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Discussion: How Different Can Perspectives on L2 Development Be?

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In this article I discuss the contributions to this Special Issue of *Language Learning* on orders and sequences in second language (L2) development. Using a list of questions, I attempt to characterize what I see as the strengths, limitations, and unresolved issues in the approaches to L2 development represented in this Special Issue. I include short commentaries I solicited from the authors of the contributions, cited as personal communications. I conclude by arguing that, while it is completely legitimate to focus research and theory construction on the first few behavioral signs of acquisition of target structures (called emergence), there should also be room for research that looks at the entire developmental trajectory for a given set of related structures, from emergence to full mastery, to be examined in relevant target populations (differing in first language, differing in explicit knowledge), using several elicitation tasks, to be administered many times over the period that it takes to reach full mastery of all structures under investigation. Arguably, if such studies are to yield nontrivial findings, second language acquisition researchers should do their best to construct theories explaining these findings.

Keywords orders and sequences; second language acquisition; interlanguage development

Introduction

In the spirit of dialogue of this Special Issue of *Language Learning*, this Discussion presents not only my reflections on orders and sequence in second language (L2) learning but also many reactions (cited as personal communication) that I received from the authors of the contributions in the Special Issue. Because it is

I would like to thank all contributors to this Special Issue, as well as Lourdes Ortega and Rob Schoonen, for their helpful comments on earlier versions of this text. All errors are mine.

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impossible to continue the dialogue in a written text to be published, I resisted the temptation to respond to these reactions. Even after I revised my text on the basis of contributors' reactions, disagreements about how to render properly authors' positions persisted in some cases. I trust that most readers will appreciate the inclusion of the personal communications, accepting the inevitable interruptions in the flow of discourse. One thing is clear: The dialogue must be continued at future fora.

Recent Shifts in the Study of Orders and Sequences

Not so long ago, the picture of acquisition orders and sequences appeared clear: Empirical studies had shown that, at least for English as an L2, the acquisition of ten free and bound morphemes (e.g., Dulay & Burt, 1974) takes place in the same order for all L2 learners regardless of differences in age and first language (L1) and that L2 learners of English follow the same steps in acquisition in a single domain, for example, negation (R. Ellis, 2015; Ortega, 2014) or question formation (Eskildsen, 2015). These appeared to be robust facts (despite early doubts expressed by Cancino, Rosansky, & Schumann, 1978), calling for explanation. It was thus the task of second language acquisition (SLA) researchers to construct theories able to account for the facts. An early explanation of the acquisition-order data was offered by Hatch (1983), who proposed a series of eight "naturalness statements for morphemes" (p. 54). Hatch summarized the statements as follows:

> (I)f the form is naturally easy, it may be acquired early even though its function is not known. On the other hand, if the function is clear (e.g., plurality), it may *not* be acquird early *if* the affix is not regular. (. . .) In other words, the naturalness rules for morphology may have to be stated in terms of if/then statements, and orders may have to be assigned to each of the rules as well. (p. 55)

Most of Hatch's statements were supported by the landmark study of Goldschneider and DeKeyser (2001), who analyzed the data of 12 English L2 studies. Their analyses suggested that acquisition orders are determined, to a large extent, by five input factors: perceptual salience, semantic complexity, morphophonological regularity, syntactic category, and frequency.

Currently, however, the picture is no longer as clear as, say, in 2001. SLA has now entered the academically exciting stage where matters become really complicated. The presentations held at the Amsterdam (2013) and Portland

(2014) conferences and the papers presented in this Special Issue of *Language Learning* reflect this complexity, which resulted from the following three developments. First, a shift of focus has obtained away from the (order of) acquisition of unrelated structures (e.g., Dulay & Burt, 1974) to (i) the steps in which a single phenomenon (e.g., negation) is acquired or (ii) the order in the acquisition of a related set of phenomena (e.g., the acquisition of five related word orders of German, as in Pienemann's 1998 early work). Second, a simultaneous shift of focus is seen from the mastery of target structures to the emergence of target structures (see the contributions of Eskildsen, Lowie and Verspoor, O'Grady, Lenzing, and Pienemann in this Special Issue). And third, with respect to theoretical explanation, whereas Hatch (1983) and Goldschneider and DeKeyser (2001) listed a number of explanatory factors without assigning a primary role to any particular one of these factors, the theories of O'Grady and Pienemann single out the construct of processing efficiency as the primary factor that drives L2 development, in a truly causal sense.

