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# A CASE STUDY OF A JAPANESE CHILD LEARNING ENGLISH AS A SECOND LANGUAGE[1,2]

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Major findings are reported here of a longitudinal, naturalistic study of the acquisition of English as a second language by a five-year old Japanese girl. The emphasis is on empirical findings based on careful distributional analyses performed on the data, rather than on any particular theoretical orientation. The major content areas discussed are 1) the problem of prefabricated patterns (Hakuta 1974b); 2) the order of acquisition of grammatical morphemes; and 3) the problem of language transfer. It is argued that there is still great need for a broader empirical data base before any serious attempts at theoretical formulations of the second language acquisition process are made.

This paper summarizes some of the major data gathered in a longitudinal, naturalistic study of a Japanese girl learning English as a second language (Hakuta 1975b). The subject in this study is Uguisu, 'nightingale' in Japanese. Her family came to the United

[1]Portions of this article are reprinted by permission from Hakuta, Kenji. 1975. Learning to speak a second language: what exactly does the child learn? In Daniel P. Dato (ed.), Developmental Psycholinguistics: Theory and Applications. Georgetown Round Table on Languages and Linguistics, Georgetown University.

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States for a period of two years while her father was a visiting scholar at Harvard, and they took residence in North Cambridge, a working-class neighborhood. The children in that neighborhood were her primary source of language input. Uguisu also attended public kindergarten for two hours every day, and later elementary school, but with no tutoring in English syntax. Most of her neighborhood friends were in her same class at school.

She was observed over a period of 60 weeks, from age 5;4, which was five months after exposure to English began, until age 6;5. Every two weeks for at least two hours, spontaneous speech was recorded and later transcribed in traditional orthography. Transcription always was done on the same day as the recording, when contextual factors were still fresh in my memory. It should be noted that, prior to the first sample, in fact from three months after her exposure to English began, I made repeated attempts to gather data, but she produced little speech. It probably would have been possible to elicit speech from her at that time by bombarding her with questions, but I chose to let her begin speaking in a natural environment, which was in a play situation with her peers. Thus, the first sample could be considered her earliest attempts at production.

## Method of analysis

With thirty bulky looseleaf notebooks filled with transcriptions, there are innumerable ways in which the data could be analyzed. And I tried practically everything, from half-hearted attempts to write grammars for given corpora, which never worked (see Brown and Fraser 1964; Rosansky, Schumann and Cancino 1974), to frequency counts. Basically, as anyone who has seriously attempted an analysis of spontaneous speech data knows, you look at what you can look at, that is, those constructions which appear in relatively high frequency in the data. In one sense, this might be called an eclectic approach, but the one commonality is that all evidence presented is distributional, which is to say neither experimental nor anecdotal. Distributional analysis is a method of descriptive linguistics. From the full pattern of a collection of related utterances, one infers, seldom with complete certainty, the grammatical and semantic knowledge of the speaker. Developmental psycholinguists add to the linguistic distributional method the power of statistics at strategic points. It is essential to realize, above all, that from the single simple occurrence of a certain construction one can never infer much about the grammatical and

semantic knowledge underlying its occurrence and, most importantly, one cannot attribute to the child all the knowledge that could usually be safely imputed to an adult. Much research in developmental psycholinguistics commits this sin, and cannot be trusted.

Essentially what one does is gather together everything in the child's speech that is in any way relevant to the construction of interest. Distributional analysis is simply the method of induction but it requires a good sense of evidence, some of the skills of a detective, and a deep knowledge of the language in question, if it is to be done well. Developmental psycholinguists did not invent the canons of induction, but it has invented and must continue to invent new ways of bringing induction to bear on the spontaneous speech of children.

## Internal and external consistency as a developmental framework

In an earlier paper (Hakuta 1975a), arguing from the basis of Uguisu's data, I made an attempt to characterize the nature of the second language acquisition process. One characteristic is the slow and gradual nature of development. That is, nowhere along the developmental spectrum do we find the sudden leaps in performance which we might infer as the acquisition of a 'rule'. Typifying this gradual development are grammatical morphemes, which are easy to score in terms of percent supplied in obligatory contexts (Brown 1973). When quantified in this way, one finds a gradual rise in the probability of that morpheme being supplied, from 0% approaching a 100% level.

That this slow development is not due either to the possibility that each of these morphemes actually consists of minute sub-rules (each of which is acquired abruptly), nor to the possibility of phonological constraints, has been demonstrated in at least one instance. A comparison of the acquisition curve for the indefinite article *a* (across all its obligatory contexts) was made with the acquisition curve for *a* in the restricted context [a/_nother], which controls, for the above two possibilities. The choice of this odd restricted context [a/_nother], incidentally, was made on the basis of the fact that there were no nouns which appeared with any frequency across all samples in order to yield a reliable curve. [a/_nother] is, however, an adequate substitute since Uguisu also occasionally used its variants, *the≠nother* and *some≠nother*, suggesting that [a/_nother] had productive usage. Figure 1 shows

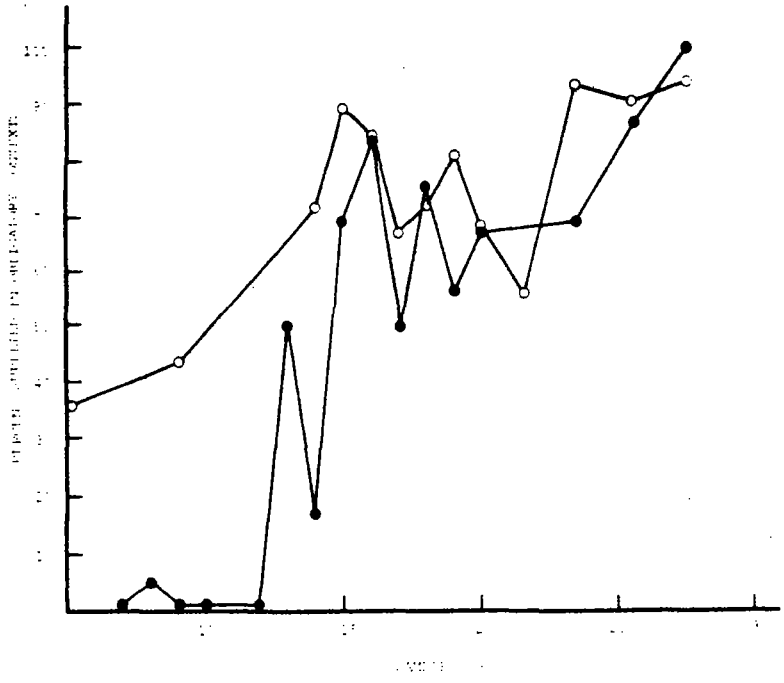

*Figure 1. Acquisition curves for the indefinite article* a *in the restricted context a/_nother (closed circles) compared to the entire range of contexts a/_NP (open circles), scored for percent supplied in obligatory contexts. The number of obligatory contexts for each data point for a/_nother ranges from 4 to 18, with a mean of 9.*

the two acquisition curves. The profiles of the two curves are strikingly similar, except for the earlier samples (prior to Sample 14) where the percent supplied for [a/_nother] is close to zero. The difference in the earlier samples, I believe, is due to instances where in total contexts, the instances of *a* which appeared were in fact simple phonological stems of other morphemes, such as *in-a* or *look-like-a*, and not necessarily productive. What the above analysis suggests, then, is that even within maximally restricted contexts, the learning involved is not abrupt and sudden, but is rather a process in which the probability of the morpheme being supplied rises only gradually. What I am implying, of course, is that this is not just true in the case of the indefinite article, but true of the development of all linguistic forms, given a careful distributional analysis on the part of the researcher. For example, when Rosansky, Schumann and Cancino (1974) quantified the various English negating devices used by their Spanish learners over the course of learning, only gradual changes as opposed to abrupt

shifts are found in the type of negating device preferred by their learners.

