with respect to economy and elegance of L3 models, the TPM does not seem to come out as the winner.

Grüter’s (2021) commentary also discusses the notion of copying. She refers to Marr’s (1982) three levels of analysis (the computational, the algorithmic, and the implementational levels), where linguistic theory is at the first level while the physical instantiations of the system (in the brain) are situated at the third level. Importantly, she then argues that as long as we ‘are firmly grounded at Marr’s first level, there is nothing problematic about the notion of creating copies of grammatical representations’ (Grüter, 2021: 2). I agree with her – as long as we are talking about L2 acquisition, where no actual process of wholesale transfer or copying needs to take place, since the L1 is always there as a hypothetical initial state, even long before any L2 acquisition starts. But I would like to point out that it is more difficult to keep copying as a metaphor in L3 acquisition, and, according to the TPM, wholesale transfer is a real physical process, taking place at a particular time after L3 acquisition has started (the ‘initial stages’), triggered by (an unspecified amount of) L3 input, and involving the parser making a language selection resulting in reduplication of a complete grammar. Furthermore, this process is argued to leave a signature in the brain, currently assumed to result in a P600 component as a reaction to ungrammatical stimuli in an L3 after very limited exposure (González Alonso et al., 2020).

Grüter (2021: 2) then argues that ‘the brain . . . has no place’ in arguments about the computational level, but here I’m afraid I disagree. While I find Marr’s levels useful, I do not think that (theoretical) linguists should be content with operating exclusively at the first level. This position is well argued in Poeppel (2017: 157), who claims that, although there is at present no known direct alignment between the primitives of theoretical linguistics and neuroscience, ‘[a]n explanatory theory of language that goes beyond observational and descriptive adequacy would need to capture [Marr’s] three

levels in a unified manner’ (see also, amongst others, Baggio et al., 2012; Johnson, 2017; Leivada, 2020). Being ‘unambiguously grounded in Chomskyan linguistic theory’ (Grüter, 2021: 2), and thus Marr’s first level, is no excuse – in fact, working within a linguistic theory which has extensively referred to concepts such as ‘innate endowment’, ‘language genes’, ‘hardwired constraints’, ‘language acquisition device’, etc., generative linguists have a particular responsibility, in my view, to ensure that theoretical models have a certain plausibility with respect to the other levels. Thus, although I believe the TPM is not correct about wholesale transfer, I think the proponents of this model are absolutely right in pursuing a research program investigating multilingualism in relation to cognitive processes and the physical brain.

In this connection, it is also relevant to discuss **Sharwood Smith’s** (2021) commentary, which bolsters the main argument of the keynote. In numerous publications over the last 15+ years, Sharwood Smith has developed the Modular Cognition Framework (MCF; for example, Sharwood Smith, 2017; Sharwood Smith and Truscott, 2014), a model that considers ‘growth of grammatical representation as an outcome of processing in real time’ (Sharwood Smith, 2021: 3), and as such it is very similar to the learning-by-parsing concept of the LPM. The MCF is partly meant as a reinterpretation of Full Transfer Full Access in a framework that is more in line with current thinking about processing and the mind/brain and which avoids older and unfortunate concepts such as copying or cloning (whether they are meant metaphorically or literally). Instead, crosslinguistic influence is considered to be ‘a matter of alternative connections within a network’ (Sharwood Smith, 2021: 3) that will be shared through co-activation rather than ‘transferred’ or ‘copied’. In the commentary, Sharwood Smith formulates the LPM in MCF terms, which briefly goes like this: As L3 structures are processed, this will result in parallel activation (to different degrees) of a number of candidate structures stored as part of the grammars of the previously acquired languages. In this process, any property can be shared (reflecting Full Transfer Potential), but ‘what is actually shared will depend on the outcome of competition between candidates’ (Sharwood Smith, 2021: 5). Finally, as he has also argued for elsewhere, Sharwood Smith advocates for abandoning the term transfer, for a number of reasons: It hails back to behaviorist ideas and the mechanistic Contrastive Analysis Hypothesis (for example, Lado, 1957), it gives misleading connotations of movement and copying, and it maintains an unfortunate division between representation and processing. He thus suggests that the term Full Transfer Potential should be changed to ‘Full Crosslinguistic Potential’ – a suggestion clearly worth considering.

III What is linguistic proximity?

