Social Influences on the Choice of a Linguistic Variant

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DURING THE YEAR 1954-1955 my wife and I were engaged in a study of child-rearing in a semi-rural New England village. In the course of the study I had occasion to record two or more interviews on Audograph discs or tapes, with each of the 24 children of our sample. Previously certain inconsistencies in the children's speech had attracted my attention, especially the variation between *-in* and *-ing* for the present participle ending. (The variation in this dialect between *-in* and *-ing* in the participle ending does not extend to words with a final -m in an unstressed syllable in standard speech. This variation is therefore probably best viewed as a case of free alternation of two allomorphs which happen to differ in respect to one phoneme, rather than as a case of phonological free variation.) Accordingly, in transcribing the discs and tapes, I decided to note the choice of these two variants, and this paper is intended to summarize and discuss this information.

To begin with, all of the 24 children, except three, used both forms to some extent at least. The three exceptions used only the *-ing* form, and since they were less loquacious than most of the other children, it is possible that a larger sample of their speech would have revealed the use of the other variant as well. This may then be regarded as a case of so-called free variation of two linguistic forms within a local speech community, and within the speech of most individual members of our sample community. In general, the choice of one or the other of the variants would not affect the denotation of acts, states, or events by the word.

"Free variation" is of course a label, not an explanation. It does not tell us where the variants came from nor why the speakers use them in differing proportions, but is rather a way of excluding such questions from the scope of immediate inquiry. Historically, I presume that one could investigate the spread of one of these variants into the territory of another through contact and migration, and this would constitute one useful sort of explanation. However, another sort of explanation is possible in terms of current factors which lead a given child in given circumstances to produce one of the variants rather than another, and it is this which I wish to discuss here.

Before discussing the determinants of selection of the variants it will be helpful to understand a little of the general background of the data. The 24 children in our sample consisted of an equal number of boys and girls, both divided into two equal age groups, ages 3-6 and 7-10. By the time the recordings were made my wife and I had been observing the children periodically for eight to ten months and most of the children were fairly well acquainted with us. Most of the children were interviewed in an office in our house, which was located in the middle of the village. Most of the children had visited our house before, some a number of times. Four younger children who had not were interviewed in their own homes. Three general types of text were obtained:

1. Protocols for all children for a verbal thematic apperception test (TAT) in which the children were asked to make up stories starting out from short sentences given by the investigator.
2. For older children only, answers to a formal questionnaire.
3. For a few of the older children, informal interviews asking them to recount their recent activities.

I shall present first some counts of variants in the TAT protocols, since this test was administered to all the children. As is shown in Table 1, a markedly greater number of girls used *-ing* more frequently, while more boys used more *-in.*

TABLE 1

NUMBER OF CHILDREN FAVORING -ING AND -IN VARIANT SUFFIXES IN TAT PROTOCOLS ACCORDING TO SEX

	-ing > -in	-ing < -in
Boys	5	7
Girls	10	2

Chi square: 2.84; 05 <P <.1 (by two-tailed test).

This suggests that in this community (and probably others where the choice exists) *-ing* is regarded as symbolizing female speakers and *-in* as symbolizing males.

Within each sex, differences in personality are associated with the proportion of frequency of *-ing* to *-in* as illustrated in Table 2.

TABLE 2

FREQUENCY OF USE OF -ING AND -IN IN TAT PROTOCOLS OF TWO BOYS

	-ing	-in
"Model" boy	38	1
"Typical" boy	10	12

Chi square: 19.67; P < .001.

The first boy was regarded by his teacher and others as a "model" boy. He did his school work well, was popular among his peers, reputed to be thoughtful and considerate. The second boy was generally regarded as a "typical" boy—physically strong, dominating, full of mischief, but disarmingly frank about his transgressions. The "model" boy used almost exclusively the *-ing* ending here, while the "typical" boy used the *-in* ending more than half the time, as shown above.

In Table 3 one may also note a slight tendency for the *-ing* variant to be associated with higher socio-economic status, although this is not statistically significant with a sample of this size. The community studied is fairly small and does not have strong class lines, vhich is prob-

TABLE 3

NUMBER OF CHILDREN FAVORING -ING AND -IN ENDINGS ACCORDING TO FAMILY STATUS

	-ing > -in	-ing < -in
Above median	8	4
Below median	7	5

Chi square (corrected): 0; P >.9.

ably why more marked results did not appear. (Most previous studies of sociological factors connected with linguistic variants have been concerned with linguistic indices of class, cast or occupational groups. Group boundaries have been regarded, implicity or explicitly, as barriers to communication analogous to political boundaries, geographical distance, etc. The emphasis in this paper is rather on variations within a face-to-face community whose members are in frequent free communication: variations between social categories of speakers and between individual speakers, and situational variations in the speech of individual speakers, as noted below.)

