Language Learning 52:4, December 2002, pp. 805–823

What Gets Processed in Processing Instruction? A Commentary on Bill VanPatten's "Processing Instruction: An Update"

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In this article we respond to VanPatten's update of the findings for processing instruction. We begin by questioning the explanatory adequacy of the model of input processing that VanPatten has proposed and that underpins his pedagogic proposals. We question both the validity of the limited-capacity, single-resource model of attention he proposes for second language classroom learning, and also the details of the mechanisms he argues are implicated in second language processing. We then argue for alternative explanations of the effects found for input processing instruction and against VanPatten's claim that the studies he reviews are true replications of earlier findings.

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Throughout we argue that further specification of cognitive resources, processing mechanisms, and conditions of learning operationalized in putative replications are essential if research into input processing instruction is to be explanatory, and cumulative, as VanPatten claims it is.

Theoretical and Definitional Issues

Bill VanPatten has proposed a model of input processing (IP), and on the basis of this model, operationalized pedagogical proposals for processing instruction (PI). Central to the IP model are assumptions about the nature of "attention," language "processing," and the structure of attentional and memory "resources." We agree with VanPatten that specifying a model of the cognitive processes implicated in second language acquisition (SLA) is important to explaining the effects of instruction, and that "it is not sufficient to speak of input in general terms" in discussing how it becomes "intake" for SLA, and that "the learning mechanisms that act upon input or interact with it [must] be spelled out in some fashion" (p. 757). We find, however, that key cognitive constructs in the IP model are vaguely defined, disjunct with contemporary cognitive theory, operationalized in ways inconsistent with the IP model, and as such a poor basis for interpreting the findings of the PI approach they are claimed to motivate. We deal first with the important role of *attention* in VanPatten's IP model, and then with his claims about *processing*.

Attention

The first two major principles of the IP model are motivated by claims about attention. Wong (2001, p. 357) summarizes the "theoretical framework" on which the IP model is based in the following way: "VanPatten (1990) was motivated by the perspective in cognitive psychology that attention is effortful and that humans have limited capacity to deal with stimuli (e.g., Broadbent, 1958; Kahneman, 1973; Wickens, 1984)." Like Broadbent

(1958), that is, VanPatten argues that capacity limits make (early) selection of input necessary, and further, as did Kahneman (1973), that attentional allocation is "effortful" and that attention is a finite resource, which nonethless can vary as a function of arousal. This position motivates the first two of his IP principles: Learners *select* content to process before form (Principle 1), unless form can be processed "at (no) or little cost to attention" (Principle 2). VanPatten (1996) summarizes this view: "Comprehension is seen as an effortful phenomenon that consumes a great deal of attentional capacity. Driven to get meaning learners first allocate attentional capacity to detect content words in the input" (p. 30). In this traditional view, as Neumann (1996) has described it, selection of input occurs largely as a functional response to "capacity limits" on attention.

But there are long-acknowledged problems with this view. Neumann (1987) and Sanders (1998) describe Kahneman's notion of "capacity" as a "*vertus dormitiva*" [*sic*], in which a vaguely defined concept, related to observable phenomena, is given theoretical status to explain those phenomena. That is, in Kahneman's (and apparently VanPatten's) model, observable facts about capacity for processing are simply "ascribed" to a vaguely defined cause, that is, limited attentional capacity. This is no explanation, since it is not specified *how* or *why* capacity is limited (Neumann, 1987).

In contrast, much recent attentional theory argues that attentional resource capacity is *unlimited* (Neumann, 1996; Robinson, in press). Such unlimited-capacity interference models specify "mechanisms" causing breakdowns in performance and processing, arguing that increasing the number of stimuli and response alternatives or the similarity between them will sometimes lead to confusion, reducing performance efficiency. This can be caused by "competition" for the same types of codes during information flow or by "cross-talk" between similar codes (Koch & Prinz, 2002; Sanders, 1998). Specifying *codes* is what Gregg (2001) has called the domain of a second language (L2) *property* theory, but VanPatten is never explicit about this issue of what codes, or L2 properties, processing operates upon, apart from the (as

operationalized in PI studies) supposed form/content distinction—a problem we address in more detail in the following sections on IP.

