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## ***Comments on Stephen D. Krashen's "Teaching Issues: Formal Grammar Instruction"***

### ***Two Readers React . . .***

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■ In Steven Krashen's Teaching Issues contribution (Vol. 26, No. 2), he responds to the question, Under what circumstances, if any, should formal grammar instruction take place? He reiterates his position that the only role for formal instruction is to permit learners to "monitor" their L2 production. In his view, when teachers and students focus on the form of the language, they may succeed in changing some superficial aspects of a learner's performance, but they do nothing to change the learner's underlying competence. According to Krashen, the only way to bring about changes in that competence is through exposure to comprehensible input.

A number of researchers and theorists (e.g., Gregg, 1984; McLaughlin, 1978; White, 1987) have questioned Krashen's hypothesis and expressed their reservations about the research findings which are cited as support for it. It is not our intention to review such general issues here. We wish instead to focus on the pedagogical implications which Krashen draws from the comprehensible input hypothesis.

Time and again, language teachers have seen new methods introduced and old ones rejected out of hand. More recently, it seemed that teachers and researchers were moving toward an agreement that we need far more information about the processes involved in language learning and that many questions need to be approached with an open mind rather than answered in a dogmatic fashion (see, e.g., Larsen-Freeman & Long, 1991; Lightbown, 1985; and articles in Hyldenstam

& Pienemann, 1985). In light of this, it is frustrating to see pronouncements in Krashen's Teaching Issues contribution such as: "Research and theory show that the best way of increasing grammatical accuracy is comprehensible input" (p. 411). Stating this in response to a pedagogical question is quite different from presenting it as a hypothesis in a research context. Our hypothesis is that while comprehensible input is an essential part of the learning environment, it will not always be sufficient to bring about developmental change or increased accuracy, even when learners are in supportive environments. We have seen evidence in our research that form-focused instruction can bring about changes in interlanguage and, furthermore, that there may be situations in which learners not only benefit from but require focused instruction to further their language acquisition.

In one series of studies, we found that form-focused instruction had a lasting effect on interrogative structures (Lightbown & Spada, 1990; White, Spada, Lightbown, & Ranta, 1991). Krashen dismisses these findings by suggesting that the tasks used to assess learners' development were "monitored" tasks and that learners were focused on form.<sup>1</sup> It is true that some of the tasks in the interrogative study quite explicitly required learners to manipulate the word order of the sentences. However, the learners displayed the same changes in their interlanguage in an oral task, showing no evidence of hesitation, mental searching, or distraction from the guessing game they were engaged in (Spada & Lightbown, 1993).<sup>2</sup> Nor was it the case, as Krashen implies, that the learners' focus had been overwhelmingly on the targeted features during the 2 weeks preceding the posttest. The instructional component of the experiment had occupied, at most, some 9 hours out of a total of nearly 50 hours of classroom activities over that period.

Krashen also comments on another study (Lightbown, 1991), in which students achieved high accuracy in using *be* rather than *have* in presentational phrases (*There's a book on the table* instead of, e.g., *It has a book on the table*). The teacher is quoted as saying she "drummed it into their little heads," and Krashen concludes that the students had

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<sup>1</sup>One of the problems in dealing with Krashen's distinction between learning and acquisition and between monitored and spontaneous language production, of course, is that it is virtually impossible to define these terms in ways which permit them to be observed or measured. For example, the Second Language Oral Proficiency-English (SLOPE) test (Fathman, 1975) essentially requires subjects to fill in the blanks in oral substitution drills. But Krashen has accepted it as a "communicative" task, apparently on the grounds that the relative accuracy of use of the grammatical morphemes by learners doing this test is comparable to that of learners using English in other, less structured environments (see Dulay, Burt, & Krashen, 1982).