A Global Overview of the Theoretical Landscape

Theories of L2 development, whether they make claims or not with respect to orders and sequences, can first of all be divided into the generative and usage-based schools. In the generative school, the following theories assume a null state at which both L1 and L2 learners do already possess some highly abstract linguistic knowledge: Input Processing theory (VanPatten, 2012), the Modular Online Growth and Use of Language framework (MOGUL; Sharwood Smith & Truscott, 2005, 2014) and, to some extent, Autonomous Induction Theory (Carroll, 2007). Language acquisition, in these theories, is seen as a matter of selecting or converging on the right grammar. In contrast, the basic idea in the usage-based school is that "the child learns language from actual 'usage events,' i.e., from particular utterances in particular contexts, and builds up increasingly complex and abstract representations from these" (Lieven & Tomasello, 2008, p. 168). Under this view, the mechanisms involved in language learning are not fundamentally different from the mechanisms involved in acquiring other cognitive abilities. Despite considerable differences, Dynamic Systems Theory (DST; as represented by de Bot, Lowie, & Verspoor, 2007; Lowie & Verspoor, 2015), Learner Varieties (Dimroth, 2013), as well as the Complex Adaptive System Principles model (CASP; Filipović & Hawkins, 2013) [1] all fall under the broad usage-based umbrella. Eskildsen (2015) also places his work in the usage-based school (to which he refers with the label Usage-Based Linguistics [UBL])

in the vein of Lieven and Tomasello (2008) and N. Ellis (2002). Sociocultural Theory (SCT) (Lantolf, 2012; Zhang & Lantolf, 2015), being a "general theory of human mental development" (Lantolf, 2012, p. 57), focuses more on the role of instruction in language learning than on the nature of the development of linguistic cognition in L1 and L2. SCT appears to be more in line with the usage-based than with the generative school. Lantolf (personal communication, August 29, 2014) gives the following comment on this characterization:

> Your comment gives the impression that instruction is not implicated in development. The point of the theory, as we explain in our new book, Lantolf and Poehner (2014), is that instruction is implicated in cognitive, and in the case of L2, linguistic development. In other words, instruction is a special form of development, or what Vygotsky called "developmental education" (Lantolf & Poehner, 2014). Said somewhat differently, education is a component of the "nature of development."

Klein and Perdue's (1997) Basic Variety (BV), while certainly to be categorized as a nongenerative, functionalist theory, cannot be classified as a genuine usage-based theory because "input features like frequency, saliency, etc. do not play a decisive role for the structure of the BV" (Dimroth, personal communication, June 25, 2014).

Processibility Theory (PT) (Pienemann, 1998, 2015) assumes that "the notion of constituency and the one-to-one mapping of semantic roles (such as agent, patient, etc.) onto the corresponding constituents" as well as the "the basic notion of predicate-argument structure" (Pienemann, 2015, p. 134) are present at the null state. Thus, PT takes the nativist side in the fundamental issue of learnability (also called the logical problem or the poverty-of-the-stimulus problem) by rejecting an unconstrained computational learning device (see the quote of Pienemann [1998, p. 1] in Pienemann, 2015). However, there is considerable evidence that, for instance, the Noun and Verb categories are not universal and that they are absent at birth (Croft, 2009). But it is true that PT assumes much less linguistic preknowledge than, say, MOGUL does (Sharwood Smith & Truscott, 2014). PT certainly does not underwrite the notion of Universal Grammar (UG), as Pienemann (2015) clearly points out. PT and MOGUL have in common that they claim to marry a dynamic processing approach to a static representational approach. However, whereas PT proposes six stages of L2 development, MOGUL only offers a framework for accommodating development, without proposing itself any developmental stages. With respect to representation, PT relies on Bresnan's (2001)

Lexical Functional Grammar. Pienemann and Lenzing (personal communication, August 25, 2014) comment on my characterization of PT as follows:

> The position that we assume is a combination of a minimal innate component and a constructivist component. Generativists argue against our position that one cannot derive more complex structures from simple structures. This was a standard position of strong rationalists such as Fodor (e.g., 1980 in Piatelli-Palmarini's edited collection). The epistemological basis of PT, therefore, differs from both empiricism and from Chomsky's approach.

O'Grady (2015) distinguishes yet another approach to language development, which he calls Processing Determinism (PD), distinct from both the usage-based and the generative school. In O'Grady's theory, the driving force underlying L1 and L2 development consists of the pressure to minimize processing cost, not input (as in the usage-based school) or UG (as in the generative school).

The term dynamic is used not only in DST (Lowie & Verspoor, 2015) but also in PT (Lenzing, 2015; Pienemann, 2015). However, in PT, the dynamic and probabilistic character of language acquisition and the resulting developmental variability within and between learners is limited by PT's linguistic architecture and its accompanying processing constraints. In contrast, Lowie and Verspoor (personal communication, July 22, 2014) argue that

> Variability is not just a random side effect but the driver of change, and studying different degrees of variability in different subsystems will give insight into the developmental process. Even though there are no predetermined constraints, eventually subsystems settle through self-organization and coordination.

