INTERLANGUAGE AS CHAMELEON

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The claim is made that Labov's "Observer's Paradox" and the five methodological axioms leading to this paradox apply to interlanguage. Thus, we may view interlanguage as a continuum of styles, which is defined by the amount of attention paid to speech; the most systematic second-language learner speech is produced when the learner is paying the least attention to speech. Yet, when we do research, the presence of the researcher and the tasks presented to the learner, lead our subjects to pay attention to their speech. Therefore, we cannot claim that research can ever observe the most truly systematic form of a learner's interlanguage—unattended or "unmonitored" speech. The methodological implications of this claim are explored, and recommendations for research are made.

As second-language acquisition researchers, I believe we have not paid enough attention to one of the inherent characteristics of language: in its use in human interaction, it varies with the subtlest shifts of situation, just as the chameleon changes color as its surroundings change. If we assume with Adjemian (1977) that interlanguage (IL) is a natural language, then we must assume that IL behaves essentially like all other languages in this respect. Though second-language acquisition researchers generally agree that IL is variable, we seem to ignore this characteristic when we set up and report experimental procedures.

I would like to examine the methodological implications of this characteristic here. The theoretical implications are many, and it is difficult to refrain from exploring them in detail. However, I have tried to limit myself primarily to the methodological implications only, and will explore the theoretical implications of this problem in another paper.

In a classic work, William Labov (1969) discussed at length the "Observer's Paradox": the aim of (applied) linguistic research is to describe the way people talk when they are not being systematically observed—yet such data can only be obtained by systematic observation. Labov outlined five methodological axioms which lead to this central paradox in the study of language. I would like to examine each of these axioms in turn, testing at each step whether they indeed apply to interlanguage and the study of interlanguage.

1Parts of this paper are based on a section of an earlier work (Tarone 1972). This is a revised version of a paper presented at the TESOL Convention, Boston, March, 1979.
**Axiom One: Style-Shifting.** "There are no single-style speakers." Every speaker shifts linguistic and phonetic variables as the social situation and topic change.

I believe the literature quite clearly shows that style-shifting occurs in IL. When we have gathered data from identical subjects at the same point in time, by means of different experimental tasks, and examined the data thus obtained, we have found that different grammatical and phonological patterns were characteristic of the IL produced in these different contexts. For example, in a series of studies on phonology, Lonna and Wayne Dickerson have quite clearly shown that IL phonology varies systematically with different testing situations (L. Dickerson 1974, 1974, L. & W. Dickerson 1977). Lococo (1976) found similar evidence of style-shifting in IL syntax. In comparing the performance of second-language learners on three different tasks, she finds significant differences across tasks, in the percentage of "errors" occurring in several grammatical categories, and also in the degree to which processes such as transfer and overgeneralization occurred. Schmidt (1978) also used several experimental tasks to gather grammatical data, and concluded at one point:

Had we used these three tasks independently of the others, we would have come out with three different sets of descriptions of the second-language learners' grammars. (p. 30)

Larsen-Freeman (1975) studied morpheme orders using five tasks (speaking, listening, reading, writing and elicited imitation). She states,

Spearman rank correlation coefficients were next computed for rank orders of morphemes across tasks for all subjects. With the comparison of ordering on one task with another task, few statistically significant correlations were found. (p. 416)

Wong-Fillmore et al. (1979) also report that syntax varies with context in interlanguage.

Of course, if such variation occurs within a single study, where identical subjects are given different tasks, it must surely occur between studies. As the setting and tasks vary, so does the IL structure.

In my own work on communication strategies, I obtained data with a picture description task. I found topic avoidance to be a particularly strong strategy for my S's—if they did not know how to describe an item in a picture, they simply did not mention it. Galvan and Campbell (1978) on the other hand, studied communication strategies by means of a "translation" task, in which children were asked to act as translators for adults who ostensibly did not know one another's language. No topic avoidance strategy occurred at all in this study. To me it is clear that the task itself influenced the strategies.
which were used—the discourse rules attached by children to the task of translation would seem to say that the translator must attempt to translate everything.