Besides asking *who* uses which variant and how much, we may also ask whether there are situational differences in *when* a single speaker uses these variants. One variant in the situation may be described as degree of formality: in the children's terms I would think of this as degree of similarity to a formal classroom recitation. The best child to examine for this variable is the "model" boy of Table 2 since he was interviewed in all three situations mentioned above and was obligingly talkative in each. As Table 4 shows, the frequency of choice of variants changed from an almost exclusive use of *-ing* in the TAT situation to a predominance of-tn in the informal interviews.

TABLE 4

FREQUENCY OF -ING AND -IN IN A 10-YEAR-OLD BOY'S SPEECH IN THREE SITUATIONS IN ORDER OF INCREASING INFORMALITY

	-ing	-in
TAT	38	1
Formal interview	33	35
Informal interview	24	41

Chi square: 37.07; P >.001.

Of course, these three situations should not be regarded as exhaustive of the frequency range of these variants in this boy's speech. In the interviews I myself used the *-ing* variant consistently and this probably influenced the informant's speech somewhat. Probably in casual conversation with his peers the *-in!-ing* ratio is even higher than in the informal interview.

Another measure similar in implication to the frequency of variants by type of interview would be differences in frequency between the beginning and later parts of a single interview. Especially in the TAT protocols, which are the most formal text, I noticed for a number of children that the *-ing* frequency was higher in the beginning of the interview and later dropped off, presumably as the child became more relaxed and accustomed to the situation. In only one child was the reverse trend noted, and there are reasons to believe that this particular child may have become more tense during the administration of the test.

A linguist might ask whether there is any association between the suffix variants and specific verbs. The corpus is not large enough to establish stable frequency indices for the suffixes of individual words, but there is certainly a trend for markedly "formal" verbs to have the *-ing* suffix and markedly "informal" verbs to have the *-in* suffix. The first boy in Table 2 above, for instance, used *-ing* in *criticizing,correcting, reading, visiting, interesting,* and used *-in* in *punehin, flubbin, ncimmin, chetcin, hittin.* For some common verbs, however, such as *play, go,* and *do* he used both alternatively. Probably only a few verbs are formal or informal enough in their connotations so that the same variant would always be used with them. Of course, the choice of verb vocabulary is itself related to personality and situational factors.

In brief, then, the choice between the *-ing* and the *-in* variants appear to be related to sex, class, personality (aggressive/cooperative), and mood (tense/relaxed) of the speakers (and doubtless of the person spoken to, although this was not investigated), to the formality of the conversation, and to the specific verb spoken. While these are "free variants" in the standard type of description of languages in which only grammatical facts and differences in none but "denotative" meaning are taken into account, if we widen our scope of study to include the meaning of these variants to the conversants we might call them "socially conditioned variants," or "socio-symbolic variants," on the grounds that they serve to symbolize things about the relative status of the conversants and their attitudes toward each other, rather than denoting any difference in the universe of primary discourse (the "outer world"). (Uriel Weinrich has suggested to me the term "symptomatic signs," after Karl Biihler, as an alternative for "socio-symbolic variant" which already has a basis in established usage. However, it seems to me that "symptomatic signs" might be in one sense too broad and in another too narrow: too broad in the sense that it might be interpreted to refer to "non-linguistic" features of speech such as general pitch, loudness, timbre, rate, etc., and too narrow in the sense that Biihler appears to regard the symptomatic function as, ideally, purely expressive of the speaker, while I am looking for a broader term which would cover this function but also include expression of the dyadic relationship between the conversants. This cannot simply be taken care of by adding in Buhler's "signal" function which deals with the "appeal" to the listener, since at least some aspects of the relationship do not exist primarily either in speaker or listener but rather *between* them, e.g., relative age, relative rank. See Buhler [1934], especially p. 28. Whether I should here introduce a term incorporating "symbol" is a further question which I acknowledge but do not discuss here, as it is complex and is not directly relevant to the main argument of the paper.) What are the wider implications for linguistics of such an analysis of social factors influencing choice of linguistic variants? For one thing, many linguists have recognized that "free" variation is a logically necessary stage in most of all linguistic change. (I find in checking over the literature that this statement seems to be based more on my impressions of conversations with linguists than on published statements. One clear statement of this principle, however, is to be found on p. 367 of Vogt [1954]. A more general statement applying to any type of cultural element, and by implication linguistic elements can be found in Linton [1936, p. 280].) Less widely appreciated but also recognized by some is another fact: Although the mechanisms of psychic economy are becoming

better understood in diachronic phonemics, they are not always sufficient to explain fully the progressive adaption of variant forms, and that people adopt a variant primarily not because it is easier to pronounce (which it most frequently is, but not always), or because it facilitates some important distinction in denotational meaning, but because it expresses how they feel about their relative status versus other conversants.