Lowie and Verspoor (2015) give the impression that, in their perspective, any variability may occur. As Pienemann (2015) notes, DST does not seem to have reached the stage of explaining or predicting developmental stages. But Lowie and Verspoor (personal communication, July 22, 2014) disagree with this rendering of their position:

> This is confusing—"not yet reached the stage" seems to imply that a theory that can predict stages is of a higher order. We have argued that at the group level stages can be predicted (and/or explained), but that this does not need to imply that these stages are predictable at the individual level. Although certainly not *anything* can happen, the developmental

> trajectory of an individual cannot be fully determined. That is at the core of DST.

The term "emergence" is used in both PT and in O'Grady's (2005, 2012, 2015) PD theory. Only O'Grady (2005), however, places his approach in the tradition called Emergentism (MacWhinney, 1999), which is usually seen as an ally of the usage-based school. Thus, PT, although a theory of emergence, should not be seen as an Emergentist theory.

The Learner Varieties View

Christine Dimroth was an active contributor to the 2013 *Language Learning* Roundtable, in which she served as a discussant of the presentations of both O'Grady and Lowie and Verspoor. Her Learner Variety (LV) position, in some respects distinct from theirs, is important and thus worthwhile recounting here. In this section, resulting from an extended personal communication exchange (June 25, 2014) and authorized by Dimroth, I summarize her main points (see also Dimroth, 2013). In her 2013 presentation, Dimroth agreed with O'Grady and with Lowie and Verspoor that the early studies of a set of unrelated morphemes were not informative for our understanding of what she called the "developmental logic" of L2 acquisition:

> It is convincing and not very surprising that one can account for the results of the morpheme studies by a combination of five quite general factors (cf. Goldschneider & DeKeyser, 2001), but this does not explain which function a given morpheme has for a learner at a given point in time. In contrast, the Learner Varieties approach examines the functional domains (e.g., expression of temporality, of spatial relations . . .) including lexical and "grammatical" means. Where O'Grady observes changes in the interpretation of a particular form, e.g., reflexive pronouns, the Learner Varieties account would rather propose to start from the function: If a child is regularly using *Mickey scratches him* in a context in which Mickey is scratching himself, one could try to capture the development in the expression of co-reference with whichever means the learner employs to this end without qualifying any of them as correct or incorrect.

Dimroth valued DST's view that variability patterns are a meaningful condition for change. She acknowledged that in the BV approach (developed

originally in the European Science Foundation [ESF] project; Klein & Perdue, 1997) this was not always the case:

> The Learner Varieties approach interprets non-linearity as resulting from reorganisation. Learning a new feature is not like adding a new piece to a puzzle which the learner has to put together. "Rather, it leads to sometimes minor, sometimes substantial reorganization of the whole variety" (Klein & Perdue, 1997, p. 307). In contrast to the Dynamic Systems approach, the focus here is clearly on language internal reasons for reorganisation.

With respect to L1 transfer, Dimroth acknowledged that the original ESF study paired two source languages with each target language in order to take care of L1 influence:

> But researchers were definitely more interested in similarities and much less in differences between learner groups with various L1s. Klein and Perdue isolated an early learner variety, the so-called Basic Variety, in all untutored adult beginners they studied, thus independently of the individual L1-L2 pairing at hand: The Basic Variety was impenetrable for L1 knowledge. This stunning uniformity may of course partly be due to the level of description: The Basic Variety was characterized by the absence of functional inflectional morphology and by a handful of very elementary word order rules, mainly based on potentially universal semantic or pragmatic principles such as "agent first" and "focus last." Even utterances at pre-Basic Varitety stages (that many theories qualify as fragments!) are shaped by universal pragmatic principles that do not surface in this pure form in neither L1 nor L2. However, these generalizations are partly due to the level of granularity of the analyses. In a current investigation known as the VILLA project (Dimroth, Rast, Starren, & Watorek, 2013), the Learner Varieties approach is applied for the study of language development through the magnifying glass. In the VILLA project, a group of researchers in five European countries investigates how learners with five different L1s acquire Polish as an L2. Given that this happens under controlled input conditions, and that we also know much about individual differences between our learners (de Bot, Lowie, & Verspoor's 2007 cognitive ecosystems) potential L1 influences on the unfolding learner varieties can be detected more easily.

With respect to O'Grady's notion of processing amelioration, Dimroth made the following remark:

> The BV has been described by Klein and Perdue (1997) as showing "a particularly natural and transparent interplay between function and form in human language" (p. 304). Maybe this makes it such an influential attractor state? Wouldn't its "grammar" also be an ideal endpoint for processing amelioration? Or, to put it in Klein and Perdue's words in their 1997 title: "Couldn't natural languages be much simpler?"