Important to the construct of *attention,* competition and cross-talk are attentionally regulated mechanisms that describe how codes are successfully translated into (or not) each other, and are thus elements of a *transition* theory (Gregg, 2001). But attentional *capacity* is not invoked as a constraint on this process in contemporary interference models, as Gopher (1992) makes clear: "[C]onsiderations of resource scarcity or the performer's ability to allocate sufficient processing efforts are irrelevant. The limits on task performance are not conceived in these terms. Attention control is constrained to a decision to engage, disengage and shift attention between tasks and the pursuit of intentions. In interference models the only limited resource is time and its derived scheduling constraints" (pp. 279–280).

Other (though less recent than unlimited-capacity) approaches to attentional constraints on performance and processing have identified *multiple,* task-differentiated attentional pools of resources (Wickens, 1984) and explained breakdowns in performance as a result of competition for resources from the same pool during processing. VanPatten (1996, p. 16) does refer to multiple-resource theories of attention, but only to support the claim that capacity is "limited" within resource pools (though this is not a necessary condition of multiple resources, since they may be divided—though unconstrained in capacity—for efficient attentional-scheduling, time-sharing reasons). For this reason, perhaps, the *structure* of resources proposed by Wickens plays no role in his proposals. Wickens's model was proposed as a solution to the observation that sometimes concurrently performed tasks lead to decrements in performance, and sometimes they do not. Wickens's argued breakdowns in dual-task performance are more likely when two tasks simultaneously draw on the same resource pool. When they draw on different resource pools, there is no competition for resources. However, we would argue, attending to form *and* content during processing for meaning during communicative

interaction is a single task drawing on the verbal encoding resource pool (as opposed, for example, to the visual manual responding resource pool), not a dual task, and that simultaneous attention to form and content is clearly possible, as evidenced, for example, by results of experimental studies of incidental learning that show learning of "some" (not all) forms during processing for meaning (e.g., de Graaff, 1997; Robinson, 2002). However, in VanPatten (1990) learners listened for content or form words in the input but had to "divide" their attention between this task and a secondary task: simultaneously making check marks on a piece of paper while listening. Importantly, dual-task performance as used by VanPatten (1990) and Wong (2001) is more difficult than single-task performance (the phenomenon Wickens's model attempts to explain), since even though they may draw on different resource pools, dual tasks require task switching to be coordinated, and such coordination is also consuming of attentional resources (Rubinstein, Meyer, & Evans, 2001). In short, in terms of both interference theory and multiple-resource theory, VanPatten creates the phenomenon (a trade-off of attention to form versus meaning) he attempts to explain within the framework of a dated "single-resource, limited-capacity" model of attention. We argue that for the effects of PI instruction to be interpretable in terms of the IP model, what we need is not so much an "update" as an "upgrade" and clarification of the role of attention. The same is true of the role of "processing" in the IP model, the issue to which we now turn.

Input Processing

Despite the prominence of processing in VanPatten's account, the status of IP as a psycholinguistically testable construct is questionable. IP is difficult to relate to current approaches to sentence processing and formulated in such a way that it is impossible to evaluate the basic claim that it reflects how learners process, or "parse," sentences (VanPatten, this issue, p. 757).

The IP parser is meaning-driven. According to VanPatten (this issue), meaning in IP is the outcome of comprehension; it is what the learner is pushed to get and has multiple dimensions. One dimension is *overall sentence meaning* (p. 759). Another important kind of meaning is *referential meaning,* which involves "some semantic concept in the real world" (p. 759). Content words are the principal source of referential meaning, but grammatical functors can also have it (p. 757). The latter is captured in the *form-meaning* mappings that the learner's "internal processors" make between real-world meaning and form. Departing from the more standard use of form to refer to the surface of the utterance, VanPatten (1996) restricts the term to bound grammatical morphemes and functors, including prepositions, articles, and pronouns (p. 10). The meaning that a form contributes to overall sentence meaning is called its *communicative value* and is a function of the form's " inherent semantic value" and "redundancy" in signaling grammatical relations (this issue, p. 759). The relative *communicative value* of the form will dictate whether it is processed.