Many of the commentaries ask additional questions about the LPM, and the perhaps most important and interesting question – most clearly formulated by **Archibald** (2021), **Flynn** (2021), and **Ionin** (2021) – is how linguistic proximity can be determined or measured. While Ionin rightly points out that this is a question for any model of crosslinguistic influence, it is of course especially important for a model such as the LPM that has given special prominence to similarities that may exist between the languages in a multilingual mind. Ionin asks how a learner can decide what is similar enough to transfer, while Archibald formulates questions of what it means for languages or structures to be

closer and how the parser will decide between the two previously acquired languages when both of them bear some similarity to the L3.

Archibald also provides a very promising direction for an answer to the question, based on evidence from phonology. Introducing the concept of ‘I-proximity’, he argues that linguistic proximity must be calculated with reference to ‘deep cues’, which operate on linguistic representations and are thus part of the I-language. A convincing example is L2 acquisition of English onset clusters [sn, sl, st] (so-called appendices), which is expected to be challenging for learners with L1s lacking such clusters, for example, Japanese, Persian or Brazilian Portuguese. However, Archibald and Yousefi (2018) find that, unlike the other two L1 groups, Persian learners do not have any difficulty acquiring these clusters, and they argue that this is due to Persian having right-edge appendix structures that can be transferred to parse the English left-edge appendices. Thus, they conclude that ‘Persian (which has appendices) is, perhaps counter-intuitively, more similar to English than Brazilian Portuguese (which lacks appendices) is’ (Archibald, 2021: 5).

Archibald’s position is that acquisition of syntax should be governed by the same properties as phonology. His proposal also resonates with the LPM argument that crosslinguistic influence is co-activation of structures in the mind/brain. In this sense, it is not dependent on a ‘decision’ that the learner makes, but on factors causing this co-activation. Whenever a learner is processing the L3 for comprehension or production, stored representations of the relevant constructions of both previously acquired languages will be activated. The strength of activation of the respective representations that enter into this competition will depend on the number of shared structures, but also other factors. Similarity and proximity are of course relative concepts, in that particular structures can be more or less similar or more or less close. This means that predicting crosslinguistic influence requires detailed knowledge of the relevant abstract structures in all three languages. Furthermore, in order to declare a winner of the competition between the activated structures, other factors may also play a role.

The lack of consideration of other factors (than linguistic proximity) in the LPM has been mentioned by a number of the commentaries – most notably **Bardel & Falk** (2021), **Wrembel** (2021), **Gabriele** (2021), and **Morales-Front & Sanz** (2021) – for example, proficiency, saliency, construction frequency, age, metalinguistic knowledge, context of use, or universal preferences. In the final section of the keynote, it is emphasized that such factors will also play a role in the complex process of multilingual language acquisition, as also argued by the Scalpel Model (Slabakova, 2017). Admittedly, other factors have not been addressed to any large extent in the LPM, but in the current version of the model their role would be the following: In the competition between the different activated structures from the previously acquired languages, linguistic proximity will be measured as the amount of abstract structure shared between (the current version of) the L3 and the previously acquired languages. However, the strength of activation may be affected by a number of these other factors, sometimes to the extent that they override linguistic proximity. Thus, when selecting a structure for crosslinguistic influence, the parser chooses the one which is most strongly activated.

This leads to another question asked by many of commentaries – either directly or indirectly – viz. how the LPM may account for non-facilitative influence from the previously acquired languages (**Bardel & Falk, 2021; Cabrelli & Puig-Mayenco, 2021;**

