A RESPONSE TO McLAUGHLIN, "THE MONITOR MODEL: SOME METHODOLOGICAL CONSIDERATIONS"[1]

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McLaughlin (1978) examines the Monitor Model and presents "a methodological critique of the research on which the Model is based" (p. 309). This paper is a response to that critique, and discusses various misrepresentations and misinterpretations of the work on Monitor Theory.

In the last issue of *Language Learning*, Professor Barry McLaughlin published what he considered to be "a methodological critique of the research on which the (Monitor) Model is based" (p. 309). In this paper, he also offered an alternative model "that more parsimoniously accounts for the data and that ties into a theory of human information processing" (p. 309). This paper is an invited response to McLaughlin's (M's) critique.[2] Because of time and space limitations imposed on me, and also because M's paper contains so many mis-statements, errors, and misinterpretations, this response will be concise. All available space will be devoted to points brought up by M, and in most cases I will be forced only to mention where counter-evidence and supporting arguments are already available in print, rather than discussing these points in detail in the text. I will therefore assume familiarity with previous work on Monitor Theory that has appeared in the published literature, and I will also assume familiarity with M's paper.

This response is divided into two sections. First, I deal with what I consider to be major points of contention—these are, for the most part, attacks on the Monitor Theory which I consider to be fundamentally misguided. A second section deals with "small points and peripheral issues," further problems in M's paper that need to be pointed out but that require less comment.

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[1]Editor's note: This an invited response to an article by Barry McLaughlin (see References) published in the last issue of Language Learning.

[2] I thank the editor of *Language Learning* for inviting me to write this response. I am deeply grateful to Michael Long and Robin Scarcella for their helpful comments. I also thank Prof. McLaughlin for sending me a draft of his paper.

Major Issues

How do you know whether acquisition or learning is being used in a given situation?

M points out (p. 317) that "it is impossible to know whether subjects are actually operating on the basis of 'rule' or 'feel'." An example of this difficulty is M's own intuitions about German:

> "While I 'feel' that something is wrong with *Ich habe nicht das Kind gesehen,* I also know that there is a rule about the placement of negatives. Similarly, while I have to have recourse to the rule to be sure that *Ich habe es ihm gegeben* is correct, I also have a feel that *Ich habe ihm es egeben* is wrong. At least in my own introspection, it is unclear whether I am working on the basis of 'rule' or 'feel'"(p. 317-318).

It may be helpful to make two different kinds of responses to this question. First, it *is* difficult for an observer to know whether acquisition or learning or some combination of both is present in someone's utterance. At this moment, we have no physiological measure that shows an acquisition-learning difference. While such concrete correlates would be useful, their absence does not weaken the acquisition-learning hypothesis or even suggest that this distinction is not real. At the moment, the acquisition-learning distinction is an abstraction that predicts many observable and concrete phenomena. In this way, second language acquisition research is identical to research in cognitive psychology, in which researchers posit an abstract hypothesis and then see if it predicts measurable phenomena. Also, no one expects the abstraction to manifest itself in every situation. When someone walks into a room, and you recognize him, you cannot tell, nor can a researcher tell you, whether you retrieved information about him from iconic, short-term, or long-term memory. This does not invalidate the hypothesis that different stages of memory exist. As is well known, special experimental conditions are necessary to bring differences out. Similarly, our failure to know in every case whether acquisition or learning is present in every utterance is not a problem unless one is opposed to mentalism in any form (see Chomsky 1965:193 for an execellent discussion).

M's observations about himself, on the other hand, seem clear to me if not to him. It seems to me that he has both acquired and learned aspects of German syntax. In the case of the first sentence, *Ich habe nicht das Kind gesehen,* his feeling of ungrammaticality was first based on aquisition; he has apparently fully acquired the rule underlying the correct negative placement. He has also learned the rule but does not need this information. In the second sentence, *Ich habe ihm es gegeben,* which involves a rule that is probably acquired much later, there has been some acquisition, but the object order rule is probably

not fully acquired. Being a language scientist and probably a good Monitor user, M has also learned this rule. Here this extra knowledge is useful, and M does in fact put this knowledge to use to check the output of his acquired competence. Thus, while he claims that in this case "I seem to operate more by rule" (p. 309), he is actually first using acquisition and then referring to the conscious Monitor. In his words:

> "I think that something is wrong with the sentence *Ich habe ihm es gegeben*, but to be sure I have to go back to a rule I have tucked away that tells me that when there are two pronouns in an indirect construction, the accusative precedes the dative" (p. 309).

All this is very consistent with the Monitor Theory.

Child-adult differences

M attempts to restate the Monitor Theory position on child-adult differences as follows:

> "Similarly, child-adult differences . . . can be viewed as resulting from different operating procedures used by learners of different ages. Adults tend to focus more on vacabulary, are often, but not always, more inhibited, tend to use formal rules to a greater extent than is true of children. It should be noted that Krashen seems to assume that children are more successful L2 performers than adults and that conscious application of the Monitor interferes with communication" (p. 326).