What are the propelling forces behind these gradual changes in the learner's system? In my earlier paper (Hakuta 1975a), I suggested two processes, internal and external consistency, which were inductively derived from Uguisu's data. My arguments are repeated here, and center around two construction types: 1) *be gonna*, as in *I'm gonna fool you*, and 2) *wh*-embeddings, as in *I know how to play hopscotch.*

*Be-gonna.* Uguisu began using this form as early as Sample 4, and with high frequency from Sample 9. She produced utterances such as the following:

She gonna kill her.
I gonna make 'nother baseball.
Oh, they gonna kill the fish.
Everybody gonna do it.
We gonna punch you.

Note that the auxiliary *be*, which is obligatory in adult speech, is missing. She eventually did begin supplying the *be*, and so I decided to score for percent supplied in obligatory contexts for all samples, tallying separately for the three allomorphs *am*, *is*, and *are*. The results of the scoring appear in Figure 2. For the moment, leave aside the strange downward swoop of the curve for *am* between Samples 9 and 14. We shall return to it later. Notice first in Figure 2 that the acquisition curves for the three allomorphs manifest a slow, probabilistic rise, just like the curves observed in Figure 1. Also note that the allomorphs *am* and *is* attain the 100 percent level of being supplied, while the allomorph *are* trails limply, never getting above the 40 percent level.

Why the difference between the different allomorphs of *be*? This is rather odd in light of the fact that Uguisu was quite good with other forms of *be*, namely the copula and the auxiliary in the present progressive. For the copula, omissions occurred rarely (about 1%), and I have argued (Hakuta 1974b, also see the next section, below) elsewhere that they are 'prefabricated patterns'. Similarly for the auxiliary: Uguisu produced *am*, *are* and *is* with equal ease, and so the problem cannot be phonological in nature.

One possibility is that the sequencing of constituents in the input may provide difficulty. That is to say, when a declarative sentence, such as *You're gonna try this one*, is transformed into the interrogative form, it becomes *Are you gonna try this one?*,

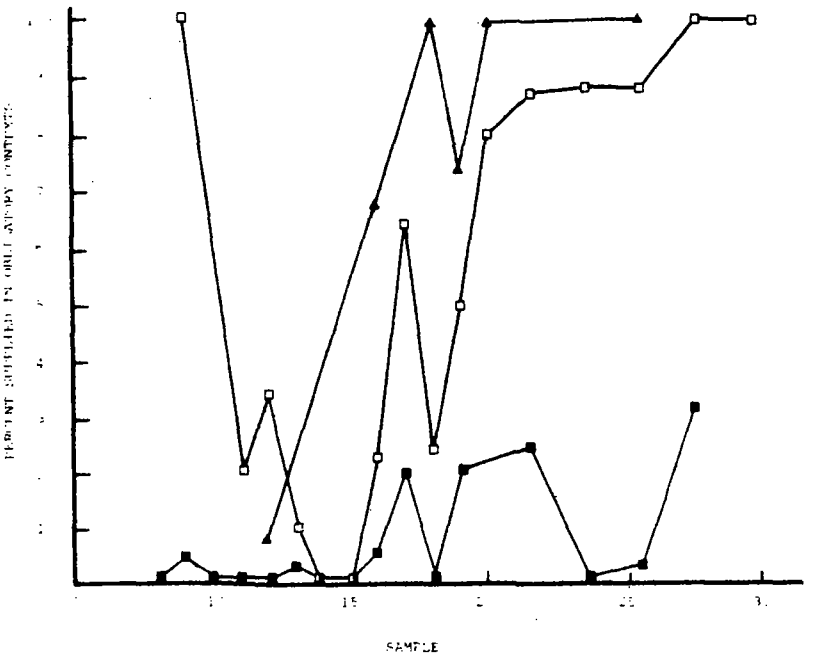

*Figure 2. Acquisition curves for the three allomorphs of* be: am *(open squares),* is *(closed triangles), and* are *(closed squares), as auxiliaries to the catenative* gonna, *scored for percent supplied in obligatory contexts. Samples are bi-weekly. The number of obligatory contexts for each data point is greater than 5, in most cases between 15 and 30.*

where the auxiliary *be* is moved out of its normal environment and placed in front of the sentence. Thus:

Pro + *be* + *gonna* + VP
*Be* + Pro + *gonna* + VP

This results in a sequence of constituents where the auxiliary *be* is in effect omitted from its position between the subject and *gonna*. It may very well be the case that this provides an erroneous model to the learner. In accounting for Uguisu's poor performance with *are*, the explanation on grounds of sequencing of constituents is an intuitively appealing one. One generally asks questions about *you* and *we*, such as *Are you gonna come with me?* or *What are we gonna do about this problem?*, both of which involve the allomorph *are*. It seems unlikely that one would ask questions with the subject *I*, that is, *Am I gonna have a tantrum?*. Questions involving a third person singular subject, such as *Is he gonna read*

*this paper?*, would also be less likely than questions with *you* and *we* as subjects.

To test for this possibility, I decided to analyze the interactor's speech taken from two distinct time periods, the first from Samples 7 through 9, and the second from Samples 17 through 22. I shall refer to these two periods as Time I and Time II. I first extracted all utterances involving the form *gonna*, and then scored them, using as categories the three allomorphs, according to whether they provided a 'good model' or a 'bad model'. A good model was defined as where the *be* is placed between the subject and *gonna*, such as *We're gonna play with playdough;* a bad model as where the *be* is not between the subject and *gonna*, but rather preposed, as in *What are you gonna do?*. The percentage of good models over total *gonna* constructions was computed for Time I and II, and the results appear in Table 1. The results show that at both Time I and II, the percentage of good models for *are* is significantly lower than for *am* and *is*. If one accepts the assumption that similar profiles appear in Uguisu's input, and I think it likely, then this analysis suggests that her apparent difficulty with the allomorph *are* had to do with her attempts to make her speech in effect consistent with what she heard in her input, a process which might be called 'external consistency'.

Let us return now to Figure 2 where, as mentioned earlier, there exists a rather strange downward swoop for the allomorph *am* between Samples 9 and 14. In Sample 9, Uguisu supplied *am* in all five instances with *gonna*. Prior to Sample 9, between Samples 4 and 8 when the *gonna* form was infrequent, Uguisu supplied *am* in 4 out of 5 instances. By Samples 14 and 15, *am* was omitted in all 22 obligatory contexts. One can well ask the question: "Uguisu, just what are you doing?" When a presumably correct form becomes deviant over time, one infers that some process of reorganization is going on. One possibility which immediately

TABLE 1

*Percent of good models over total* gonna *constructions in interactor speech from two time periods.*

| | Time I | Time II |
|---|---|---|
| am | .80 (15/19) | .95 (18/19) |
| is | .74 (14/19) | .74 (23/31) |
| are | .29 (22/75) | .33 (29/87) |

presents itself is that the function of the form *gonna* is quite similar to that of other catenatives, *have to* and *wanna*. They all signal 'intentionality' or 'imminence' (Brown 1973:318). Of these three catenatives, however, *gonna* is the only one in which an auxiliary *be* is required. Did Uguisu have the other forms *wanna* and *have to? Wanna* was present from the very first sample; more interestingly, the form *have to*, though existing infrequently from Sample 5, went through a 'peak usage' between Samples 9 and 12, where approximately 14-15% of her total constructions used this form. This compares to an approximate 4% usage in the later samples. When a form undergoes such overuse, it suggests some process through which the form is being actively 'tried out' by the learner. Interestingly, this period of overuse of *have to*, which lasted from Sample 9 to 12, corresponds to the period when the downward swoop for *am* in *gonna* is observed, between Samples 9 and 14. This observation leads me to speculate that Uguisu was attempting to make her *gonna* form consistent with her other two catenative forms, *wanna* and *have to*, thereby dropping the *am* in *gonna*, a process which might be called 'internal consistency'. Uguisu was trying to keep related linguistic forms within her system consistent with one another.

