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Creoles, interlanguages, processing and transfer: A response to Sprouse

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1. Introduction

- 1 In his article in the first issue of this journal, Rex A. Sprouse (2009) presents a critique of two of my recent papers (Plag 2008a, 2008b) in the *Journal of Pidgin and Creole Languages*, in which I put forward the hypothesis that creoles are conventionalized interlanguages of an early stage. Sprouse's critique would deserve a detailed treatment, which however, due to the space limitations imposed on this response, has to wait for another occasion. It may be worthwhile, however, to clarify at least some of the more important points raised in his article, one methodological, two theoretical. Sprouse accuses me of an 'uncritical acceptance of Pienemann's Processability Theory', of entertaining a 'traditional conception of transfer', and of an 'unwillingness to confront the rich generative literature on transfer'. In the following I will comment on each of these points.

2. Processing

- 2 It can be generally stated that the debate on the role of second language acquisition in creole languages is often not as well informed as it should be. For example, I feel that in creolist circles results of modern SLA research, especially pertinent theoretical insights, are not always appreciated and incorporated. Sprouse and I seem to be largely in agreement on this point.¹
- 3 When presented by the editor of the *Journal of Pidgin and Creole Languages* with the opportunity to reflect on topics of my own choice over a series of four articles ('Columns'), I decided to devote my Columns to the role of SLA in creole formation, and to introduce a hitherto neglected perspective into this debate. The perspective chosen was that of processing, involving four important domains of grammar and lexicon (inflection, syntax, phonology, and word-formation), each treated in one Column. The two Columns

on inflection and syntax are at issue here (i.e. Plag 2008a, 2008b; see Plag 2009a, 2009b for phonology and word-formation, respectively).

- 4 The reason for this particular choice of perspective was that the role of processing has never been really focused on in this debate, in spite of the fact that issues of, for example, relexification or transfer, are heavily dependent on notions such as access and selection, both in turn largely determined by processing (see, for example, Plag 2000). For instance, the selection of a particular item as the basis for relexification presupposes access to important parts of the L2 lexical representation of that item, which in turn requires the ability to process at least parts of the lexical and grammatical structures in which the item occurs in the L2 speech signal. Hence my focus on the hitherto neglected processing issues.
- 5 In my first two Columns I wanted to compare certain phenomena in creole inflectional morphology and syntax with potentially parallel phenomena in interlanguages. In particular, the idea was to test the interlanguage hypothesis by applying the predictions of a processing-based theory of morpho-syntactic development in L2 to the pertinent creole phenonema. To my knowledge (and in accordance with standard textbooks on SLA theories such as van Patten & Williams 2006), there is only one such fully-developed, production-oriented theory around, namely Processability Theory (e.g. Pienemann 1998, 2005),² and this theory makes rather clear predictions about the make-up of interlanguage morpho-syntax and its development in the learner. Inevitably, such a methodology involves using the theory employed as it stands. Whether this means ‘uncritical acceptance’ I leave to the reader to decide.
- 6 In any case, that a practitioner of Chomskyan-type generative linguistics does not like my choice of theory for my particular undertaking is understandable, but his criticism is beside the point. If anyone wants to use a different theory to test the interlanguage hypothesis, they are most welcome to do so, and I commend Sprouse for devoting himself to exactly this task in his paper. Eventually, we would hope to see which theory (or rather set of theories) is best able to account for the interlanguage facts as well as for the creole facts. Notably, the final results of his study, based on Chomskyan SLA theory, and of my study, based on LFG-based, psycholinguistically-informed Processability Theory, are ultimately the same. We both conclude that the creole phenomena show strong parallels to what we find in interlanguages. Of course, there are important differences in the two accounts, too, to which I now turn.

3. Transfer

- 7 With regard to transfer, Sprouse claims that I “fail[ed] to engage with the reconsideration and redefinition of transfer that occurred within the generative tradition in the 1990s” (Sprouse 2009, p. 275f). I did, however, refer to Schwartz and Sprouse’s Full Access/Full Transfer Hypothesis (FAFTH, e.g. 1996), but, again in line with my processing-based approach, I decided to employ an alternative, processing-based hypothesis, i.e. Pienemann’s Developmentally Moderated Transfer Hypothesis (DMTH, e.g. Pienemann *et al.* 2005). I write that the “crucial difference between the two positions is the timing of transfer, not the occurrence of transfer as such. The Developmentally Moderated Transfer Hypothesis does not deny transfer effects but claims that transfer is constrained by L2 processability”.³ In contrast, under the Full Transfer Hypothesis transfer is