González Alonso & Rothman, 2021; Schwartz & Sprouse, 2021a). This has not been fleshed out in much detail in previous work, and space does not allow much elaboration here, but in brief: As stated in Westergaard et al. (2017: 671), non-facilitative influence from one of the previously acquired languages will occur when ‘learners misanalyse L3 input (and/or have not had sufficient L3 input)’, the former typically applying in processing for comprehension and the latter in processing for production. An example of such misanalysis could be a German learner of Norwegian interpreting a definite noun phrase as a bare noun, due to the definite article being a suffix in Norwegian (but not in German), for example, *huset* ‘house.DEF’ compared to German *das Haus* ‘the house’. Another example would be a Norwegian learner of English interpreting subject–auxiliary inversion as an example of the Norwegian V2 rule (which also applies to lexical verbs) – that is, upon hearing a sentence such as ‘What will you eat?’ (which has identical word order in Norwegian), a Norwegian learner will activate the Norwegian V2 rule and falsely assume that verb movement across the subject would also apply to lexical verbs, in sentences such as ‘*What eat you?’ The other context for non-facilitative influence (when there has not been sufficient input in the target language) is when learners are processing for production. As stated in the keynote (Westergaard, 2021a: 11), we would expect non-facilitative influence to be more frequent in production than in comprehension, since when ‘the target representation is lacking (or too weak) . . . , learners will typically use a structure from [one of the previously acquired languages].’ This is the context where we will often see typological/lexical similarity overriding structural similarity, which has so frequently been found in studies supporting the TPM; for example, Rothman and Cabrelli Amaro (2010), where L1 English L2 Spanish learners of L3 French transferred Spanish pro-drop syntax instead of using English, which would have been facilitative in this case. The LPM take on such phenomena would not be that these early learners are using an L3 representation that has been copied from Spanish together with every other property of Spanish grammar (which is the TPM explanation). Instead, we would argue that when processing for production, the learner will activate the corresponding structures in both previously acquired languages, but because there will also be lexical activation, the typological/lexical similarity between the L3 and one of the previously acquired languages will cause stronger activation of the syntactic structure of this particular language. Thus, in the Rothman and Cabrelli Amaro (2010) study, the lexical similarity between L3 French and L2 Spanish will also (more strongly) activate Spanish syntax, to the extent that it will win over English syntax, as long as there is no stable L3 representation to block this (that is, typically at early stages).¹ This then accounts for the role of superficial/typological similarity as a factor in the LPM, and it hopefully explains more fully the expectation expressed in Westergaard et al. (2017: 677) that ‘as exposure to the L3 grows, the role of overall typological proximity should decrease, while the role of more abstract structural similarities should increase.’

So, how can we determine linguistic proximity in multilingual language acquisition? The short answer is that the linguistically most proximate structure is the one that has the strongest activation, typically because it shares the most abstract structures with the L3. However, the long answer is that in order to predict what will transfer in a particular case, we need to have detailed knowledge about the abstract representations involved and control for a number of other factors (see also Section VI below).

IV Terminological issues

It is possible that there is still some confusion about the terms property-by-property transfer and partial transfer (as in the partial transfer models of the 1990s), as Schwartz and Sprouse (2021a: 11) claim that property-by-property transfer is ‘many iterations of partial transfer’. Thus, in their commentary, **Schwartz & Sprouse** list a number of studies in L2 acquisition that show evidence for Full Transfer, and thus by extension ostensible evidence against Full Transfer Potential. Similarly, **White** (2021: 3) refers to a study by Grüter and Conradie (2006), arguing that transfer of *wh*-question syntax from L1 Afrikaans to L3 German provides ‘direct and convincing evidence in favour of FTFA’, presumably since a functional domain is affected, which was typically argued by the partial models to be inaccessible to transfer. However, as **Stringer** (2021: 4) points out, property-by-property transfer is ‘quite distinct’ from partial transfer, since ‘[a]ccording to the LPM/Scalpel Model, nothing is inaccessible; everything in the pre-existing languages of the mind is potentially available for transfer’ (unlike in partial transfer models such as Vainikka and Young-Scholten, 1994; or Eubank, 1996). Thus, while the studies listed by Schwartz and Sprouse may provide evidence against partial transfer, none of these studies can distinguish between Full Transfer (as wholesale copying) and Full Transfer Potential.²

Bardel & Falk (2021: 5) also raise concerns with the use of the term L3 in the LPM and argue that the original Westergaard et al. (2017) study cannot be considered an L3 study as it uses data from simultaneous bilingual children learning a foreign language at a later stage. For them, ‘there are differences between acquiring an L1 and learning a non-native language (L2, L3, etc.)’, and they argue that a language can only be called an L3 if the learner has previously acquired an L2 after the native language. However, as pointed out by **Stringer**, there are many different acquisition as well as attrition contexts. Importantly, the same types of crosslinguistic influence can be found across these different contexts and, referring to Gürel’s (2002) work on L1 English learners of L2 Turkish and L1 Turkish learners of L2 English, **Stringer** (2021: 4) argues that there may be ‘no qualitative difference between L1 effects on the L2 and L2 effects on the L1’. For this reason, the learning context itself is not the central part of the LPM; instead, the linguistic properties of the three languages take central stage, as these will be responsible for the co-activation of structures in the learner’s mind. However, other factors will play a role in the competition between the various activated structures, and order of acquisition is presumably one of them.