*Wh-embeddings and wh-questions.* While the notion of 'internal consistency' is still fresh in mind, I shall go on to the next problem of *wh*-embeddings and *wh*-questions, which I think speaks more directly to this issue.

As early as Sample 5, Uguisu made the following set of utterances:

I know how to do it.
I know how to do read it this.
I know how to read it this.
I know how to make.
I know how to draw it cat.
I know how to draw (it) butterfly.
I know how to draw it boy.

What appeared at that time to be quite grammatical constructions of embedded *how*-questions, however, disintegrated over time into forms such as the following, which she produced at the very last session:

First I gotta write it and show you how *do you* spell 'Debra'.
I know how *do you* spell Vino.
We only know how *do you* make it like that.
I know how *do you* write this.

What one finds here once again is a progression, from presumable grammatical utterances to a deviant form. This progression, from *how to* to *how do you*, is also a gradual and not a sudden process.

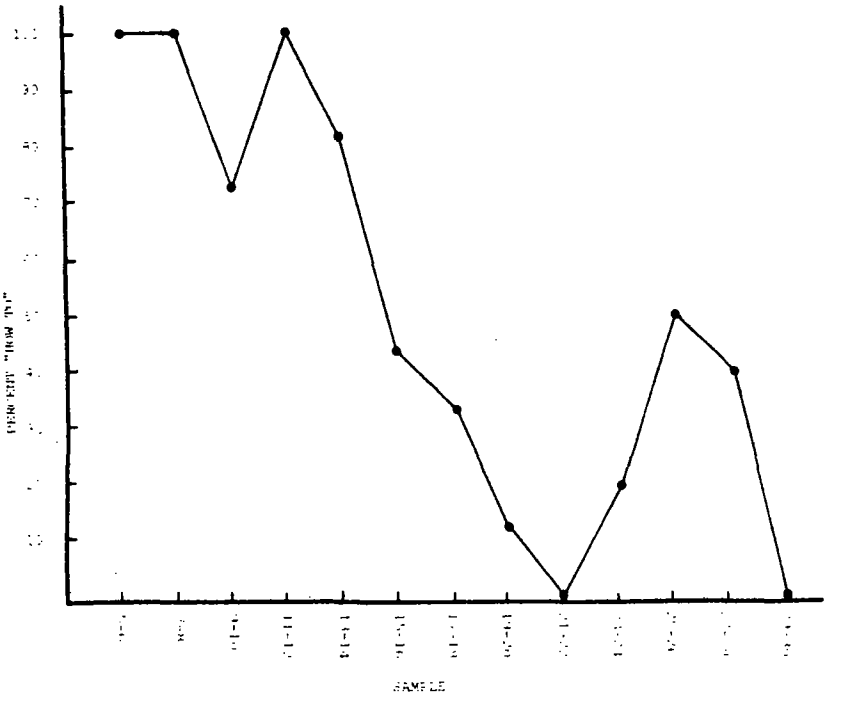

*Figure 3. Proportion of correct how-embeddings (*how to*) over total how-embeddings. Bi-weekly samples are paired.*

Figure 3 plots the story, and the graph can be read as follows: 'Given the instances when embedded how-questions were used, what percentage took the proper form *how to?*' Once again, one may well ask the question: 'Uguisu, what are you doing?' We can get a glimpse of the process by looking at other *wh*-embeddings used by Uguisu. Table 2 gives an exhaustive list of embedded *where*-questions used by Uguisu. The form starts out with the configuration 'Sentence + Question', as in *We know where is this.* Through a gradual process, the question becomes 'uninverted', as in *I don't know where the bathroom is,* after some redundancy, as in *You will see where is your house is.* A similar progression is observed with other embedded *wh*-questions, but with less frequency of occurrence.

The *wh*-questions produced by Uguisu complete the picture. From the first sample on, Uguisu was able to construct *where*-questions of the following sort:

Where's purple?
Where is thenose?
Where is potato?

TABLE 2

*Exhaustive list of embedded "where" questions produced by Uguisu.*

| Form* | Sample | Sentence |
|---|---|---|
| I | 1 | I don't know, where is money. |
| I | 7 | We know where is this. |
| I | 7 | I don't know where is it. |
| I | 10 | My father tell me where is here. |
| I | 10 | I didn't know, where is, um, doctor's room. |
| U | 11 | I know where it is. |
| I | 11 | You have to close your eye and you have to see where is it. |
| U | 12 | I don't know where she is. |
| I | 13 | I don't know where is your house. |
| I | 14 | I didn't know where is it. |
| I | 16 | You know where is my house. |
| I | 16 | You will see the house where is it. |
| R | 16 | You will see where is your house is. |
| R | 16 | I don't know where is the telephone number is. |
| R | 18 | I don't know where is the woods is. |
| I | 18 | I know (it) where is it. |
| U | 18 | I know where it is. |
| U | 24 | I don't know where the bathroom is. |
| U | 26 | I know where it is. |

*I = inverted
U = univerted
R = redundant

She was also able to construct *how*-questions of the following sort:

How do you make it bread?
How do you play this?
How do you put it on?

It seems that Uguisu was forming her *wh*-embeddings by attaching her *wh*-question to a sentence, except for the *how*-embeddings. The gradual progression from the proper form *how to* into the deviant *how do you*, then, suggests Uguisu's attempt once again to maintain the internal consistency of her linguistic system. I suppose that, had Uguisu remained in the United States, her now deviant *how*-embeddings would have returned through a gradual process to the proper *how to* form, just as her deviant *where*-embeddings gradually became uninverted into the proper form. Presumably, the move away from the internally consistent 'Sentence + Question' configuration is motivated this time by some propelling forces toward the maintenance of an external consistency, that is, the input that Uguisu hears.

The general conception of the second language acquisition process conveyed above is that it is a dynamic, fluid process in which the system of the learner is constantly shifting: shifting in a slow and gradual manner either toward the maintainance of an internal consistency within the structures which the learner possesses, or in the direction of an external consistency, where the learner attempts to fit the internal system into what is heard in the input. Needless to say, this conception still remains largely impressionistic and crude. For example, we lack specification of the dimensions along which the child comes to maintain the internal or external consistency of his/her system. The significance of the above observation, however, lies in the fact that it reveals both internal and external factors at work in second language acquisition, and that any serious theory of acquisition must take both factors into account. We shall return to this question in the concluding discussion to this paper.

I would now like to turn to three major aspects of Uguisu's development, which might be considered within the perspective offered above. The next section deals with the problem of prefabricated patterns, which are essentially imitations and are products of the process of external consistency. The following section deals with the development of grammatical morphemes, and a partial explanation for the order of acquisition is attempted in terms of internal and external consistency. And finally, we will deal with evidence of language transfer from Japanese, which might be considered a problem of internal consistency, that is, maintainance of consistency with her already internalized L1.

## Prefabricated patterns

The second language learner is necessarily older than the child learning a first language, and we would expect that, with advanced semantic development and yet no form with which to express such thoughts, the need to learn the various linguistic structures is especially acute. One way in which the learner might cope with this problem is by employing a strategy which 'tunes in' on regular, patterned segments of speech and uses them without knowledge of their underlying structure, but with the knowledge as to which particular situations call for what patterns. This argument is based on the consideration that a developed processing span enables memorization of longer speech segments, and segments, for example, like *this is* would be not too different from individual lexical items. I have called such forms 'prefabricated patterns'. The

distributional evidence for the existence of prefabricated patterns in Uguisu has been presented in an earlier paper (Hakuta 1974b), and will not be discussed here. Essentially, it was shown that (1) patterns using the copula including all allomorphs of *be;* (2) the pattern *do you* as used in interrogatives; and (3) the pattern *how to* as in embedded how-questions (discussed earlier) were all prefabricated in the sense that they all showed a characteristic rigidity in usage and lack of variability, as well as frequent misuse in linguistically inappropriate contexts. Furthermore, at least in the initial stages of learning, prefabricated patterns constituted a significant proportion of Uguisu's utterances (over 50%).