M then comments:

> "This runs counter to some strong evidence that adolescents (who are in the stage of formal operations and would be expected to be heavy Monitor users) are superior to children and adults in 'naturalistic' L2 learning (Snow and Hoefnagel-Hohle 1978)" (p. 326).

M later states that he is "not sure" there is a critical period.

First, M has not represented my position with any completeness or clarity. Monitor Theory predicts child-adult differences in the following way (again, I can only be very brief: for more detail, see Krashen 1979a):

(1) Formal operations are hypothesized to be responsible for the birth of the extensive conscious Monitor (granting that children may have some meta-awareness of language, as documented by Hatch 1978a).

(2) Formal operations also have certain affective consequences, which may be aggravated by biological puberty. These affective changes affect our ability to acquire (they strengthen the "affective filter" posited in Dulay and Burt 1977). Acquisition remains, however, the most effective and central means for internalizing language for adults as well as children.

This point of view is quite consistent with the published literature on child-adult differences. The literature does *not* say that adolescents

are superior in second language acquisition or performance. What it does say is that they are *faster* in initial stages. Studies that focus on *eventual attainment* (Oyama 1976, 1978, Seliger, Krashen, and Ladefoged 1974, Asher and Garcia 1969) agree that children are superior to adults. Studies that measure *rate of acquisition* (Snow and Hoefnagel-Hohle 1978, Asher and Price 1967) agree that adults and older children are superior in early stages. I have discussed this briefly elsewhere (Krashen 1977b), noting that the initial superiority of adults and older children/adolescents may be due to the fact that they tend to use the first language as a "substitute utterance initiator" on occasion. Using the L1 (along with the conscious Monitor to "repair" the utterance so that it conforms to the surface structure of the target language) in this way allows older performers to produce sentences right away, without actually acquiring anything, and this can give them a "head start." (See below)

The important point is that M has misinterpreted me, and is not clear on what the child-adult literature has to say. (Also, it is difficult to react to arguments such as "I am not sure there is a critical period." None of us are sure about anything in L2 research. What counts is what we *hypothesize* and whether our evidence is consistent or inconsistent with our hypotheses).

The morpheme studies

M states the Monitor Theory explanation of variation in the morpheme studies correctly on page 327. He then describes the argument as "circular." If by this he means that I have allowed new data to alter my research hypotheses, then the argument is circular—the circle goes from data to hypothesis to new data and back to new hypotheses. This is called progress. In this particular case, our data has led us to hypothesize that Monitor use is more limited than we originally thought. Fuller's (1978) dissertation, along with the Houck, Robertson, and Krashen (1978) study, was the new data that forced some rethinking. Very briefly, I had hypothesized at first that the only requirement for Monitor use was *time:* Larsen-Freeman's subjects (Larsen-Freeman 1975) produced unnatural orders, I hypothesized, because they were given pencil and paper tests and thus had more time to access the conscious grammar. Dulay and Burt (Personal communication) insisted that this was not sufficient—a more important condition for Monitor use is that the subject be "focussed on form," or concerned with correctness. Even with time, many subjects will not Monitor because they are interested in what they are saying, not how they are saying it. The results of our composition study supported this

notion (Krashen, Butler, Birnbaum, and Robertson 1978). We found natural orders even when subjects had all the time they needed. One interpretation of this was that our subjects were less concerned with form and more concerned with communication. In the Houck et al. study, we found natural orders despite the fact that the task asked subjects to correct their own written output. We suggested then that performers might need an extreme discrete-point test to focus them on form.

Fuller (1978) found natural order for both oral and written SLOPE test scores, which confirms this. Despite M's comments on page 327, the SLOPE test is not an extreme discrete-point test: In such a test, subjects would be focussed on just one item or rule at a time. This is not all the case with the SLOPE test, as subjects are asked to fill out a sentence with a complete phrase, in response to a picture. Larsen-Freeman's reading and writing tasks, which are essentially multiple-choice or fill-in type, are much closer to being discrete-point which is consistent with our hypothesis that it takes a real discrete point test to bring out the conscious grammar in force.

M points out that Fuller's (1978) results differ from what is reported in Krashen, Sferlazza, Feldman, and Fatham (1976). In this study, we ran an oral and a written SLOPE on adult second language acquirers; the former condition produced a clear natural order, agreeing with Fatham's (1975) child second language order. The written version, described as a pilot, could not be analyzed due to profound ceiling effects: Subjects simply performed too well and were bunched near the top. We did note that the III singular morpheme was higher in rank (8/20) than in the oral version, which is consistent with the hypothesis that the Monitor was invoked. Keyfetz-Fuller did not find a huge III singular jump in rank from oral to written conditions, but it did increase in rank somewhat (three ranks out of 20) and in overall accuracy (about 10%). This was not enough to disturb the natural order. In any case, our pilot was admittedly inconclusive. Since Keyfetz-Fuller's written study did not suffer from ceiling effects, and since it was in all respects a superior and more carefully designed study than ours was, I base my current hypotheses on her data. Again, I do reserve the right to change my hypotheses in light of new data.