Dimroth observed that Lowie and Verspoor (2015) claim that (i) all subsystems of language are equally important and meaningful; (ii) this does not only involve language internal subsystems like the lexicon, morphology, or syntax but also language external ones like input, motivation, learning strategies, etc.; and (iii) DST researchers aim at investigating the complex interplay of all subsystems that ultimately lead to development. Dimroth commented on this as follows:

> One reason for making the comparison between the approaches so difficult is to be found right here: Who would deny that complexity increases over time—from an LV perspective it is just not particularly interesting to measure this kind of complex overall development. Given the heuristic goals of the LV approach, the question seems just too broad to be handled in a meaningful way. Instead, we need to concentrate on restricted subsystems whose functioning and development is still far from being understood. The VILLA project (Dimroth et al., 2013) aims to uncover the developing internal structure of nascent learner varieties and relate it to input properties.

Questions to Be Asked in the Evaluation of Theories of L2 Development

Not all theories are equally explicit on what their positions are on some fundamental issues related to L2 development. In this section, I list some questions that may help readers in giving a place to current frameworks and theories in SLA's theoretical landscape. The questions pertain to six issues (see below).

Let me begin with what I see as a preliminary question, concerning the representation of linguistic regularities. There is regularity in every human language. Linguists describe regularities with two basic tools: abstract categories (such as phonemes, parts of speech [e.g., N and V], syntactic functions

[e.g., subject, predicate]), and rules or constraints that specify relationships among, and/or the linear ordering of, instances of abstract categories (e.g., subject–verb agreement, Subject-Verb-Object). Any theory of language acquisition and use has to say: (1) whether proficient language users (both L1 and L2 users) do or do not possess a mental grammar consisting of categories and rules (i.e., a question of representation), and—if mental grammars are claimed to exist in users' minds—(2) how these categories and rules/constraints come into existence (i.e., a question of acquisition). Researchers in the generative and usage-based schools take different positions with respect to the null state in L1 acquisition, as pointed out above. With respect to advanced stages, researchers in the generative tradition assume that language users possess a mental grammar (constrained in one way or another, as by UG). According to the usage-based school, language users possess constructions, which can account for regularity in language users' reception (comprehension) and production of language. As Eskildsen (personal communication, June 30, 2014) points out:

> Constructions, at their most abstract level, *are* abstract categories—but they are also "symbolic" form-meaning pairings. So the abstractions posited by UBL are just as abstract as, say, combinatorial rules of syntax, but they're not semantically void. The abstractions are thought to be arrived at by language learners on the basis of recurring exemplars of the same construction. [. . .] Because I work with developmental issues ("trajectories") and I keep in mind that Cancino et al. (1978) taught us not to use "stable grammars" to account for what is inherently dynamic ("development"), my take on it is that if it seems empirically valid to talk about these abstractions for people learning a language, then I will. In Eskildsen (2015), I do argue that Carlos might have a "schematic representation" that sanctions his uses of WH-questions; in Eskildsen and Cadierno (2007) we also made the same kind of argument for Carlos' *do*-negation construction.

Asked whether adult native speakers possess a mental grammar of their L1, Lowie and Verspoor (personal communication, July 22, 2014) responded with

> "NO" to the UG kind of pre-determined mental grammar, but we would totally agree with Eskildsen's view, in which the "schematic representation" emerges through use; In DST terms this would be considered a basin of attraction.

O'Grady (2005, 2012, 2013, 2015) appears to be unwilling to grant language users the possession of a traditional mental grammar (i.e., a grammar of any linguistic school, using categories and rules/constraints). I can understand why he does not ascribe to the generative view of learners selecting a grammar from a set of logically possible grammars and also that it is not the "goal" of the developmental process "to produce a grammar" (O'Grady, 2012, p. 116). But how do learners arrive at abstract categorical knowledge of, for example, Negation, Quantifier, Subject, Object, and Verb? Is this knowledge the work of the processing routines? Furthermore, when the processing routines are said to "do the work traditionally assigned to grammatical rules" (O'Grady, 2015), does that mean that these routines, and their entrenchment, are responsible for the predictable regularities in advanced learners' language-production and language-reception behavior? Why is O'Grady hesitant to grant (advanced) L1 and L2 learners a mental grammar? Dimroth, in her 2013 presentation, brought up the same point: "If we call the result a grammar or a set of processing routines, this might not reflect more than a terminological preference." O'Grady (personal communication, August 23, 2014) comments in the following way:

> On the question of why I don't posit a grammar, the simple answer is because it is not necessary to do so. (. . .) The key idea is that once the processing routines that are independently needed for speech and comprehension are identified and properly characterized, there is nothing left for a grammar to do. (. . .). My point assumes that the term "grammar" is being used in a non-vacuous sense. Here's a quote from the first chapter of *Syntactic Carpentry* on this point: "[Processing] routines are not just grammatical rules under another name. Routines correspond to real-time *processes*, whereas rules describe *patterns* of elements (Jackendoff, 2002:57). Rules say what the structure is (ibid.:31); routines say how it is built. These are not the same thing, as the literature on grammatical analysis itself repeatedly emphasizes (e.g., Jackendoff, 2002:197)" (O'Grady, 2005, p. 9).
>
> [W]ith respect to Dimroth's point [see above], here's the issue. If in fact the term "grammar" just means "whatever it is that allows us to build and interpret sentences," then of course anyone who has a language also has a grammar. But if it means "rules that say what the structure is," then the matter is not so easy to decide. A parallel point can be made with respect to Universal Grammar, which some commentators are now taking to be simply whatever it is that allows children to learn language, thereby guaranteeing that it exists (Chomsky, 2013, p. 34; Nevins, Pesestky, &

> Rodrigues, 2008, p. 357; Rooryck, Smith, Liptak, & Blakemore, 2010, p. 2651). But, once again, matters are not so clear if we think of UG in its more traditional sense of an inborn system of grammatical principles.

Having dealt with the preliminary issue of the representation of linguistic regularity, let me now, as promised, list the six questions and corresponding issues I consider fundamental in order to place current frameworks and theories for developmental sequences in SLA's theoretical landscape:

Issue 1: Abstract Grammatical Knowledge

As just explained, my opening questions are concerned with abstract grammatical knowledge: How does the theory account for the emergence and growth of abstract knowledge? How does the theory account for the fact that mature L1 speakers and proficient L2 speakers produce new utterances, never produced before, exhibiting regularities that linguists describe with the use of abstract categories and rules?

Issue 2: Reception and Production

Is the theory explicit about development of reception and production? Note that almost all empirical research pertains to language production. An exception is Spinner's (2013) study, which provided support for PT with respect to language production but not with respect to language reception. Input Processing theory (VanPatten, 2012) and MOGUL (Sharwood Smith & Truscott, 2014) include language reception but do not propose stages of L2 learning. O'Grady's (2005, 2012, 2015) PD theory focuses on language reception (comprehension). As O'Grady emphasizes in various places in his contribution in this Special Issue, an understanding of how the ability to comprehend language emerges is absolutely central to the investigation of development and to the development of language production: "Conclusions based on production data need to be tempered by the insights afforded by controlled experiments, especially those involving comprehension" (see also O'Grady, 2005, p. 7). Pienemann and Lenzing (personal communication, August 14, 2014) make the following comment:

> Hulstijn is concerned that PT focuses only on production. Although the main focus of PT related research has been on oral speech production, it is argued that the same processes are involved in the process of syntactic decoding (i.e. reception/comprehension). Lenzing (2013, 2014) proposes that there is one single syntactic processor underlying both

> comprehension and production processes in L2 acquisition. First results indicate a correlation between the two processes.

Issue 3: Variability

How do theories account for variability in the transition from one stage to the next? Variability has always been a problem in the study of L2 development. Ortega (2014) calls variation "a Trojan's horse in interlanguage" (p. 195). Deterministic approaches, such as PT (Lenzing, 2015; Pienemann, 2015) and PD theory (O'Grady, 2015), are less concerned with variability than with orders of emergence. PT allows for variability, moderated by: (1) attitudinal factors (Meisel, Clahsen, & Pienemann, 1981), (2) discourse topic (Pienemann, 1998), and (3) task (Pienemann, 1998), but variability is constrained by the universal processing hierarchy. Pienemann (2015) emphatically presents PT as a theory accounting not only for the nonvariable aspects of development but also for its variable aspects. Yet, in my view, what makes PT unique and different from other approaches to language development is its stance on the nonvariable and universal stages of acquisition. In contrast, MOGUL (in the generative tradition; Sharwood Smith & Truscott, 2014) as well as UBL (Eskildsen, 2015) and DST (Lowie & Verspoor, 2015) can handle variability much better, it seems. Pienemann and Lenzing (personal communication, August 25, 2014) comment on this characterization of PT as follows:

> We think that your wording "allows for" and "moderated by" understates what PT does. Instead, PT provides an approach to explaining variability in SLA from a structural and language-external perspective, and this occurs within the overall constraints of the psycholinguistic framework.