The clearest and most comprehensive statement of social factors in linguistic change which I have encountered is found in an article by Martin Joos (19S2) dealing with medieval sibilants. (Others have separately recognized the importance of fashion in linguistic change, especially in the spread of standard dialects, and to a lesser degree have recognized the complementary process of using distinctive linguistic features to emphasize social exclusiveness. J. O. Hertzler (1953) gives a bibliography including studies of both sorts. Joos's statement however appears to me to be unique in his recognition that the two processes combine to constitute a self-perpetuating cycle. Since Joos is noted for his rigorous definition of the scope of linguistics proper it is perhaps all the more interesting that he should throw in this "sociological" aside.) He speaks of "the phonetic drift, which was kept going in the usual way: that is, the dialects and idiolects of higher prestige were more advanced in this direction, and their speakers carried the drift further along so as to maintain the prestige-marking difference against their pursuers. The vanity factor is needed to explain why phonetic drifts tend to continue in the same direction; the 'inertia sometimes invoked is a label and not an argument.'" This protracted pursuit of an elite by an envious mass and consequent "flight" of the elite is in my opinion the most important mechanism in linguistic drift, not only in the phonetic drift which Joos discusses, but in syntactic and lexical drift as well. (Incidentally, this flight-pursuit mechanism might be regarded as an explanation of the constant rate of decay of basic "non-cultural" vocabulary postulated by Morris Swadesh's theory of glottochronology. To make it suffice one would also need to assume that all societies possess some form of elite group—if only the "ideal conformist" in some societies—and that mass envy of the elite and ambition to join them

are everywhere the same. These assumptions may seem radical and against common sense, but they are not as easy to refute as one might think. Needless to say, one would not assume that the elite is always a property or authority elite. In politically and economically undifferentiated societies, the most important criterion might be technical skill and productivity in consumer goods, admired personality traits, etc.) The study of social factors in linguistic drift is in the field of the sociology of language rather than linguistics proper. However, this study can not reach ultimate fruition without certain linguistic studies by competent linguists. I refer here to studies of individual variations in linguistic forms in small, face-to-face speech communities, and of variations in these forms in the speech of single individuals in a range of social situations. Studies of this sort constitute tasks of respectable magnitude which have, in the main, been neglected. (The classic study in this field is Gauchat [1905]. Other references are cited by von Wartburg [1946, p. 33]. Modern techniques, of course, open entirely new perspectives for research.) A student of social factors in the choice of linguistic variants would wish to know for a fairly large stratified sample of a speech community how often members of a given sub-group used a sizable sample of series of socially significant variants, and for at least some of the sub-groups one would want to know how these frequencies of choice of variants changed under different situations and in the presence of conversants of different social status and personal relationships. A linguist as such would not wish to analyze these social factors in great detail. But it would be well within the scope of linguistics to identify individual informants in a unitary speech community by name or code number and group them according to their similarity or dissimilarity in the use of variants in some standard situation, say, in conversation with the linguist. The psychologist and sociologist could then take these groups and see what sense they made in their terms. In practice, of course, such a rigorous separation between linguistics and the more general social sciences is not required since linguists and other laymen are presumably capable of making a number of distinctions of considerable sociological interest, such as male versus female, etc.

A word about the relation of the proposed study to dialectology is appropriate here. It has generally been the aim of dialectologists to describe linguistic variations between groups which are separated by some communications barrier, especially geography or social class. What I am advocating here is the study of linguistic variations within small groups where there is free and relatively intense communication, so that as far as possible the lack of contact between speakers is not a reason for failure to use the same forms. Of course in a large society such as ours, small closed groups are rare, and some of the variation among the individuals of any group picked for study will be due to the fact that they have different contacts outside the group. But this empirical fact does not reduce the importance of studying variation within the face-to-face community, although it suggests that the best place to study such variation would be on a remote Pacific atoll with a small, long-established population. What I am proposing might be called comparative idiolectology rather than dialectology. Ideally, a thorough description of a single dialect would be based on the study of a sizable sample of the idiolects in a local speech community, in the same way that a through description of a language would be based on the study of a sizable sample of its dialects. In comparative idiolectology one might, as a device of field work, still concentrate on a single informant, but one would want to follow him around with a portable recording machine and note changes in his speech in different settings and situations and with different conversants. Moreover, since phenomenologically language is as much listening as speaking one would be led to analyze what was said comprehensibly to him by others as well as what he said himself.

