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ON PARADOXES, RULES, AND RESEARCH IN
SECOND-LANGUAGE LEARNING¹

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After a detailed consideration of Saporta's (1966) "paradox of second-language learning" as a point of departure, the nature of paradoxes in general is discussed, particularly the conclusions they force upon a researcher. The nature of Saporta's paradox is then dealt with in terms of the linguistic aspects of second-language learning. This leads to the proposal of a research strategy involving profiles of idealized learners with specific types of individual differences. Detailed suggestions are provided for experiments relevant to the way *individuals* actually learn second languages.

1. Introduction

What is the exact relationship between linguistics and applied linguistics? If, for instance, one limits one's concern to the relationship between generative theory and the solution of practical language-related problems, one quickly sees a variety of opinions and views. Richards (1967-68), for example, critiques Chomsky for not turning his "critical discernment so brilliantly exercised . . . to more pressing linguistic tasks," i.e. to the solution of practical problems. Richards in the title to his article makes it clear that, in his opinion, generative grammar *does not* help in the solution to such problems. Chomsky would seem to agree with this latter statement, but would go on to claim that linguistics (and psychology, for that matter) *cannot* help in the solution to such problems because:

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it is difficult to believe that either linguistics or psychology has achieved a level of theoretical understanding that might enable it to support a 'technology' of language teaching. (Chomsky 1966a: 43)

Actually, Chomsky's position is more complex, as is evidenced by the following:

The applied psychologist and the teacher must certainly draw what suggestions and hints they can from psychological research, but they would be well-advised to do so with the constant realization of how fragile and tentative are the principles of the underlying discipline. (1966a:44)

Chomsky makes it clear that this view holds for linguistic research as well. On the other hand, Thomas (1965) states rather arbitrarily and without presenting any evidence that:

it seems apparent that any pedagogical grammar must be based on the best available scientific grammar. (1965:5)

In this regard, Thomas has written a book that recommends the direct teaching of linguistic rules.

No matter what the conclusion and no matter how well argued, this sort of discussion has generally failed to prove convincing, because considerations of teaching are generally not kept distinct from considerations of learning (Selinker, forthcoming) and because so little is known about variables related to *learners* of second languages. This latter sort of knowledge, to be gained only through empirical research, is absolutely essential, we would argue, before one can proceed to an understanding of the relationships described above. Saporta is quite right when he says:

Once we have all declared ourselves in favor of better grammars, however, there is still a need for a sober statement of precisely what the unique contribution of linguistics is to second-language learning, and how this contribution combines with a realistic learning theory to yield useful pedagogical grammars. (1965:551)

It is our view that such a "sober statement" will not be forthcoming until much more is discovered about second-language learners, especially as to how they learn the linguistic aspects of second languages. To date, these discoveries have defied the direct application to second-language learning—data of theoretical approaches to linguistics and psychology (cf. Kline 1970). It seems clear then that a new empirical (especially, experimental) approach to the study of second-language learning is called for—an approach which

would concern itself with the psychological processes of such learning and would attempt to illuminate them. It also seems that the ideal place to begin our quest is in the same article by Saporta where he presents the "paradox of second-language learning":

Language is rule-governed behavior, and learning a language involves internalizing the rules. But the ability or inclination to formulate the rules apparently interferes with the performance which is supposed to lead to making the application of the rules automatic. (1965:548)³

Although to our knowledge, nothing to date has appeared in print concerning this paradox, we know that a number of people have expressed interest in it. The present article attempts to shed some light on the issues raised in the paradox, (1) by considering the nature of paradoxes in general, particularly the conclusions they force upon a researcher; (2) by considering the nature of this particular paradox in terms of the researcher in the psychology of second-language learning; and (3) by considering in respect to (1), (2), and some other relevant variables, a strategy for research into the processes of second-language learning.

2. Paradoxes and Research

It is obvious that a paradox, when discovered in a scientific context, shows that something is awry with our understanding of the subject. If it occurs in a mathematical context, it shows an inconsistency in the theory. A famous example of this type of paradox is Russell's "class of all classes" paradox; Russell demonstrated that this paradox arose from the basic principles of the set theory forming the basis for the mathematical theories of Frege. A paradox, however, may occur in the context of empirical science; a consistent theory may simply not represent reality sufficiently well, and may generate results at odds with experience. This is surely the case with Zeno's famous paradoxes where the results of the analysis of distance and motion, for example, are not in accord with our intuition and experience in similar situations. We believe that this is also the case with Saporta's paradox, occurring as it does in the empirical realm of linguistics.