V Research methodology suitable for the LPM

White questions the LPM rejection of the mirror-image design for L3 acquisition studies, a design which is considered to be the gold standard for L3 studies by proponents of FTFA and the TPM (for example, Puig-Mayenco et al., 2018; Schwartz and Sprouse, 2021b). The mirror-image design compares two groups with opposite L1s and L2s (for example, L1 Spanish L2 English vs. L1 English L2 Spanish), and it was proposed about a decade ago in order to tease apart results that could be interpreted as supporting both the TPM and the second language status factor (L2SF; that is, in cases where the L2 is

also the typologically closest language), for example, in the Rothman and Cabrelli Amaro (2010) study mentioned above. It thus became important to have a design that distinguished between the L2 and the typologically/lexically more similar language, as in, for example Rothman (2010), a study which compared L1 English L2 Spanish and L1 Spanish L2 English groups learning Brazilian Portuguese as an L3. It should be emphasized that the mirror-image design is intended to distinguish order of acquisition from other factors. It is therefore somewhat surprising that the proponents of the TPM and FTFA (still) embrace this design, considering the fact that Schwartz and Sprouse (2021a: 13) seem to completely dismiss the L2SF, claiming that ‘there is no conceptual motivation for the assumption that (adult) L2-Interlanguage grammars . . . will always serve as the initial state of L3 acquisition.’ Puig-Mayenco et al.’s (2018) systematic review also reveals that only 28.2% of the L3 studies investigated may be accounted for by transfer from the L2. Furthermore, for the mirror-image design to provide support for the TPM, it has to show that the two groups behave the same (that is, transfer from the same language, whether it is the L1 or the L2 of the participants). And, while null results are not impossible to interpret statistically, it is not straightforward to argue that lack of significant differences between the groups means that order of acquisition does not play a role (for a similar argument, see Lago et al., 2021). Finally, depending on the language combinations, mirror-image groups are very difficult – and in many cases impossible – to find.

Importantly, the mirror-image design cannot distinguish between models that assume influence from one or both of the previously acquired languages, such as the TPM vs. the LPM or Scalpel Model. In order to do this, it is necessary to use a subtractive language group design (Westergaard et al., submitted). In such a design, the L3 group is compared with two L2 control groups, where the L3 is kept constant and each of the two previously acquired languages is subtracted in the L2 groups. An example of this would be if the L1 English L2 Spanish L3 French learners of the Rothman and Cabrelli Amaro (2010) study mentioned above would be compared with an L1 English and an L1 Spanish group of learners of French as an L2. This methodology would allow us to isolate the effects of each language in L3 acquisition. That is, if there are significant differences between the L3 group and the two L2 groups, we can reject the null hypothesis that the subtracted language does not exert any influence on the L3 and, by extension, state that this language does influence the learners’ behavior in the L3.

This subtractive design was only briefly mentioned in the keynote, and **González Alonso & Rothman** (2021: 3) have commented on it in the following way:

for a theory that explicitly assumes that ‘anything may transfer’ as opposed to ‘everything does transfer’ . . ., expecting that the influence of the L3 group’s L2 will *always* be detectable in a given linguistic property *sampled pseudo-randomly* from the grammar amounts to conceding that transfer is so pervasive that we might as well assume it affects every single property. (emphasis added)

This leads me to other important aspects of the methodology: the timing of data collection and the selection of properties. It is of course not the case that the LPM assumes that L2 effects on a given property can be detected at any time in the learning process; nor do

we argue that L3 studies should sample linguistic properties pseudo-randomly. On the contrary, when selecting properties for the original Westergaard et al. (2017) study, we made an effort to choose a property that was known to cause problems in L2 acquisition at a particular time and therefore selected non-V2 word order, shown in Westergaard (2003) to be problematic around age 12 in Norwegian learners of English. The intention was to compare this to a property known to be problematic for Russian learners of L2 English at the same age. As we did not have any relevant previous research to rely on at the time, we unfortunately chose a property that was already acquired in this age group; that is, we found a ceiling effect (both in the L2 and the L3 group).