Unfortunately, other naturalistic studies of second language acquisition have largely overlooked the phenomenon of prefabricated patterns, although scattered evidence of their existence can be found in the literature. Butterworth (1972), for example, reports patterns such as *It's time to + Verb*, *You can + Verb*, and *I want you to + Verb*, in the speech of a 13-year old Spanish adolescent Ricardo. Adams (1974), in a diary data study of 10 Spanish speakers in an English immersion program at Culver City, reports such prefabricated patterns as *Do you got X*, and *Do you have X?*

Joseph Huang (1971) has paid more attention to the process of imitation than other studies in his analysis of the speech of a 5-year old Taiwanese boy Paul. Huan noted the extraordinary well-formedness of some of Paul's early utterances, such as *Get out of here*, *It's time to eat and drink*, and *This is + NP*, and attributed them to the process of imitation. But by the second month of observation, Paul began creating ill-formed utterances, such as *This ++ kite* (the notation ++ indicates a brief pause). Such utterances Huang attributed to the strategy of 'rule-formation'. An important point that Huang makes is that the strategy of sentence imitation did not disappear altogether when Paul began producing utterances out of his own syntactic system. He reports variability between the two strategies. Unfortunately, Huang views imitation and rule formation as two independent processes, as he makes no attempts to establish a relationship between the two.

The emphasis on rule-formation and analyses of various structural types (for example, interrogatives, negation), along with a de-emphasis on the analysis of prefabricated patterns, I believe, is due to the strong focus on language *structure* in the developmental psycholinguistic research of the 1960's. The process of imitation, generally associated with the linguist's taboo on Skinner's (1957) paradigm, has largely been ignored; if recognized, such pre-

fabricated patterns have generally been considered 'clutter' in the data.

What is the significance of prefabricated patterns? As mentioned earlier, they enable learners to express functions which they are yet unable to construct from their linguistic system, simply storing them in a sense like large lexical items. I think it is also important to note that, if learners always have to wait until they acquire the constructional rules for forming an utterance before using it, then they may run into serious motivational difficulties in learning the language, for the functions that can be expressed (especially in the initial stages of learning) would be severely limited. It might be important that the learner be able to express a wide range of functions from the beginning, and this need is met by prefabricated patterns. As the learner's system of linguistic rules develops over time, the externally consistent prefabricated patterns become assimilated into the internal structure. This process of internal consistency, it seems, is a slow and gradual process as suggested by the gradual 'extinction curve' of the pattern *how to* in Uguisu (Figure 3), as well as by Huang's report on the variability between his subject's two strategies.

It seems to me that future research in second language acquisition must bear in mind the problem of prefabricated patterns. Notice here, incidentally, that in general prefabricated patterns will reveal themselves only in longitudinal studies, as in cross-sectional studies they might simply be overlooked. To regard them as mere clutter in the data would be a mistake, for a theory of second language acquisition must be able to account for everything that the learner produces. The only justification in not accounting for them would be if we were to take so narrow a definition of language as being 'what can be assigned linguistic descriptions'. And by doing that, we end up with nothing but a collection of facts which deserve to be accounted for simply because they fit into *our* assumptions about what language is.

## Grammatical morphemes

The acquisition of grammatical morphemes in English by L1 children has been studied by Brown (1973), and the dramatic success of his method of analysis in uncovering an invariant sequence of development across children is by now well-known. Researchers in second language acquisition, ambitious to find a similar invariant sequence in their learners, have embarked in producing their own parade of studies on grammatical morphemes

(Dulay and Burt 1973, 1974a, Bailey, Madden and Krashen 1974, Gillis 1975, Hakuta 1974a, 1975).

Uguisu's order of acquisition covers a broad spectrum of grammatical morphemes, some 17 in all. I will not attempt to account for the full ordering, only an attempt at partial ordering, along with detailed analyses of some of the more revealing morphemes. Finally, I will make some comparisons of Uguisu's order with those reported in other studies.

It should be pointed out here that this order of acquisition was determined using Brown's (1973) scoring methods and the criterion of acquisition which he used, where the point of acquisition was defined as "the first speech sample of three, such that in all three the [morpheme] is supplied in at least 90% of the contexts in which it is clearly required" (Cazden 1968: 435). Thus, this is a longitudinal order of acquisition, as opposed to a difficulty ordering which is obtained in cross-sectional studies. The importance of this distinction lies in the simple fact that the two methods appear to yield rather different orders, a fact which Rosansky (1976) recently pointed out.

The morphemes scored and their acquisition order are displayed in Figure 4.

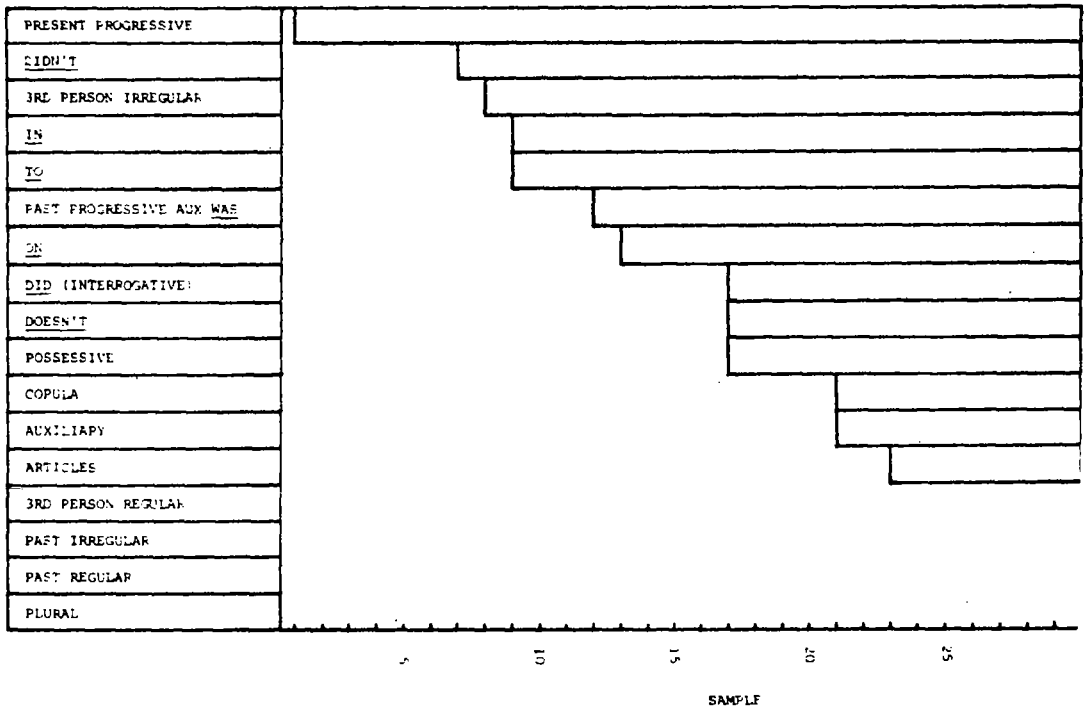


*Figure 4. Acquisition points for all grammatical morphemes scored in Uguisu.*

*Overt marking of underlying semantic relations.* Slobin's (1971) Operating Principle E states: "Underlying semantic relations should be marked overtly and clearly." In this regard, he writes: "children scan adult sentences for cues to meaning, and are aided by overt morphological markers which are regular and perceptually salient. Such markers probably play a similar role in production, helping the children keep track of where he is in the transition from thought to utterance." (p. 201). I disagree with Slobin's use of the sentence "children scan adult sentences" because it gives too strong an impression of deliberateness, but I think it would be not too great of an insult to modify Slobin's remark to something like "children are sensitive to overt morphological markers in the input, and such markers will appear early in the child's production of utterances."