It would seem then that interlanguage consists of many styles, and that the linguistic and phonological characteristics of IL change as the situation changes.

**Axiom Two: Attention.** It is possible to range the styles of a speaker along a continuous dimension defined by the amount of attention paid to speech.

While there has been much discussion in the literature of the existence in IL of the opposite poles of attended and unattended IL—in monitored and unmonitored utterances—our lack of information about the situations in which IL data have been gathered makes it impossible to verify that a *continuum* does in fact exist in the speech of second-language learners. Krashen (1976, 1977, 1978) postulated the two opposing poles of second-language learner speech—monitored and unmonitored. Dulay and Burt (1978) hypothesized that different morpheme orders result when the second-language learner is required by different experimental tasks to change the focus of his/her attention. Task focus (attention) affects the morpheme order obtained. I believe that it is not helpful to postulate only two extreme opposite ends of what I contend here is a continuum. More appropriate is the hypothesis that IL, as a natural language, consists of a continuous range of styles which are defined, as Labov suggests, by the degree of attention paid to speech. Labov's axiom suggests that attention is not an "on-off", all-or-nothing matter, but a matter of degree. We must take more care in careful description of the experimental situations in which our data originate so that we can clearly establish whether this hypothesized continuum of styles exists, or whether IL consists simply of two opposing modes of monitored and unmonitored language.

**Axiom Three: Vernacular.** In the "vernacular" style, where the minimum amount of attention is given to speech, the most regular and systematic phonological and grammatical patterns are evidenced. Other styles tend to show more variability.

Labov went on to state that whenever a subordinate and a superordinate language variety are in contact, in any formal speech situation, speech patterns will shift "from the subordinate towards the superordinate in an irregular and unsystematic fashion." (p. 50) In formal test situations, speakers of subordinate language varieties have been shown to be unable to perceive accurately the non-standard rules which they themselves use in informal situations. In general it is not possible for most speakers to direct their attention toward the non-standard rules—after extensive contact with the superordinate
variety, speakers of the subordinate variety "no longer have clear intuitions about the vernacular available for inspection." (p. 51)

It seems to me that the second-language learner is clearly in a situation where there is a subordinate and a superordinate variety to contend with. The superordinate variety of the language classroom is the target language (TL). The TL may also be the standard of the community in which the learner lives. I suggest here that the subordinate variety is the learner's own interlanguage. As A&emian (1977) hypothesized, this IL rule system is permeable with regard to the TL rule system. It can be invaded by TL (superordinate) rules in as-yet unknown ways. If Axiom Three can be shown to apply to IL, then the IL rule system should be least permeable in informal situations, when little attention is paid to speech, and most permeable and most variable in formal situations, when the learner focuses attention on speech.

