Processing the Content of Input-Processing and Processing Instruction Research: A Response to DeKeyser, Salaberry, Robinson, and Harrington

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In the unenviable position of having to reply in approximately 1,000 words to several noted scholars about their comments on my article “Processing Instruction: An Update,” I apologize in advance for the inability to address all of their comments in detail.

Input Processing

The purported weaknesses of the current model of input processing (IP), as outlined in DeKeyser et al. (this issue), stem from the use of an outdated model of attention as well as a parser that does not consider the standard accounts in first language (L1) parsing. My response is essentially this: We cannot import wholesale models from L1 studies without concern as to whether such wholesale importation is appropriate for second language acquisition (SLA). Let’s take parsing first. That L1 parsing relies on structure precedence is predicated upon ambiguity resolution in L1 sentence processing (e.g., Clifton, Frazier, & Rayner, 1994; Cuetos, Mitchell, & Corley, 1996). However, developing second language (L2) learners do not have intact L2 parsers like L1 speakers. L2 learners may eventually wind up with a parsing mechanism that can be described and tested using L1 models, but this does not mean they begin with those parsers.
acquirers certainly don’t (e.g., Crain, Ni, & Conway, 1994). L2 learners must build L2 parsers, a point that DeKeyser et al. ignore.

In short, the problem of a “meaning-based parser” is not necessarily a contradiction for the L2 context as learners attempt to make sense of input. Additionally, one would hope that the first-noun strategy is a structurally based parsing strategy, since subjects and objects are structurally assigned during parsing.

As for capacity, although it may be true that some models of attention no longer describe fixed resources, again these models are based on L1 speakers and generally are not even models of the role of attentional resources in language processing. Only Just and Carpenter (1993) have proposed a model of capacity limitations that is comprehension-oriented. If a language-based L1 model suggests that comprehension is capacity-robbing, we can only begin to imagine the drain on resources for the L2 learner attempting to comprehend in a new language and the impact this has on processing.

The point here is that whatever the “problems” with the current IP model, they will not be solved by borrowing cognitive constructs wholesale from psychological research that has nothing to do with either language or with SLA (see also Schmidt, 1992, and VanPatten, 1994, on this point). Finally, the IP model was never meant to be a final-state model of parsing (if I am reading the criticisms correctly) such as those used in L1 studies; SLA is developmental in nature, and the model attempts to capture this.

As one more matter, even if we concede the purported problems in the model, the fact remains that the four processing principles describe what L2 learners actually do. Relying heavily on VanPatten (1990) to argue against the capacity “problem,” DeKeyser et al. ignore that there is both published and unpublished research using on-line and off-line measures that show that learners do indeed prefer lexical items to grammatical markers for getting semantic information (e.g., Lee, Cadierno, Glass, & VanPatten, 1997; Musumeci, 1989), that they do rely on a first-noun strategy (e.g., see Allen, 2000; VanPatten, 1996), that there are position preferences for processing (e.g., Barcroft & VanPatten,
1997; Klein, 1986; Rosa & O’Neill, 1998), and so on. These phenomena cannot be discarded.

Replication

DeKeyser et al. miss the point regarding the issue of design and replication. My point has always been and still is that processing instruction (PI) cannot be reduced to input/comprehension and that traditional instruction (TI) cannot be reduced to output/production (see, for example, VanPatten, 1996, 2000, as well as Sanz & VanPatten, 1998). PI is one kind of comprehension approach to focus on form and TI is one kind of output-oriented approach to form. In their criticism, DeKeyser et al. continue to conflate input/output with the two instructional types, attributing claims/hypotheses to me that I have not made. Their argument obscures the point that a replication is not a replication if it does not investigate the same underlying hypothesis as the original study did. Their discussion of Cheng (in press) to question the tenets of IP [sic], then, is simply off the mark: my (our) original claim was (and still is) that PI is better than TI, and on that score, the evidence is overwhelmingly in favor of the claim, including some of the research DeKeyser et al. cite to refute it (e.g., Benati, 2001). Finally, TI is a well-defined construct (e.g., Lee & VanPatten, 1995, in press), and for those working in foreign language teaching in the United States, it is readily recognizable and ubiquitous (Wong & VanPatten, 2002).