However, whether or not the theory concerned is consistent, if it can be made to generate a paradox it must be scrapped, for the unstated but obvious reason that we assume all phenomena to be consistent and explainable, and a theory which does not do both is,

³The reader should be aware that Saporta (personal communication) has recently denied any investment in the ideas expressed in the two references cited here (Saporta 1965, 1966).

in essence, trivial. One is then left with the conclusion that a paradox is in some sense a *reductio ad absurdum*, wherein some premise of a theory is shown to be either logically inconsistent (as in the case of a mathematical paradox), or literally untrue (as in the case of a data-contradicting paradox), by demonstrating that it leads to either logical or real contradictions.

Since we have concluded that a paradox has no real-world existence, but only a theoretical one, we need to investigate the conclusions that are forced upon a theoretician who has discovered a paradox. We are concerned here only with the data-contradicting type, since Saporta's statement does not deal with the internal consistency of the theory, but with observable facts. What decisions must a researcher reach regarding the tenability of a theory on which a paradox is based? On reflection, it turns out that three (not necessarily mutually exclusive) conclusions are possible:

- 1) The principles or theory which form the groundwork for the reasoning leading to the paradox are false; or
- 2) The train of reasoning leading to the paradox is fallacious, although the postulates underlying it may still be sound; or
- 3) There is no paradox at all, and the conclusions reached in the theory are in fact true; the observations of the phenomenon, then, must be in error.

Saporta, in discerning a "paradox" in second-language learning, apparently feels that if we accept his premises and follow his reasoning, we are led to the conclusion that it should be impossible to learn a second language. He has then left us to draw one or more of the following specific inferences:

- 1a) Language is *not* "rule-governed behavior," or learning a language does *not* involve "internalizing the rules";⁴ or
- 2a) "The ability or inclination to formulate the rules" does *not*, in fact, "interfere with the performance which is supposed to lead to making the application of the rules automatic"; or
- 3a) The premise and the reasoning are both valid, and it is therefore apparently impossible to learn a second language.

We must immediately reject (3a), of course. The undeniable fact is that it *is* possible to learn a second language, for some to the

⁴Les Dickinson (personal communication) has pointed out that between the first and second sentences of the original paradox, there should appear the following suppressed premise: "The process of internalizing the rules, in a second-language learning situation, necessarily involves first formulating them" since the notion of formulating, i.e. making the rules explicit, does not occur in the first sentence of the original paradox.

level of native-speaker competence, and for most at least to the level of "communicative competence" (Jakobovits 1970). It would appear, then, that there is a true paradox involved, and it remains for us to examine the reasoning leading to the conclusion, and the premises underlying the reasoning.

As to (1a), we wish to state that it is not the purpose of this paper to take issue with the contention that "language is rule-governed behavior." (cf. Miller, Galanter, and Pribram 1960, and Hoard 1967). We have reservations, however, about its relevance to the domain of second-language learning. There are, for example, problems with such notions as "strategies of learning" and "strategies of communication" (Selinker, forthcoming); one has to account for the fact that it is just where there is knowledge of target language rules that strategies seem to arise (Elaine Tarone, personal communication), creating in turn new kinds of knowledge in the learner which are difficult to relate to "the rules" referred to in the paradox.

Furthermore, we do take serious issue with the absoluteness of the statement "learning a language involves internalizing the rules." As we hope to demonstrate, this statement has little meaning outside of its theoretical framework.

In (2a), it is not clear exactly what kind of "performance" is meant in the statement of the paradox. From the context, it seems to us to mean "doing second-language drills," a definition which is surely far from that intended by Chomsky (1965) in the dichotomy between competence and performance.