The overall, referential, and communicative meanings are thus carried in two kinds of mappings between surface elements and underlying functions: word-meaning mappings for content words and form-meaning mappings for free and bound grammatical morphemes.[1] In addition to these, VanPatten (1996) also posits *relational meaning,* which "refers to such things as who did what to whom" (p. 33). An important way in which this relational meaning is encoded is through interpreting the first noun as the agent of the sentence. VanPatten posits a built-in bias for the use of this strategy.[2]

According to VanPatten, variation in the amount and types of meaning will dictate how, and more surprisingly, when an element is parsed. The latter is an extraordinary processing claim: Although Principles 1 and 2 (given above) of the IP model possess a categorical certainty that is admirable, they make sense only if one assumes that "meaning" in Principle 1 refers only to the *referential meaning* encoded in content words and, in word order

languages, *relational meaning* coded in the first noun. For this to work the parser would need to discriminate between referential meaning appearing in the content words and that appearing in the forms. This would require some kind of "preprocessing" stage in which the parser sweeps through the input string and categorizes ("detects") each word as content or form, initially processing the content and inhibiting form until a second stage of processing.[3] Note that we are talking only about processing familiar language here and have not considered how the processor would deal with novel input, nor have we considered how the parser would interface with the subsequent intake stage.

Is such a parser possible? Perhaps, but no current sentence-processing models are even remotely related. Meaning-driven parsing is a contradiction in terms for the large class of structural-precedence models that rely heavily on grammatical and lexical information from the outset in processing (e.g., Frazier & Clifton, 1996; in the L2, Juffs & Harrington, 1995). Even approaches that emphasize the importance of meaning in processing assume that structural information is used from the outset as one of many constraints on interpretation (Boland, 1997; see review in Harrington, 2001). It is particularly odd that VanPatten (1996) identifies the competition model as the one that shares the most affinity with IP (pp. 51–52). The competition model is a subsymbolic approach to language and cognition and thus makes no principled distinction between the various cues (content, grammatical, pragmatic, etc.) that are available in the input: They are multiple sources of information that all contribute to understanding (Bates & MacWhinney, 1989).

Is the informal way in which VanPatten uses constructs like *attentional capacity, processing, meaning,* and *form* a cause for concern? Probably not, if the IP account is intended as a simple description of L2 learning outcomes (e.g., that there is a limit to the amount of information one can process at a time, and that words that are more salient in the input are learned more easily). These are elements that L2 instruction certainly needs to accommodate (see "Implications"). On the other hand, the underspecified

and often idiosyncratic use of these notions is a problem if IP is intended as a viable account of sentence processing that provides a psycholinguistic foundation for L2 theory or pedagogy.

Design and Operationalization Issues

Not only are there problems with the conceptualization of *attentional capacity, processing, meaning,* or *form,* as shown in the previous section; there are further problems with the operationalization of these concepts in many studies carried out by VanPatten and other proponents of PI. In a variety of studies, the difference between "processing for meaning" and "processing for form" coincides with other, less exciting, but not less obvious, differences. Listening for "key lexical items" in VanPatten (1990) and listening for a "content word" in Wong (2001) does not lead to a decrease in text comprehension scores, whereas scanning for grammatical morphemes does. Hence the inference that elements of meaning (content, lexicon) do not interfere with comprehension, whereas any attention to form, even in the superficial sense of scanning for certain morphemes, does. But is the form-meaning distinction what is really at play here? How about the obvious distinction between a full-fledged word that is easy to notice and a morpheme of one or two letters that is easy to skip over? To make matters worse, in the case of both studies, the target word was a close cognate (English L1 *inflation* and Spanish L2 *inflación* in VanPatten, 1990; French L1 *inflation* and English L2 *inflation* in Wong, 2001), so that the detection task here is extremely easy, much easier than scanning for barely noticeable morphemes, at least in the oral condition (note that in Wong's written condition, where arguably detection of grammatical morphemes is easy, too, no decrease in comprehension scores was found when learners were required to scan for grammatical morphemes). Note also that Bransdorfer (1991, quoted in Wong, 2001) did not find any interference from scanning for elements of grammar/form when these were operationalized as whole words such as *está* (one of the two copulas in Spanish). All of this seems to suggest that what

underlies these results is not a form-meaning processing distinction but rather an easy-difficult scanning distinction.