Is the Monitor Model restricted to intermediate stages?

M, citing an unpublished conference paper (Krashen 1975), comments that I restrict the Monitor Model to intermediate stages (P. 318). This deserves some explanation. Production using the *Monitor Model,* represented in figure one, does indeed have to wait until the performer

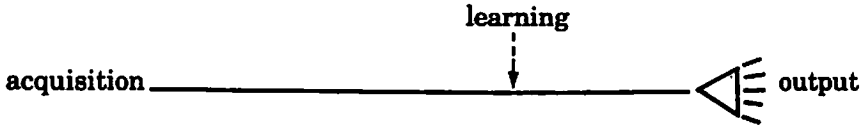

Figure 1. The Monitor Model for Adult Second Language Performance.

has acquired enough of the target language in order to use what he has acquired to initiate utterances. If he or she is called on to produce complete sentences before enough has been acquired, the *theory* predicts he will "fall back" on his first language (Newmark 1966, Krashen 1977b). The performer can also apply the Monitor to his output, in an attempt to make it conform closer to his conscious view of what the surface structure of L1 looks like. This is performance without acquired competence. (The child L2 acquirer often substitutes a "silent period" for this "stage").

While production using the performance model in figure one may have to wait, then, until some acquisition has taken place via input, pseudo-language output via, L1 surface structure and some Monitor use can take place (again, the supporting data is in Krashen 1977b). *Monitor Theory*, which is a set of hypotheses (ie the acquisition-learning hypothesis, the natural order hypothesis, etc. see appendix), does not have to wait for intermediate stages, but has implications for all stages of second language acquisition.

This should handle M's question on page 329:

> "Researchers have indeed found more interference in classroom than in naturalistic settings . . . it is not clear to me why the Monitor Theory predicts this . . .".

Monitor Theory predicts more "interference" (or better: "first language influence") in situations where less acquisition has taken place, which is unfortunately usually the case in the classroom (it need not be: see e.g. Terrell 1977, M. Long 1976, Krashen 1979b), when instruction focusses on conscious rules.

M's very next statements make no sense to me:

> "Why is it that the use of the Monitor leads to interference? Cannot interference occur in the acquisition process as well? The theory says nothing about this and consequently all attempts to explain interference phenomena in these terms is *ad hoc*" (p. 329).

Nowhere is it claimed that use of the Monitor leads to interference (although it may be the case that we see more use of the first language where there is less "acquisition," and this may occur in classes where the focus of attention is on conscious rules). "Interference" is hypothesized to be falling back on the L1, exactly as Newmark (1966)

claimed. In order to criticize this position, one would need to find fault with the supporting evidence, produce counter-evidence, and/or make a better generalization that is more consistent with the data. M does none of these things.

Need conscious learning come first?

M's introspections tell him that

> "We initially approach complex tasks, such as learning a second language or tennis, deliberately and consciously" (p. 318).

Elsewhere he states:

> "One conciously and deliberately learns the rudiments and then learns more complicated motor patterns as performance improves. Krashen's contention, that acquisition is central and learning periperal . . . does not seem to correspond to experience" (p. 326-327).

This seems to be a version of the hypothesis that "learning becomes acquisition," the principle on which the cognitive code method is based. There are good reasons for questioning this view, which admittedly seems reasonable to a lot of people. These reasons are given in detail in Krashen (1979c), but I list them here briefly:

(1) We often see acquisition in cases where learning never occurred, that is, there are many performers who can perform complex structures in a second language who do not know and never did "know the rule" consciously in any sense (Krashen 1978, Stafford and Covitt 1978, Kounin and Krashen 1979). Unless all these instances are due to L1 use or to routines and patterns, this shows that previous learning is not necessary for acquisition.

(2) We also see learning that never seems to become acquisition. Many fine ESL performers, while they have acquired a great deal of English, may also know many conscious rules. They nevertheless make "careless" errors on simple, but late-acquired rules, such as the third person singular ending on regular verbs in the present tense. "P", described in Krashen and Pon (1975), made errors in casual speech on easy rules that she had known consciously for 20 years. P was an excellent Monitor user, but she had not acquired some simple (to learn) rules that are typically acquired very late. Thus, learning does not necessarily become acquisition, even with easy rules that are learned very well.

(3) Even the best learners master only a small sub-set of the rules of a language.