“reconceptualiz[ed] purely in terms of mental representations” (Sprouse 2009, p. 276), *i.e.* not in terms of processing.⁴

- 8 One consideration why I did not apply the Full Transfer hypothesis is nicely illustrated in Sprouse’s paper, when he talks about the problem of lack of initial transfer of SVO in English-Japanese word order. While the DMTH states that SOV can be processed in the first stages of acquisition, hence that no transfer should be expected, the FAFTH would predict the transfer of SVO word order from the beginning. However, this is not what can be observed, which is a severe problem for the FAFTH. Not so for Sprouse. He explains the lack of transfer as follows: “Where the evidence is straightforward and robust, even without instruction, rapid develop[ment] should be expected.” (p. 284). If this is so, I would be curious to learn what would count as a falsifying instance for the full transfer hypothesis, since any non-occurrence of predicted transfer can then be regarded as the natural consequence of ‘rapid development’.
- 9 With regard to basic word order in creoles, Sprouse complains that in my paper “there is no suggestion as to how S AUX V O order might have arisen in Berbice Dutch in the absence of this word order pattern in both the substrate and the lexifier” (p. 283). It is unclear to me how Sprouse can have overlooked my pertinent remarks on the notions of ‘canonical word order’ and ‘unmarked alignment’. In the relevant part of my Column I explain how, under the processing constraints early L2 learners must operate, SVO or SOV patterns emerge irrespective of the basic word orders of the languages involved in the contact. From these considerations it follows that if creoles are conventionalized early interlanguages, we should find SVO and SOV word orders in creoles irrespective of the word orders of superstrate and substrate. And this is indeed what we seem to find cross-linguistically. I do not deny that transfer from an SVO language may converge with the universal, processing-based tendency towards SVO and SOV to produce rather straightforwardly an SVO creole, but the crucial test cases are constellations such as those we find with Berbice Dutch.
- 10 Obviously, Sprouse does not like my explanations and prefers a transfer-based account across the board, which, for the reasons outlined in the previous paragraphs, I in turn do not find particularly convincing.
- 11 But which theory of transfer is the correct one was not the topic of my Columns anyway. Rather, in the two Columns I wanted to show that there are important SLA phenomena observable in creoles that go *beyond* transfer. Unfortunately, Sprouse has apparently missed this point, perhaps due to his own focus on transfer in SLA, or perhaps due to my failure to express myself adequately. The phenomena I discuss in their majority exhibit universally unmarked structures, which I attribute to the universal processing constraints second language learners operate under. If it is true that “Schwartz and Sprouse propose that the most straightforward evidence for transfer would be differential developmental paths” (Sprouse 2009, p. 277), then the rather uniform structures exhibited across creoles call straightforwardly for the kind of non-transfer-based explanation that I provide.

Acknowledgements: I would like to thank Rex Sprouse for taking up the issues raised in my two JPCL papers. In doing so, he continues a debate between the field of second language acquisition and the field of pidgin and creole studies that can be fruitful for both parties involved. I also very much like to thank the editors of AILE-LIA, Daniel Véronique and Sandra Benazzo, for the

opportunity to respond to Sprouse's critique, and for their and another (anonymous) reviewer's helpful comments on an earlier draft of this paper.

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NOTES

1. I would, however, not subscribe to the view that SLA theories are ‘invisible’ in pertinent creolist studies, as Sprouse formulates it in the title of his forthcoming paper on ‘The invisibility of SLA theory in mainstream creole linguistics.’ (Sprouse in press). Perhaps his notion of ‘theory’ is more restricted than that of other people, such as myself.
 2. Sprouse refers to Dekydtspotter’s (2001) ‘Universal Parser’ as an alternative SLA processing model, one that is based on the tenets of Chomskyan-type generativism. Dekydtspotter’s programmatic study of the comprehension of a single lexical item in French interlanguage is, however, nowhere near to what I would call a *processing-based theory* of morphosyntactic interlanguage development. In addition, it is concerned with parsing, not with production. I was interested in the production side.
 3. I leave it to the reader to decide whether Sprouse is on the right track in his characterization of the DMTH as “traditional” (e.g. p. 273) and “essentially behaviorist” (p. 276), and of his own hypothesis as the “newer cognitively based understanding of transfer”.
 4. Contrary to what Sprouse suggests, the jury is still out on the question which of the two hypotheses is correct. Sprouse cites Bohnacker (2006) as the apparently final blow to the DMTH, but has apparently missed Pienemann and Hakansson’s (2007) reply to that article in the same journal.
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