Other studies are equally open to alternative interpretations. In VanPatten and Oikkenon (1996), three groups were compared: explicit information only, structured input only (including comprehension exercises and feedback), and "regular PI," the last being a combination of explicit explanation, systematic practice, and explicit feedback referring back to the rules. Object pronouns in Spanish as a foreign language were the target of instruction. On the comprehension posttests, the last two groups virtually coincide, leaving the first far behind. VanPatten and Oikkenon conclude from these results that it was structured input and not explicit information that was helpful to the learners, but it is clear from their description of the treatments that the structured-input-only group must also have engaged in explicit learning. Even though learners in this group were never given the rules, they were constantly given yes/no feedback, which must have led them to figure out the system (which boils down to a simple morphological alternation, along with a word order that is different from English). Rather than an implicit group, then, this is an explicit inductive group. On the other hand, the explicit-information-only group was never given any relevant practice. In other words, instead of an explicit and an implicit treatment, there was a good explicit-inductive and a poor explicit-deductive treatment. The order of performance of the three groups, then, is as one would expect: good explicit-deductive ("processing"), good explicit-inductive ("structured input"), and poor explicit-deductive ("explicit information only"). It may very well be, in other words, that the explicit rule teaching (or, more precisely, the explicit rule learning) that takes place in PI is what is beneficial, in interaction with the many examples in the structured input, of course, rather than this input by itself. Just as in the lexical/grammatical morpheme distinctions mentioned above, the treatment distinction as labeled/intended in VanPatten and Oikkenon (1996) is probably not the treatment distinction the learners experienced subjectively, and the latter, of course, is all that matters. Future research

would do well to document learners' subjective experience of the treatments received.

Note also that in the most frequently quoted PI study, VanPatten and Cadierno (1993), it appears that the "PI" group was given more explicit rule information than the "traditional"/output group. Here again, the degree to which information about grammar was processed explicitly may have had a substantial impact on the results, especially given that the PI students were given more explicit information than they needed for the production test (which boils down to a simple morphological alternation), whereas the traditional instruction (TI) students did not have their attention drawn explicitly to the word order differences that make the comprehension tests difficult for a native speaker of English.[4]

We cannot agree, therefore, with Benati (2001) when he says that "processing instruction does not aim at raising learners' consciousness about grammatical form" or that "the ultimate scope of processing instruction is not about raising consciousness awareness [*sic*] about a grammatical form but to make the learner appreciate the communicative function of a particular form and consequently enrich the learner's intake" (p. 99); how exactly would this nonconscious "appreciation" occur anyhow? We do agree with VanPatten, however, when he says that "it is more appropriate to view [PI] as a type of focus on form or input enhancement" (this issue, p. 764).

Replication Issues

The empirical evidence gathered so far increasingly points toward the rejection of the main tenet of the IP hypothesis. For instance, replication studies that analyzed the effect of IP on semantically more complex targets such as the subjunctive contradict the theoretical claim about IP made by VanPatten. Indeed, Collentine (1998) and Farley (2001) showed that both PI and output-processing (OP) groups improved after instruction on the uses of the Spanish subjunctive. Similarly, Cheng's (2002) study on the acquisition of the Spanish copulas *ser* and *estar* shows

equally beneficial effects for both PI and output-based treatments. And the same can be said of studies that extended the analysis of the effects of PI to other languages (and also different target items). Benati's (2001) data revealed that both the TI group and the PI group improved their performance (compared to a control group) in both the interpretation and the production of the Italian future tense. More importantly, the analysis of the use of the French causative from Allen (2000) reveals not only that the TI group improved as much as the PI group in the interpretation task, but that the TI group was significantly better than the PI group in the production task.

VanPatten reconciles the above-mentioned findings by pointing to a different operationalization of the TI treatment group in these studies (which incorporated a significant amount of meaningful communicative tasks) compared to the operationalization of the same construct in previous studies (i.e., Cadierno, 1995; VanPatten & Cadierno, 1993). This post hoc argument raises two important questions: How do we define TI, and how do we operationalize it in the actual treatment condition? Salaberry (1997) argues that "the methodological problem of the research design of the studies supporting IP is that their results show interaction effects between their proposed treatment variable—input or output practice—and one or more intervening factors" (p. 428). It is not difficult to see what some sources of additional variation are. For instance, Cadierno (1995), a study frequently cited as empirical evidence to support the IP hypothesis, acknowledged two additional variables, other than mode, in her operationalization of the treatment conditions: (a) differential degrees of emphasis on meaning, and (b) the sequential versus the paradigmatic presentation of past-tense verbal morphology. Cadierno justified this difference by pointing out that "this variation as to the types of activities is a direct reflection of what is commonly presented in Spanish textbooks" (p. 190). Cadierno's characterization of traditional teaching practice may be accurate, but that does not invalidate the concern about possible confounding of treatment

variables. Interestingly, VanPatten advances the same type of argument in his latest review (see below).