My explanation for these phenomena (in Krashen 1979c) is that while learning may often precede acquisition, it need not, and in fact may not even help. Rather, we acquire along a fairly predictable

natural order, and this occurs when we receive comprehensible input. Occasionally, we learn certain rules before we acquire them, and this gives us the illusion that the learning actually caused the acquisition.

The main point I wish to make here is that I present a hypothesis with evidence. M does not present any real counter-evidence other than his own intuition, a practice he faults me for (See below).[3,4]

Interestingly, in the same paragraph in which M asserts that complex tasks are first approached with conscious rules, M changes his position (or presents an alternative):

> "It may be, however, that they (second language acquirers) initially work with the L1 and the rules of the L2, as Krashen elsewhere . . . seems to imply" (p. 318).

This possibility was discussed earlier in this paper. I wish only to point out here that this is in fact my position, but it is by no means a claim that this is the universal pattern. Some performers fall back on the first language some of the time, for some aspects of grammar, and some generalizations can be made for when this is most likely to happen, as discussed above and in Krashen (1977b).

The nature of supporting evidence

My view of scientific method is simple. We look for generalizations, abstractions, that predict real world phenomena. We can arrive at these generalizations any way we like (intuitions, data, etc.), but

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[3] M does cite Duškova (1969) in support of his position:

"Many recurrent errors reflect no real deficits in knowledge, since most learners know the pertinent rule and can readily apply it, but the mechanical application does not work automaticaly." (Duškova 1969:16).

This quote is meant to support the hypothesis that controlled processes need to precede automatic processes. At the risk of restatement (see page 9 of this paper), the Monitor Theory interpretation of this statement is very clear. Duškova's English as a foreign language students had learned certain rules but had not acquired them, and thus could only perform them correctly when there was time and when their attention was called to the rule in question. This sort of thing tends to happen most typically with late-acquired, simpler morphology, which I have hypothesized (Krashen et al. 1978) is the usual domain of the conscious Monitor, and Duškova's data supports this.

[4] M explains individual variation in Monitor use within his model as follows:

"Rather than saying that some individuals are optimal, some under- and some over-users of the Monitor, one could say that people vary in the extent to which they use controlled processes in L2 learning" (p. 326).

This interpretation implies that under-users (or anyone with less than a total conscious grasp of the L2) will never acquire significant portions of an L2, since M's position is that controlled processes are a necessary first step in gaining "automatic control," and "we initially approach complex tasks, such as learning a second language, deliberately and consciously" (p. 16). This prediction appears to be false (Stafford and Covitt 1978, Kounin and Krashen 1978, Krashen 1978): see also point (1), page 157 of this paper.

our generalizations need to be able to predict. They are not merely categories for previously existing data, but must fit data gathered after the hypotheses were formulated. The way we test our generalizations, then, is to see whether they predict new data. If they do, we are still in business, but if they do not, we have to change the hypothesis, alter it. If these alterations cause major changes in the fundamental assumptions in the original generalizations, make it too *ad hoc*, too cumbersome, we may have to abandon the hypothesis.

Note that according to this approach, we can never prove anthing, we can only look for supporting eivdence. When we fail to find supporting evidence, or when we find counter-evidence, we are in trouble. Even when we do find supporting evidence, when the hypothesis makes correct predictions, a critic can always say that we have not found "enough." Thus, a scientist, professionally speaking, is never able to *believe* anything. All he or she can do is have a current hypothesis that he or she is interested in testing.

At the moment, the hypotheses of the Monitor Theory appear to me to be consistent enough with the existing data to deserve consideration. This is what I mean when I state my "current position."

A central hypothesis of the Monitor Theory is that conscious learning is not available for initiating utterances, but is only available as a Monitor. This hypothesis is consistent with a great deal of data; it predicts variation in accuracy order in grammatical morpheme studies, is consistent with results of research in language aptitude (Krashen 1979a), predicts phenomena related to individual variation (Krashen 1978), L1 influence (Krashen 1977b), etc. Oddly, M claims that ". . . he does not provide evidence" in support of this idea (p. 318). If M means that there is not enough evidence to satisfy him, fair enough. There has been enough evidence *for me* to accept this hypothesis as one worth further investigation (there will never be enough for *belief*, speaking as an investigator). Hopefully, future research will provide even more evidence. If M means I have not provided *proof*, he will always be able to make this accusation. If M means that there is a better model, he must show how this alternative handles the same data the Monitor Theory does and can solve problems the Monitor Theory cannot. This he has not done. If he truly thinks I have not provided evidence, it seems to me that we are working with such different ideas as to how progress is achieved in science that real communication between us is impossible.

Another criticism is that Monitor Theory only "restates" known phenomena, e.g.:

> "What Krashen has done is simply to show that one can talk about certain phenomena in terms of the acquisition-learning distinction. There may, however,

> be other ways of talking about these phenomena that are equally valid and more parsimonious" (p. 325).