The clearest evidence for the axiom of vernacular shifting in interlanguage may be found in studies by Gatbonton (1975) and Felix (1977). Gatbonton found among other things, fewer target language variants in IL casual speech than in formal or read speech—evidence of style-shifting toward the TL norm in formal speech, and away from it in less formal speech. Then, in Felix's (1977) paper based on data from a study in Kiel, Germany, the claim was made that the most regular IL patterns were to be found in those data produced freely in family interactions. To summarize Felix's findings: there was much less interference in this "spontaneous" speech than in speech produced in more formal experimental situations; some structures seemed to occur only in spontaneous speech and others only in test situations; the order of acquisition suggested by spontaneous speech data was different from that suggested by elicited data; and in experimental situations very different strategies were used, resulting in unusual and aberrant grammatical structures which never occurred in spontaneous speech. The data called "spontaneous" in Felix's study were gathered in informal interactions with family members in the subjects' home. I suggest that speech produced in this situation is equivalent to a vernacular of interlanguage; obviously, this speech style was much more regular than speech produced in the relatively more formal experimental situations used in the study. Felix hypothesizes that the experimental situation forced the learners to go beyond their competence, thereby encouraging errors. My hypothesis is that the IL is functioning as a subordinate language variety, and that therefore the vernacular style of this variety is more regular than the formal style, in keeping with Labov's third axiom cited above. The interlanguage rule system seems to become more permeable in formal language
situations—permeable to invasion from the superordinate rule system of the target language (see Gatbonton). An interesting question for research is raised by the fact that IL seems also to be more permeable in these situations to invasion from the native language (NL) rule system (see Felix). Could it be that there are two superordinate norms operating for the second-language learner? The NL norm and the TL norm? Or, perhaps, as Deborah Pirani suggests (personal communication), individuals may differ with regard to whether NL or TL is the superordinate norm. These data would seem to suggest that in formal situations, there may be more influence from both the TL and the NL rule systems, and that this influence makes the IL rule system more irregular in formal than in informal situations. As Labov's third axiom might suggest, this shifting toward the TL (and, if there is transfer, toward the NL as well) seems to occur in an "irregular and unsystematic fashion." We must conclude that research data obtained in formal test situations do not reflect the most systematic IL competence of the learner. If Labov's third axiom can be shown in further studies to apply to the IL rule system, then we must conclude further that in formal test situations the speakers of an IL do not have conscious access to the more regular rule system which operates in informal situations. In particular, attempts to obtain direct access to the intuitions of IL speakers as to the rules underlying their vernacular style, via grammaticality judgments, must be called into question. If Labov's third axiom applies to second-language learners, as I suggest it does, then second-language learners cannot have clear systematic intuitions about their vernacular style. Their intuitions can only gain access to what Bialystok (1979) termed their "metalinguistic knowledge" about their IL, but not to the implicit knowledge which is actually used to generate their IL in situations where they are not paying attention to language.

It would seem that, for a variety of reasons, second-language acquisition researchers should attempt to study the vernacular as it occurs in informal situations. I hypothesize it to be the most systematic IL style, and the style least permeable to invasion by TL or NL rules. This hypothesis is of central importance to the current discussion on the relationship of pidginization to second-language acquisition (see Gilbert 1979); there is a need for clear evidence of a systematic internal IL norm, influenced as little as possible by style-shifting toward TL or NL. The ability to measure the vernacular is also important for language proficiency assessment in programs such as French Immersion in Canada, where we want to measure informal usages of the IL (see Genesee 1979 and Swain 1979).

A crucial question for research is, then, what constitutes a formal situation as opposed to an informal one.
Axiom Four: Formality. When a speaker is systematically observed, a formal context is thereby defined, and the speaker pays more than the minimum amount of attention to speech.

A formal situation is one in which we systematically observe the speaker. However, in the literature on second-language acquisition, an incredibly broad spectrum of experimental situations have been referred to as "informal," and the so-called "spontaneous speech" which is used by second-language learners in all these situations can be considered neither informal, in Labov's sense, nor equivalent in style. I believe we have been too lax in our definition of what constitutes informal style, or the vernacular. We are not consistent in what we mean by "formal context" and "informal context," and this has hurt the field when we have tried to make sense out of data generated in our studies.

For example, Naiman (1974) was one of the first to suggest the use of elicited imitation tasks to obtain data more or less equivalent to "spontaneous production." However, this is how his "spontaneous production" data were obtained:

... The S's were shown the pictures used for the comprehension task and asked to talk about the relevant picture ... E attempted to elicit the relevant syntactic structure from the child. (p. 8)

Although the location of the study was not indicated, I assume it must have been at school. My question is, can speech data obtained in this way really be considered comparable to speech data generated in the absence of adult E's and the presence of peers in informal situations? Not according to the fourth axiom above. While Naiman makes no explicit claim that his "spontaneous production" data are equivalent to what Krashen would call "unmonitored speech" and I would call the vernacular, he does go on to suggest that because systematic relationships were found in his study between these spontaneous production data and elicited imitation data, that we should use elicited imitation tasks instead of "recording hours of spontaneous speech in the hope that the child will use a particular sound or grammatical structure." (p. 33) Assumed in this recommendation is either that IL is the same in all contexts (a violation of Axiom One), or that style-shifting between formal and informal contexts is systematic (contrary to Axiom 3, which I have suggested applies to IL).