Ceiling effects, and corresponding floor effects, should of course be avoided in any study of L3 acquisition, as our goal is to find meaningful and statistically significant differences between properties and groups of learners. However, this requires careful piloting and selection of properties to determine the optimal age of testing. This is obviously a complex matter, as the optimal age of testing is almost certain to be (slightly or considerably) different for different properties. For example, in a large-scale study comparing the same populations as in the Westergaard et al. (2017) study, Jensen et al. (in progress) finds that out of seven properties tested, only three turn out to show significant differences between the groups, two properties are near significance, while two show a ceiling effect. This means that the truly optimal methodology for the LPM would be a longitudinal one, presumably also for other models, and the appearance of the Cumulative Input Threshold Hypothesis (Cabrelli and Iverson, submitted) is therefore very welcome in the field. In any case, it seems to me that the timing of testing is not only crucial for the LPM, but for all theories of L3 acquisition. This is in fact highlighted by **Cabrelli & Puig-Mayenco**, who refer to the recent study using event related potentials (ERP) by González Alonso et al. (2020), where two groups of L1 Spanish L2 English speakers were exposed to one of two artificial languages: Mini-Spanish and Mini-English (both with grammatical gender). The TPM prediction was that wholesale copying of one of the previously acquired languages would be reflected in a P600 component as a reaction to ungrammatical stimuli, but only in the group that had been exposed to Mini-Spanish and thus assumed to have transferred Spanish at the initial stages of L3 acquisition (that is, the only previously acquired language that has gender). However, after a couple of hours of exposure and training, no such ERP signature was found in either of the groups, and the authors speculate that ‘the training phase . . . fall[s] short of the actual amount of exposure required to trigger any kind of representational transfer from a previous language’ (González Alonso et al., 2020: 13).³

VI Predictions

Bardel & Falk, Cabrelli & Puig-Mayenco, Flynn, Gabriele, González Alonso & Rothman, and White all question the ability of the LPM to make testable predictions, especially in cases where non-facilitative influence is expected. As stated in the keynote, LPM predictions are not at the macro-level – i.e. cross-linguistic influence (CLI) is not always from the same language – but are dependent on the proximity of specific structures in the three languages involved. Thus, predictions are made for specific studies with a carefully selected design (as discussed above) and based on what we know from

previous research about the linguistic properties at hand and L2 acquisition of the relevant languages.

Predictions of the LPM are outlined in Westergaard et al. (submitted), but a brief sketch is provided here: In a subtractive experimental design such as the one described in the previous section, we should compare at least two linguistic properties, A and B, where property A is shared by the target language and one of the previously acquired languages (and is different from the other), while property B is shared by the target language and the other previously acquired language (and is different from the language with which the target language shares property A). We would then expect to find that the L2 groups differ in the following way: The L2 group with property A is expected to outperform the L2 group without this property and, conversely, the latter L2 group is expected to outperform the former L2 group on property B. Furthermore, we predict the L2 group with property A to perform better on this property than property B, and the reverse should hold for the L2 group sharing property B with the target language. For the L3 group, the LPM would predict that the L3 learners will be more accurate or equal (in case of a floor effect) on property A than the L2 group without this property, and less accurate or equal (in case of a ceiling effect) to the other L2 group (that is, the group sharing property A with the target language). For property B, we should see the reverse pattern. This means that we would not expect the L3 group to score higher on any L3 property than the L2 group with which it shares this property, or lower than the L2 group with which it does not share this property. Such results would falsify the model.

If the timing of the experiment is right, the L3 group will typically perform in between the two L2 groups on both properties A and B. And if the score of the L3 group is significantly different from both groups, this can be interpreted as support for cumulative activation of both previously acquired languages and combined cross-linguistic influence. This is what we see in Kolb et al. (forthcoming), a direct follow-up study to Westergaard et al. (2017). In this study, Russian–German learners of L3 English are compared with corresponding L2 groups. As the original study yielded inconclusive results for one of the conditions (see previous section), two further properties were added: determiner use (where German and English are similar) and adverb placement (where Russian and English are similar). The results showed that the two L2 groups were significantly different from each other, scoring better on one of the conditions (the one that was similar to their L1) and correspondingly lower on the other (the one that was different from their L1). The L3 group was significantly different from the L2 learners, scoring between both L2 groups on both conditions, as shown in Figure 1 (from Kolb et al., forthcoming). This strongly suggests that the L3 learners were influenced by both Russian and German.