Also, in discussing first language influence:

> "Although Krashen regards certain findings concerning interference as 'evidence' for the Monitor Model . . . it does not seem to me as much evidence as restatements of known phenomena in the terminology of the model" (p. 329).

These statements deserve some comment. It is not the case that the Monitor Theory simply restates certain phenomena. What it has done is to state testable hypotheses concerning a wide range of phenomena in terms of a general theory. Most important, these hypotheses are supported by empirical data. Respectable criticism should produce counter-evidence, and/or propose other generalizations, and give the evidence in support of the alternatives that the original hypothesis cannot handle. This has not been done in this case.

Also related to his point is this complaint:

> "I believe that the Monitor Model does have a basis in subjective experience, but I do not believe that subjective experience should be the testing ground for a theory of language acquisition processing" (p. 329).

I agree that subjective experience should not be *the* testing ground for a theory of language acquisition or processing. However:

1) Intuitions and feelings are very valuable in helping one arrive at hypotheses.

2) It is certainly not counter-evidence when research results lead to hypotheses that are consistent with one's intuitions.

3) The Monitor Theory and the Monitor Model for performance are based on far more than subjective experience.

4) M makes liberal use of the subjective experience and his personal beliefs in supporting his position, with a minimum of empirical evidence. Here is a sampler:

> "Incidentally, *I believe* that schemata develop all levels of linguistic functioning—the semantic, the phonological, pragmatic, etc." (p. 321).
>
> *"I believe* that the language learner possesses certain discovery procedures that are used to work on input and generate schemata" (p. 321).
>
> *"Introspectively at least, it seems* that we initially approach complex tasks, such as learning a second language, deliberately and consciously" (p. 318).
>
> (emphases mine)

The fact that M is relying on his subjective experience and intuitions is not the problem. The problem is that he is relying on little else. If substantial empirical evidence is available to support his views, he does not provide it.

I hope my position is clear: Intuitions about how second languages are acquired and used do not supply crucial evidence for the validity of our theories. They are, nevertheless, very valuable for arriving at our

hypotheses. I am therefore very interested in introspective reports, diary studies, etc. and am constantly examining my own experiences as a language acquirer.

Smaller Points and More Peripheral Issues

In this section, I list errors of interpretation, inaccurate restatements of my position, and my objections to certain rhetorical techniques used by M. I follow the order in which these points appeared.

On page 311, M claims that statements on the relationship between "Monitoring" and "monitoring" are "contradictory." M does not tell the reader which statements these are, a technique which leaves a responder with no defense. The idea of using a small "m" to refer to general awareness of language, self-correcting using conscious or subconscious knowledge, while reserving the captial "M" to specifically the use of the conscious grammar, belongs to Earl Stevick. It has not, to my knowledge, appeared in print, but it is a useful distinction. All I can do is repeat that in several papers we point out that the use of conscious rules for editing is not the only means available for self-correction; performers can use acquisition as well—in fact, most native speakers' repairs of slips of the tongue are probably done this way. The central hypothesis of the Monitor Theory is that *learning* has only this function: it cannot be used as an utterance-initiator. The Monitor Theory does not postulate that acquisition cannot be used for self-correction. But this has been said before. It is made very explicit in Stevick's recent commentary and expansion of the Monitor Model for performance (Stevick, in press).

On page 313, M attributes a hypothesis to me which I never held in print, in ditto, in class, or even in conversation: The reason morphemes such as III singular and regular past are more difficult in Monitor-free situations (or the reason they are late acquired?), and the reason they rise in rank under Monitored conditions is that "these rules are redundant and therefore unnecessary for communication." This may be the case, and I have heard this suggestion from many people over the years. I myself have taken no position on why things are ordered the way they are, nor does Monitor Theory make any predictions, other than to try to account for why accuracy orders vary under certain conditions. There is a great deal of excellent work in this area (Slobin 1973, Ervin-Tripp 1973, Clark and Clark 1973, Dulay and Burt 1975, Wode 1977, Hatch and Wagner-Gough 1976), but I have not contributed to it, other than to hypothesize *what* the natural order for grammatical morphemes might be for second language. The "affective filter," which M refers to on page 314, is incorrectly attributed to me. I

would like to take credit for this idea, which I think is very interesting and insightful. It comes, however, from Dulay and Burt (1977), and I have been careful to cite this source whenever I refer to the filter, (see, for example, Krashen 1979a, 1979b, 1979c). Also on page 314, the statistical independence of attitude and aptitude are asserted to "follow from" the Monitor Model. I never claimed this. The statistical independence of attitude and aptitude was established long before I arrived on the scene (Gardner and Lambert 1972). This fact, however, is *consistent with* the acquisition-learning hypothesis and the hypothesis that attitude is more directly related to acquisition and aptitude more directly related to learning. It should also be noted that Krashen (no date-b), cited in M's paper, was a dittoed working paper distributed among students at USC. The main points of this paper are stated in Krashen (1977b), *not* 1977a, as stated by M on page 315. While I do not retract anything that is in that paper, it should be pointed out that this unpublished (and unsubmitted) paper was cited by M without my permission. The practice of citing unpublished working papers discourages scholars from exchanging ideas.