Beyond this, we wish to take issue with some portion of Saporta's reasoning. In his valuable 1966 article, after an introduction dealing with the relation between theory and pedagogy (cf. Wilkins 1968) Saporta arrives at his paradox apparently *ex nihilo*. The section immediately preceding the statement of the paradox deals with the "alleged role of memorization as a technique for learning" and Saporta seems to conclude that rote memorization and internalization of rules are mutually exclusive processes. We are not so sure.

Let us begin by examining the phrase which occurs three times in the statement of the paradox, "the rules." Given the recent chaotic trends among the theoreticians of generative grammar, one is led to wonder whether those rules which are *the* rules will ever be made available, even to the sophisticated reader. John Lackstrom (personal communication), points out that we often allow ourselves to forget that rules are not matters of fact, but theoretical constructs more or less supported by facts. It is also true that Chomsky sees no relevance to language teaching of the

implications of four areas of current research: "the 'creative' aspect of language use; the abstractness of linguistic representation; the universality of underlying linguistic structure; the role of intrinsic organization in cognitive processes" (Chomsky 1966a:46). It is important to ask why this should be so.

Although this topic has not been dealt with directly in the literature, it is not difficult to demonstrate that generative grammar is concerned with an idealized form of language behavior rather far removed from that considered in second-language learning research. Generative grammarians have, in fact, made a number of so-called "counterfactual" assumptions⁵ in order to delimit a well-defined field of interest for theoretical purposes; they have, in effect, dismissed several obvious facts about language behavior as irrelevant to theory construction. This is certainly proper for a theoretical discipline, but, as we argue below, at least some of the facts thus excluded are crucial to any serious analysis of second-language learning. Some examples:

(1) Individual differences among speakers, and thus learners, of a language are denied consideration in generative theory by Chomsky's statement that "linguistic theory is concerned primarily with an ideal speaker-listener." (1965:3) But such differences form a crucial set of variables in second-language learning.

(2) The "ideal speaker-listener" is "in a completely homogeneous speech-community." (Chomsky 1965:3) But it is clear that such considerations as the common heterogeneity of the speech-community are important in determining how people learn second languages.

(3) Consideration of the time factor in learning a second language is not possible in present models of generative theory. (Chomsky 1965:202) But second-language learning is obviously developmental in nature and such factors must be handled somehow in order to discuss it.

(4) The visual nature of some people's memories is not a relevant factor in generative grammar (Chomsky 1966b). But the reader should compare Rivers (1964:105) for a careful argument for the importance of recognizing that many second-language learners have visual memories. As will be seen below, we are interested in the seemingly visual coding of linguistic rules presented visually to the learner.

⁵cf. Walters 1967 and Goodman 1955 for a general discussion of various types of counterfactuals. One type, which is background to many discussions among philosophers and linguists (Chomsky 1966b), is the statement in an "if . . . then" form which is used to delimit one's domain of inquiry. We are concerned with counterfactuals which define the relevant data and restrict the extent of the predictions derived from a theoretical apparatus. This type is the one referred to in the text; specifically, any counterfactual which limits the applicability of a scientific theory.

(5) Limitations on storage or on short-term memory on the part of the speaker or listener are not relevant to generative grammar. (Chomsky 1965:3, 10ff) It should be obvious how extremely relevant such limitations are to second-language learning theory; considerations of memory limitations will be discussed in section 4.

By all of the above we do not, of course, intend to imply that generative grammarians deny the existence of these facts, nor that they deny their relevance to a theory of second-language learning; we merely wish to demonstrate that some caveats are in order for anyone who wishes to extrapolate from generative theory. In light of this, one wonders exactly what Saporta means when he says:

The main contribution of generative grammar is precisely that to the extent that it provides the most meaningful statements about the relevant data, it enables textbook writers to base their material on the most adequate description. (Saporta 1966:88)

There are seemingly two claims here; one concerning second-language learning and one concerning second-language teaching: the former, that generative grammar provides to some unspecified extent data relevant to the psychological processes of second-language learning (cf. Selinker 1969); and the latter, that generative grammar provides the most adequate description for textbook writers. We must dispute these claims on the grounds that generative grammar acting within its own domain most probably does *not* describe "the relevant data" of the psychology of second-language learning.