Issue 4: Explicit Grammatical Knowledge

Does the theory have anything to say about explicit grammatical knowledge and how such knowledge may affect L2 development? Does the theory have anything to say about the role of form-focused practice in L2 development? Do form-focused L2 learners travel through the same developmental stages as learners who do not take L2 lessons and/or learners who are not form focused? It seems as if only SCT (Zhang & Lantolf, 2015) and Pienemann's (1984) Teachability Hypothesis explicitly address these related issues.

With respect to the relationship between the Teachability Hypothesis (TH) and PT, Pienemann (2015) points out that Zhang and Lantolf (2015) are amiss when presenting the TH as a "corollary" of PT. Pienemann sees the TH as an "application" rather than as a corollary of PT. Historically, Pienemann is right

because he proposed the TH in a article that was published in 1984, while PT was presented in his 1998 book. But for me, the question remains whether, in terms of content, the TH could after all be seen as a corollary of PT by others, which does not mean, as Zhang and Lantolf are careful to observe, that a falsification of the TH would automatically imply that PT had been invalidated.

Issue 5: Social Factors

Does the theory have anything to say about the roles of social factors (e.g., interaction with interlocutors) and psychological factors (e.g., motivation) in L2 development? Can these factors alter acquisition orders or sequences? Only SCT (Zhang & Lantolf, 2015) appears to explicitly address this issue, while UBL (Eskildsen, 2012, 2015) argues that social factors (such as language use, affordances, etc.) are key factors in understanding L2 development in general.

Issue 6: Crosslinguistic Influence

Does the theory have anything to say about crosslinguistic influence (CLI)? Can all target structures be affected by L1 interference or does the theory pose limitations on CLI? In PT, the processing hierarchy is universal; therefore, CLI cannot affect the acquisition order of the morphosyntactic phenomena that fall under the processing hierarchy (Pienemann & Keßler, 2012, p. 231); PT allows CLI in all other structures. UBL researchers (including DST) do not appear to pose restrictions on CLI. According to O'Grady (2015), "learners have a propensity to transfer the entrenched routines of their first language to a second language, but only if that does not increase processing cost in the second language." Pienemann and Lenzing (personal communication, August 25, 2014) comment on my characterization of PT's stance on CLI as follows:

> PT contains an explicit hypothesis on L1 transfer. This is the DMTH (Håkansson, Pienemann, & Sayehli, 2002; Pienemann, Di Biase, Kawaguchi, & Håkansson, 2005). It *constrains* the possible effect of the L1 on L2 acquisition. It states that learners can only transfer L1 structures if they are processable by the current interlanguage system. This prediction is fundamentally different from a no-transfer assumption.

The theories represented in this Special Issue of *Language Learning* leave various questions in the six issues listed in this section unanswered. For instance, it seems to me that, for O'Grady, issues 2 to 4 are less relevant than the question of which inherently linguistic processing mechanisms drive L2 development in the first place. It is up to the members of SLA's research community to decide

whether the study of L2 development should be restricted to this question. R. Ellis (personal communication, July 1, 2014) comments on this as follows:

> It seems to me there is a deeper issue here—namely whether the goal of SLA should be the development of a comprehensive theory (which you seem to suggest it should be) or whether the goal of SLA should be to develop theories that are purposeful (i.e., address the concerns of particular users) as I have argued in my contribution to the Special Issue and elsewhere. For example, PT is not purposeful where language teaching is concerned because what is important here is mastery not emergence. I don't think a comprehensive theory is either possible or practical.

Lantolf (personal communication, August 29, 2014) adds to this:

> I agree with Ellis here to a point, but because of what I said in my earlier comment, our perspective on language teaching is that SCT not only addresses the purposefulness of language instruction as the achievement of mastery, it also addresses the matter of how L2 acquisition emerges (or at least can emerge) in the specific environment of the classroom.

Prediction, Falsification, and the Scope of Theories

A remarkable feature of both Pienemann's PT and O'Grady's PD theory is their (partially) deterministic character. In times of probabilistic approaches to the solutions of almost all questions in science and technology (Oaksford & Chater, 2007), the partially deterministic nature of these two theories, predicting universal hierarchical orders of acquisition (i.e., in an implicational scale) is particularly noteworthy. Perhaps, with respect to the explanation of phenomena of orders and sequences in L1 and L2 acquisition (and phenomena of children's development in cognitive domains other than language), the most crucial question is whether probabilistic accounts suffice, or whether developmental phenomena must be explained (as PT does) partially in deterministic and partially in probabilistic ways.