On page 317, M notes, "when teaching is directed at elucidating what the student has already acquired, the result, according to Krashen, may be a very gratifying 'Eureka' experience on the part of the student." This is, in fact, a correct restatement of what is expressed in part of Krashen (1979a). It should be pointed out, however, that this is not what I consider "good teaching" (nor does M imply that this is my position). One of the most interesting, and from one point of view, the most important area of work involving Monitor Theory, is the question of application, which is dealt with to some extent in Krashen (1979a, 1979b). Very briefly, I think that Monitor Theory is consistent with conclusions reached by many teachers over the last few years: the classroom is a place to give students the input they need for language acquisition via communicative activities that the students are interested in, in which the anxiety level is low, and that involve language the students can understand and use for further language acquisition. The outside world may not provide such input for adults. This is a vast over-simplification of my position, which rests heavily on previous work by Stevick (1975) and Hatch and her colleagues, (for example, Wagner-Gough and Hatch 1975). The use of the classroom to provide "learning" where acquisition already exists is "language appreciation" and quite peripheral to the main function of the classroom, in my view.

M repeats the frequently discussed notion that second language performance is more variable than first language performance on page 323: This is due to the observation, which is correct, that order of

acquisition studies in areas such as grammatical morphemes, questions, and negation seem to show more variation in second language. This may be true, but it should also be considered that in these areas there has simply been more done in second language; there are what Hakuta (1976) refers to as a "parade" of second language morpheme studies, while in first language, we refer only to Brown's 1973 longitudinal study and de Villiers and de Villiers' (1973) cross-sectional study for normal development (it should also be noted that the variation in second language morpheme development is much less than has been claimed, according to my analyses in Krashen 1977b). For negation and questions, most second language studies cite only Klima and Bellugi (1966) as their touchstone. Here again, there have been many second language studies. More first language studies might produce more variation.

M's comment on the importance of vocabulary (p. 324) contains another interesting mis-citation. The cite is of Hatch (1978b), and the page number, for those interested, is page 430. Here are her words, "... it seems clear that vocabulary is an important concern of second language learners ... Krashen too has commented that such a finding is not surprising for, after all, adults carry around dictionaries, not grammar books (when learning languages)." M attributes this insight to Hatch herself! Actually, Hatch heard this from me at a USC-UCLA second language meeting, and I was repeating what one of my students had pointed out to me. Unfortunately, I do not remember which student said it, and I hope he or she forgives me for failing to give him or her full credit for this interesting observation. More important, both Hatch's and my position imply clearly that vocabulary acquisition is very important. In the "old days," we said that vocabulary should be de-emphasized in order to concentrate on syntax. My current position (Krashen 1979b) is that more vocabulary means more comprehension of input, which in turn means more acquisition of syntax. I have no official position on how vocabulary can best be acquired.

I have mentioned M's model only in passing. It is difficult to discuss in detail because his hypotheses are not stated explicitly enough to allow real comment. There is, for example, ample evidence that the processes (discovery procedures) he lists, such as simplification, avoidance, etc. do exist, but it is difficult for me to see how they function as part of his acquisition/performance model. (These discovery procedures can easily be "restated" in terms of the Monitor Theory in more precise ways; see, for example, Krashen and Scarcella (1978) for comments on imitation). I will, therefore, await more specific statements of M's hypotheses and their supporting evidence rather than risk mis-stating his position.

APPENDIX—SOME HYPOTHESES

1. The acquisition-learning hypothesis: Adults have two means of internalizing "rule" in a second language. *Acquisition* is subconscious and is similar to the way children acquire first and second languages. *Learning* is conscious. It is "knowing about" language, or "formal" knowledge of a language. Other scholars have used different terminology to express this dichotomy:

acquisition	*learning*
implicit knowledge	explicit knowledge (Bialystok & Frohlich, 1977)
mechanisms that guide "automatic" performance	mechanisms that guide (Lawler & Selinker, 1971) puzzle- or problem-solving performance
expression rules	reference rules (Widdowson, 1978)