One could, however, argue that the data relevant to both domains do in fact overlap. For example, take the distinction "complex" vs. "simple" noun phrases (Ross 1967). A surface fact in English that is relevant to both domains is: an adverbial constituent of a certain type must follow a direct object constituent unless the object constituent is composed of a complex NP. That is, (1) and (3) are grammatical, whereas (2) is not, and these facts are established by native-speaker judgment.

1. He bought the book $\left\{ \begin{array}{l} \text{on Tuesday} \\ \text{in Seattle} \end{array} \right\}$.
- 2.* He bought $\left\{ \begin{array}{l} \text{on Tuesday} \\ \text{in Seattle} \end{array} \right\}$ the book.
3. He bought $\left\{ \begin{array}{l} \text{on Tuesday} \\ \text{in Seattle} \end{array} \right\}$ the book which you told him about.

It seems to us that there are three things going on here; first, the second-language learner who produces correct surface order in

English must have internalized the distinction between the two types of NPs; this fact is surely relevant to a psychology of second-language learning. Second, Ross' distinction in technical terms is as follows: "A noun phrase is complex if it dominates the node S." (Ross 1967:28) This "explication" is used in conjunction with "the principle for S-pruning: delete any embedded node S which does not branch (i.e., which does not immediately dominate at least two nodes)" (1967:26). Ross makes a very strong case for the indispensability for linguistic theory of this principle of "node deletion" or "tree pruning." But we would insist that the relevance of this technical concept to second-language learning is far from clear and would have to be demonstrated. Third, now that the surface facts seem clear, a "rule" could be set up to describe the distinction between complex and simple NP's. It is important to tell second-language teachers and textbook writers about this rule; these people should somehow be able to relate this information to the second-language learner.

As a general point, the textbook writer and the teacher should note that Saporta has claimed that:

recent studies in generative grammar . . . have made explicit the kind of capacities a language learner must have if he is even to approximate the competence of a native speaker, capacities such as the ability to distinguish grammatical from ungrammatical sentences and to produce and comprehend an infinite number of the former, the ability to identify syntactically ambiguous sentences and more generally the interrelation of sentences, etc. (1966:82)

If "language learner" in this case means second-language learner, we dispute this claim as well. It is just not clear how or even whether these abilities are acquired by successful language learners, nor is it clear how facts uncovered by generative grammar and the rules created to explain them relate to the "capacities" or "abilities" Saporta describes. To integrate such notions into a theory of second-language *learning*, a major relaxation of counterfactuals would have to occur. We are not, in principle, calling for such a relaxation. On the contrary, all sorts of insights and speculations, interesting and useful to those vitally concerned with second-language *learning*, are constantly being made available through the current outpouring of generative studies (cf., for example, Rutherford 1968). What we are advocating is extreme caution in extrapolating the findings established in one well-defined domain to another.

At this point it seems to us that more acceptable phrasing of

the initial sentence of the paradox would be: "Language is rule-governed behavior, and learning a second language involves, *among other things*, internalizing language rules of various kinds." But even with this rephrasing, there are problems. Focusing upon "rules," one might ask: in terms of a training procedure, how to people best internalize those second-language rules necessary for acceptable performance, when the learner is trying to express meanings that he may already have? Is practice always necessary and if so, in what form? Clearly, the correct answer depends upon the types of rules involved and many other considerations, some of which are discussed below. But we need to ask also: what does "internalizing" mean? (cf. Rivers 1968) We think it means that language rules are coded in some cognitive or intellectual structures. Heuristically, it seems that we can postulate for our purposes two distinct types of cognitive structures: (1) those mechanisms that guide "automatic"⁶ language performance and (2) those mechanisms that guide puzzle- or problem-solving performance (cf. Neisser 1967, Chapter 11). It is not very clear to us what the role of rote memorization would be in a training procedure designed to create second-language competence when speed in expressing one's meanings is an important performance factor.