I regard PT as by far the most specific developmental theory of L2 acquisition. At the same time, however, it surprises me that no falsifications of its claims seem to have been obtained and published yet, given the considerable amount of research conducted under the PT framework. (Some concerns with PT were expressed in Bohnacker [2004], Dyson [2009], Glahn et al. [2001], and Spinner [2013].) Is it true then that, as of today, nobody has found that

a morphosyntactic phenomenon requiring information exchange between constituents emerged before a structure requiring information exchange within a constituent? PT makes deterministic claims with respect to structures falling under PT's universal six-level processing hierarchy, but it does not make predictive claims about the acquisition order of the structures that PT places at the same processing level. For most languages, there must be dozens of structures at the within- and between-constituent processing levels each. Pienemann and Lenzing (personal communication, August 25, 2014) comment on this consideration as follows:

> It is not a necessary assumption in PT that all features of a stage emerge in a regular order. The emergence of a particular structure from a stage reveals the capacity to process any of the structures that require that level of processing, but does not mandate that all those structures must be produced, let alone that there is a necessary stage-internal sequence.

Furthermore, PT accepts empirical evidence in its support only when three instances (sometimes two or even one instance) of oral production in spontaneous speech can be observed. I emphatically acknowledge that there is nothing wrong for a theory to limit its scope in this way. Pienemann and Lenzing (personal communication, August 25, 2014) comment on this consideration as follows:

> The key point is not the number of instances, but the methodological focus on the difference between chunks and productive usage. In the case of morphology the acquisition criterion also includes a check of lexical and morphological variation, again to exclude chunks in determining productive use. The idea behind this is to identify the point in time when a specific morphological or syntactic structure is used productively and can be considered to have been acquired by the learner. Despite the minimal nature of the acquisition criterion the strength of the evidence depends on the number of contexts for the given structure.

When Pienemann (2015) dismisses production data elicited with the rather demanding elicited imitation task used by Zhang and Lantolf (2015), we can conclude that apparently there is no inherent empirical link between PT and speech produced with this task. Everyone in the research community can then make up their mind whether this is a virtue or a vice. Pienemann and Lenzing (personal communication, August 14, 2014) made the following comment with respect to the use of elicited imitation:

> We consider the elicited imitation task used by Zhang & Lantolf (2015) problematic in this context not because it is "demanding" (as Hulstijn states), but because the design of elicited imitation tasks leads to the elicitation of formulaic utterances rather than production of utterances based on the acquired L2 system (Pienemann, Keßler, & Lenzing 2013).

Pienemann (2015) writes "(D)ata obtained through elicited imitation cannot be compared one-to-one with spontaneous speech production data. In terms of language processing, the two types of data tap into different psycholinguistic mechanisms." Note that this is an assertion. Empirical research on the question of what the task of elicited imitation measures has a 40-year long tradition in L1 and L2 acquisition research. For a recent overview see Ellis et al. (2009). The discussion about the status of formulaic utterances and the status of repetitions of stimulus utterances in instructional tasks (such as elicited imitation) should not be dismissed as only a matter of measurement. The matter is crucial for our understanding of language acquisition and I expect and welcome a continuation of this discussion.

By focusing on the emergence of processing constraints, PT creates the following paradox. If only three instantiations (in free oral production, excluding formulaic language), as in PT, are needed to conclude that a learner is able to process linguistic information at a given stage, the question arises why learners should not soon apply that procedure in all obligatory contexts so that production quickly rises to almost errorless performance. PT does not make claims with respect to relative mastery of related structures after their emergence, thus remaining silent on the long developmental journey after emergence and on possible differences in the duration of that journey between different processing stages. This paradox may apply also to O'Grady's PD theory. Pienemann and Lenzing (personal communication, August 14, 2014) comment on this as follows:

> It is important to distinguish the methodological function of the emergence criterion for acquisition from the actual learning device. Recent work within PT has begun to explore the learning device and so far it appears to be heavily lexically driven. This would be consistent with substantial variation between individuals in actual learning. We see no paradox between a clear and universal criterion for recognizing the emergence of a particular feature within the learner's system and learners

following variable pathways through the total language space; indeed that idea is central to the elaboration of the Hypothesis Space with PT.

"Tallying of Frequency and Accuracy Studies" Back on the Research Agenda?

Given the three trends listed in the opening section of this article, interest in the L2 developmental studies of the 1970s and 1980s seems to have disappeared. I plead for a renewed interest in the research practice of studies at the time that tallied frequency and accuracy of use, *in addition to* the work on the emergence of new structures in learners' L2 production. What I mean is the absolute and relative frequencies with which targetlike as well as nontargetlike interlanguage forms are used, so that the rise and fall of a related set of structures (such as the five word orders in German described in PT or the set of negation structures of English) can be examined over time. If one is interested in L2 development, one should test (a) groups of learners on (b) both their receptive and productive use of (c) a set of related (interlanguage or target) structures (d) over a longer period of time. One could then compare the resulting developmental patterns in different populations of L2 learners, differing in any potentially relevant dimension, (e.g., first languages, natural versus instructed setting). What I mean can be illustrated with reference to Figure 3 in Pienemann's article, depicting hypothetical developmental trajectories of three related structures. The information provided by the pattern of the three curves together, over time, tells us a much more complete story of L2 development than either only the order in which the three structures emerged or only the order of (almost) errorless mastery.