VII Empirical support for the LPM

Cabrelli & Puig-Mayenco and **Schwartz & Sprouse** express some concern that there is so far little empirical support for the LPM, citing only one published study (Westergaard et al., 2017). In response to this, I would like to point out that the LPM builds on previous research in the field, especially on the strengths of the TPM and the Cumulative Enhancement Model (CEM; for example, Flynn et al., 2004). That is, the LPM is very similar to the TPM, in that it has adopted linguistic similarity (sometimes superficial at

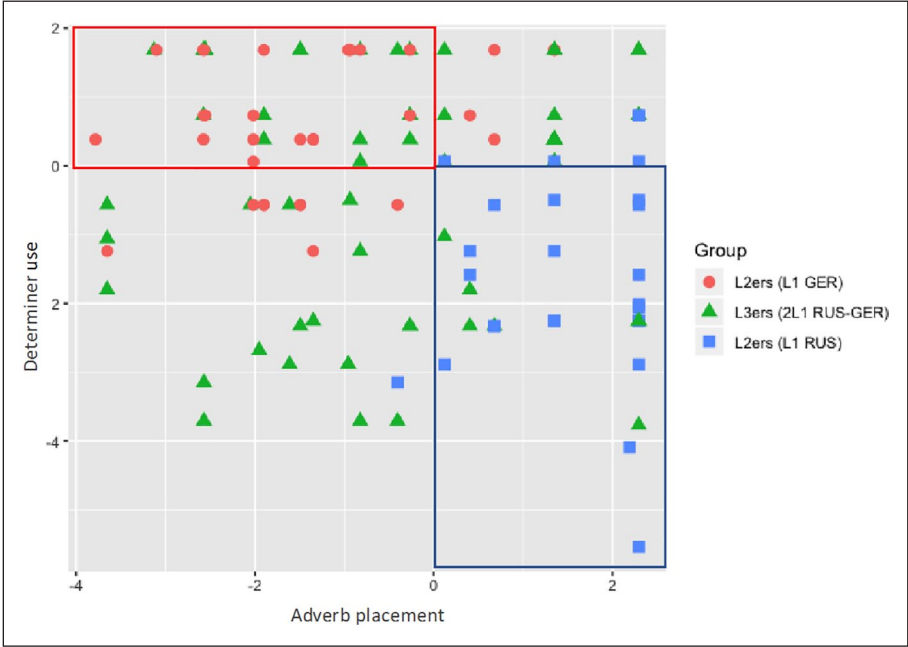


Figure 1. Distribution of random effect sizes for two critical conditions (Determiner use and Adverb placement), showing that Russian–German L3 learners of English are different from both of the corresponding L2 groups.

early stages) as the main factor for crosslinguistic influence, although it rejects wholesale transfer and the four-way hierarchy. The LPM has instead adopted the cumulative aspect of the CEM, but given the reliance on processing (for comprehension as well as production, see Section III), the LPM (unlike the CEM) also predicts non-facilitative influence to occur in L3 acquisition. Thus, the LPM should in principle be able to account for results that have previously been argued to support these two models. Furthermore, in the keynote it was argued that the 17 studies in the systematic review of Puig-Mayenco et al. (2018) that display hybrid transfer could be interpreted as general support for the LPM.

However, **Cabrelli & Puig-Mayenco** (2021: 2) claim that it is an ‘unsound practice’ to rely on existing data from other studies to argue for a particular model, the reason being that ‘these data are in fact compatible with multiple theoretical accounts.’ This is of course true for many data sets, as mentioned for the Rothman and Cabrelli Amaro (2010) study mentioned above. I would nevertheless contend that it is not objectionable to build on previous research; in fact, I would even argue that it is a preferable strategy.

Cabrelli & Puig-Mayenco further point out that in the original Westergaard et al. (2017) study – where the L3 group scores in between the L2 Norwegian and the L2 Russian groups on a word order phenomenon where L3 English is similar to Russian (despite English being typologically/lexically more similar to Norwegian) – this result is not necessarily evidence of hybrid transfer. Instead, they argue that it is also ‘possible that the learners had originally transferred Norwegian [wholesale] and are in the process of