In this light, it is worth investigating the conclusion seemingly drawn by Saporta that rote memorization and internalization of rules are mutually exclusive processes, in the statement which appears just prior to the paradox:

the student who makes the most progress by adopting rote memory as a strategy will presumably be the most reluctant to abandon it, and failure to abandon it means failure to learn a language. (1965:548)

Although verbalizing, memorizing, and consciously trying to apply grammatical rules to sentences have led to generations of second-language learning failure for many students, it has nevertheless obviously led to success for some. After all, people have always learned second languages and, it turns out upon reflection, some individuals have done so no matter what the method. As Saporta would admit, other variables are clearly involved. How is one then to evaluate the statement just quoted? The situation is much more complex than this quote would lead us to believe; it seems that the validity of this statement clearly depends on one's definition of "learning a second language" and also upon what the student

⁶That is, performance (language drills included) where speed and spontaneity are crucial and the learner has no time to consciously apply linguistic mechanisms.

actually memorizes. For instance, it is often not self-defeating for a rule to be memorized if the learner has as much *time* as he wishes to apply that rule, even if that rule should happen to be coded in a puzzle-solving mechanism. It is not too difficult to imagine those types of meaningful performance that allow the second-language learner time to apply rules: reading, writing, listening to a tape on a recorder with a slow-down device, communicating with a native speaker who has patience, and so on. It seems that the point has been reached in our discipline where we must include the processes of reading and writing in our definition of "learning" (more precisely, "having learned" or "knowing") a second language, whether or not learning theory (cf. Neisser 1967: 105-137) or linguistics have anything coherent to say about them. Thus, an essential consideration in defining *successful* second-language learning is that performance in reading and writing for the most part implies time for the learner to consciously apply rules. Crucially, it seems that many second-language learners, at least partially, approach their reading and writing tasks consciously applying certain types of grammatical rules in a puzzle-solving sense, and will continue to do so, not necessarily because of any major deficiency in their training programs, but probably because of their reliance on visual memory. It appears, upon observation, that grammatical rules are most often presented as visual input to cognitive systems, either in a text or on a blackboard or in notes taken by the second-language learner. It thus seems reasonable to postulate that the use of such visual coding, whether or not aided by oral statement of the rule by the learner, is at times closely related to puzzle-solving mechanisms.

What is the situation for the researcher when second-language performance involving "automatic" application of rules under conditions of speed and spontaneity is of concern? This is the type of performance that applied structural linguists have focused upon from the days when Fries (1945:3) stated:

A person has "learned" a foreign language when he has thus first, *within a limited vocabulary* mastered the sound system . . . and has, second, made the structural devices . . . matters of automatic habit.

Saporta does not even discuss the concept of "habit" apparently believing, as most of us probably do today, that the concept of "language as habit" was destroyed long ago in favor of the concept of "language as rule-governed behavior." And instead of discussing second-language performance related to automatic habit, or second-language performance related to the expression of meanings,

he discusses in the paradox "performance which is supposed to lead to making the application of the rules automatic"; we stated above that the notion "performance" as used in the paradox seems to mean doing second-language drills. But Saporta states that "performance" is "apparently" interfered with by "the ability or inclination to formulate the rules." Once again, though holding some element of truth, we believe that the statement is too absolute, and thus misleading. A more realistic appraisal would involve a consideration of individual differences. Some individuals, while consciously attempting to follow certain types of grammatical rules in a second language, have a certain type of performance interfered with, specifically the subtypes of second-language performance necessitating an element of speed and thus automatic and spontaneous application of rules: i.e. successful performance of drills in the classroom and something akin to ordinary conversation. But it is also true that if the given performance situation allows an individual, perhaps the same individual, time to puzzle out the correct answer, he might be able to formulate other types of grammatical rules as well as consciously and successfully follow them. Note that one premise behind the preceding sentence is that such an individual does not have severe memory limitations and that he *can* learn grammatical rules by rote memory; such an assumption is of course not always warranted. In the second-language learning situation, some individuals are not helped at all by explicit formulation of rules, while others are, and can apply these rules whenever time permits. It seems to us that in light of the preceding discussion, a more realistic phrasing of this part of the paradox would be:

When performance involving automatic application of rules is desired, the ability or inclination to formulate rules *does not necessarily guarantee* the performance which is supposed to lead to making the application of these rules automatic.

Furthermore, we find that since there seems to be little or no evidence that "the ability or inclination to formulate the rules" must *always* "interfere with the performance which is supposed to lead to making the application of the rules automatic," it is the second possible conclusion to be drawn from the analysis of the paradox, i.e. (2a), which is indeed the proper one. That is, there is every reason to believe that at least *some* students benefit from such ability.