Dulay and Burt (1978) defined "natural communication" as the "use of 'natural speech' where the student's focus is on communicating something" as opposed to a focus on a particular rule of the TL. I certainly believe the former would be closer to the vernacular than the latter. However, Dulay and Burt went on to claim that such "natural communication," may occur in either of two different "natural com-
munication tasks”—“structured communication,” in which an examiner asks a student questions designed to elicit target structures “naturally and systematically,” or “unstructured communication,” in which a student and examiner or other person converse with no inter to elicit specific structures. It is implied that both structured and unstructured communication tasks can elicit equivalent “natural communication.” But clearly this cannot be the case if IL functions according to the axioms outlined above. In fact, I would suggest that a situation in which an adult asks a child “specific questions designed to elicit target structure”, however “naturally and systematically” s/he may attempt to do so by definition is a more formal situation than one where the experimenter does not have a set program of discourse—and that both of these situations are more formal than a situation where the student converses at home with, for example, a peer rather than an adult E, on whatever topics come up.

Krashen (1978) also asserted that there is no difference between morpheme-order data generated by the Bilingual Syntax Measure (which elicits “structured communication” in the framework outlined above) and “adult free speech.” It is not clear what “adult free speech” means in the context of our discussion. If by “adult free speech” Krashen means anything like the vernacular, we must question his assertion in light of the axioms outlined here. If IL is a natural language, speakers of IL must style-shift as they move from an experimenter-subject question-answer context in a school testing situation, to peer discourse in a tavern or at home.

As a final example of the way in which we have neglected to pay adequate attention to the formality of the testing situation, I would like to refer to a longitudinal study by Cazden et al. (1975). Here data were obtained using elicitation techniques, “spontaneous speech” (which here means that an experimenter, transcriber and tape recorder were present in the subject’s home), and “preplanned sociolinguistic encounters” (meaning that the E and the S took trips). In spite of the admirable variety of situations in which data were collected, this study combined the data resulting from all these contexts and analyzed it together. I have not seen any attempt to tease out the relationship among bodies of data produced by each of these methods, though such an attempt may have been made. Based on the axioms outlined here, and my suggestion that they apply to IL, I would predict significant differences in the IL structures produced by these varying elicitation techniques—quite apart from the variation in the data occurring over time due to learning.

To summarize then, it would seem that most of the “spontaneous IL speech” data described in the literature have been gathered in more
or less "formal" situations, in Labov's sense. In fact, the term "spontaneous" seems to apply to a very wide range of speech styles indeed. Almost all our "spontaneous" or "natural" data have been obtained in situations where the speakers has been more or less systematically observed in more or less obvious ways by an E. Thus we must assume that the speakers were paying more or less attention to their speech in these contexts, and style-shifting more or less toward a superordinate speech variety.

**Axiom Five: Good Data. The best way to obtain enough good data on any one speaker is through an individual tape-recorded interview: a formal context.**

Here we have the crux of the paradox. We must observe and get clear data if we are to do research. We must be present in the room with the subject. And, the best physical data is recorded data—ideally, with a microphone as close to the speaker's mouth as possible. If we have recorded data we can check and recheck our perceptions as researchers. We can validate our perceptions by checking with other researchers. We can go back to the raw data.

Hence the paradox. If we get good recorded data, we get bad data in the sense that the speaker has focused attention on speech and style-shifted away from the vernacular, which is the most systematic IL style and therefore what we may want to study.

**Now What?**

In light of the hypothesis just outlined—that Labov's five methodological axioms apply to the study of IL, and that therefore as IL researchers we are caught in the "observer's paradox"—what can we do to avoid or minimize the effects of this paradox? I do believe that research into the systematic nature of IL is possible—but only if we proceed with the constant awareness of the chameleon-like nature of IL and its extreme sensitivity to context.