overcoming non-facilitative transfer' (Cabrelli and Puig-Mayenco, 2021: 4); this is a possible analysis that has also been suggested by Schwartz and Sprouse (2021b). That is, as they rightly point out, it is difficult to disentangle transfer from acquisition. While this is certainly true, it nevertheless remains a question on such a scenario why the L3 group scores so much better than the L2 Norwegians; for the TPM, this would presumably either be a case of what is referred to as residual transfer or perhaps simply a cross-linguistic effect. But it becomes more difficult for a wholesale-transfer account to explain such in-between results in the L3 group in cases where the effect of the allegedly non-transferred language is non-facilitative, since such influence cannot be the result of learning. This is the case for one of the conditions in the Kolb et al. (forthcoming) study mentioned above (see Figure 1): Although English is typologically/lexically closer to German than to Russian, the L3 group scores lower than the L2 German group on determiner use, arguably as a result of non-facilitative influence from Russian. A similar result is attested in a recent study by Lloyd-Smith (2020), who has studied two word-order phenomena in German–Italian bilinguals learning English as an L3 in comparison with corresponding L2 groups (L1 Italian and L1 German). She finds a certain amount of non-facilitative influence from both previously acquired languages in the L3 group and concludes that transfer from both languages has taken place, thus supporting the LPM proposal that both linguistic systems are available for crosslinguistic influence in L3 acquisition.

In Westergaard et al. (submitted), we provide a non-comprehensive overview of recent studies supporting property-by-property influence in L3 acquisition, including a series of studies by Stadt et al. (2016, 2018, 2020) investigating speakers of L1 Dutch L2 English learning either L3 French or L3 German, with results indicating that both previously acquired languages are activated and available for transfer in L3 acquisition. Several of the commentaries have also provided additional data: **Gabriele** (2021) argues that the Kulundary and Gabriele (2012) study, although not showing hybrid transfer, does provide evidence for transfer property by property. **Archibald** provides data from Benrabeh (1991), showing that L1 Algerian Arabic L2 French speakers produce French vowels and Arabic consonants in their L3 English, which he convincingly accounts for in terms of the I-proximity of the feature hierarchies (Archibald, 2019). Finally, **Wrembel** provides evidence from a number of large-scale studies in phonology, such as Wrembel (2015), Wrembel et al. (2019), and Kopečková et al. (under review), indicating that CLI is gradual and structure-dependent and stemming from both previously acquired languages. She also points out that the LPM and the Scalpel Model resonate well with the Natural Growth Theory of Acquisition (Dziubalska-Kołodziejczyk and Wrembel, 2017, forthcoming), which 'assumes a gradual dynamic emergence of Ln phonology, shaped by the input from L1 and other languages (L2, Ln), and influenced by universal preferences understood as preference generalizations as well as typology and context of use' (Wrembel, 2021: 5).

VIII Unfortunate formulations and possible misinterpretations

The keynote may have contained some unfortunate formulations, as it seems to have caused some misinterpretations, which I would like to take the opportunity to clarify here. One example of this is **Flynn**, who takes issue with my concept of UG, as the keynote

repeatedly states that the innate endowment for language *enables* the child to parse the input. But this does not mean that UG *is* the parser; in fact, at the beginning of the keynote (Westergaard, 2021a: 6), UG is described as a genetic endowment which may ‘contain categories, features, principles, and constraints, as often assumed in the literature, but crucially no parameters’. Thus, I agree with Flynn that ‘without knowledge of language in some form, the language learner would be unable to parse a sentence’ (Flynn, 2021: 5). Furthermore, **Morales-Front & Sanz** have commented on the possible division between the grammar and the parser, but I would like to emphasize here that the keynote argues against such a division in connection with the – in my view unnecessary – distinction between representational copying and crosslinguistic effects in the TPM. Thus, the grammar and the parser interact, in that ‘the parser operates on representations in the grammar’ (Westergaard, 2021a: 18). **Stringer** (2021: 2) has also noted what he calls ‘very much a case of overreach’ in the claim that ‘parsing is the only mechanism for language learning’ (Westergaard, 2021a: 12), pointing out that this cannot be the case for lexical learning. Thus, the keynote should have clarified that this claim was directly related to the question of parsing vs. copying of morphosyntax, where parsing alone takes care of the acquisition process and there is no need (let alone motivation or evidence) for shortcuts such as representational copying, as argued in the TPM (for example, Rothman et al., 2019).