3. Toward a strategy for research

We would like to suggest a direction that research into the learning of the linguistic aspects of a second language might take.

Those sets of variables which we consider directly relevant to theory construction are: types of rules, types of learners, and types of performance. The last factor is the easiest to handle since we wish to consider only two types: (1) performance in a second language in which the learner has time to consciously apply grammatical rules no matter how such rules are coded, and (2) performance in a second language in which automatic application of rules under conditions of speed and spontaneity is necessary. In the latter case, coding in terms of those intellectual structures which control automatic performance in language seems essential, whereas in the former case, it does not seem to matter how the rules are coded for an approach to successful performance.

As regards types of rules, the situation is more complex for the researcher; for many types of rules have appeared in recent generative discussions. Generally speaking, what types of rules would one wish to distinguish from the point of view of second-language learning? It is certainly true that there are some types of rules (phonological feature manipulations, different kinds of phonological and syntactic rule schemata, some of the more involved treatments of semantic features, etc.) whose value to the language teacher is doubtful; it is equally true that other rules, for various reasons, are extremely useful in the classroom—the problem is to distinguish one from the other on theoretical grounds, without resorting to an actual test of each combination of rules under classroom conditions. The latter procedure would be self-defeating, in view of the plethora of new rules, and new *types* of rules, which would have to be constantly tested.

One could make several heuristic distinctions, but it is obvious that there is in generative theory no criterion which could be used to judge the pedagogical worth of a rule. Nor is there any reason to suppose that such a criterion *should* be a part of a theory of generative grammar. Nevertheless, such a criterion, if developed, could be of enormous value to us and might well provide illuminating insights into the nature of rules in general. This constitutes a major research goal, in our opinion.

Even should we be able to determine from a principled point of view the pedagogical worth of a rule, is there any reason to believe that this would demonstrate anything relevant about the psychological processes undergone by a second-language learner? What it might do is provide a basis for distinguishing two other concerns which are regularly confused in the literature: a) the *subconscious* psychological processes undergone by the learner of a second language whether he attempts to express meanings that he may have in a language he is in the process of learning, and

b) the *subconscious* psychological processes undergone by the successful learner whenever he *consciously* applies rules or uses strategies that he hopes will lead him to successful performance and, hence, clear proof of successful learning in a second language.

As regards types of second-language learners, the situation is even more complex. It is clear that there are at least four types of learners we might wish to isolate: (1) individuals who cannot learn a particular verbalized rule; (2) individuals who can learn a particular verbalized rule and apply it automatically without difficulty; (3) individuals who can learn such a rule and not be able to apply it with or without time to do so; (4) individuals who can learn such a rule and apply it only when they have time to do so consciously. For some of the latter, the time factor will diminish with practice until it reaches automatic application; for others it will diminish only slightly or not at all. Of course, one should add that the combinations of these variables increase greatly when different types of rules and performance are considered. As was stated above, one of the assumptions of generative grammar that must be violated in a serious discussion of second-language learning is the notion that visual memory is not relevant. The fact is that some people use visual memory to organize intellectual structures in a second language, often combining visual support with memorization of rules. Carroll (1966:105) points out that experimentation into verbal learning has demonstrated that "other things being equal, materials presented visually are more easily learned than comparable materials presented aurally." This conclusion seems to approximate our intuitive feeling, but we are not sure how to extrapolate from studies on verbal learning since such studies are not concerned with learning materials comparable to those we are concerned with and, furthermore, do not seem to pay enough attention to individual differences.

4. A strategy for research

What we are calling for, then, is a series of profiles of idealized learners who differ from one another in terms of the types of variables discussed above. Furthermore, these profiles, to be truly meaningful, should be related to a theory of second-language learning relevant to the way in which *individuals* actually learn second languages—this theory to be framed in an experimental setting, if possible.