What is meant by 'overt marking'? We should be clear about this, for it would be tautologous to say that what is acquired early is overtly marked. I have two possible dimensions in mind. One such is along the line of syllabic/nonsyllabic, the former being more overtly marked. For example, *-ing* is more salient than the possessive *'s*. The second dimension, which is perhaps a continuum, could be drawn along the line of what I shall call 'root changes' versus affixing. A root change would be where the content word being modulated undergoes a drastic change of form, such as *go/went, don't/didn't, have/has*, and so forth. I would argue that a root change is more salient than, say, a simple affixing of a morpheme, such as the plural *-s* or the past *-ed*, because affixes can be filtered out more easily in perception than root changes.

Notice in Figure 4 that there are certain morphemes which express the same semantic relations. One set, consisting of *didn't, did*, the auxiliary to the past progressive, and past irregular, and the past regular, expresses the past tense. A second set, *has, doesn't*, and the 3rd person regular, expresses the third person singular indicative. For just these two sets of morphemes, we are at an advantage to tease out the role of the *form* of the marking, because the function expressed remains constant.

For the past tense markings, the order of acquisition is as follows:

| | |
|---|---|
| 1. didn't | (Sample 7) |
| 2. aux past progressive | (Sample 12) |
| 3. *did* | (Sample 17) |
| 4. past irregular | (after Sample 30) |
| 5. past regular | (after Sample 30) |

It is surprising how early didn't is acquired, by Sample 7, and the

past irregular and regular forms never attain criterion. Uguisu, by Sample 17, was consistently marking the past tense in everything except for the regular and irregular forms on the main verb. I think, however, that we are doing somewhat of an injustice in scoring all past irregular verbs under that one category, for it covers a large portion of English verbs, and they must all be learned by rote. Some irregular verbs, such as *said*, *made* and *forgot*, were consistently marked from early samples. As the lexicon grows, new verbs are acquired, and their past markings must also be learned. For just the past irregular, then, I think that the acquisition points for the past tense marking on different irregular verbs would be widely spread out over time.

Notice that, except for the past regular, which comes last in our order, the forms all manifest overt marking. They are all root changes, for example, *don't/didn't*, *am-are-is/was*, *do/did*, and the past irregular. This order supports the contention of early acquisition of overtly marked semantic intentions.

How about the third person singular indicative markings? The order of acquisition is as follows:

| | |
|---|---|
| 1. *has* (3p irreg) | (Sample 8) |
| 2. *doesn't* | (Sample 17) |
| 3. 3rd person reg. | (After Sample 30) |

This order, once again, supports the contention of overt marking appearing early. *Has* and *doesn't* are both root changes, whereas the 3rd person regular is an affix.

For the forms in the analysis above, it is not the complexity of the underlying relations which matters, for they are held constant. Rather, it is the salience of the form by which such relations are marked. The more overtly marked, the earlier it is acquired. Such overtly marked forms perhaps penetrate the attention of the learner. If the learner is motivated to make his production match what is heard in the input, those are the first to be acquired, because they are salient to the learner. Salient forms make themselves easily available to the process of external consistency. Of course, I have not been able to suggest reasons as to the ordering within the overtly marked forms; I can only speculate that it is perhaps some combination of frequency and the degree of salience of the form.

*The semantic of the form.* In the above section, we considered different forms which expressed the same semantic. Now we shall contrast a pair of morphemes which share identical forms but differ along the dimension of the semantic: the

possessive and the plural. Both morphemes share the same allomorphs, /-s, -z, -iz/, and they both are attached to nouns. In terms of frequency, they differ; the plural is high in frequency, the possessive low. More importantly, Japanese has an obligatory particle for the possessive *-no*, which is postposed to the noun which is the possessor, but it does not have a morpheme which expresses plurality.

Uguisu attains criterion for the possessive by Sample 17, but the plural never attains criterion, and is the last morpheme on our order of acquisition. This, I believe, is evidence for transfer from Japanese. If frequency of the morpheme in English were important, the reverse order would have been predicted. However, it is worth noting that, even though the possessive was acquired before the plural, on the entire scale of grammatical morphemes, it is a relatively late acquisition, most likely due to its low salience.

It is of interest to compare Uguisu's data on possessives with an analysis by Cazden (1968) where she contrasted the performance of Adam, Eve and Sarah on the possessive in elliptic versus non-elliptic (noun-noun) contexts. Because the number of contexts for the possessive was low, Cazden summed the scores for all samples up to the point when they attained the 90% criterion. Cazden found that, in all three children, the morpheme in the elliptic context was supplied with far more frequency than the non-elliptic. I performed the same analysis on my data, taking the scores from Samples 1 thru 16. The results appear in Table 3, along with Cazden's data. Uguisu performed equally in both contexts, suggesting a difference between first and second language learners. Possibly, the non-elliptic form requires more processing space because it consists of two nouns, while the elliptic only takes the possessor noun. First language learners, having a more limited processing span, may omit the morpheme when they have to process two nouns for the non-elliptic form, but in the elliptic

TABLE 3

*A comparison of the possessive inflection in two linguistic contexts for Adam, Eve, Sarah (Cazden 1968) and Uguisu.*

| | With Noun | Elliptic | Total |
|---|---|---|---|
| Eve | 0.07 (9/138) | 0.69 (11/16) | 0.13 (20/154) |
| Adam | 0.16 (21/130) | 0.86 (37/43) | 0.34 (58/173) |
| Sarah | 0.06 (2/33) | 1.00 (8/8) | 0.24 (10/41) |
| Uguisu | 0.66 (104/157) | 0.68 (32/47) | 0.67 (136/204) |

form may find it possible to "fit in" the morpheme since there is only one noun to process. On the other hand, Uguisu, with a more developed processing span, may have been able to handle the loading of two nouns with as much ease as single nouns, and therefore performed equally in both contexts.

*The articles.* The development of articles in Uguisu is of particular interest because Japanese does not have obligatory linguistic devices to make the specific/non-specific distinction. At the same time, the English articles are highly salient, hence very likely to be noticed by the learner. In Figure 4, *a* and *the* were both lumped together under the category 'articles'. This was done because Brown (1973), in this study of Adam, Eve and Sarah, did not score separately for the definite and indefinite articles, and I was interested in comparing his order with Uguisu's. However, in my analysis of Uguisu's articles, I tallied the two separately, since I was present at every sampling session with Uguisu and was always aware of the context. I found it possible to identify the obligatory contexts for the respective forms of the articles in about 90% of the cases.

Uguisu's control of articles in earlier samples is difficult to assess. It seems that in many instances where articles are supplied, they are not actually segmented morphemes but rather are phonological features of verbs (for example, *look-like-a*) and prepositions (for example, *in-a*) and other words. Brown (1973) seems to have found a similar problem in studying articles, as he writes: "Before the attainment of the 90% criterion I have found that the child's use of articles cannot support any inferences about his control of semantic and grammatical rules. This is partly because seeming articles in earlier samples probably are not organized as separate morphemes at all but are rather features of the pronunciation of particular words." (p. 355).

Perhaps because articles appear with high frequency and are syllabic, hence more perceptually salient, articles tend to be retained in the learner's speech in some form, either as pronunciation features of particular words or as a schwa. Brown (personal communication) warns, however, that telling against their salience is the operation of liaison in English which causes articles to be frequently slurred and hard to tell apart, *That's a* becoming *Thassa*, and *Put the* becoming *Putta* or *Pudda*.