The discussion of micro-variation across similar L1 varieties and the variability found in learner grammars may also have been unclear, as **Bardel & Falk** (2021: 3) claim that equating ‘the well-known variation of interlanguage with (micro)variation in L1 or in certain dialects of native speakers is farfetched.’ However, these issues are not directly related: In the keynote (Westergaard, 2021a: 18) I argue against wholesale copying of grammar representations as a shortcut to a complete L3 grammar, as this would predict a relatively stable L3 grammar already at an early stage (although not target-consistent). The fact that early learners typically display a great deal of uncertainty in the L3 grammar is thus taken as some evidence for an approach where L3 grammars are instead built up incrementally, and representations are strengthened and stabilized by continued input and use. The following paragraph discusses a different issue, a claim by Schwartz and Sprouse (2021b) that step-wise development would somehow be incompatible with generative grammar. However, it is not difficult to show that there is well-known micro-variation in stable linguistic systems (for example, closely related varieties) as well as developing grammars (acquisition and change/attrition) that may easily be accounted for within a generative model.⁴

Finally, **González Alonso & Rothman** discuss a question asked in the keynote why the brain would create a situation for ‘massive unlearning in L2 acquisition, when this is avoided in L1 acquisition’ (Westergaard, 2021a: 10). Explaining how ‘[a]ll cognitive-based . . . approaches to L2A acknowledge that there is a significant amount of restructuring/unlearning implied’, González Alonso and Rothman (2021: 5) then argue that ‘all theories must explain why that happens.’ The LPM acknowledges the strong effect of the L1 on the L2 by the concept of Full Transfer Potential. Nevertheless, the question in the keynote should instead have been formulated in the following way: Given that all languages in a bi-/multilingual mind are always (to various extents) activated and must be inhibited when not in use, why does the brain create an *additional hurdle* by making a complete representational copy of the L1 (in L2 acquisition) or one of the previously acquired languages as a

starting point for L3 acquisition? Assuming that copied representations are somehow more stable than transient representations that are the result of processing (if not, what would be the point of arguing that representations are copied?), one would expect them to be harder to unlearn. And if, at later stages of L3 acquisition, the other (non-copied) language should turn out to have structures that may cause facilitative crosslinguistic influence, then the originally copied structure would be a serious disadvantage.

IX Concluding remarks

In this response to the commentaries, I have revisited the central issues, responded to some of the questions and, in so doing, extended the theoretical and methodological foundation of the LPM. It has been emphasized that according to this model, crosslinguistic influence is due to co-activation of corresponding structures in the previously acquired languages, where linguistic proximity of abstract structures plays the major role, while other factors may affect the strength of this activation. L3/Ln acquisition is a complex phenomenon, and experiments must therefore be carefully designed with respect to participant groups, timing, and selection of linguistic properties. Needless to say, numerous issues remain, but the commentaries indicate that there is a promising future ahead for this increasingly important field.

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Notes

1. This is arguably also what happens when learners are exposed to grammatical and ungrammatical sentences in acceptability judgement tests, often used in L3 studies, where there is a mismatch between lexical and structural similarity of the languages involved.
2. **Schwartz & Sproue** (2021a: 3) have apparently also missed the division of learning by parsing into processing for comprehension and processing for production (see Section III above and Westergaard, 2019: 11), since they claim that:

[i]t is unlikely that a ‘need’ would ever have arisen for a micro-cue that licenses any of these properties to be transferred from the L1 grammar into the L2-Interlanguage grammar, because there is no *TL [target language] input* that could be successfully parsed by recruiting such a micro-cue. (emphasis added)

See also their recent keynote (Schwartz and Sprouse, 2021b) about this issue, as well as my commentary (Westergaard, 2021b).

3. Interestingly, the study does find a significant difference between the two groups (an early positivity in the group exposed to Mini-Spanish), and the authors conclude that:

lexical similarity [between the L3 and one of the previously acquired languages] stands out as the only obvious candidate for the difference noted, which is compatible with models of transfer that focus on structural similarity as the main conditioning factor (such as the TPM and the LPM). (González Alonso et al., 2020: 15)

4. Despite this fact, it is certainly possible, as claimed by **Morales-Front & Sanz** (2021: 2), that both the Micro-cue Model and the Linguistic Proximity Model may ‘face some drag by being presented under the aegis of GG [Generative Grammar]’, since in their view both models are ‘closer to a Usage-Based Approach . . . to language development than to the original spirit of GG’. Both the Micro-cue Model and the LPM have aspects of generative as well as usage-based theory – which I have considered a strength. It is my hope that the future of our field will see more inspiration and collaboration across this (in my view unnecessarily wide) theoretical divide.

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