It is well known that psychological rules often apply to groups of individuals. It is entirely reasonable to hypothesize that such groups exist with regard to the four types of learners listed above. One of these four classifications with simple kinds of rules could

perhaps be the jumping-off point for a research study. It should certainly be possible to do a descriptive study in depth on one individual or a comparative study on two or more individuals, carefully describing all relevant variables in the analysis, rewriting from a linguistic description, say, ten surface structure rules for a second-language learner and establishing several tests of performance to demonstrate whether each rule has been internalized or not. One type of test might imply successful performance where time is not a crucial factor. In the latter case, the investigator might notice that with practice one learner greatly reduces the time necessary for automatic performance whereas another learner does not. The researcher should carefully collate all seemingly relevant information in the two cases, including introspective judgments as to what is easy or difficult and why, what learning strategies toward the material the learner consciously uses, and so on. The fact is that we are presently in need of a great deal of truly relevant data.

Secondly, to take an example drawn from our experience, which has largely been as teachers of English to foreign students: The topic of subordinate clauses in English is a complex one, dealing as it does with several different types (and levels) of rules. We have noticed that relative clauses seem to be easier, in general, for foreign students to assimilate than NP complements, and that *that* clauses seem more readily learned than infinitive or gerund complements. But there are individual differences here as well, perhaps due primarily to presentation in a visual rather than an oral mode, and these differences can be tested. In any case, if our general observation is correct, then this phenomenon might be due to the type of rule involved, or to some other factor, independent of any theoretical consideration. The point is that there are no valid theoretical grounds for assuming that this or any other second-language learning phenomenon arises from any one cause. Since we have no way of knowing in advance just what factors might be significant, we feel that experimental elimination of those that are not significant is called for.

We believe that, in principle, an experiment could be undertaken to test the manner and degree of learning English subordinate clause types by different types of learners under different performance conditions. It would be necessary to make the experimental group as homogenous as possible in language background and previous English experience. The group of subjects could then be taught selected English clause types, and then tested exhaustively to determine how and to what degree they have internalized the rules. Correlations should be made with, among other things, type

and level of rule involved, manner and order of presentation, time factor involved in application of rules, oral vs. written performance, and results of any standard personality inventory administered to students. The results of these correlations should, hopefully, prove enlightening as to the relationships between the variables of type of rule, type of learner, and type of performance.

The reader should note that variables of the type listed in the last paragraph, with the exception of type and level of linguistic rule, have been specifically excluded from theoretical models of language and language acquisition of the assumption of the counterfactuals mentioned above. It is our contention that in order to determine the best application of the results of theory, we must first know just how the universe of discourse of the theory is related to the real world; we must know just how far counterfactuals have placed linguistic theory from the relevant facts of second-language learning.

A third area of research concerns an issue raised previously in this paper with respect to a quote from Saporta: What role does memory play in internalizing rules? To speak of "rote memory" in the context of second-language learning is to beg the question; whatever memory is involved, it is not simply "rote." We have noticed that a student not gifted with eidetic recall can ordinarily repeat only the last part of a long sentence in an unknown language; the same student, when asked to repeat a long sentence in a language he is learning (i.e., that he partially knows), will repeat the first part—the last part often escapes him. It may be the case that a different type of memory is involved in second-language learning, at least for some individuals. Furthermore, memory span itself becomes an important variable in the domain of second-language learning. Lado (1965) has reported a number of studies which seem to show that memory span is shorter in a second language than in the native language. While Donaldson (1970) disputes Lado's contention, it should nevertheless be useful to know how memory span is related to groups of individuals distinguished by types of rules and types of performance. One could repeat the experiments reported in Lado's paper with provisions made for the isolation of individual variables. Our goal, once again, would be the establishing of profiles of types of individuals.

Memory, of various kinds, is surely related to the issues raised by consideration of Saporta's paradox, and would have to be integrated into the lengthened, though more realistic, reformulation we now present:

Language is rule-governed behavior, and learning a second language involves, *among other things*, internalizing language rules

of various kinds, coded in any one of several possible intellectual structures. Successful internalization is demonstrated by being able to bring these rules to bear on desired performance. When performance involving automatic application of rules is desired, the ability or inclination to formulate rules *does not necessarily guarantee* the performance which is supposed to lead to making the application of these rules automatic.

This restatement reveals that there is no paradox at all; one thing it does indicate is the possible truth of one of Saporta's premises, i.e. that the best scientific grammar is not necessarily the best pedagogical grammar. In the final analysis, therefore, this "paradox" does not represent a dead-end in learning theory as applied to a second language, but rather becomes an interesting sidelight on the seeming perversity of human learning processes, as well as a spur to research which might help to answer the question posed in the first sentence of this paper.