With the above in mind, I decided to score samples centering around Sample 20, which I impressionistically determined was about the point where articles were coming into full bloom. Samples 14-28 were scored, and I also scored Sample 9, which was

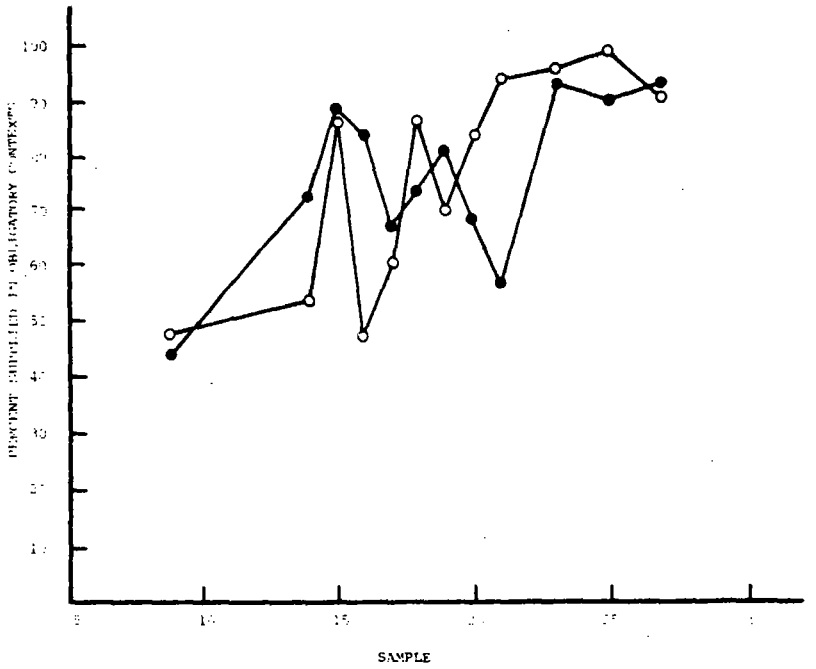


*Figure 5. The development of the definite (open circles) and indefinite (closed circles) articles in Uguisu, scored for presence in obligatory contexts.*

one of the largest samples (3 hours), to give some idea of Uguisu's performance in early samples. The results of the scoring appear in Figure 5. After a good deal of fluctuation, both *a* and *the* attain the 90% criterion by Sample 23. This analysis supports the contention (Brown 1973:351) that the articles are acquired as a system, suggesting learning along similar dimensions for the contrasting pair.

The story of the development of articles in Uguisu, however, is incomplete with a simple scoring for percent supplied in obligatory contexts. Uguisu frequently supplied the inappropriate article, as in the following examples:

Oh . . . then you're not a boy . . . oh, are you *the* girl?
It's a sun, and *a* sun can't see that man taking that apple off.
He was inside *a* hospital taking that apple off.
And I'm *the* girl too.
You draw *a* pretty.
I gonna make *a* eyelashes.
They only *a* . . . baby ducks.

It is clear that many of the errors involved violations of the specific/nonspecific distinctions, as well as violations of the

restriction that *a* can be used only with singular nouns. We would like to know, then, not just how well Uguisu performed with respect to obligatory contexts, but also the success rate that Uguisu had in striking the appropriate semantic target whenever she used either form of the articles. In other words, we would like to know how frequently the errors of commission occurred, rather than errors of omission. I decided to score for the proportion of correct usage over total usage for the respective forms of the articles. In this case, we are not considering instances of absence, but simply cases where the forms appeared, and what percent of the forms when used were appropriate. Figure 6 displays the results.

What Figure 6 suggests is that, although errors do not disappear altogether, by the time both *a* and *the* attain the 90% criterion for percent supplied in obligatory contexts, inappropriate usage also diminishes. However, it is worth noting that Uguisu performs better on *the* than on *a*, which suggests that many of the *a*'s were in actual fact, schwas. The extent to which these errors of commission occur is surprising. At some points, such as Sample 14 for *the* and Samples 9 and 20 for *a*, only a little better than 50% of the respective forms of the article used were appropriate. It seems likely that a good deal of the seeming articles used by

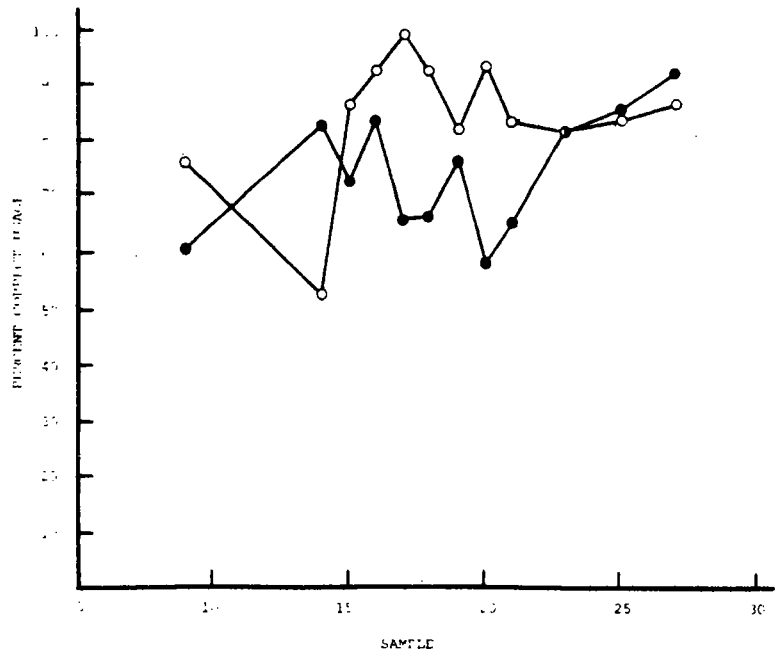


*Figure 6. Percentage of correct usage of articles when used by Uguisu. Closed circles represent* a, *and open circles represent* the.

Uguisu were simply fragments retained in her speech due to the salience and frequency of articles, and that not until much later in development did Uguisu have full control of the semantics of the articles.

This late acquisition of full control seems to be the result of that distinction not being marked in Japanese. To support this is an incidental finding by Frauenfelder (1974), who studied the acquisition of French gender by English-speaking children in a French immersion program in Toronto. English and French both have the definite and indefinite articles, but French has the additional complication of gender. Frauenfelder found that, after one year of immersion, the children still had trouble with gender, often overgeneralizing the masculine into the feminine, but that the children used, almost without exception, the correct form of the article with respect to the definite/indefinite distinction. These English-speaking children, Frauenfelder argues, were transferring along the specific/nonspecific dimension from L1 to L2, hence acquiring that distinction early in L2. Uguisu, since she had no distinction in Japanese, had to painstakingly learn the appropriate discrimination. Clearly, salience of articles in English played a role, and articles appeared in her speech quite early, but without the proper discrimination necessary for full control.

*Internal and external consistency and order of acquisition.* The process of external consistency, since it depends on the input, is perhaps very sensitive to forms which are salient, that is, overtly marked. We have seen that, for the past tense and the third person singular indicative markings, the overtly marked forms were acquired first. Also, the present progressive and the prepositions, which are overtly marked, were acquired early. Articles, though full control of the semantics was acquired late, also appeared in Uguisu's speech with a good deal of frequency, albeit often incorrectly. They all speak to the issue of the early acquisition of overtly marked forms, at least on the level of an externally consistent system. Conversely, markings which are not salient (for example, the possessive, the plural, the past regular, and the third person regular) were acquired late. Such minimal markings are perhaps more difficult to decipher in the input, hence making themselves less readily available to the process of external consistency.