<sup>2</sup>Although it is true that Krashen will not have had access to the paper in which the oral performance results are reported in full, the paper to which he does refer (White et al., 1991) mentions these findings in the concluding section (p. 429).

been “drilled” on a single point until they had learned this one thing, which they were then able to monitor. This explanation seems implausible in light of the fact that, 1 year later, the students were still highly accurate in the use of this form in spontaneous communication. This was *not* true of another group of students whose high accuracy on this structure had, in fact, been achieved through decontextualized drill (Lightbown, 1987). This difference was emphasized in the article. The “drumming” was often done with humour, was always in the context of ongoing communicative interaction, and was *never* framed as drill. It seems quite plausible that the teacher was actually drawing the learners’ attention to a language feature such that they understood that what they were saying was not what they meant to say. Seen in this way, the form focus may have helped make that particular phrase genuinely comprehensible in the input. It seems odd that Krashen is so determined to rule out a role for instruction that he does not propose this interpretation himself. Our interpretation of these findings was that contextualized form-focused instruction may be not only beneficial but essential under certain conditions and for certain features of the second language (see also Swain, 1985; White, 1991).

Krashen’s dismissal of Pienemann’s research is even more puzzling. In the first place, while Pienemann’s “teachability hypothesis” makes reference to possible effects of instruction, its primary objective is to define the *limits* of such effects. Pienemann tested the prediction made by the teachability hypothesis (that instruction is effective only if timed to match the learner’s developmental stage) in two experimental studies in which L2 structures which were taught to children (Pienemann, 1984, 1989), and university students at different stages of L2 acquisition (Pienemann, 1988), and in a longitudinal study of the formal acquisition of an L2 by university students (Pienemann, 1988, 1989).

Pienemann’s first experimental study tested the prediction that learners who are developmentally “ready” to learn a structure (in this case, inversion) can learn it through formal teaching, while those who are not ready will not learn it, even if they are exposed to the same instruction. The study is based on pretests and posttests for 5 Italian children learning German as a second language. Some 20 hours of naturalistic conversational data confirmed Pienemann’s hypotheses: The 2 learners whose interlanguage had reached the stage immediately prior to inversion did learn it, while the 3 whose interlanguage was at an earlier stage did not. Krashen argues that Pienemann’s claim for the effectiveness of well-timed instruction is “based on very little data” (p. 410). He suggests specifically that it is based largely on the data from only 1 of the 5 children whose German L2 acquisition was examined in this study. In claiming that the whole case for the effect of teaching is based on 1 informant, Krashen overlooks the fact that, because

German inversion is a highly frequent structure, within the several hours of speech observed and recorded in this study, each learner had hundreds of opportunities to produce utterances which would contradict the predictions made by the teachability hypothesis. In fact, they did not deviate from the predictions even once.

Krashen does not mention an extended replication of the experimental study (Pienemann, 1988). Pre- and posttests were carried out for 12 university students of German as a second language. Thirty minutes of conversational data were elicited from each subject and analyzed in terms of five different structures. In this case the objective was to synchronize the teaching of grammar as well as possible with the learners' interlanguage grammar. This study confirmed for most of the informants and structures that their interlanguage grammar did progress in response to form-focused teaching if the interlanguage was developmentally ready for the structures taught.

Krashen also overlooks another study of adult learners of German as a foreign language in a formal classroom setting. Although the study is reported in one of the papers Krashen refers to (Pienemann, 1989), it is reported more fully in Pienemann (1988), which displays data from spontaneous language samples produced by 3 informants over a period of 1 year (some 30 hours of recorded speech). The study systematically compares the teaching objectives and the classroom input the students received with the language produced by the students themselves. The main finding was that the order of acquisition in students' interlanguage did not coincide with the objectives, the sequence, or the frequency of structures in the input. Instead, their production of German sentences proceeded along the lines predicted by the teachability hypothesis. That is, formal instruction which was not appropriate to learners' developmental stage did not change their spontaneous language performance.

We feel that second language acquisition (SLA) researchers are just beginning to make real progress in understanding how instruction affects the development of linguistic skills and knowledge. Krashen, on the other hand, seems ready to make strong claims about pedagogical implications of his hypotheses. Although we are optimistic that SLA research has the potential to provide insights into the successes and shortcomings of teaching methods, it is our view that we must avoid suggesting that we have found final answers.