2. The natural order hypotheis: Adults acquire (not learn) grammatical structures in a predictable order. (This does not imply strict invariance, does not preclude the possiblity that some structures are acquired in "blocks," and does not necessarily imply that second language order is identical to the order of acquisition for first language, although there are some similarities.
3. The Monitor hypothesis: Conscious learning is available only as a Monitor. Monitor use is possible only when the performer has enough time, and it can only happen when the performer is concerned with form. This hypothesis does not predict that all self-corection behavior comes from the conscious grammar. It does imply that our fluency in second languages in due to what we have acquired.
4. The imput hypothesis: Language acquisition (not learning) occurs when the acquirer understands input language. If an acquirer is at stage i, he or she can move to state $i + 1$ by understanding input at the $i + 1$ level (with the aid of context or extralinguisitic information). Speaking aids language acquisition indirectly, by encouraging understandable input. (Understanding input is a necessary but not a sufficient condition; see hypothesis 5 below).
5. The attitude-acquisition hypothesis: Attitudinal variables such as motivation type and personality characteristics, shown to be related to success in second language acquisition, relate primarily to acquisition and not to learning. Their presence encourages a low "affective filter" (Dulay and Burt 1977). Positive attitudes encourage the acquirer to get more input and "allow it in." Directly related to this is Stevick's concept of "depth" (Stevick 1975). (This does not mean that all personality characteristics are related to acquisition;

some, such as "attitude toward the classroom and teacher" and the presence of an "analytic personality," may relate to learning).

6. The aptitude-learning hypothesis: Two of three of Carroll's components of aptitude (grammatical sensitivity and inductive ability) relate directly to learning, not acquisition (Carroll 1973)
7. The L1 hypothesis: The first language can be used as a substitute utterance initiator, substituting for acquired L2 competence, when the performer needs to produce a structure but has not yet acquired it.

Empirical data supporting these hypotheses can be found in Krashen 1976, 1977a, 1977b, 1978, 1979a, 1979b, 1979c.

REFERENCES

Asher, J. and Garcia, R. 1969. The optimal age to learn a foreign language. Modern Language Journal 8:334-341.

Asher, J. and Price, B. 1967. The learning strategy of total physical response: Some age differences. Child Development 38:1219-1227.

Bialystok, E. and Frohlich, M. 1977. Aspects of second language learning in classroom settings. Working Papers on Bilingualism 15:1-26.

Brown, R. 1973. A First Language. Cambridge, Mass.: Harvard Press.

Carroll, J. 1973. Implications of aptitude test research and psycholinguistic theory for foreign-language teaching. Linguistics 112:5-13.

Chomsky, N. 1965. Aspects of the Theory of Syntax. Cambridge, Mass.: MIT Press.

Clark, H. and Clark, E. 1977. Psychology and Language. New York: Harcourt, Brace, Jovanovich.

de Villiers, J. and de Villiers, P. 1973. A cross-sectional study of the acquisition of grammatical morphemes in child speech. Journal of Psycholinguistic Research 2:267-278.

Dulay, H. and Burt, M. 1975. A new approach to discovering universal strategies of child language acquisition. In Dato, D. (ed.) Developmental Psycholinguistics: Theory and Applications. Georgetown University School of Languages and Linguistics. pp. 209-233.

Dulay, H. and Burt, M. 1977. Remarks on creativity in language acquisition. In Burt, M., Dulay, H., and Finocchiaro, M. (eds.) Viewpoints on English as a Second Language. New York: Regents. pp. 95-126.

Duškova, L. 1969. On sources of error in foreign language learning. International Review of Applied Linguistics 4:11-36.

Ervin-Tripp, S. 1973. Some strategies for the first two years. In Dil, A. (ed.) Language Acquisition and Communicative Choice. Stanford: University Press. pp. 204-238.

Fatham, A. 1975. Language background, age, and the order of acquisition of English structures. In Burt, M. and Dulay, H. (eds.) New Directions in Second Language Learning, Teaching, and Bilingual Education: On TESOL '75. Washington: TESOL. pp. 33-43.

Gardner, R. and Lambert, W. 1972. Attitude and Motivation in Second-Language Learning. Rowley, Mass.: Newbury House.

Hakuta, K. 1976. A case study of Japanese child learning English as a second language. Language Learning 26:321-351.

Hatch, E. 1978a. Second language Acquisition. Rowley, Mass.: Newbury House (Introduction, pp. 1-18).

Hatch, E. 1978b. Discourse analysis and second language acquisition. In Hatch, E. (ed.) Second Language Acquisition. Rowley, Mass.: Newbury House. pp. 401-435.

Hatch, E. and Wagner-Gough, J. 1976. Explaining sequence and variation in second language acquisition. In Brown, H. D. (ed.) Papers in Second Language Acquisition. Special Issue #4 of Language Learning. pp. 39-58.

Houck, N., Robertson, J., and Krashen, S. 1978. On the domain of the conscious grammar: Morpheme orders for corrected and uncorrected ESL student transcriptions. TESLO Quarterly 12:335-339.

Fuller, Janet Keyfetz. 1978. An Investigation of Natural and Monitored Morpheme Difficulty Orders by Non-native Adult Students of English. Ph.D. dissertation, Florida State University.

Kounin, T. and Krashen, S. 1978. Approaching native speaker proficiency from two different directions. In Blatchford, C. and Schachter, J. (eds.) On TESOL '78: EFL Policies, Programs, Practices. Washington: TESOL. pp. 205-212.