More recent studies are beginning to recognize additional sources of variation explicitly by including them in the research design of their studies (with potential effects on the operationalization of the theoretical construct). For instance, Farley (2001) compared the effects of PI with regard to what he called "meaning-based output instruction." The latter was explicitly distinguished from other output-based treatments "in that there is no component containing traditional, output-based mechanical drills" (p. 291). Farley's study showed that both IP and OP groups improved after instruction. In the case of Cheng (2002), a reinterpretation of previous findings from her dissertation (1995) led her to conclude that her "traditional instruction group" may have focused on meaningful practice (to account for the nonsignificant differences between treatments). Finally, Collentine's (1998) nonsignificant differences between input- and output-based instructional treatments on the use of subjunctive in Spanish led him to conclude that "in all likelihood, some factor other than mode led to the two experimental groups' similar outcomes" (p. 584). In a more recent analysis, Collentine (2002) speculated that one intervening factor may be the complexity of the target grammatical item (see also Allen, 2000; DeKeyser & Sokalski, 1996).

Thus, considering the emphasis that VanPatten puts on research design issues in replication studies, his justification of the operationalization of one of the key treatment variables in his studies (i.e., TI) is not entirely clear to us. VanPatten claims that TI is composed of strictly mechanical drills given his analysis of "10 major Spanish textbooks" and claims made by graduate teaching assistants who attended his workshops, etc. First, note that his analysis of the state of the art of the field of L2 instruction is open to criticism given that language teaching practice does not necessarily mirror the pedagogical approach presented in textbooks, nor does it necessarily reflect what teachers claim they do in a language class (Long, 1991). Be that as it may, it appears that VanPatten is confounding the analysis of language teaching

practices (and policies) with the operationalization of one of the treatment conditions in empirical studies. This is unfortunate, because any research design with such a vague definition of a key construct is bound to introduce a great deal of spurious results.

Moreover, the above-mentioned concerns about the appropriate operationalization of the TI treatment condition extend to the operationalization of PI as well. For instance, Farley (2001) questions Collentine's PI materials because they do not "keep the learner"s strategies in mind" (p. 290). Collentine (2002) rejects this claim and contends that his own instructional materials may be "much more compatible with processing instruction's ultimate goal," given that Farley's tasks attempted to raise the acoustic salience of the subjunctive, whereas Collentine's attempted to raise the subjunctive's communicative value (with greater ecological validity). Interestingly, although VanPatten initially acknowledges the results from Collentine (1998), he points out that (a) Collentine's PI materials did not include affective activities and that (b) the informational load in his activities was "heavy." VanPatten concludes that these are important constraints that limit the scope of the IP hypothesis and suggests that they be emphasized more, so that future studies will be consistent with the claims made by IP. We take the latter statement, apart from underlining the variability in operationalization of the IP treatment across studies, to mean that the IP hypothesis may need substantial changes to be generalized to more complex target grammatical items.

Finally, it is important to point out that intended but also unintended modifications of the research design originally used by VanPatten have uncovered important empirical findings that lead to further questioning some of the basic tenets of IP. To make this point clear, let us review the findings from Cheng (2002). In this study, the results of an interpretation task, a sentence production task, and a guided composition on the uses of the copulas *ser* and *estar* all revealed significant improvements for both treatment groups (i.e., PI and TI) in comparison with a control group. There was no significant difference, however, in the improvement

made by both the PI and the TI groups. Only when one of the copulas (*estar*) was isolated (the data from *ser* were taken out) did the results favor the PI group for the improvement from posttest 1 to posttest 2, for the interpretation and production tasks only. There were no significant differences, however, between the PI and TI groups in the immediate posttest. And more importantly, both treatment groups outperformed the control group in both posttest 1 and posttest 2 in the more open-ended task, the guided composition. Much can be said about these complex results. For instance, one could argue that any true system-wide effects will be revealed more clearly in the discursive task and not a sentence-based task, in which case no difference is detected either in the short or the long term. As far as the sentence-based task is concerned, one could argue that, given that both groups improved in the immediate posttest, the OP group may simply need more practice to maintain their gains (but not necessarily that there is a qualitative difference in the type of knowledge generated by each condition; see below). More importantly, Cheng's own post hoc explanations reveal potential inadequacies of some of the basic tenets of IP. For instance, note that Cheng claims that "the [copula's] low communicative value in general may have contributed to the lack of significant difference between processing and traditional instruction in this study." But is it not the purpose of PI to make meaning-form connections, especially with target grammatical items of low communicative value? In passing, we note that Cheng (2002) appears to supersede the overly strong claim about the benefits of PI made by Cheng (1995), which is used by VanPatten (this issue) as strong empirical evidence to support his claim.