Interacting with the above is the process of internal consistency. When we controlled for the form, contrasting the plural and the possessive, we found that possessives, which are marked in Japanese, were acquired before the plural. This is

despite the fact that plurals occur with higher frequency than possessives. Full control of articles, the specific/nonspecific distinction not being marked in Japanese, was acquired late despite the salience of the forms. Perhaps these markings followed some general principle of internal consistency with markings which Uguisu had in Japanese. The exception is the third person singular marking on *has*, which was acquired as early as Sample 8; Japanese verbs do not inflect for person. Certainly, its salience helped, but articles, which were acquired late, are also salient. One possibility is that articles take two forms (*a* and *the*), whereas *has* simply remains invariant, but there is no way to tell. I can offer no simple solution at this point.

The most honest statement possible is that the interaction of these two processes accounts to a fair extent for Uguisu's order of acquisition. But until we look at the order of acquisition obtained from speakers of other L1 backgrounds, we must leave ourselves open to other possible alternatives.

*Comparison with other studies.* Comparing across studies is no simple task. This is particularly true because the studies differ with respect to method of data collection, as well as with respect to scoring procedures. Studies using the Bilingual Syntax Measure (BSM) show strikingly high invariance in their difficulty ordering (Dulay and Burt 1973, 1974a, Bailey, Madden and Krashen 1974). This order, although different from the L1 order, holds not only across a variety of L1 backgrounds, but also in children (Dulay and Burt 1973, 1974a) as well as in adults (Bailey *et al.* 1974). However, it is still unclear whether that order is a simple artifact of the BSM elicitation device and its unique scoring method, or whether this order is true of second language learners in general (see Rosansky 1976).

If the latter is the case, Uguisu is certainly a unique learner, for her order of acquisition is very different from the BSM order (Spearman rho = +.20, for the 9 morphemes in common). There is at present no way to tell, but a number of morpheme studies on longitudinal data currently under way should shed light to this issue.

A study by Gillis (1975) should also be included in the discussion here. Gillis studied two Japanese children learning English in a naturalistic setting in Canada. Unfortunately, she restricted her analysis to only those morphemes which were related to verbs. There were seven such (past regular and irregular, 3rd person regular and irregular, copula, auxiliary and progressive). Since none of her morphemes attained criterion for acquisition

during her period of observation, Gillis pooled scores for the morphemes in order to obtain her difficulty ordering. For our present purpose, I have taken the liberty of performing a rank order correlation between her two subjects (Haruo and Akio). Results: Haruo X Akio = +.54; Uguisu X Haruo = -.79; Uguisu X Akio = -.19. Comparing Haruo and Akio's order to the BSM order, there are 6 morphemes in common. Results: Akio X BSM = +.14; Haruo X BSM = -.54.

The results of these comparisons are not necessarily encouraging to those who would like to see a stable order of acquisition across L2 learners of English. Of particular interest is the above comparison between the three Japanese learners. It is interesting to note that, even within the same L1 background, the order seems variable, at least for the verb-related morphemes. It is unfortunate that Gillis did not investigate the articles and plurals in her subjects, where negative transfer would be expected.

In any event, the 'morpheme controversy' in second language acquisition is likely to continue for a while.

### Language transfer

We have already seen some strong indications of language transfer in Uguisu's order of acquisition of grammatical morphemes. In this section, I will present some of the more provocative interference errors which appeared in the protocols of Uguisu. Then I will present a suggestive finding of 'structural avoidance', and this I will demonstrate by comparing Uguisu's frequency of relative clauses to that of Marta, a 5-year old Puerto Rican girl learning English.

*Interference error: 'mistake'.* When Uguisu says *you're mistaking* or *I just mistake it*, she is using the English noun, *mistake*, as a verb. This error is directly traceable to Japanese. The meaning that is captured in the English verb phrase *to make a mistake*, is expressed in Japanese by a main verb, *machigaeru*. The total usage of *mistake* which appears in Uguisu's protocols are shown in Table 4. With one anomalous exception, at least through Sample 19, Uguisu used *mistake* (or *mustake*, as she often pronunced it) as a verb. I can think of no other verbs used by Uguisu where there is a mismatch between Japanese and English, although subject to a linguist's inspection, and it seems that Uguisu erred where the grammatical categories differed between the two languages.

TABLE 4

*Exhaustive list of uses of "mistake/mustake" in Uguisu.*

| Sample | Utterance |
|---|---|
| 7 | Not there, I mustake. |
| 8 | Oh no, I mistake. |
| 9 | don't give me more because you're mustaking. |
| | No, I'm sorry. I mustake. |
| | Because I mustake. |
| | Anytime I mustake. [anytime=everytime] |
| 10 | If . . . you do like this mustake you won. [?] |
| 15 | Oh, this is mistake. |
| 16 | Because I just mistake it. |
| 19 | If you mistake, you have to cross(ed) it out. |
| | I always mustake (over there). |
| | I just mustake, and I just skipped. |
| 25 | I made a mistake. |
| 29 | I made a mistake. |

*Interference error: reflexives.* In Japanese, there is an invariant reflexive form, *jibun*, and where English uses the preposition *by*, Japanese uses the instrumental preposition (*-de*) equivalent to *with*. Table 5 lists all reflexives used by Uguisu; not an overwhelming lot, but enough to speak for my case. There are in Table 5 two possibilities of transfer. First, the fact that Japanese uses the invariant *jibun*, argues for Uguisu's use of *self*, with no pronoun, through Sample 12. Telling against this is the possibility that it may just as well be a case of simplification, and has nothing to do with her invariant Japanese form. Here we are left with an ambiguity, a problem which constantly plagues classification of errors. The second possibility, however, is a sure case. Except for the two cases in Sample 29, all forms of the reflexive in standard adult English call for the preposition *by*. Rather, starting at Sample 20, Uguisu began using the preposition *with* (instrumental), which is what Japanese would require.

*Structural avoidance.* Schachter (1974) has insightfully conducted a study which taps at a rather subtle form of language transfer. She took texts of English compositions written by adult learners of English as a second language, and analyzed the frequency of relative clauses that they contained. The learners came from two different types of L1 backgrounds: the first group consisted of native speakers of Persian and Arabic, both languages which place the head noun of relative clauses to the left of the clause, that is, the same as in English; the second group were

TABLE 5

*Exhaustive list of reflexives used by Uguisu.*

| Sample | Utterance |
|---|---|
| 11 | You have to do self, because remember I do self? |
| | I will do it self. |
| | I can do it self. |
| | Give me that, I can do it self. |
| 12 | You have to make it self, it's not hard to make. |
| 20 | He did it he-self. |
| | He did he-self. |
| | They have to do it with their-selfs. |
| | The shoes is walking with their-self. |
| 23 | Make it with your-self over here. |
| | You can write it with your-self. |
| 25 | You could drive with your-self. |
| | You can drive with your-self, couldn't you? |
| 27 | I can make toast with my-self. |
| 29 | He's scared of self. |
| | His-self because he's scared of dog. |

speakers of Chinese and Japanese, which place the head noun to the right of the relative clause, that is, different from English. Schachter's results, briefly, were that Persian and Arabic speakers used twice as many relative clauses as the Japanese and Chinese speakers.

This type of 'structural avoidance', Schachter argues, is another manifestation of language transfer, undetectable by simply looking at errors made. Although shown only in adults (and that only in written composition), it seemed to me entirely possible that the same would hold true for children in speech.

To test this possibility, I obtained permission to use transcripts from a study of Spanish speakers learning English, a study which was being conducted by Schumann, Rosansky and Cancino under Courtney Cazden at Harvard. In particular, I got the protocols of their 5-year old subject Marta. The Schumann *et al.* group had collected their data using the same method as Uguisu, and so the two children were easily comparable. As Spanish is also a language which places the head noun to the left of the relative clause, it was hypothesized that Marta would be producing more relative clauses than Ugùisu.

In attempting this analysis, I ran into a problem which plagues (and will probably continue to plague for some time) second language acquisition research: the lack of an index of development, such as Mean Length of Utterance in first language

research. Nothing tried so far works. This is a problem in this particular study because we would like to see the number of relative clauses produced at a similar stage in development for both children. In our case, of course, even simple chronological age is no predictor at all.