REFERENCES

- Carroll, John B. 1966. The contributions of psychological theory and educational research to the teaching of foreign languages. In A. Valdman (ed.), *Trends in Language Teaching*, 93-106. New York: McGraw-Hill.
- Carroll, John B. 1968. Contrastive linguistics and interference theory. In James E. Alatis (ed.), *Proceedings of the Nineteenth Roundtable Meeting*, 113-122. Washington, D.C.: Georgetown University Press.
- Chomsky, Noam. 1965. *Aspects of the Theory of Syntax*. Cambridge: M.I.T. Press.
- Chomsky, Noam. 1966a. Linguistic theory. In Robert G. Mead (ed.), *Northeast Conference on the Teaching of Foreign Languages*, 43-49. Menasha, Wisc.: George Banta Co., Inc.
- Chomsky, Noam. 1966b. UCLA Linguistic Institute lectures.
- Donaldson, Pat. 1970. The foreign-native memory span discrepancy. Unpublished paper, University of Washington.
- Fries, Charles C. 1945. *Teaching and Learning English as a Foreign Language*. Ann Arbor: University of Michigan Press.
- Goodman, Nelson. 1955. *Fact, Fiction and Forecast*. Cambridge: Harvard University Press.
- Hoard, James E. 1967. On the foundations of phonological theory. Unpublished Ph.D. dissertation, University of Washington.
- Jakobovits, Leon A. 1970. *Foreign Language Learning: A Psycholinguistic Analysis of the Issues*. Rowley, Mass.: Newbury House.
- Kline, Helen. 1970. Research in the psychology of second-language learning. Unpublished paper, University of Minnesota.
- Lado, Robert. 1965. Memory span as a factor in second-language learning. *International Review of Applied Linguistics* 3.123-129.
- Lyons, John. 1968. *Introduction to Theoretical Linguistics*. Cambridge: Cambridge University Press.
- Miller, G. A., E. Galanter and K. Pribram. 1960. *Plans and the Structure of Behavior*. New York: Holt, Rinehart & Winston, Inc.
- Neisser, Ulric. 1967. *Cognitive Psychology*. New York: Appleton-Century-Crofts, Inc.
- Richards, I. A. 1967-68. Why generative grammar does not help. *English Language Teaching* 22.3-9,101-106.
- Rivers, Wilga. 1964. *The Psychologist and the Foreign-Language Teacher*. Chicago: University of Chicago Press.
- Rivers, Wilga. 1968. Grammar in foreign language teaching. *Modern Language Journal* 52.205-211.
- Ross, John R. 1967. Constraints on variables in syntax. Unpublished Ph.D. dissertation, M.I.T.
- Rutherford, William E. 1968. *Modern English: A Textbook for Foreign Students*. New York: Harcourt, Brace & World, Inc.
- Saporta, Sol. 1965. Review of *Language Teaching: A Scientific Approach*, by Robert Lado (McGraw-Hill, 1964). *Language* 41.547-551.
- Saporta, Sol. 1966. Applied linguistics and generative grammar. In A. Valdman (ed.), *Trends in Language Teaching*, 81-92. New York: McGraw-Hill.
- Selinker, Larry. 1969. The psychologically-relevant data of second-language learning. Paper delivered at Second International Congress of Applied Linguistics, Cambridge University.
- Selinker, Larry. *Interlanguage* (forthcoming).
- Thomas, Owen. 1965. *Transformational Grammar and the Teacher of English*. New York: Holt, Rinehart & Winston, Inc.
- Walters, R. S. 1967. Contrary-to-fact conditions. In *The Encyclopedia of Philosophy*, Vol. 2, 212-216. New York: MacMillan Co. and the Free Press.
- Wilkins, D. A. 1968. Review of *Trends in Language Teaching*, ed. by A. Valdman (McGraw-Hill, 1966). *International Review of Applied Linguistics* 6.99-107.
- Witkin, H. A. 1949. The nature and importance of individual differences in perception. *Journal of Personality* 18.145-170.