The untrained listener will not, of course, generally be able to reproduce or identify the differences in the speech of others whom he encounters, unless he is an accomplished mimic. But he does react to these differences by making interpretations about the social situation on the basis of them and will be able to tell when a speaker is talking like a woman, like an upper class person, like a relaxed person, etc., even though he cannot specify all the variant forms on which he bases his judgment. (The "tape experiment" described by Putnam and O'Hern [1955] investigates language and social status in this manner, although the speakers were not members of a single face-to-face community, so the complication of barriers to communication is introduced.) (This is not to deny the presence or importance of other "non-linguistic" features of speech as well as things entirely unconnected with speech such as dress, physical appearance, gestures, etc., which also serve as cues for judgments of the conversational situation.) In analyzing socio-symbolic variants there will obviously be a certain amount of association between variant series. In many of the series at least one variant could be distinguished as "formal," and another as "informal." But it is a question for empirical investigation whether this distinction applies to all variant series, and, if so, with how much force. I have suggested above a number of factors which influence the *-in j-ing* distinction. Conceivably they all bear on formality, that is, compliance, tenseness, femaleness, and high class all make for formal behavior. But even if this is true for these factors in American culture, are they a unitary complex in all cultures, and may there not be other social factors affecting socio-symbolic variants which are independent of the formality complex? Are variants associated with being female always associated as well with formality ? In three languages with which I am acquainted, English, Japanese, and Ponapean, I can think of a number of instances where this link is found, but there also appear to be exceptions. In Ponapean, for instance, a minority of women have an unusual allophone for the *r* phoneme, but this seems to have no relation to the degree of formality. Lisping in English is regarded as feminine, but would indicate little about degree of formality.

Even where the same factor determines the choice of alternants in several series of variants, the breaking point for each series will probably be different. For instance, in the TAT texts discussed above, three of the children used the pronunciation *ty* for the indefinite article *a*. This pronunciation can be regarded as formal to the point of being artificial and is much more restricted for speakers in this community than the *-ing* variant of the present participle ending, yet the direction of social symbolism is the same, though not the intensity. In other words, *ty* in itself is more a sign of formality

than -*ing* though both are signs of formality. The "formality" index of a given text would be determined by the variant chosen in several series of socio-symbolic variants, each of which would have a different socio-symbolic level with respect to formality. Presumably these series could be ordered in terms of increasingly greater thresholds of formality required to bring about the shift from the informal to the formal form.

I have been stressing here the synchronic implications of socio-symbolic variants. The diachronic implications are at least equally interesting. Obviously the threshold for a given variant does not *necessarily* remain the same, generation after generation. If a particular variant has for whatever reason greater prestige, it will gradually be adopted in more situations by more people: its threshold will be lowered. But as its threshold is lowered and approaches universality in the speech community, its socio-symbolic load is reduced and eventually vanishes. One could hardly convey much of an air of informality, for example, by saying a for the indefinite article, though saying *ey* would be quite stilted. But presumably new series of variants keep arising to replace those which achieve uniformity in this way.

Now what is meant by "variants of greater prestige" ? One could determine which of a pair of variants had the greater prestige by noting which tended to "spread" when two conversants who in other situations differed in their choice came together. But the grounds of prestige clearly vary according to individuals and societies. A variant which one man uses because he wants to seem dignified another man would reject because he did not want to seem stiff. Societies likewise have characteristic average value preferences. Using the variable of formality, it is quite possible that one society would show a tendency, at least in some situations, to show a preference for adoption of formal forms of speech, and another in analogous situations show a preference for informal forms. These preferences could in turn be related by persons so inclined to social structure. One would end up with a statement not simply of the direction of linguistic drift, but what this drift meant psychologically and what social changes might check it. It would be very interesting, for instance, to find and examine cognate variants from some related societies with differing descent practices, and see whether the current drift is in the direction of feminization or masculinization. Such data would not only illuminate the mechanism of linguistic drift, but would provide students of social structure with extremely valuable indices of the distribution of envy and cross-segmental identification in the communities speaking the language studied.

REFERENCE NOTE

For related references on variation and social perception of speech, see the article by Gumperz in Part VII.

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