Krashen, S. 1975. A model of adult second language performance. Paper presented at the Linguistic Society of America, December, 1975, San Francisco.

Krashen, S. 1976. Formal and informal linguistic environments in language learning and language acquisition. TESOL Quarterly 10:157-168.

Krashen, S. 1977a. The Monitor Model for adult second language performance. In Burt, M., Dulay, H. and Finocchiaro, M. (eds.) Viewpoints on English as a Second Language. New York: Regents. pp. 152-161.

Krashen, S. 1977b. Some issues relating to the Monitor Model. In Brown, H. D., Yorio, C. and Crymes, R. (eds.) Teaching and Learning English as a Second Language: On TESOL. '77. Washington: TESOL. pp. 144–158.

Krashen, S. 1978. Individual variation in the use of the Monitor. In Ritchie, W. (ed.) Principles of Second Language Learning. New York: Academic Press. pp. 175-183.

Krashen, S. 1979a. Attitude and aptitude in relation to second language acquisition and learning. In Diller, K. (ed.) Individual Differences and Universals in Language Learning Aptitude. Rowley, Mass.: Newbury House.

Krashen, S. 1979b. Relating theory and practice in adult second language acquisition. In Felix, S. (ed.) Recent Trends in Research on Second Language Acquisition. Tubingen: TBL Gunter Narr.

Krashen, S. 1979c. Adult second language acquisition and learning: A review of theory and applications. In Gingras, R. (ed.) Second Language Acquisition and Foreign Language Teaching. Arlington, Virginia: Center for Applied Linguistics.

Krashen, S. and Pon, P. 1975. An error analysis of an advanced ESL learner: The importance of the Monitor. Working Papers on Bilingualism 7:125-129.

Krashen, S., Sferlazza, V., Feldman, L., and Fatham, A. 1976. Adult performance on the SLOPE test: More evidence for a natural order in adult second language acquisition. Language Learning 26:145-151.

Krashen, S., Butler, J., Birnbuam, R., and Robertson, J. 1978. Two studies in language acquisition and language learning. ITL: Review of Applied Linguistics. 39–40. 73-92.

Krashen, S. and Scarcella, R. 1978. On routines and patterns in language acquisition and performance. Language Learning 28:283-300.

Larsen-Freeman, D. 1975. The acquisition of grammatical morphemes by adult ESL students. TESOL Quarterly 9:409-419.

Lawler, J. and Selinker, L. 1971. On paradoxes, rules, and research in second language learning. Language Learning 21:27-43.

Long, M. 1976. Encouraging language acquisition by adults in a formal instructional setting. ELT Documents 3.

Long, M. 1978. Towards a neurolinguistic explanation for the effect of learner age on secondary language acquisition. Unpublished Paper, UCLA TESL Department.

McLaughlin, B. 1978. The Monitor Model: Some methodological considerations. Language Learning 28:309-332.

Newmark, L. 1966. How not to interfere with language learning. In: Language Learning: The Individual and the Process. International Journal of American Linguistics 40:77-83.

Oyama, S. 1976. A sensitive period for the acquisition of a non-native phonological system. Journal of Psycholinguistic Research 5:261-285.

Oyama, S. 1978. The sensitive period and comprehension of speech. Working Papers on Bilingualism 16:1-17.

Seliger, H., Krashen, S., and Ladefoged, P. 1975. Maturational constraints in the acquisition of second language accent. Language Sciences 36:20-22.

Slobin, D. 1973. Cognitive prerequisites for the development of grammar. In Ferguson, C. and Slobin, D. (eds.) Studies of Child Language Development. New York: Holt, Rinehart, and Winston. pp. 175-208.

Snow, C. and Hoefnagel-Hohle, M. 1978. Age differences in second-language acquisition. In Hatch, E. (ed.) Second language Acquisition. Rowley, Mass.: Newbury House. pp. 333-344.

Stafford, C. and Covitt, G. 1978. Monitor use in adult second language production. ITL: Review of Applied Linguistics 39-40. 103-125.

Stevick, E. 1975. Memory, Meaning, and Method. Rowley, Mass.: Newbury House.

Stevick, E. The Levertov Machine. In Scarcella, R. and Krashen, S. (eds.) Research in Second Language Acquisition. Rowley, Mass.: Newbury House. In Press.

Terrell, T. 1977. A natural approach to second language acquisition and learning. Modern Language Journal LXI. 325-337.

Wagner-Gough, J. and Hatch, E. 1975. The importance of input data in second language acquisition studies. Language Learning 25:297-308.

Widdowson, H. 1978. The significance of simplification. Studies in Second Language Acquisition 1:11-20.

Wode, H. 1977. Free versus bound forms in three types of language acquisition. Arbeitspapiere zum Spracherwerb No. 19. Englisches Seminar der Universität Kiel.