Grammatical morphemes are a possibility, but here again, in most cases, one runs into difficulty: Spanish has articles, Japanese does not; Spanish has plurals, Japanese does not; and so forth. I finally settled on a rather arbitrary criterion: the acquisition point of *doesn't*. I felt this to be as fair as any, because neither Japanese nor Spanish inflects the negative auxiliary for person, so it is at least language-free. However, the results to follow should be interpreted in light of the fact that this developmental milestone has been arbitrarily selected.

The Schumann *et al.* group had already determined the acquisition point for *doesn't*, and so had I for Uguisu, so I took 3 consecutive samples prior to the acquisition point (*prior* because Marta acquired it in her very last sample), and decided to compare those for the frequency of relative clauses.

Here, I ran into a second difficulty: utterances in second language learners are often difficult to determine, often conjoined on and on by *and*, and other coordinations. Utterances simply do not 'crack' apart as they do in Stage I, II and III children learning their first language. My solution was to count the number of main verbs in the transcripts, as a rough index to the size of the corpus of utterances. This measure works, I think, because verbs are rarely if ever absent in the sentences of both Uguisu and Marta. In order to hold constant the size of the corpus across all samples, I decided, prior to counting relative clauses, to use as the base *n* the number of main verbs in the smallest protocol. This, it turned out, was 374 main verbs; so each protocol was cut at the utterance involving the 374th main verb.

A count of the relative clauses for the two children appears in Table 6. The results indicate that Marta uses relative clauses with far greater frequency than Uguisu. If they are indeed at a common point in development, the implications of such 'avoidance' by Uguisu reveals an aspect of transfer which can only be illuminated by studying not just the errors made, but the distribution of different structural types.

Studies investigating language transfer in second language acquisition have generally looked for interference errors in which the source of error is directly traceable back to the L1 structure (e.g., Dulay and Burt, 1973; Selinker, Swain and Dumas, 1975). It

TABLE 6

*Frequency of relative clauses in Marta and Uguisu.*[1,2]

| | Time I | Time II | Time III |
|---|---|---|---|
| Marta | 7 | 14 | 10 |
| Uguisu | 0 | 5 | 1 |

[1] Each sample consists of 374 main-verb utterances.
[2] For Uguisu, Time I = Sample 14, Time II = Sample 15, Time III = Sample 16.

so happens that interference errors are seductive to the researcher because they fit into our present conceptions about language structure, and are predictable to some extent. However, it is becoming increasingly clear that interference errors are not the only manifestations of the process of language transfer. As has been suggested by Uguisu's data, the order in which certain structures develop may very well depend on the L1 of the learner. Another manifestation might be structural avoidance. Other possibilities remain, such as the overall rate of development as a function of the L1 background. The point of this paragraph is that we must broaden our perspective in looking into the effects that the native language has on second language acquisition. In fact, it is impossible to prove that there is *no* language transfer, for there always remains the possibility that the researcher is simply looking at the wrong place. Finding a low percentage of interference errors, or even finding a uniform order of acquisition for a restricted set of grammatical morphemes, is no license to jump to the conclusion that everything must be due to 'universal cognitive mechanisms'.

## Conclusion

Working with spontaneous speech samples is frustrating. One always feels that further analysis remains to be done. Lurking behind the researcher's mind is the constant fear that some important piece of evidence is being overlooked. This fear is real; the richness of information in a two-hour speech protocol is indescribable. Thirty such protocols are enough to keep anyone busy for several years (as they have done to me), and still unsatisfied. It is a kind of work not unlike that of an archaeologist, as opposed to a physicist.

Yet there remains a certain sense of satisfaction associated with longitudinal, naturalistic studies, especially in a primitive field

like the study of second language acquisition, where the issues are still poorly spelled out. More than anything else, one gets a sense of the incredible complexity of the problem, and given this amount of confusion, any theory which is stated in loose terms will be able to find support. And it turns out that the two existing theories are rather loose.

The 'interlanguage hypothesis' (Selinker 1971, Selinker, Swain and Dumas 1975) is one theory. The most recent statement of this view is as follows: "It is the main tenet of the Interlanguage hypothesis that second-language speech rarely conforms to what one expects native speakers of the target language to produce, and that it is not an exact translation of the native language, and that it differs from the target language in systematic ways. That is to say, it is proposed that the form of the utterances produced in the second language by a learner is *not* random." (Selinker, Swain and Dumas 1975). In sum, the hypothesis states that the system of the learner can be accounted for in its entirety neither by transfer nor by the nature of the target language standing alone, but, is rather a distinct entity consisting of the interaction of the two languages, hence 'interlanguage'. This theoretical system specifies at least three central processes, language transfer, overgeneralization of the target language rules, and simplification of the target language.

The 'creative construction hypothesis' (Dulay and Burt 1974b) is the other theory, and its essential position, as far as I can tell, is that language is not learned through habit, but rather through an active process in which the child constructs the L2 system, guided by the nature of the L2 rather than the L1.

For both the above theories, it is difficult to think of the kinds of evidence you would need to disconfirm them. Take, for example language transfer. If it turned out that we find quite a bit of transfer taking place, the 'interlanguage hypothesis' would claim it as supportive evidence. On the other hand, as I have argued earlier, there is no way to prove conclusively that there is no transfer. The creative construction hypothesis could not be hurt by evidence of language transfer since, as Dulay and Burt themselves have pointed out (in Tarone *et al.* 1974) that they are amenable to the notion of language transfer as past experience playing a significant role in new learning experiences.

Lacking any strong theoretical framework, the present study becomes what might be criticized as a sort of fishing expedition. But I did not set out on this expedition without appropriate tools. The tools employed were the modern inventions of developmental psycholinguistics. Although I have used my evidence to support

and to criticize certain studies, and to argue certain directions for future research, the evidence was presented in the most quantitative and explicit form possible such that it could be used in the future to formulate theories which address themselves to real issues. This is predicted on the belief that we are nowhere near deciphering the problem of second language acquisition, and until we have some more facts on which to base our speculations, theorizing, although intrinsically fun, may be misleading.

The findings reported here were categorized under the crude labellings of being products of internal and external consistency processes. These two processes were inferred from certain characteristics of Uguisu's data, but the problem of internal versus external factors in development is also a classic in psychology. Clara and William Stern (in Blumenthal 1970) in 1907 had insights and wisdom from which we in the 1970's might well benefit, and I quote them at great length:

> As with all mental development, the main issue concerning language acquisition can be formulated as follows: what part of the developmental process is accounted for by external factors and what part by internal factors? This question was posed ages ago concerning the origins of speech itself. And just as two extreme viewpoints evolved whereby speech was conceived as arising either through *nature* or through *convention*, so it is with the modern discussion of the appearance of speech in children.
>
> It might be assumed, of course, that there is an observable correlation between a child's speech and its environment, and that consequently the process of language acquisition by a child would be considered simply as the mechanical acceptance of external speech forms and meanings through imitation. In contrast, those who emphasized the internal contributions a child makes to its own speech looked for productions having nothing to do with imitation. They sought so-called word-inventions or early manifestations of self-produced logical activity. Both views are capable of obscuring the real situation.
>
> We believe that the proper position is a synthesis of these two opinions. In his form of speech of a child learning to speak is neither a phonograph reproducing external sounds nor a sovereign creator of language. In terms of the contents of his speech, he is neither a pure associative machine nor a sovereign constructor of concepts. Rather, his speech is based on the continuing interaction of external impressions with internal systems which usually function unconsciously; it is thus the result of a constant "convergence". The detailed investigations pertaining to the development of speech and thought should determine the relative participation of both forces and also show how they accommodate each other. (p. 86-87)

It is worth an evening of pondering over.

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