VanPatten (this issue) argues that "as long as classes and materials are meaning-oriented and avoid mechanical and display language, acquisition is fostered and PI is no better than any other meaning-based instruction with a form focus. I would not disagree with this statement at all" (p. 798). Although we would not disagree with such a statement either, we are concerned, however, with the lack of an appropriate operationalization of the

theoretical construct that underlies the findings purported to support VanPatten's claims. Note that the role that VanPatten and Cadierno (1993) attributed to OP is qualitatively different from the one they assigned to IP. Crucially, only the latter type of processing, they argue, leads to "alter the nature of the developing system." More specifically, IP leads to acquisition, whereas "traditional instruction results in a different knowledge system" (p. 238). Unfortunately, VanPatten has little to say about these two different types of knowledge in his latest reanalysis. His focus on broad issues such as input versus output (who would deny that both are aspects of the process?) confuses the issue. We submit that the right operationalization of the theoretical construct that VanPatten is after may not necessarily be mode. A more appropriate question is, for instance, how can we lead students to affect their developing system through various pedagogical manipulations (be they PI, task-based, etc.), irrespective of mode? We recognize that VanPatten acknowledges the above-mentioned concerns as he tries to limit the generalization of his hypothesis. He restricts his claim to the analysis of his own version of the type of instructional treatments with a focus on input. Notice that he points out that critics of PI are not accurately assessing his claim "unless one liberally interprets TI as meaning any kind of output-based instruction or PI as any kind of input-based instruction" (p. 796). Even though we welcome this first attempt at limiting the scope of his hypothesis, we believe that the concerns we have raised about the lack of operationalization of the treatment conditions remain valid.

Pedagogical Implications

The main conclusion to be drawn from our discussion is that it would be at best premature at this point to draw sweeping conclusions about what kind of processing students need to engage in to acquire various structures. First of all, very little if any research on "PI" can even claim to address acquisition and not just the learning of monitored knowledge. More importantly, however,

even when only this kind of monitored knowledge is concerned, one should be very careful in drawing conclusions about the learning of any structure in any language. The issues of noticing, understanding, remembering, and retrieving the relevant knowledge, even considering declarative knowledge alone, are very different depending on the transparency of the form-meaning connection, the abstractness of the structure involved, the similarity with the native language, and the nature of the skill required (comprehension versus production). Some complex structures are easy to recognize but hard to produce correctly. Others may be easier to produce correctly when monitored but are harder to perceive, let alone process correctly and speedily. There is room here for a great many studies to investigate the contribution of various kinds of processing activities to the learning and acquisition of various kinds of structures in various languages. Such research is needed not only from an applied point of view, but also to provide the necessary database for induction of broader principles of L2 learning and acquisition.

Conclusions

Bill VanPatten has made a very important contribution to the field by drawing attention to the importance of providing students with activities that engage them in processing crucial form-meaning links, in particular, in comprehension activities. As is often the case in the field of SLA, however, there has been a rush to overgeneralization and overinterpretation, which threatens to overshadow the very important message of PI. This commentary should be seen as an invitation to contribute to a more finely articulated understanding of this very important issue for both theory and practice. What elements of input can be processed simultaneously, given what we know about processing and capacity limitations? How does that depend on the nature of the structure and the nature of instruction? How much can the teacher hope to achieve in this respect for a given type of student? These are questions that still await empirical answers from rigorously

designed experimental research with clearly defined and controlled treatments and sizeable samples of students learning a variety of structures in a variety of languages.

Revised version accepted 22 April 2002

Notes

[1]Although the IP model is a capacity-constraint model in which resource demands are crucial, only relative resource demands of form are specified (by variation in communicative value). The resource demands of content words are not assumed to vary.

[2]This kind of meaning is encoded differently across languages. Word order is crucial in English for distinguishing between, e.g., *Bob kissed Betty* and *Betty kissed Bob.* Overt case markers (i.e., grammatical form) perform the same function in languages like Japanese and Korean. Thus, according to VanPatten, thematic relations are a type of referential meaning in case-marking languages like Japanese but a type of relational meaning in word order languages like English!

[3] This would involve some kind of affix-stripping mechanism that detaches bound forms from their content stems, but how such a mechanism might work is an open question. Since both content words and forms can have referential meaning (semantic value), the presence or absence of semantic value cannot be the basis for the decision. The preprocessor could use a threshold value, but it would have to be defined in such a way that forms with the greatest possible "inherent semantic value" still had less referential meaning than the least meaningful content word.

[4]We owe this observation to an anonymous reviewer.

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