We are not in agreement with Krashen that "the effect [of grammar learning] is peripheral and fragile" (p. 409). Nor would we claim that the opposite has been demonstrated for a wide variety of language structures. We argue, however, that in view of the limitations of the existing research, the effect of instruction must be explored further. Declarations about the sufficiency of any single source for successful

language learning (whether it be comprehensible input or the memorisation of grammar rules) are not likely to be borne out in teachers' experience. Statements of absolutes which contradict their observations will make teachers more sceptical of the possible importance of SLA research for their practical concerns. If researchers acknowledge the developmental status of our understanding of SLA and continue to carry out research in cooperation with teachers, we may hope that teachers will still be listening to us as our recommendations about pedagogical practice become more precise and more complete.

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## ***The Effect of Formal Grammar Teaching: Still Peripheral***

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■ Lightbown and Piertemann raise three issues: (a) They argue that the oral test in White, Spada, Lightbown, and Ranta (1991) did not focus students on form, (b) they question my interpretation that children in Lightbown (1991) overlearned (rather than acquired) the *there is* construction, and (c) they accuse me of “dismissing” Pienemann’s research and the teachability hypothesis.

### **A TEST OF ACQUISITION OR LEARNING?**

In my view, the data from White et al. does not tell us whether direct instruction resulted in acquired or learned competence. To show that learned knowledge “becomes” acquired or underlying competence, one must demonstrate an impact on tests that tap only acquired competence.

Lightbown and Pienemann maintain that evidence from Spada and Lightbown (1993) shows that the oral test was indeed a test of acquired competence because children showed “no evidence of hesitation, mental searching, or distraction.”

This apparent fluency is indeed suggestive, and I have utilized similar observations in discussing monitor use (Krashen, 1991). But these observations are just that, observations, and are only suggestive. No comparison was made with the spontaneous speech of these subjects, nor was a comparison made between fluency in asking questions (the target structure) and using different kinds of sentences.

Lightbown and Pienemann also maintain that the focus on form in class had not been excessive. In my view, 9 hours of direct instruction on questions in only 2 weeks is a lot of focusing on form, even if it was less than 20% of the total class time. In addition, after just completing 2 weeks of instruction on questions, and taking other tests that clearly focused the students on questions (White et al., 1991), and being prompted when they failed to produce *wh* questions (“When students did not spontaneously produce a *wh* question after two questions, the interviewer prompted them [e.g., Can you ask me a question with *what?*]” [Spada & Lightbown, 1993, p. 211]), it is reasonable to suggest

that students interpreted the oral test not as a simple guessing game, but as another test on question formation.

I thus hold to my view that the oral test may have satisfied conditions for monitor use (focus on form, know the rule, time).<sup>1</sup>

An interesting finding in Spada and Lightbown (1993) is a long-term effect for direct instruction, extremely rare in second language acquisition literature for any kind of test. In fact, their subjects continued to improve in question formation, albeit at a slower pace, after all English instruction stopped. We need to know if these children had any exposure to English during this time: the *there is/are* issue.

Lightbown (1991) is the only other study I know of that finds a long-term effect for focusing on form. Lightbown and Pienemann respond to my claim that subjects, after so much drill on the *there is* form, “specialized” in it and monitored it all the time by stating that another group had “decontextualized drill” and did not exhibit such sustained accuracy.

But no group had so much work on *there is*. There was so much emphasis placed on *there is* in the class in question that “it had become a kind of joke in the classroom” (Lightbown & Spada, 1990, p. 435). The teacher not only “drummed it into their little heads” but “got so tired of hearing ‘you have a this’ and ‘you have a that’ that *every time* [emphasis added] somebody said ‘you have . . . ’ I said, ‘I do? Where?’” (Lightbown, 1991, p. 207).