First and foremost, we should take more care in reporting how data are gathered. In addition to the descriptions of the subjects which are currently routinely reported, the following dimensions should be specified in the procedures section of any study:

**Task.** _Exactly_ what was the subject asked to do? We need to know precisely what the subject was told about why the E was there and what s/he was testing for. We need to know what task the S was focusing on as the IL data were produced. We should refer to the "vernacular" as _only_ that data produced with no elicitation device and under the conditions specified under Point Two below. We should
specify what we mean when we use the term "spontaneous speech" in our studies.

**Interlocutors.** Who was present in the experimental situation when the data were gathered? What was their relationship to the S? Sex, age, past experience with the S—all are relevant.

**Physical Surroundings.** Were they relatively formal (e.g., classroom, office or school context) or informal (e.g., S's home, a tavern, etc.)?

**Topic.** If there was a discussion, what was the topic?

Until these variables are clearly specified, we should not be surprised at the fact that our studies produce data which is systematic only in the most global and uninteresting sense, hard to interpret and almost impossible to replicate. We should not be surprised either to find it impossible to tease out the relative roles played by the processes of transfer, overgeneralization, etc., in shaping IL—when these processes operate variably with situation (Dickerson 1975, LoCoco 1976).

Second, if we want to gain access to the "vernacular IL," we can follow some of the procedures listed by Labov as encouraging style-shifting toward the vernacular even while an experimenter is present. Sociolinguists have been able to obtain data which is both good recorded data and highly vernacular even while informing S's of their rights and turning on a tape recorder—by using the variables of physical surrounding, interlocutors and topic to divert the S's attention away from speech and focus it on other matters. For example, the setting should be as informal as possible—in the S's home, in the kitchen rather than the living room, on the floor with kids rather than sitting on chairs, eating snacks rather than not. The tape recorder should be unobtrusively placed, and the initial 5 to 10 minutes of conversation after the tape recorder has been turned on should be discarded. One of the best ways to elicit the vernacular is to simply use the normal interactions of the peer group rather than relying on a one-to-one interview. Various topics can encourage the vernacular—the interview situation may be interrupted by someone or something unexpected so that the S forgets about the tape recorder. The S may be asked to deal with topics which recreate strong emotions felt in the past—e.g., "Have you ever been in a situation when you were in serious danger of being killed?" Any topic dealing with strong emotion—births, sex and sex roles, fear, etc.—seems to work. Labov is the only researcher I know of other than Guiora who has used alcohol in a language study. Conversations over beer at the kitchen table seem to move quickly toward the vernacular. There is obviously room for creativity here. Just as obviously, one can never be entirely sure that what one is observing even under the best of conditions is true
vernacular style. But these techniques seem to have worked well in the past sociolinguistic research, and are certainly worth a try in our field. We are able to approximate the vernacular much more closely than we have in the past.

Third, implied in the above recommendations is the assumption that only through individual longitudinal studies of IL in use in the most informal contexts will researchers ever be able to gain insights into the truly systematic nature of interlanguage. If we are to find the chameleon we must separate him out from his surroundings, and we must be very clear as to what his surroundings were when we report our findings. And we must attempt to capture him in his most natural, unguarded state.

Finally, elicitation devices and research in the classroom should not be discarded. They obviously have their place. As measures of formal language proficiency, elicitation devices are very useful. Where they fall short is as research tools which purport to elicit "vernacular" or "unmonitored" speech. What we have discussed here should encourage research in the classroom rather than discourage it. For we have seen that IL in the classroom is probably markedly different from that which is used outside the classroom; it is probably more unsystematic and contains more NL variants, but it also seems to contain more TL variants as well. One wants to know the shape of the classroom IL and its precise relationship to the vernacular IL. And, most particularly, one wants to know how TL variants move from the former style, the classroom style, into the latter, the vernacular, "unmonitored" speech. This is, in fact, the goal of language teaching after all—to facilitate precisely this movement.

REFERENCES