Misinterpretation of my work also appears in DeKeyser et al.’s discussion of VanPatten and Oikkenon (1996). Explicit in their criticism is that we cannot show that one group learned explicitly and one group learned implicitly. Nowhere in VanPatten and Oikkenon was this a research question or a hypothesis. What we sought to investigate was the role that explicit information played in PI, not implicit versus explicit learning in general. What we found (and currently are finding in other studies) is that explicit information plays little or no role in PI compared to structured
input. This cannot be construed as a claim about implicit versus explicit learning.

Conclusion

My final remark is simple. A careful reading of DeKeyser et al. (this issue) reveals the very problem I discuss in my article: misinterpretation and misrepresentation that result in distortion. Although the result is the chance to indicate these distortions in a forum such as this and to clarify issues for the reader, the downside is that the issues can never be addressed completely, and one wonders just what the reader walks away with.5

On the other hand, I'm honored that DeKeyser et al. think IP and PI have made a contribution to the field, and I would like to end where we all seem to converge: Namely, if we all agree about the fundamental role of input in acquisition, then we need to look at what learners do to input, why they do it, and what insights this may provide for instruction if any (e.g., Carroll, 2001). This is to be found only in research on IP in an L2 context.

Notes

1Certain results are reported incorrectly in their response. For one, Bransdorfer did indeed get differential results between the content lexical item and the copular verb that carried strong stress, and in another Bransdorfer study I cite in my 1996 book, it is clear that it is not the case that the issue is one of “easy/difficult” rather than “form/meaning.” In addition, Cheng did get initial results similar to VanPatten and Cadierno when she appropriately focused on the real acquisition problem, the verb estar (see VanPatten, 1996, pp. 116–119), although there was some improvement for the TI group. In short, even though TI resulted in improvement on the tasks, PI was still better than TI. In Benati’s study, the PI group was significantly better on the interpretation task, similar to VanPatten and Cadierno (1993). Again, PI is better than TI.

2Interestingly, Norris and Ortega (2000) classify PI as focus on form but classify the treatments in DeKeyser and Sokalski (1996) and Salaberry (1997) as focus on forms. I take this as further evidence that these particular replication studies are comparing their apples with our oranges. In addition, nowhere do I define TI as “composed of strictly mechanical drills” (DeKeyser
et al., this issue, p. 816), and nowhere is TI operationalized in my studies as such. Finally, two of the authors are the originators of the conflation of PI versus TI with input versus output (in their current essay, “modality”) and are, in essence, arguing against their own invention and certainly not one that my colleagues or I created or intended, as I have already stated in my article.

3 The authors use IP in some places to mean PI and PI in some places to mean IP. Thus, one has to sometimes deduce what their point of reference is.

4 Here is the classic description of TI that underlies most current approaches to grammar instruction in foreign language classes: “[A] grammar lesson should consist of grammatical rules which explain the particularities of the structural pattern to be learned plus a series of drills from a mechanical level to a communicative in order to give students optimum practice in language production” (Paulston, 1976, p. 4). In one guise or another, this approach has been advocated rather recently by Nunan (1999), Hadley (2001), and Stevick (1996), among others.

5 Because DeKeyser et al. are attempting to refute claims and conclusions in my research, I believe it is appropriate to examine their response for factual content. I have counted minimally 15 instances of misrepresentation and misinterpretation of what I have said. I leave it to the reader to ponder this observation and again apologize for not being able to address their criticisms entirely in the limited space available for a response.

References


Wong, W. & VanPatten, B. (2002). *The evidence is IN and drills are OUT*. Unpublished manuscript.