Lightbown’s data also bears on the question of whether form focus is necessary. Although the other classes she studied were not as accurate in the *there is* form, Lightbown notes that in these classes, students who were the most accurate were those who were in general the most advanced in English. This suggests that this construction is typically acquired a bit later. In fact, 5 out of 10 high-scoring students one year older appear to have fully acquired it (see her Table 15.2, p. 208); low-scoring sixth graders, however, did not appear to be moving toward mastery of this structure (see also her Table 15.3, p. 210).

## TEACHABILITY

Crucial to Pienemann’s research is his teachability hypothesis. As I understand it, the teachability hypothesis has two parts:

T1: Form-focused instruction will not work if the acquirer is not ready for it, in other words, if it is not at the acquirer’s  $i + 1$ .

T2: Form-focused instruction will work if it is at  $i + 1$ ,

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<sup>1</sup>In my Teaching Issues contribution, I noted that White et al. (1991) do not indicate whether there was any time pressure on the test. Lightbown and Pienemann do not provide any information on this point, nor is it mentioned in Spada & Lightbown (1993).

In my view, Pienemann's studies fully support T1 and confirm earlier research results that also show that direct teaching does not affect the "natural order" of acquisition (see Krashen, 1985, p. 19). The issue is whether there is evidence to support T2.

In Pienemann (1984), the evidence for T2 rests nearly entirely on the case of Giovanni's supposed acquisition of one grammatical structure. Pienemann (1989) adds the performance of three acquirers on the English copula. Two of these subjects show some evidence of backsliding: increases in accuracy after instruction followed by subsequent declines (pp. 61–63). As I stated in my Teaching Issues contribution, Pienemann only provides us with percentages and no raw data: We have no idea how many obligatory occasions of the target structure were produced. Lightbown and Pienemann do not supply any additional details in their response.<sup>2</sup>

Nearly all the data in Pienemann (1988) address (and support) T1. I present here the entire portion of the text that addresses T2: "The second point is that it was possible to add the morphemes *-er* and *-bar* to the interlanguages at the predicted points in time, if the interlanguage was sufficiently developed" (pp. 61-62).

The results of teaching the *-er* and *-bar* morphemes to 12 university students of German as a foreign language are given in Pienemann's Table 10, which does not present any raw data, percentages, or acquisition criteria. It only indicates whether or not the target structure was acquired.

The pattern in Table 10 is consistent with T2, but the data is sparse and open to other interpretations. According to Pienemann, acquisition of the principles underlying verb separation (SEP) and passive needs to take place before it is possible to acquire *-er* and *-bar*, respectively. Of 7 subjects who had acquired SEP, there were only 4 cases in which direct instruction resulted in the apparent mastery of *-er*. In several other cases, *-er* was acquired later, which may or may not have been a result of formal instruction. Only 1 subject acquired the passive, and this subject was the only one who appeared to have mastered *-bar*. Five clearly did not acquire *-bar*, 3 apparently did not produce enough obligatory occasions for it, and in 3 other cases *-bar* was present after instruction, but was not present 3 months later.<sup>3</sup>

Finally, in none of Pienemann's studies does he inform us how the target structures were taught (see also Ellis, 1990, p. 158). This is

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<sup>2</sup> If Pienemann followed the procedure used in Meisel, Clahsen, & Pienemann (1981), there were at least four contexts for each structure. This is a very low figure; one "performance" error can change a subject's score by 25%.

<sup>3</sup> Five out of 12 subjects showed evidence of having mastered verb separation, 4 of 12 inversion, and only 2 out of 12 mastered the passive after direct instruction on these structures. Some subjects acquired verb separation and inversion before instruction.



crucial. When instruction “works,” it may be because target structures are presented as part of an interesting message, as comprehensible input at  $i + 1$ . It is theoretically possible to teach language this way, but it is inefficient, as noted by Ellis (1990). I have argued (e.g., Krashen, 1981) that it is much more efficient to supply large amounts of comprehensible input, both aural and written, and  $i + 1$  will be automatically covered.

I remain unrepentant. In my view, the research says that the effects of direct instruction are typically short lasting and do not become part of acquired competence. The effects of grammar teaching still appear to be peripheral and fragile.

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