A good example of the type of research I advocate is the study conducted by Klein Gunnewiek (2000)—a study referred to by Pienemann et al. (2005).[2] Klein Gunnewiek tested the production of five word orders of German (derived from PT) in 24 Dutch learners of German over the course of 28 weeks in which German was first taught in the school curriculum. Students were tested in intervals of 4 to 5 weeks, each time with the same three task types. Using multilevel analyses (distinguishing target structure, time of task administration, type of task, and individual learners as independent variables), the researcher was able to investigate the question of whether a common developmental pattern could be observed despite minor individual differences. The development of the five structures hardly seemed interdependent and no stage-like theory was supported by the data. It was also found that mean proficiency levels as well as rate

of development were highly task dependent. In my view, this kind of research remains a relevant target of SLA research. Pienemann (2015) comments on this study as follows:

> As Hulstijn indicates, Klein Gunnewiek (2000) was unable to identify developmental patterns using quantitative methods based on average correctness scores and group mean scores. [. . . T]his demonstrates the weakness of the analytical approach rather than constituting empirical evidence of the absence of developmental patterns. (p. 134)

I acknowledge that, for researchers, teachers and learners alike, it is important to know that there are theories (namely PT and O'Grady's PD theory) that predict the order in which word order and accompanying morphological patterns will emerge in spontaneous speech production. But after structures falling under these processing constraints (PT) or under the pressure to minimize processing cost (O'Grady's PD theory) have emerged—and this may be a matter of weeks or months in tutored settings, depending on typological distance between L2 and L1 and amount of practice—there is likely to follow a much longer period of time until these structures appear more or less stable at, say, 95% correct in a variety of obligatory contexts. SLA needs to provide answers to the following kind of questions: Will the initial order of acquisition, according to the emergence criterion of three spontaneous instances, be maintained over time? Will we find structures (among a set of related structures) that, although they did not emerge as number one, finished first nevertheless? Of all the hundreds of morphological and syntactic regularities that learners have to acquire, which ones will quickly rise to perfection and which ones will continue to cause problems? How large will individual differences be? Despite individual differences, would we nevertheless be able to reliably conclude that, say, for the large majority of learners in given populations (e.g., Japanese adolescent learners of L2 English in Japan or learners of L2 English with L1 Spanish, living in the United States), accuracy in the production of structure X rises relatively quickly over time, showing a steep accuracy curve, while accuracy in the production of a related structure Y exhibits a flatter and more prolonged increase in accuracy? Furthermore, with respect to the explanation of observed accuracy orders, for which target structures in which languages will, for example, Hatch's (1983, p. 54) "naturalness statements for morphemes" and Goldschneider and DeKeyser's (2001) input factors (perceptual salience, semantic complexity, morphophonological regularity, syntactic category, and frequency) and Filipović and Hawkins's (2013) principles be valid? I believe

that theories of SLA, in order to provide a comprehensive understanding of L2 development all the way from emergence to targetlike plateau stages, have to include an account of data obtained in the type of studies proposed here. In my interpretation of R. Ellis's (2015) paper, it is this broad picture that holds for the large majority of learners of a given L2, that forms the baby that we do not want to see thrown out with the bathwater when studying L2 development. Scientific inquiry is concerned with abstracting away from details, trying to discern general patterns across individuals, and using appropriate statistical techniques to guard researchers against the danger of claiming evidence for general trends that cannot be reliably supported.

In conclusion, I plead for a renewed interest in the study of how learners come to master (or not) related target structures (as qualified above). This study should be conducted in addition to the detailed work on emergence of new structures in the oral production of individual learners and the explanation of emergence in deterministic and/or probabilistic terms. Hawkins and Filipović (2012) also plead for a combined perspective:

> We share his [Pienemann's] view that a lot of learning can and should be explained in terms of processing. But it is important not to lose sight of the fact that learning and processing are nonetheless distinct, in principle, and that we do need theories of both, even though their effects may sometimes be hard to tease apart. (p. 70)

I sincerely hope that there are SLA researchers around who deem it potentially relevant to conduct the type of research advocated here.

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Notes

1 Limited journal space does not permit a lengthy discussion of this important theory of SLA.
2 Unfortunately, this study was never published in an international journal.

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